The

THEORY OF PRICES

VOLUME II

The

THEORY OF PRICES

A Re-Examination
of the
Central Problems of Monetary Theory

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VOLUME II

TO MY MOTHER AND FATHER

THE Cathedral is the synthesis of the land. . . . All our France is in our Cathedrals, just as all Greece was in the Parthenon. . . . I must pay tribute to these stones, . . . stones so tenderly assembled into masterpieces by devout and learned artisans. . . . What a reserve of strength and glory the modern world could find in it! I want to teach everyone to love this spacious art, I want to help to save all of it that remains intact, and to preserve for our children the great lesson of the past which the present misunderstands. But I cannot say it all. Go and see for yourselves. And above all look at the cathedrals submissively, simply. Learn humility and application.

—Auguste Rodin, *Les Cathédrales de France*

AND so we see that Peri, Caccini, and their colleagues actually initiated very little. . . . Their achievement was almost entirely negative, consisting in the neglect of traditional resources rather than in the invention of new ones; and the factitious appearance of novelty that their work presents is less the consequence of what they created than of what they destroyed.

—Cecil Gray, *The History of Music*

EVEN the physical sciences made but little progress in their youth: partly because they had no adequate apprehension of the vastness of the area which lay beyond their knowledge. But by patience and perseverance each generation of workers has corrected, and brought certainty into, doctrines which had previously been faulty and uncertain . . . The experience of economics during the six or seven generations in which it has been studied seriously, has been similar, though cast in a smaller mould. . . . The combined constructive efforts of students in the chief countries of the western world have made the area of economic certainties fully twice as large as it was in say, 1860. . . . Those matters as to which there is no longer uncertainty are but little discussed; while conflicts of opinion are prominent over the widening area of uncertainties. The quiet agreements do not attract the attention of hasty critics; the turmoil of conflicts does.

—Alfred Marshall, "A Note on Economic Study"
(*Industry and Trade*, 673 f.)

Preface

IN THE first volume of this work, I set myself the task of welding into a single, unified organon the disparate, and often apparently contradictory, analytical approaches to certain "central problems of monetary theory." In the present volume, I have set myself the task of merging the organon so constructed into a still more comprehensive organon. The purpose of this more comprehensive formulation is to integrate "monetary theory," in the narrower sense of the term, with the "general" Theory of Value. This volume, moreover, like the first volume, has a further purpose: namely, that of providing a documented illustration of the processes by which the resultant body of analysis has come to be what it is.

By some readers of the first volume, the combination of this further purpose with the central constructive purpose of the work was felt to have produced a literary form which was most "strange." But the "strangeness" thus attributed to my exposition is surely no greater than the strangeness of the academic standards which are nowadays not only implicitly followed but also formally advocated. Is it not remarkable that "exactitude," "solidity," and "exhaustive scholarship" should be regarded not as virtues, but as vices which can be shown to be such by the simple expedient of prefixing derogatory epithets before "exactitude" and "solidity," and by dismissing "exhaustive scholarship" as proper only to a "bygone age"?[1] I, on the contrary, rest my case on the proposition that if the qualities of "exactitude," "solidity," and "exhaustive scholarship" are indeed characteristic *only* of a "bygone age," that fact constitutes a condemnation of our own age and a commentary on our current needs.

I am aware that, in attempting to combine the work of

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[1] Cf. the review of Volume One in the *Economic Journal* for September, 1939, pp. 495 f.

construction with the work of historical re-exploration, I
have made the going for the reader harder than it would
have been if I had been content to follow a more con-
ventional style of exposition—if for no other reason, be-
cause of the sheer physical bulk imposed upon the work
by the method I have deliberately adopted. But I can
only repeat what I said in the preface to the first volume:
namely, that "the physical compass of a work is not a
criterion for judging its usefulness; it is something im-
posed by the nature of the task which the author sets
himself." To criticize Wagner's *Ring*, as the anti-Wagne-
rians did, because it is not of the physical dimensions or
the musical texture of *Carmen*, or to criticize the frescoes
in the Sistine Chapel because their figures are not of the
dimensions of the figures in a Dutch interior, would be
as absurd as to reverse the criticism on analogous grounds.
I venture, therefore, to point the moral with a parable,
the basis of which has been provided by Professor Gunnar
Myrdal:

"In the older countries," he writes, "social policy has been growing
as some of the old cathedrals grew: chapels and towers were added in
different periods and in different styles, walls were moved, windows
opened, and the general plan, if there ever was one, was lost for long
periods. We are now constantly searching for means of rationalizing
and coordinating the historical outgrowth into some sort of integrated
system." [2]

Surely the symbol of the construction of a cathedral
is even more applicable to the way in which an "integrated
system" of *analysis* may grow out of the labor of successive
generations. From the "devout and learned artisans" who
"so tenderly assembled" their stones into the masterpieces
that are the Gothic cathedrals, we should learn that the
most enduring contributions have most often been made
by those who worked slowly and patiently, and not by
those who, in Goethe's words, "have taken up a notion
that they must and will erect a tower, and who yet expend
on the foundation no more material and labor than would be
sufficient for a hut." From these "devout and learned

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[2] G. Myrdal, *Population*, p. viii.

artisans," who often used the *stones* of Roman monuments to build structures which differed completely from the Roman monuments themselves, we may learn that to build monuments to Tradition we need not, and indeed should not, imitate the external forms of another age, simply out of fear that a new structure, consciously built to serve the needs of our own age, may be dismissed as "strange."

The parable extends, indeed, even to specific details. The builders of the cathedrals covered the façades with lines of the Prophets and Galleries of the Kings—sculptured tributes, all of them, to those who had built that faith without which the cathedrals themselves could not have come into existence. But in thus paying tribute to the great figures of the Past, the builders of the cathedrals showed also, by their sculptured choirs of angels and their sculptured visions of the Kingdom of Heaven, that they were mindful of the teaching of the ancient sage who admonished his fellows to "seek out the wisdom of the ancients" precisely in order that they might "be occupied in *prophecies.*" Nor, in their concern with Past and Future, did they forget the "practical" needs of the Present. On the contrary, their architectural plan insisted, from first to last, upon devices that would make possible both Strength and Light. And not the least miraculous aspect of their achievement was that, in all their concern with detail, the finest examples of Gothic cathedral architecture stand as monuments to a single plan, unified not only architecturally but also symbolically: a plan dominated by the ideal of service to the Truth, as it is given us to see the truth.

What would be said of those who, viewing a Gothic cathedral for the first time, disapproved of it solely because it did not conform to previous architectural patterns; who, "distracted" by the intricacy and profusion of the sculptures on the façade, would fail to appreciate the symmetry of its broad architectural design; who, ignorant of the principles of architectural construction, would regard flying buttresses as an unnecessary obstacle to a view of the whole? The least that could be done for such observers, surely, would be to give them advice on How to View a Cathedral. They would be advised, first, to be

content with viewing the broad sweep of the whole. For some this would be sufficient; but for others, to miss the sculptured detail would be to miss half of the achievement. Such observers, therefore, would be encouraged to study the sculpture at close range; and if they objected that such close-range observation was made inconvenient by the *location* of the individual figures, they would be reminded that to have located these figures differently would have prevented the broad architectural plan from appearing. And the select few for whom it seems important not only to admire the final result, but also to understand how this result was obtained, may then be invited to study at still closer range the structural logic of the Gothic arch and the flying buttress.

So (in a spirit of humility much greater than might seem to be implied by the grandeur of the metaphor), I invite my readers to read this book. To obtain a broad over-all view, the reader is urged to confine his first reading to the material in large type; he will then discover that he has to deal with the equivalent of a book of less than one hundred fifty pages. Some readers will then go on to the fine print sections, the constructive relation of which to the general architectural design may be tested by reading such sections as those devoted in Chapter Seven (pp. 368 ff.) to The Meaning of Period Analysis, and to the description, in Chapter Nine (pp. 478 ff.), of a three-dimensional model of a Moving System of Economic Quantities. And the reader interested in the more detailed problems of construction and doctrinal evolution will go on to the material in the footnotes.

To some readers of Volume One, on the other hand, its principal element of "strangeness" was the amount of attention devoted to the writings of Mr. J. M. Keynes. But I must insist that what a future generation may come to refer to as "the Keynesian episode" is itself "strange" to the point of uniqueness in the history of economics. In full awareness of the danger of magnifying the importance of the "episodes" of one's own generation, I submit the following considerations in support of this contention:

1. The assaults on the received corpus of economic theory by writers such as Comte, Schmoller, Veblen and their disciples were assaults by *"outsiders"*; they were assaults on economic *theory* as a *method of investigation.* In both the *Treatise* and the *General Theory,* on the other hand, Mr. Keynes's *Dunciad* (as Professor Hicks has called it) has been directed, not against economic theory as such, but against certain of its specific weapons and conclusions. The result has been the inauguration of a struggle which is *internecine*; and this has meant the need for a type of defense and counter-attack for which parallels must be sought elsewhere than in the struggles inaugurated by the writers I have mentioned.

2. None of the previous internecine struggles within economic *theory* can compare in scope or consequences with the struggle inaugurated by Mr. Keynes's attack. Jevons, for example, launched his attack on a much narrower front than the front over which Mr. Keynes has launched his; hence Marshall could confine his defense to his celebrated Note on *Ricardo's Theory of Value.* Moreover, Jevons's heterodoxy (itself of the utmost mildness in comparison with the Keynesian heterodoxy) had only modest consequences because neither Jevons nor his followers had the immediate and overwhelming success in academic circles that Mr. Keynes has had. The same thing must be said of Marx, whose followers among professional teachers of economic theory have to this day remained relatively few in number.

In contrast, the unprecedented success of Mr. Keynes in converting professional economists to what he is proud to regard as his heresy has been to create an example of the "noxious influence of authority" compared with which the "noxious influence" attributed by Jevons to the authority of Mill, and by later writers to the authority of Marshall, is as nothing. Within four years of the publication of Keynes's *General Theory,* its disciples have become so convinced of its plenary inspiration that they are prepared to characterize its position, without qualification, as the "modern" position, and to insist confidently that the attainment of this "modern" position has introduced an "unprecedented

rate of obsolescence in economic theory."[3] The supporters of Mr. Keynes have insisted upon the authoritative character of his versions of "orthodox" doctrine with an assurance for which it would be hard to find parallels in the earlier controversies, even if one could point to examples of misrepresentation of "orthodox" doctrine as flagrant as those of which Mr. Keynes can be convicted.[4] Indeed, if one needs further proof of the "strangeness" of the Keynesian episode, one need ask only at what other time, since Adam Smith, a position avowedly presented as revolutionary and heterodox has become for so large a number of professional economic theorists a new ("modern") orthodoxy in so short a period.

Confronted by so "strange" and unprecedented a situation, what should have been the response of the non-Keynesians? To attempt to draw around the Keynesian "heresy" a *cordon sanitaire,* in the form of a wilful refusal to consider the details of Mr. Keynes's charges and his alternative analytical proposals, would have been scientifically despicable. Worse than that, like all *cordons sanitaires* in the field of ideas, it would have been incredibly stupid. When a work attains the degree of influence that the *General Theory* has attained within five short years, it would be not only intellectually contemptible, but also utterly blind to suggest that the best way to treat ideas distasteful to us is to allow them to sink into an unknown grave. Even if it were clear (as it is certainly not) that an early grave stands ready to receive the ideas of the *General Theory,* the grave will not be "unknown": on the contrary, it will be marked forever in the history of economics by the kind of monument deserved by those whose ideas, however one may disagree with them, have had the rare virtue of being capable of stirring the minds of men.

I must insist, therefore, that some of us, at least, were

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[3] See A. P. Lerner, in the *Canadian Journal of Economics and Political Science,* VI (1940), 575 ff.

[4] Cf., for example, Mr. R. F. Harrod, in the *Political Quarterly,* VII (1936), 294: "[Mr. Keynes's] knowledge of the development of economic doctrine is far-reaching; he is well acquainted with the ground occupied by his adversaries, not merely with the form which their arguments usually take, but with the foundations on which they rest."

morally bound to take an attitude toward the "Keynesian episode" which is the very antithesis of the attitude of the dear lady who protested, in a letter to the newspapers, that too much "publicity" was being given to the utterances and exploits of Adolf Hitler, and that if only we would "ignore his empty threats," he would "sink back where he belongs—into oblivion." I am prepared, moreover, to defend the thesis that polemics are justified whenever they are an integral part of a broad attempt at further construction and reconstruction. I am also prepared to question the implication that there is something altogether "strange" in the combination of polemics and construction in a single work. "Every one with a scientific instinct," wrote H. D. Macleod, "can at once perceive that Adam Smith's work is pervaded with a combative air; that every part of it is evidently written *at* something preceding, and that it is intended to overthrow a prior system." [5] And it is only the shortness of our memories that has made us forget that Ricardo's *Principles* was criticized by some of its contemporaries on the ground that its partaking "somewhat of the nature of a running comment upon the writings of preceding authors" prevented it from affording "a clear and well arranged view of the science"—such as could be obtained, for example, from James Mill's avowed "schoolbook," *The Elements of Political Economy*! [6]

I must repeat, therefore, that the criterion for judging the form of a given work must be its adequacy for the purposes which it sets itself, and for the needs of its time, and not its differences from the form of earlier works. And I believe that, so viewed, the present work may fairly claim some degree of the "*architettura interna ordinatissima*" which a most generous Italian critic, with all recognition of the difficulty of the work, was gracious enough to accord to the first volume. [7] Thus, it was logical that the first volume, which was concerned principally with problems of "monetary theory" in the narrower sense of the

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[5] Macleod, *History of Economics*, 35.

[6] See the review of Mill's *Elements* in the *Westminster Review*, II (1824), 291.

[7] Luigi Einaudi, in the *Rivista di storia economica*, IV (1939).

term, should also have been concerned primarily with Keynes's *Treatise,* rather than the *General Theory.* For it was the *Treatise* that launched the more violent attack upon both the received frameworks for dealing with the problems of "monetary theory" and the received solutions of those problems; and the *Treatise* covered a vastly wider range of issues within "monetary theory" than is covered by the *General Theory.*

Similarly, it is logical that this second volume, which is concerned with the integration of "monetary theory," in the narrower sense, with the so-called "general" Theory of Value, should be concerned primarily with the *General Theory* rather than the *Treatise;* for whereas the *Treatise* did not even so much as raise the problem in formal terms, the *General Theory* bases its principal claim to a "revolutionary" character precisely on its concern with this problem of integration and synthesis. But the consistency of this division of the material with the requirements of an over-all unity of treatment may be tested by the number of references in this volume to the findings of the first volume with respect to specific problems of "monetary theory," in the narrower sense of the term, as well as to the *Treatise;* and it may be tested further by the number of references in the Index to the first volume (pp. 614 and 619) to "Value theory and monetary theory" and to the *General Theory.*

In neither case, moreover, have I allowed Mr. Keynes's choice of the battle terrain to be the decisive determinant of either the design or the scope of this work. To the kindly critic of Volume One who remarked, somewhat impishly, that in some places my effort to direct the argument toward Keynes's *Treatise* "appears forced—as, for example, where Keynes is taxed for having failed to say anything upon a theme concerning which Marget would like to write a chapter," I must therefore reply by pointing out that very considerable "chapters" in the *present* volume are concerned with matters on which (unhappily) Mr. Keynes has "failed to say anything." [8] For the fact that

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   [8] The quotation is from the review of Volume One in the *Journal of Political Economy,* XLVI (1938), 873.

Mr. Keynes *has* "failed to say anything" on these matters is precisely what invites us to attempt to fill the gap: partly, to be sure, as defense-preparation against the next assault, but also, and primarily, because the filling of "gaps" is precisely the task of those whose very respect for the work of the earlier prophets who laid the foundations for our own work demands that we build *further* upon the foundations they laid.

On the other hand, wherever it appears that Mr. Keynes has been among the builders of the foundations—as he most certainly has been in a large number of cases, whether as prophet or as the Adversary whose challenge has forced a constructive response—I have regarded it as strictly in accordance with the plan of this work that he should be given his unquestioned due. I have no illusions that my efforts in this direction will satisfy those for whom the only peace which is possible is a *pax Keynesiana*—any more than the "true" Jevonians were satisfied with the place accorded to Jevons in the Marshallian synthesis. But I continue to cherish the hope that, when the tale will finally have been told, it will be seen that at least as much was done to assure the gratitude of later generations of economists to Mr. Keynes by those of us who have tried to separate the dross from the gold in his writings, as has been done by those who have either turned their backs upon some of his most pregnant suggestions, or have accepted certain of his pronouncements in the spirit of those imitators of Byron of whom it was said that they imitated their model in nothing but his limp.

In one final respect the principles of construction under- lying this work find a parallel in the history of architecture. It was rarely the case that the plan laid down for the cathe- drals at the very outset was adhered to down to the last detail over the long period of years which the cathedrals required for their erection. On the contrary, even when the broad plan remained essentially unchanged, it happened very often (in Professor Myrdal's words) that "walls were moved" and "windows opened": this was done, indeed, whenever it was felt that by moving walls and opening windows it was possible to add more Strength and Light,

or whenever it was felt that the external and internal symmetry of the final structure would be improved by the change of plan. To such a change of plan is to be attributed the deferment, to a later publication, of some of the material announced for the present volume in the preface to Volume One. Specifically, I now present these two volumes on *The Theory of Prices* as the central unit in a structure which, unified by the broad purpose of analyzing the effects of Money on the *functioning of the economic system,* will nevertheless have two further wings, one to be entitled *Money and Production,* and the other to be entitled *Money and Interest.*

My concern with the latter problem, though it has as yet resulted in little formal publication, has been more or less continuous over the last eighteen years; it thus antedates, so far as one can discover from his published writings, Mr. Keynes's concern with the problem.[9] I did, indeed, experiment with the possibility of including much of the material on this subject in the present volume; and this fact accounts for some of the delay in completing the volume. The results of these experiments, however, were such as to convince me that the requirements of both symmetry and solidity would be better served by deferring most of this material to a separate publication, and that I should have to be content, in these two volumes, with only very general indications of the way in which this later material will be related to the material thus far presented.[10]

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[9] The principal documents which I have prepared on the subject of *Money and Interest* are three: (1) *The Loan Fund: A Pecuniary Approach to the Problem of the Determination of the Rate of Interest* (Ph. D. thesis, Harvard, 1927); (2) *Four Lectures on the Natural Rate of Interest* (delivered at the London School of Economics, April and May, 1933); (3) *The Present State of Interest Theory and Mr. Keynes* (paper presented at the meetings of the American Economic Association, December, 1937). The last item indicated has been mimeographed; the other two items remain in manuscript, although publication of the first item was arranged for more than ten years ago. None has been published, therefore; although a number of writers have done me the honor of referring, in their own publications, to one or another of the documents mentioned. The only things I have formally published which bear directly on the question of *Money and Interest* are in the way of reviews or review-articles: such as the article, "Irving Fishers Theorie des Zinses," *Zeitschrift für Nationalökonomie,* II (1931).

[10] See the Index, under Interest.

The same thing must be said of the material on *Money and Production.* To those who have seen, in the choice of the title *The Theory of Prices* for the present work, evidence of a lack of concern with the effect of monetary expansion and contraction upon the level of Output and Employment as a whole, I must point out that the analytical structure presented in these two volumes is not only organically related to the problem of the effect of Money on the level of Output and Employment as a whole, but is an indispensable first step toward a treatment of the latter problem which would go beyond the type of analytical nihilism represented by a mere stressing of "the brute fact that prices do actually rise in booms and fall in slumps," and beyond loose banalities such as that "the effect of a change in the flow of money payments is predominantly on the volume of goods sold, and not on prices." [11]

Indeed, so far from apologizing for the deferment of the material on *Money and Production* to a later publication in which an attempt will be made to treat the problem with all the care that its complexity deserves, I am prepared to warn the reader of the possibility that I may find it desirable to construct a further corridor between these two volumes and the volume tentatively entitled *Money and Production,* in the form of a monograph on *The Generation and Utilization of Money Income.* Readers of these two volumes should not be in doubt as to the nature of the positive solution of this problem which is sponsored in these volumes.[12] Nor should they be in doubt as to the rôle I am prepared to assign to elements such as Saving and Investment and Liquidity Preference in the solution of the problem.[13] The "Multiplier," indeed, is the only type of

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[11] Cf. the references to Messrs. Harrod and Kaldor, respectively, given below, p. 545, n. 48, and p. 344, n. 67; and see the Index, under Output, effect of money upon.

[12] See the Index to the present volume, under Income, generation and utilization of.

[13] See the Index, under Investment and Saving, and under Liquidity Preference. It is my intention to distribute whatever remaining material I wish to present on these topics between the projected work on *Money and Interest,* on the one hand, and that on *The Generation and Utilization of Money Income,* on the other.

analytical device brought into recent discussion by the Keynesian influence with which, in the present work, I have not attempted to deal in detail; and even here the reader of these two volumes should have no difficulty in surmising the nature of the position I should adopt with respect to that concept.[14]   I cherish no illusions as to the reaction, to this revision of the general plan, of those to whom a concept such as the Multiplier represents, from the standpoint of precision and power, the superseding last word in monetary and business-cycle theory; but again I prefer to leave judgment on this matter until I am ready to present my own position on the issues involved, in a setting which satisfies my own sense of adequacy and proportion.

As I announced in the preface to Volume One, it is my intention to follow the present volume by a textbook, in which virtually no specific references to the efforts of other writers (including Mr. Keynes) will be made.   For whereas the present work has been written (in the words of a generous and sympathetic reviewer of the first volume) for "those who have read much and meditated long on monetary problems and intend to continue such reading and meditation," the textbook to follow will be directed toward a different audience.[15]

The problem of adequate acknowledgment to those who have helped me in one way or another in the preparation of the present volume is, if anything, more difficult than that which I experienced in connection with Volume One. I cannot, for example, even if I knew how to, acknowledge adequately the gratitude I feel toward all those scholars who, in correspondence and by word of mouth, have given me the kind of encouraging comment on the first volume which means so much.   In addition, however, to the specific acknowledgments made in the preface to Volume One, I should like to express my particular gratitude to George J. Stigler, John K. Langum, Alexander L. Hart, and Manuel Gottlieb for their kindness in consenting to read parts of

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[14] See the Index to the present volume, under Multiplier; but see especially what is said below, pp. 471 ff., 476 f.

[15] The quotation is from the review of Volume One by Gustavo Del Vecchio in the *Giornale degli economisti* for October, 1938.

the manuscript of this volume—for the substance and form of which, needless to say, they bear no degree of guilty responsibility whatever.

Of the publishers of works cited in this volume, as in Volume I, who have given me permission to quote from works published by them, I wish to mention particularly Harcourt, Brace and Company, publishers of Mr. Keynes's works in this country; Farrar and Rinehart, Inc., publishers of E. Lindahl's *Studies in the Theory of Money and Capital*; and the Macmillan Company, publishers of Irving Fisher's *The Purchasing Power of Money*.

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PART ONE

MONETARY THEORY AND VALUE THEORY IN ECONOMIC LITERATURE

CHAPTER ONE

Monetary Theory and Value Theory in Earlier Economic Literature

I

THE CHALLENGE OF KEYNES's *General Theory*

THE CHAPTER of Keynes's *General Theory of Employment, Interest, and Money* which purports to summarize Mr. Keynes's own results on the subject of the Theory of Prices begins with an inclusive condemnation of economists in general for having failed to tie up in a satisfactory manner the Theory of Money and Prices, on the one hand, and the Theory of Value, on the other. "So long," writes Mr. Keynes, "as economists are concerned with what is called the Theory of Value, they have been accustomed to teach that prices are governed by the conditions of supply and demand." "But," he goes on to say, "when they pass in Volume II, or more often in a separate treatise, to the Theory of Money and Prices, we hear no more of these homely but intelligible concepts." [1]

Mr. Keynes confesses, to be sure, to a personal guilt in this matter in the past.[2] He believes, however, that in so doing he was doing merely what economists of the "traditional" stamp had always done. "We have all of us become used to finding ourselves sometimes on the one side of the moon and sometimes on the other, without knowing what

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[1] *General Theory*, 292. It will be observed, from the rest of the passage on the page indicated, that, according to Mr. Keynes, it is this characteristic of received doctrine on the subject of the Theory of Money and Prices which has brought it about that in the "more sophisticated" discussions "we are lost in a haze where nothing is clear and everything is possible." Cf. Volume I, p. 1, of the present work.

[2] See, for example, the *General Theory*, page vi: "When I began to write my *Treatise on Money* I was still moving along the traditional lines of regarding the influence of money as something so to speak separate from the general theory of supply and demand."

route or journey connects them, related, apparently, after the fashion of our waking and our dreaming lives." [3]   "One of the objects" of the argument of the *General Theory*, therefore, "has been to escape from this double life and to bring the theory of prices as a whole back to close contact with the theory of value," thus destroying the "false division" of economics "between the Theory of Value and Distribution on the one hand and the Theory of Money on the other hand."

It is clear that Mr. Keynes has issued a double challenge to those whom he identifies, without further qualification, as "economists."   In the first place, they are challenged by Mr. Keynes, as they have been challenged by others of our generation, to disprove the suggestion that economists in general have in fact been guilty of allowing a serious "hiatus" to exist between the "general theory of value," on the one hand, and the theory of the "value of money," on the other.[4]   In the second place, Mr. Keynes regards the specific results which he believes he has obtained through his insistence upon bringing "the theory of prices as a whole back to close contact with the theory of value" as being among the most important results of the argument of the *General Theory*; and he has been supported in his estimate by commentators on that argument.[5]   This means, ob-

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[3] *General Theory*, 292.

[4] For a particularly emphatic statement of this charge by a writer of our own generation other than Mr. Keynes, see B. M. Anderson, Jr., *The Value of Money* (1917), 46 ff.; (cf. also the summary on p. xiv of the same work). As a passing commentary on the suggestion that intensive discussion of the problem of the relations between the theory of "The Value of Money and the General Theory of Value" is a development of only the last few years, it may be observed that all of Part One of Anderson's book, constituting about a fifth of the whole, was devoted to the topic indicated. For further examples of challenges, by contemporary authors writing prior to the publication of Keynes's *General Theory*, in terms similar to those indicated in the text, see A. Aftalion, *Monnaie, Prix et Change* (1927), 164; G. Myrdal, "Der Gleichgewichtsbegriff als Instrument der geldtheoretischen Analyse," in *Beiträge zur Geldtheorie*, edited by F. A. Hayek, pp. 371 ff., 376 (10 ff., 18, of the English translation published in 1939 under the title *Monetary Equilibrium*); and the further references given below, p. 52, n. 1. On developments *since* the publication of the *General Theory*, see what is said below, p. 9, n. 12.

[5] For an example of the suggestion, by a commentator on the *General Theory*, that particular importance attaches to the specific aspect of the work which is here under discussion, see J. R. Hicks, "Mr. Keynes'

viously, that these specific results must be subjected to a close examination by anyone who would evaluate the Theory of Prices presented in the *General Theory* in the light of received doctrine on the subject.

It is proposed here to meet both of these challenges. This chapter, however, together with the two which follow, is designed specifically to test the validity of Mr. Keynes's generalizations with respect to what "traditional" economics has had to say on the subject of the relation between the Theory of Money and Prices, on the one hand, and the general Theory of Value, on the other.

It is of some importance to call attention to the fact that it is proposed to discuss only those aspects of the problem that *fall within the range of topics with which the present work is concerned.* As it happens, one of the principal results which Mr. Keynes himself believes to follow from his attempt to bridge the alleged gap between the two bodies of theory in question is that he has thereby succeeded in "pushing monetary theory back to becoming a theory of *output as a whole,*" in the sense that he has succeeded in demonstrating that an adequate treatment of "the problem of what determines output and employment as a whole" requires "the complete theory of a Monetary Economy." [6] From the statement in the Preface to this volume regarding the modifications made in the original plan with respect to the scope of the present work, it should be clear that a complete examination of the adequacy of the particular "theory of output as a whole" presented in the *General Theory* must be left for another occasion. Nevertheless, the following comments may be presented here with respect to both Mr. Keynes's

General Theory of Employment," *Economic Journal,* XLVI (1936), 238, on the *General Theory* as "bringing money out of its isolated position as a separate subject into an integral relation with general economics." Cf. also the comments by F. Vito on this aspect of the argument of the *General Theory* in the *Rivista internazionale di scienze sociali,* XLIV (1936), 655, and the same author's *Risparmio forzato e cicli economici (Pubblicazioni della Università Cattolica del Sacro Cuore,* Series III, vol. xvii [1937]), 25; also R. J. Saulnier, *Contemporary Monetary Theory* (1938), 8 f., 373, 375, and A. P. Lerner, "Some Swedish Stepping Stones in Economic Theory," *Canadian Journal of Economics and Political Science,* VI (1940), 581, where reference is made to the "recent union" of "general economic theory and monetary theory" in a context suggesting that this "recent union" is due almost entirely to the efforts of Mr. Keynes in his *General Theory.*

[6] Cf. the *General Theory,* pp. vi and 293 (italics mine). On the possible suggestion that this fact makes irrelevant much of the material presented, in the present chapter and the one following, with respect to the treatment by earlier writers of the relation between the Theory of Money and Prices, on the one hand, and the "general" Theory of Value, on the other, see what is said below, p. 35, n. 95.

generalizations concerning what "traditional" economics has had to say on the subject and his claims for the relevant aspects of his own work:

1. (*a*) The implied claim to have been the first to produce a "theory of output as a whole" is patently absurd in the light of the vast literature on the subject of industrial fluctuations, which includes the literature on the subject of secular stagnation and therefore of enduring "under-employment."

(*b*) At best, Mr. Keynes's claim could be interpreted as meaning only that there has not been sufficient *integration* of the results obtained in the "general" Theory of Value, on the one hand, and the "theory of output as a whole," on the other. This involves raising the question of the degree of usefulness of analytical devices developed originally in connection with what Mr. Keynes calls the "Theory of the Individual Industry or Firm," in dealing with the problems involved in the "Theory of Output and Employment *as a whole*." [7] It is precisely this question which is discussed at several points in the present volume as a result of the fact that it is necessarily involved in any attempt to establish the nature of the rôle played by money in the theory of the determination of money prices.[8]

(*c*) The implication that some novelty attaches to the suggestion that an adequate theory of "output and employment as a whole" must do justice to the influence of money, and therefore requires the full use of the substance of monetary theory, is as absurd as the implication discussed under (1), in the light of the plain facts of the history of economic doctrine with respect to the influence of monetary expansion and contraction upon the level of "output as a whole." [9]

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[7] Cf. the *General Theory*, 293.

[8] See especially Parts Two (pp. 137 ff.) and Three (pp. 521 ff.) of the present volume.

[9] That there is still room for an adequate history of doctrine on this subject no one familiar with the literature can deny. All that is here contended is that there *is* a literature, the vastness of whose proportions is not adequately indicated even by such an extended survey as that of F. Burchardt, "Entwicklungsgeschichte der monetären Konjunkturtheorie," *Weltwirtschaftliches Archiv*, XXVIII (1928). For our present purpose, in any case, it should hardly be necessary to remind a generation familiar with the writings of Mr. R. G. Hawtrey that the problem of the effect of money upon "output as a whole," and therefore the problem of the relation of monetary theory to the theory of output as a whole, was not first posed by the *General Theory*. It is highly doubtful, on the contrary, whether any economist of standing would have been prepared to deny, prior to the appearance of either the *Treatise* or the *General Theory*, that "it is no longer possible to distinguish clearly between monetary theory and business cycle theory" (Saulnier, *Contemporary Monetary Theory*, 7). Evidence of this, indeed, is provided by the fact that Mr. Keynes himself was prepared, in the un-"revolutionary" days of his *Monetary Reform* (pp. 21, 30, 36 ff.), to remark, quite casually, that "it has *long been rec-*

2. It would be extremely easy to present, and support, a similar series of propositions with respect to the further problem, associated with the relation between monetary theory and "general" economic theory, to which the *General Theory* may be regarded as having called attention, but whose adequate discussion must likewise be left for another occasion: namely, the relation of money to the determination of the rate of interest.[10] It happens, however, that Mr. Keynes himself has made concessions, with respect to the existence of a literature on the subject of the relation between money and interest, of a kind which make it unnecessary to labor the proposition that such a literature does exist, and that an evaluation of Mr. Keynes's contributions toward a solution of the problem would require an adequate consideration of what was available in this literature before the *General Theory* was published.[11] As already indicated, this is a task that lies outside the plan on which the present volume is constructed. Enough has been said, however, to indicate why this fact is not to be taken as evidencing an unwillingness to meet the challenge of the *General Theory* to received doctrine on the subjects mentioned.

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ognized, by the business world and *by economists* alike, that a period of rising prices acts as a stimulus to enterprise" and that falling prices act to bring about "depression" and "unemployment" (italics mine). On the treatment of the problem of the effect of monetary expansion and contraction upon output as a whole by economists characterized by Mr. Keynes as "classical," see what is said below, pp. 37, 49, n. 133, and also pp. 64 f., 74 f., 83.

[10] The reasons for deferring to a later occasion a more nearly complete discussion of the problem indicated are stated in the Preface to the present volume. See, however, the incidental comments on the treatment by earlier writers of the relations between money and interest in n. 11, immediately following, and the forward references there given; also what is said on this matter below, pp. 63 f., 66, 71, 75 ff.

[11] Mr. Keynes has been prepared, for example, to recognize Irving Fisher as "the great-grandparent who first influenced" him strongly "towards regarding money as a 'real' factor" in the determination of the rate of interest (see Keynes's "Alternative Theories of the Rate of Interest," *Economic Journal,* XLVII [1937], 242 n.). This fact takes on particular interest in view of Fisher's own acknowledged indebtedness to so "classical" an economist as John Stuart Mill (see below, p. 50, and especially n. 134 thereto; also the reference to Ricardo, in this connection, below, p. 38, n. 102). Mr. Keynes has stated also that he would even have been willing to adopt Wicksell as a "great-grandparent" in this respect if it had not been for the addiction of Wicksell, elsewhere characterized by Mr. Keynes without qualification as "unorthodox" (cf. p. viii of the German [1936] translation of the *General Theory*), to the heinous sin of "*trying* to be 'classical'" (Keynes, "Alternative Theories," *loc. cit.,* 242 n.). This fact likewise takes on particular interest in the light of the plain facts with respect to the relation of the substance of Wicksell's doctrine to that of Ricardo. See what is said on this matter below, pp. 38, n. 102, 76, n. 62, 77, n. 63.

II

"VALUE THEORY" AND THE VALUE OF MONEY FROM
ARISTOTLE TO CANTILLON

If the determination of something called the Value of
Money is not the sole problem with which monetary theory
is concerned, it is at least the problem which has bulked
largest in most of the versions of the Theory of Money and
Prices that have come down to us.   It is only reasonable,
therefore, to construct an historical account of earlier at-
tempts to establish a *modus vivendi* between the Theory of
Money and Prices, on the one hand, and the general Theory
of Value, on the other, upon a framework suggested by this
simple historical fact.   More specifically, the framework
indicated is one designed to determine to what extent, if any,
earlier writers sought to establish such a *modus vivendi* by
regarding the problem of the Value of Money as a special
case of the general Theory of Value, in the sense that the
analytical devices developed within the latter field were
formally applied to the solution of the former problem.

This procedure can be followed without prejudice to dis-
cussion of (1) the degree of significance, if any, which may
be held to attach to the statement of the problem of the
Value of Money in terms of the apparatus developed within
the general Theory of Value; and (2) the question whether
other methods of establishing a *modus vivendi* between the
two bodies of theory which can be found in earlier economic
literature are not, in fact, more significant than the method
indicated under (1).   Our first task, however, must be to
establish, in broad outline, the facts of doctrinal history
with respect to the relation between the Theory of Money
and Prices, on the one hand, and the general Theory of
Value, on the other, when the problem is regarded as taking
the form of applying to the problem of the Value of Money
the analytical devices developed within the "general" Theory
of Value.

The only conclusion possible upon the basis of facts such
as those adduced below is one that will surprise those who
would otherwise have been inclined to accept without ques-

tion Mr. Keynes's proposition that in this respect "tradi-
tional" economics has been leading a kind of "double life." [12]
For the simple truth of the matter is that this "double life"
is largely a myth. What is true is rather that, from the very
beginning of economic science, there has not been a genera-
tion in which some writer of importance has not insisted
upon treating the problem of the Value of Money in terms
of the analytical devices represented by whatever general
Theory of Value the particular writer in question happened
to hold.

The evidence for this conclusion is provided below. It
is of considerable importance, however, to emphasize that
the main purpose of the historical account which follows is
not to issue a bill of indictment against Mr. Keynes or others
who have made equally irresponsible statements with re-
spect to what "traditional" economics has had to say con-
cerning the relation between the two bodies of theory. Its
main purpose is rather to demonstrate that from the study
of doctrinal history certain *lessons* may be derived which, if
they had been learned in time, might have made unnecessary
a very large part of the controversy that has taken place in
recent years.

In what follows, therefore, an attempt will be made, in
each case, to establish the significance, for the subsequent
development of monetary theory, of the results obtained by
a given author or group of authors. These lessons, in turn,
are summarized in Chapter Three, section II, of the present
volume, where forward references are given to our later dis-
cussion of those aspects of Mr. Keynes's argument to which
these lessons may be held to be relevant. The reader, there-

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[12] There *is* considerable evidence to indicate that the effect of Mr.
Keynes's statements on this head has been to strengthen, rather than
weaken, the general impression as to the reality and pervasiveness of the
"double life" that is alleged to have characterized this aspect of economic
literature. See, for example, the really extraordinary series of statements
with respect to the lack of "connection" that is alleged to have existed
"until recently" between "what is usually taught as Monetary Theory and
the General Theory of Prices (or Value)," in section 1 of the otherwise
interesting and suggestive article by J. Marschak, "Money and the Theory
of Assets," *Econometrica*, VI (1938), 311 f.; and cf. also the reference given
above, p. 5, n. 5, to Mr. Lerner's statement with respect to the "recentness"
of the "union" between "general economic theory and monetary theory."

fore, who is interested only in these "Lessons of Doctrinal History" and their application to current discussion may turn at once to Chapter Three, section II, and the forward references there provided. For the rest of the present chapter, as well as the chapter following, will be concerned with the presentation of the evidence which may be held to support the general conclusion stated above with respect to the treatment, in earlier economic literature, of the relation between the Theory of Money and Prices, on the one hand, and the general Theory of Value, on the other.

1. *Aristotle and the Schoolmen.* There are those who, since they consider Aristotle to be, if not "the first analytical economist," then at least the author of propositions out of which "the whole Science of Economics is to be evolved, just as the great oak-tree is developed out of the tiny acorn," might insist that any test as to what has been said on the subject of the relation between the general Theory of Value and the Value of Money "from the very beginning of economic science" must begin with no less a name than that of Aristotle.[13] It is therefore rather amusing to observe that historians of monetary theory, writing without benefit of the stimulus provided by Mr. Keynes's inclusive generalization with respect to what "economists" have said or failed to say concerning the relation between the two bodies of theory, had already been prepared to summarize Aristotle's theory of the Value of Money by the proposition that he had "assimilated" his "theory" on this head "to his general theory of value."[14] If, moreover, it is fair to regard Aristotle as not only having "gathered together the whole knowledge of economics in antiquity," but also as having "anticipated, in his presentation of it, nearly, if not quite all, that was achieved during the middle ages," it is also fair to ask whether the economics of the Schoolmen imitated this aspect of Aristotle's argument also.[15] Here also, therefore, it is rather amusing to observe that the same historians of monetary theory have pointed out that, as a group, the Schoolmen, instead of regarding money as subject to *"special* laws of value," were prepared to insist that "the value of money varies" in response to the same type of controlling factor as does the value of "other things."[16]

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[13] The quotations with respect to the place of Aristotle in the history of economics are taken from E. Roll, *A History of Economic Thought* (1939), 33, and H. D. Macleod, *The History of Economics* (1896), 51, respectively.

[14] So, for example, A. E. Monroe, *Monetary Theory before Adam Smith* (1923), 8 f.

[15] The quotation is from L. Cossa, *Introduction to the Study of Political Economy* (p. 134 of the English translation of 1893).

[16] See Monroe, *Monetary Theory before Adam Smith*, 25 f., and the references there given.

As was suggested above, however, what really matters is not the establishment of such facts of doctrinal history so much as the lessons that might have been drawn from them for the subsequent development of monetary theory. From this point of view, the following comments with respect to the Aristotelian "achievement" and its influence are in order:

In the first place, the "assimilation" of the problem of the Value of Money to the "general" Theory of Value is a small achievement indeed if the particular variant of the "general" Theory of Value involved is itself one of extreme crudity.[17] Whenever, therefore, a given author, in his "assimilation" of one body of theory to the other, falls back upon a variant of the "general" Theory of Value which is itself retrograde, the result of the "assimilation" must itself necessarily be retrograde.

In the second place, the whole history of the discussion, by later writers, of Aristotle's "assimilation" of the two bodies of doctrine shows that, as often as not, the concern with the formal problem of "assimilation" has not only overshadowed a concern with genuine issues of substance, but on occasion has actually obscured these issues as the result of an insistence upon a statement of the problem which throws no light whatever on the matters which are really in controversy.

In the case of Aristotle, for example, the particular consequence which was drawn from those passages which might be interpreted as having "assimilated" his theory of the Value of Money to his "general" Theory of Value was that money was to be regarded as a "commodity" and therefore subject to the Laws of Supply and Demand determining the value of "commodities." [18] So stated, the proposition came to be re-

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[17] I have reference here, of course, to Aristotle's "general" Theory of Value when judged from the standpoint of the advances made since his day; I have no desire to minimize its *historical* merits, which have been placed very high by competent historians of economic doctrine. See, for example, Travers Twiss, *View of the Progress of Political Economy in Europe since the Sixteenth Century* (1847), 85, on Aristotle as having held a theory of the "exchangeable value of commodities" which was based upon the concept of "demand founded on utility," and the quotation from Senior's Lectures given by M. Bowley, *Nassau Senior and Classical Economics* (1937), 205, on the merits of "Aristotle's description of value as depending on demand." Cf. also H. R. Sewall, *The Theory of Value before Adam Smith,* in "Publications of the American Economic Association," Third Series, Vol. II, No. 3 (1901), p. 57, on the interpretation of Aristotle given by Montanari (1680); and see, finally, the comments on Aristotle's Theory of Value in J. Schumpeter, "Epochen der Dogmen- und Methodengeschichte," *Grundriss der Sozialökonomik,* I, 1 (1924), 23.

[18] Cf. Monroe, *Monetary Theory before Adam Smith,* 8. It happens that a controversy which was in any case doomed in advance to sterility was made the more confused by an initial disagreement concerning what Aristotle did in fact say with respect to the so-called "commodity" character of money. It was, of course, such disagreement which underlay Roscher's suggestion that Aristotle's discussion was typical of those based on the second of the "wrong" types of "definition" of money indicated by Roscher's

garded by a very large number of economists as one which was so nearly self-evident that it could be accepted by them, as one of their number put it, "with eyes closed." [19]   If anything is clear, however, it is that (1) the obscurity as to what was implied by the statement that money is a "commodity" remained as great after the acceptance of the proposition as it was before; (2) the substance of the issues really in dispute between those who appealed to this "fundamental principle" and those who protested against its misuse (as in the bimetallic controversy) was completely untouched by the introduction into the argument of the "principle" itself; and (3) there was not a single problem involved in these disputes—whether it was the question of the theoretic possibility of fiat money, or the effect upon the value of money of monetary legislation (including the conferring of the legal tender quality) and of the arts demand—which could not have been discussed with more precision and a deeper understanding of the issues that were substantively important, *without* any direct reference to the so-called "commodity" character of "money."   And it is anything but clear that those who denied that money is a "commodity" would always, or even usually, have been found on the wrong side of the dispute if the dispute itself had in each case been transformed from a question of words into a question of substance.

2. *Bodin.*   Few economists would dispute the suggestion that if there was any writer, other than Bernardo Davanzati, active during the "period of transition from Canonist doctrine to mercantilist theory," who deserves to be called "an enlightened economist," that writer is

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own famous (and itself much overdiscussed) proposition that "the wrong definitions of money may be divided into two classes: those which convey the idea that money is more than a "commodity," and "those which imply that it is less" (Roscher, *Principles of Political Economy*, Book II, Chap. III, section cxvi, n. 5 [I, 342 f., of Lalor's translation]; on Roscher's proposition itself, see Wicksell, *Interest and Prices*, 33 f.).   The reader interested in seeing the lengths to which such disagreement with respect to Aristotle's meaning could be carried may care to consult the references given in R. Gonnard, *Histoire des Doctrines Monétaires*, I (1935), 31 ff., as well as the attempt at reconciliation made by Roll, *History of Economic Thought*, 36. It is certain, at any rate, that the disagreement in question is what made possible, from the eighteenth century to our own day, appeals to the authority of Aristotle by representatives of both sides in the perennially recurring application, to disputes with respect to monetary policy, of the ancient and futile dispute regarding the "commodity" character of money. Cf. the reference to Mirabeau's appeal to Aristotle in S. D. Horton's "Historical Material for, and Contributions to, the Study of Monetary Policy," in *U. S. Senate Executive Document No. 58*, Forty-Fifth Congress, Third Session, 297, and the further references to the bimetallist literature in Palgrave's *Dictionary of Political Economy*, I, 54, as well as the attempted application to the Knappian controversy by A. Gray, *The Development of Economic Doctrine* (1931), 27 (contrast Roll, *loc. cit.*).

[19] So A. Landry, "La Loi de L'Offre et la Monnaie," *Révue d'économie politique*, XI (1897), 487.

Jean Bodin.[20]   Still fewer would be prepared to deny that the name of
Bodin is the first really great name in the history of doctrine with respect
to the nature of the forces determining the Value of Money.[21]   It is
worth noting, therefore, that Bodin's discussion has been characterized
by historians of monetary theory as representing a direct application to
the problem of the Value of Money of Bodin's own "general law of
value." [22]

Bodin's "general law of value" was simply the unsophisticated propo-
sition that "c'est . . . l'abondance qui cause le mépris." [23]   It is clear
that he intended this "law" to be understood as applying not only to
the consequences of an "abundance of gold and silver," but also to an
"abundance" of "all things."   The historians of doctrine who have
characterized his "theory of the Value of Money" as a direct application
of his "general Theory of Value" are therefore correct.   What makes
Bodin a figure of importance in the history of monetary theory, how-
ever, is not the mere fact that he "assimilated" his theory of the Value
of Money to the "general" Theory of Value: after all, this had been
done before him by Aristotle and the Schoolmen.   What makes Bodin
important is rather the fact that his ultimate finding (namely, that an
increase in the quantity of the precious metals was an important cause
of their depreciation) is one which can stand on its own merits as a
contribution to our understanding of the forces determining money
prices, in the sense that it called attention to an element the importance
of which, for all its simplicity, had not been adequately appreciated by
the better-known writers on the value of money before Bodin's time.

3.   *Davanzati.*   There can be no question that the only writer on
monetary theory in the sixteenth century whose contributions bear
comparison with those of Bodin is Bernardo Davanzati.   It happens
also that Davanzati has been regarded as one of the first writers on the
"general" theory of value who moved within the range of ideas suggested
by what would now be called "utility" analysis.[24]   It is worth noting,

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[20] The quotations are from Roll, *History*, 60, 89.

[21] Cf. pp. 9 and 96 of Volume I of the present work.

[22] See, for example, F. Hoffmann, *Kritische Dogmengeschichte der
Geldwerttheorien* (1907), 13 f.   In the light of Monroe's use, throughout
his work, of the expression "the commodity theory" of the Value of Money,
the same interpretation of Bodin's position must be held to be represented
by the former's characterization of Bodin as a "commodity theorist"
(*Monetary Theory before Adam Smith*, 57 f., 101, 113, 144).

[23] Bodin, *La Response de Jean Bodin à M. de Malestroit*, p. 10 of H.
Hauser's edition (1932); cf. A. E. Monroe, *Early Economic Thought* (1924),
127 f.   The proposition in question is characterized as a "law" by H.
Baudrillart, *J. Bodin et son Temps* (1853), 170.

[24] See A. Graziani, *Storia critica della teoria del valore in Italia* (1889),
30 ff., and cf. Sewall, *The Theory of Value before Adam Smith*, 53 ff., on
Davanzati's argument as being capable of translation into the propositions
that "the values of goods relatively to one another depend . . . upon their
subjective utilities" and that "utility is a function of quantity and desira-
bility."   See also Hoffmann, *Kritische Dogmengeschichte*, 17, where

therefore, that one of the reasons why Davanzati's *Lezione delle monete* (1588) has been characterized as an "imperishable masterpiece of clear, firmly incisive analysis, illuminating all individual phenomena in the field by the help of a single explanatory principle" is that he presented "a 'metallist' theory of money, *on the basis of a general theory of use-value*, that can be retained even today." [25]

One has, however, only to consult histories of monetary theory on the nature of Davanzati's achievement to discover a lesson which, if it had been learned, would have made unnecessary a very large part of the later discussion that has arisen as the result of an insistence upon confusing issues of substance with issues that are entirely factitious. The substantive result for the Theory of Money Prices that was represented by the part of Davanzati's argument under discussion here is his recognition, with Bodin, of the importance of changes in the quantity of the money metal—such as those resulting from the discovery of America—in the determination of the level of money prices.[26] This is a result which, as has been suggested, "can be retained even today" by monetary theorists. It is also a result, however, which remains completely uncontradicted by, and itself does not contradict, those further aspects of Davanzati's argument which have been characterized as evidencing a "quantity conception" of the determination of money prices, in the sense that they represent a first attempt to pose the problem in terms of what later came to be called a "mutual impact of relevant flows" of money and of goods, respectively.[27]

The chief objection to Davanzati's method of stating the problem was, of course, that he thought, not of an impact of "flows," but rather of *stocks*, and thereby laid himself open to the charge of having neglected the range of problems which were later summarized under the head of the concept of "velocity." [28]   Yet if objections are to be made to

Davanzati is grouped with Bodin as a writer who undertook to "set up a theory of value on a psychological basis."

[25] So J. Schumpeter, "Epochen der Dogmen- und Methodengeschichte," *loc. cit.*, 36 (italics mine).

[26] See especially, in this connection, p. 35 of the edition of Davanzati's *Lezione* contained in Custodi's *Scrittori classici italiani di economia politica, Parte antica,* Vol. II.

[27] For an example of a characterization of this part of Davanzati's argument as evidencing a "quantity conception" *(Quantitätsauffassung),* in the sense indicated, see Hoffmann, *Kritische Dogmengeschichte,* 17.

[28] The statement of Davanzati to which reference is here made is, of course, his famous proposition that "all these [earthly things which satisfy men's wants] are, by the consent of nations, worth all the gold . . . that is wrought" (cf. Monroe, *Monetary Theory before Adam Smith,* 59; the passage appears on p. 32 of the Custodi edition of Davanzati's *Lezione).* For examples of criticism of Davanzati's argument for having failed to do justice to the element of velocity, see Graziani, *Storia critica,* 31, and M. Pantaleoni, in Palgrave's *Dictionary of Political Economy,* I, 483; and on the significance of the point for the general Theory of Money and Prices, see Volume I, p. 345, of the present work, and the references given in n. 3 thereto.

Davanzati's "quantity conception" of the problem, it must be made on such grounds, which are grounds of substance, and not on the purely formal ground that the very introduction of the "quantity conception" represents an objectionable duality in the monetary theory of Davanzati, who may thus be said to have "operated in a different way with the two parts of his theory." [29]   For to shift the argument to this other ground is to assume what must be demonstrated: namely, that all aspects of the theory of the determination of money prices are best approached in terms of an application of the categories of the "general Theory of Value" to the problem of the Value of Money, and that a theory of the Value of Money which runs from first to last in terms suggested by these categories is necessarily inconsistent with, and superior to, a theory of the Value of Money which makes use of these categories whenever they show themselves capable of providing substantive results unobtainable by different methods, but not otherwise.   That this conclusion does not necessarily follow is, indeed, *the* great lesson to be learned from the history of attempts to establish a *modus vivendi* between the Theory of Money and Prices, on the one hand, and the general Theory of Value, on the other.   It is a lesson whose importance far transcends any that can be drawn from a demonstration that Davanzati did or did not succeed in stating his theory of the Value of Money in terms of the categories provided by his "general" Theory of Value.

4. *Petty.* If not everyone will agree with Marx's characterization of Petty as the "founder of political economy," it will certainly be agreed that his is one of the great names of the pre-Smithian era.[30] And within the "general" Theory of Value, it is, of course, Petty's articulate emphasis upon the element of cost of production that makes his work important.[31]   It is worth observing, therefore, that Petty himself proceeded immediately to apply his general principle with respect to the rôle of cost of production to the problem of the value of the money-metal.[32]   Historians of doctrine have therefore been correct in characterizing his treatment of the problem of the Value of Money as amounting to a subjection of this special case to the "general law of value." [33]   Again, however, what gives importance to Petty's treatment of the problem of the Value of Money is not its "assimilation" to his

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[29] So Hoffmann, *Kritische Dogmengeschichte,* 17.

[30] For Marx's characterization of Petty as the "founder of political economy," see the references given by Roll, *History,* 102, n. 1.

[31] Cf. the comment by J. R. McCulloch, *The Literature of Political Economy* (1845), 318: "He [Petty] has in different parts of this tract [*A Treatise of Taxes and Contributions*] indicated, with considerable distinctness, the fundamental principle, by establishing which Mr. Ricardo gave a new aspect to the whole science."

[32] See, for example, Petty's *Treatise of Taxes and Contributions,* Chap. IV, secs. 14–15, and Chap. V, sec. 10 (I, 43 f., 50 f., of Hull's edition of *The Economic Writings of Sir William Petty*).

[33] So, for example, Hoffmann, *Kritische Dogmengeschichte,* 35. Cf. also Monroe, *Monetary Theory before Adam Smith,* 106, 144.

"general" Theory of Value, in and of itself. It is rather that, in stressing the importance of the cost of production of the money-metal as an element affecting its value, he succeeded in introducing into the theory of the Value of Money an element which not only can stand on its own feet as a contribution to that theory but had also been either neglected or given inadequate emphasis by earlier writers on the nature of the forces determining the value of money.

5. *Locke.* In view of the fact that Locke's *Considerations of the consequences of the lowering of Interest, and raising the value of Money* has been characterized as a treatise which, "though nominally on the currency, is to a large extent a general discourse on the general principles of economics," it is of some interest to determine whether Locke made any attempt to associate his discussion of the forces determining the "Value of Money" with his "general principles" with respect to the forces determining the "value" of all things.[34] On this point Locke could hardly have been more emphatic. "Money," he wrote, "in buying and selling . . . [is] perfectly in the same condition with other commodities, and [is] subject to all the same laws of value."[35] Again, therefore, those historians of doctrine are correct who have insisted that Locke "undertook to subject the value of money to the general law of value."[36]

Unfortunately, the discussion by later writers of Locke's position on this head has by no means displayed the unanimity that one might have expected on the basis of apparently unequivocal statements of the kind just quoted. The reasons for this lack of unanimity turn, however, not upon differences of opinion with respect to issues of substance, but upon the same kind of logomachy to which attention was called above in the case of discussions concerning the implications of the position attributed to Aristotle with respect to the "commodity" character of money.

The "laws of value" to which Locke himself believed "money" as well as "other commodities" to be "subject," were, of course, those summarized by his proposition that "that which regulates the price" of commodities is "nothing else but their quantity in proportion to their vent."[37] On the other hand, the particular proposition which has given trouble to later commentators (to the point of leading them to conclude that Locke also held that the value of money is determined by a "special law" of its own, so that in fact he held simultaneously "two theories of the value of money" which "can in no way be reconciled"), was that money differs from other commodities in that its "vent," unlike the "vent" of other commodities, "is always sufficient, or more than enough"; and the corollary that, "this being so, its quantity alone is enough to regulate or determine its value without considering any

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[34] The characterization of Locke's *Considerations* in the terms quoted in the text is that of J. Bonar, in Palgrave's *Dictionary*, II, 635.

[35] Locke, *Considerations*, p. 243 of the Ward, Lock and Company edition.

[36] So, for example, Hoffmann, *Kritische Dogmengeschichte*, 97.

[37] Locke, *Considerations*, 242, 245 f.

proportion between its quantity and vent, *as in other commodities.*"[38]

The *substantive* meaning of the latter proposition need not concern us here; for, unfortunately, most commentators have not been concerned with the soundness of Locke's position when it is translated in terms of questions of substance.[39] What they have been concerned with, for the most part, has been a mere logomachy, arising from a failure to agree upon what is meant by the statement that money is subject to the same principles of "supply and demand," or to the same "laws of value," as those which operate in the case of other commodities.[40] For if by this is meant that the *particular conditions* of supply and demand are held to be the same in all instances, then, of course, the case of the Value of Money will be regarded as quite different from the case of the value of other commodities whenever one can point to conditions affecting the supply of money (or, in the case of Locke, affecting the "vent of," or the demand for, money) which are different from the conditions present in the case of other commodities. When, however, the "general principles of supply and demand," or the "general laws of value," are regarded as *including* all the special conditions of "supply" and "demand" which occur in economic life, it becomes perfectly possible

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[38] Locke, *Considerations,* 249 (italics mine). The other passages quoted are from Hoffmann, *Kritische Dogmengeschichte,* 23 ff. Contrast, however, the statement by Hoffmann cited above, p. 16, n. 36, and see also the following note.

[39] I venture to suggest, as reasonable translations of Locke's substantive meaning, the following propositions: (1) the demand for (or "vent" of) money is such that money is less likely to lose its value entirely than are most other commodities; or (2) the probability of sudden and short-period changes in the "demand" for money (or its "vent") is much less than the probability of such changes in the supply of money (or its "quantity"), with the result that no great harm would come from directing attention primarily to the changes in the supply of money, when only short periods are taken into account. It is clear that neither of these propositions justifies the suggestion that Locke believed that the very terms "supply and demand"—or, as he called them, "quantity" and "vent"—have no meaning in the case of the Value of Money. It is equally clear, from a study of Locke's essay, that he cannot possibly be interpreted as having meant that the "demand" for money (or its "vent") is of no importance in determining its value. For if we regard the forces determining the ("absolute") demand for money as summarized by the expression T/V, it is necessary only to call attention to (1) the importance of Locke as a figure in the history of emphasis upon the importance of monetary "velocity" (cf. Volume I of this work, p. 96, and the reference given in n. 55 thereto); and (2) the fact that Locke himself was careful to insist that the "value of money," in the sense of its purchasing power over other things, "depends" not *only* "on the plenty, or scarcity of money," but on its "plenty, or scarcity . . . *in proportion to the plenty and scarcity of those things*" (*Considerations,* 239 [italics mine]).

[40] See, in this connection, the excellent comments of C. Rist, *Histoire des doctrines rélatives au crédit et à la monnaie depuis John Law à nos jours* (1938), 326 ff.

to argue that money is subject to the same principles of supply and demand as are other commodities, even if the particular conditions of supply and demand are different from those of most "ordinary" commodities. To argue otherwise, indeed, is to put the argument on the level of economic illiteracy represented by such propositions as that "because of the appearance of a monopoly 'the law of supply and demand has been repealed.' " [41]   The whole discussion, therefore, has little significance beyond the facts (1) that it should have been regarded as having provided a lesson that might have prevented a very large amount of unnecessary controversy by later writers concerned with the relation between the problem of the Value of Money, on the one hand, and the "general" Theory of Value, on the other; and (2) that it provides further support for the generalization that, as often as not, the statement that the case of the Value of Money is only a special case of the "general" Theory of Value has been merely a source of additional and purely factitious difficulties, rather than a means for resolving difficulties presented by the facts of economic life.

6. *John Law.* No one could write the history of monetary theory in the eighteenth century without considering the writings of John Law. Nor would it be possible to write the history of "general" value theory during the eighteenth century without mention of Law—whether one regards his utterances on this head as having "anticipated" the "theory of subjective value" or as representing nothing more than a particularly explicit statement of what has been called a "quantity-and-demand theory of value." [42]   Again, therefore, there can be no question of the correctness of the statement by historians of monetary theory that Law subsumed the case of the Value of Money under what he believed to be the "general Laws of Value." [43]

Again, however, what gives importance to Law's argument on this head is the fact that he actually made a contribution to the *substance* of received doctrine with respect to the forces determining the Value of Money. This was in the form of a statement—clearer and more explicit than had been made previously—of the proposition that the *monetary* use represents an "additional" element in the "demand" for, and there-

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[41] Cf. the comments on this type of proposition in F. B. Garver and A. H. Hansen, *Principles of Economics,* p. 97 of the revised (1937) edition.

[42] For an example of the first characterization of Law's "general" Theory of Value, see L. Mises, "Die Stellung des Geldes im Kreise der wirtschaftlichen Güter," in *Die Wirtschaftstheorie der Gegenwart* (1932), II, 310 (cf. Roll, *History,* 119) ; and, for the second characterization, see E. Cannan, *A Review of Economic Theory* (1929), 159 f.

[43] See, for example, Hoffmann, *Kritische Dogmengeschichte,* 31, 97. For Law's statement of the principles determining "How Goods are Valued," see his *Money and Trade Considered* (1705), 4 ff. (2 ff. of the first volume of Harsin's edition of Law's *Oeuvres Complètes*) ; and, for examples of his application of these principles to the value of money, both metallic and nonmetallic, see *Money and Trade Considered,* 6, 10, 63 ff., 69 ff., 84 ff., 89 ff., 117 ff. (*Oeuvres Complètes,* I, 6, 12, 86 ff., 94 ff., 114 ff., 120 ff., 158 ff.).

fore the value of, whatever material may be used for money.[44] As compared to this contribution of substance, the fact that Law went out of his way to insist that "the value of money obeys the same laws as other goods" is of altogether secondary importance.

Indeed, it is even an open question whether the more formal aspects of Law's discussion of the relation between the theory of the Value of Money, on the one hand, and the "general" Theory of Value, on the other, did not serve to obscure, rather than illuminate, the true nature of the issues which were most important for his argument. It is, for example, something of a commentary on the essential lack of significance attaching to the fact that Law may be said to have subsumed the case of the Value of Money under his "general" Theory of Value, that D'Aguesseau, one of Law's most determined antagonists, was just as emphatic as Law in insisting that the "value of money obeys the same laws as other goods, rising or falling in proportion to changes in supply and demand." [45] Nor is the force of the paradox lessened by the fact that D'Aguesseau's "general" Theory of Value was essentially identical with that of his opponent.[46] For D'Aguesseau, as for Law, the general theory of value—or, as he put it, "the general principle of the value of all things which enter into trade"—could be summed up by the proposition that "the relative value of things depends on the proportion . . . between quantity and demand"; and D'Aguesseau was no less emphatic than Law in insisting upon applying this "general principle" to the case of the Value of Money.[47] The whole episode, indeed, is interesting chiefly as providing the type of warning that should have been heeded— but, characteristically, was not—by those proponents and opponents of "the quantity theory" who, more than a century later, vied with one another in asserting that their positions rested on the high authority of "the general law that value is determined in the relation of demand and supply," while the real issues in the "quantity theory" controversy went by default.[48]

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[44] In this connection, see the comments by Mises, *The Theory of Money and Credit*, 106, n. 1.

[45] So Monroe, *Monetary Theory before Adam Smith*, 199. Cf. also the article by W. Qualid, "D'Aguesseau économiste" (*Révue d'histoire des doctrines économiques et sociales*, II [1909], 278 f.) to which Monroe refers.

[46] Cf. P. Harsin, *Les Doctrines Monétaires et Financières en France du XVIe au XVIIIe Siècle* (1928), 214.

[47] For D'Aguesseau's statement of his "general principle," see his "Considérations sur les Monnoies" (in Vol. X of the 1777 edition of the *Oeuvres de M. le Chancelier D'Aguesseau*), 4, 6; and for his application of this "general principle" to the case of the value of money, see 11 ff., 24 f., 27, 37 of the same work.

[48] Of the defenders of "the quantity theory" who appealed most emphatically and repeatedly to the authority of the "general law" as stated above, the most notable, perhaps, was Francis Walker. See, for example, his article "The Quantity-theory of Money," *Quarterly Journal of Economics*, IX (1895), 372, 374 (I, 211, 213 of Walker's *Discussions in Economics and Statistics*); "The Value of Money," *Quarterly Journal of Economics*,

Nor is this the only lesson that can be drawn from a formulation such as that of Law, according to which the value of money, like that of other goods, is to be regarded as determined by the "proportion between supply [or "quantity"] and demand." That the "general Theory of Value" involved is one of extreme crudity can best be seen by comparing the algebraic formulations given to this proposition by Italian writers of the eighteenth and early nineteenth centuries, on the one hand, with a mathematical formulation of the theory of "demand" of even the degree of simplicity of Cournot's $D = F(p)$, on the other.[49] Nor should there be any doubt as to the reason for its crudity. The prob-

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VIII (1893), 63, 74 (*Discussions,* I, 196, 205); "The Relation of Changes in the Volume of the Currency to Prosperity," *Economic Studies* of the American Economic Association, I (1896), 27 (*Discussions,* I, 221: cf. also I, 248); *Political Economy, Advanced Course,* p. 128 of the third (1888) edition; *Political Economy, Briefer Course* (1884), 106. For an equally emphatic statement, on the other hand, that "those who deny the validity of the quantity theory . . . cannot by any stretch of the imagination be regarded as having denied the principle of demand and supply"—that, on the contrary, " 'they have accepted the fundamental principle, and have rejected the theory, simply because it seemed to them inconsistent with the principle' "—see J. L. Laughlin, *The Principles of Money* (1903) 323, and the reference to W. C. Mitchell there given. For further examples of a simultaneous appeal to the authority of the "Law of Demand and Supply" by *both* "quantity theorists" and "anti-quantity theorists," see the references given by P. Lambert, *La Théorie quantitative de la Monnaie* (1938), 91.

[49] For the early algebraic formulations to which reference is made in the text, see the citations from Frisi (1772), Ortes (1774), Valeriani (1806, 1816–1817), Ressi (1817–1825), and Fuoco (1825), given by M. Fasiani in his "Note sui saggi economici di Francesco Fuoco," *Annali di statistica e di economia* (Genoa), V (1937), 98 ff., 164 ff. In this connection, cf. the comments by H. Schultz, *The Theory and Measurement of Demand* (1938), 5 ff., especially n. 2. It may be observed, in passing, that one of the "early non-mathematical economists" who "had the correct schedule notion of demand and supply," in the sense that he "meant to say in nonmathematical terms that the quantity demanded is a decreasing function of price" (Schultz, *op. cit.,* 6, n. 6), was Ferdinando Galiani, who wrote, several years before the publication of the works of his compatriots cited above, that "whatever is cheaper is more readily taken for consumption; and thus price, which arises from scarcity, regulates consumption." See Galiani's *Della Moneta* (1750), Book I, Chap. II (Vol. III, p. 87 of Custodi's *Scrittori classici, Parte moderna;* the passage is to be found in English translation on p. 296 of Monroe's *Early Economic Thought*). It may also be observed here that the statement in the text as to the superiority of formulations of the type $D = F(p)$ must not be taken to mean that there are not problems, particularly in monetary theory, in which the type of emphasis suggested by formulations of the type $P = D/S$ provides a necessary complement to that provided by the other type of formulation. In addition to what is said below, p. 21, n. 51, see below, pp. 46 ff., and the forward references given in nn. 123 and 126–128 thereto.

lem (once economics had reached the stage of realizing that the economic problem is not that of deciding how prices *should* be determined, but how they are *in fact* determined, and that prices are, in fact, "determined" by "supply" and "demand" instead of by arbitrary fiat) is obviously that of indicating the nature of the forces which make "supply" and "demand" as large as they are in any given case. If, therefore, Law is to be regarded as having contributed in a significant way to the "general" Theory of Value, it can be only because he is interpreted as having contributed, in other parts of his argument, to an understanding of *why* "demand" is as large as it is, by advancing a proposition which can be regarded as having in some degree "anticipated" the theory of "subjective" value, just as Petty may be regarded as having contributed to our understanding of why "supply" is as large as it is by his emphasis upon cost of production.

If, therefore, Law is to be regarded as having contributed to our understanding of the forces determining the Value of Money, it is only because he contributed to our understanding of the *forces determining* the "demand" for money. The mere statement that the value of money is determined by the "proportion between supply [or "quantity"] and demand" not only tells us virtually nothing concerning the forces determining the "demand" for money, but is actually less illuminating than the kind of formulation with respect to the "demand" for money that could have been constructed, in Law's own day, upon the basis of what Locke and others had already said concerning monetary "velocity" and what amounts to the "volume of trade" of the Fisherine Quantity Equation.[50] This conclusion may be tested, indeed, by comparing the latter type of equation with the type of equation which is provided by the early algebraic translations of the equivalent of Law's "general" Theory of Value to which reference has already been made.[51] The point is of very great importance for an evaluation of later "contributions" to the Theory of Money and Prices, based upon an "assimilation" of the case of the Value of Money to the general Theory of Value, which have been supposed to represent a great advance over the familiar

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[50] On this aspect of Locke's work, cf. the last sentence in n. 39, p. 17, above; and on the general significance of the successive "discovery" of the equivalents of the different variables of the Fisherine equation, see Volume I of this work, pp. 93 ff.

[51] In this connection, see F. Lavington, *The English Capital Market* (1921), 23 f., where, having presented, as a "quite general expression" (for price as determined by "demand" and "supply")—one "which is applicable not only to money but to any kind of commodity"—the formula $P = D/S$, in which D *represents* "Demand" and S represents "Supply" (and which is therefore virtually identical with the early Italian formulations cited above, p. 20, n. 49), the author goes on to translate this "very simple" expression into a formulation which is still "simple," but is much more illuminating as a statement of the forces determining the Value of Money, and which turns out to be the virtual equivalent of a Quantity Equation of the Fisherine form.

Quantity Equations and the type of reasoning that they may be held to summarize.[52]

7. *Galiani.* There can be little doubt that the monetary theorist of the eighteenth century who has the most nearly unequivocal claim to a position of importance in the development of the "general" Theory of Value by virtue of his "anticipation" of the "theory of subjective value" is, not Law, but Ferdinando Galiani, of whose *Della Moneta* it has been said that "it reads, in part, like a modern textbook." [53] For it was Galiani who, in passages of unprecedented clarity and articulateness, laid down, as a general "Explanation of the Principles which Govern the Value of All Things," the proposition that Value, itself a "ratio," "is compounded of two ratios, expressed by the names *Utility* and *Scarcity.*" [54] Nor could any historian of doctrine deny to Galiani's discussion of *monetary* problems the qualities attributed to his work as a whole by J. B. Say: "genius united with erudition, carefulness in uniformly ascending to the nature of things." [55] It is therefore of some importance to observe that there cannot be the slightest doubt as to the accuracy of the statement by historians of monetary theory that Galiani regarded the case of the Value of Money as "simply a case of value in general." [56] It is difficult, indeed, to see how he could have been more explicit on this head. For it was his desire to establish more firmly "the foundations of the science of money" that led him in the first place to "discuss the utility of things" in general; and no reader of his work can deny that he lived up to his promise to "apply to money a hundred of times" the conclusions he had established with respect to the "fundamentals of value" *(principi stabili del valore)* in general.[57]

As always, however, what makes this part of Galiani's discussion important in the history of monetary theory is, not his formal "assimilation" of his theory of the Value of Money to his "general" Theory of Value, but the substantive results he obtained from this process of

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[52] Cf., for example, what is said in this connection below, pp. 652 ff., 659 ff., and 729 ff.

[53] Schumpeter, "Epochen der Dogmen- und Methodengeschichte," *loc. cit.*, 36.

[54] See especially Galiani's *Della Moneta,* Book I, Chap. II (pp. 58 ff. of the Custodi edition; 283 ff. of Monroe's *Early Economic Thought*).

[55] Cf. the Introduction *(Discours préliminaire)* to Say's *Traité d'économie politique* (p. xxxi of the English translation published in Philadelphia in 1836, to which all subsequent citations of Say's *Treatise* refer).

[56] See Monroe, *Monetary Theory before Adam Smith,* 209; and cf. also Sewall, *The Theory of Value before Adam Smith,* 92. The statement of Hoffmann, *Kritische Dogmengeschichte,* 87, that Galiani did not provide a "clear" statement of his position on this head seems to me inexplicable in the light of statements such as those cited from Galiani in the following note; and it is not supported by the quotations given by Hoffmann himself *(loc. cit.).* Contrast, in any case, the statement on p. 97 of Hoffmann's book.

[57] See *Della Moneta,* Book I, Chap. II (pp. 59 and 91 of the Custodi edition; 284 and 299 of Monroe's *Early Economic Thought*).

"assimilation" for the theory of the forces determining money prices. With respect to the *supply* of the money metal, for example, his treatment of the effect of changes in the cost of production of the metal upon its value *by way of its effect upon the "scarcity"* of the metal has the same flavor of modernity and sense of balance as that which characterizes his discussion of the rôle of cost of production in the determination of the value of "things" in general.[58] On the side of *demand*, similarly, it is difficult to find in any earlier writer as clear a statement with respect to the importance and mode of operation of the arts demand for the money metal as is provided by Galiani.[59] And that his concern with the arts demand did not blind him to the nature of the forces underlying the monetary demand is sufficiently demonstrated by the fact that his emphasis upon what has been called by later writers the "bearer of options," as well as the "store of value" function of money, has been assigned by a contemporary historian of monetary theory a *"place de choix"* among earlier recognitions of the proposition that one of the most important characteristics of money is its ability to act as a "bridge between the present and the future." [60] All these propositions are in

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[58] For an example of Galiani's reasoning with respect to the effect of cost of production upon the value of "things" in general, see *Della Moneta*, Book I, Chap. II: "The fact that this beauty of glass and crystal is the product of art rather than of nature does not alter the price, except by altering the scarcity" (pp. 66 f. of the Custodi edition; p. 287 of Monroe's *Early Economic Thought*). For an example of an application of this reasoning to the value of the money metals, see his comment on the consequences that would inevitably follow if "alchemy" should succeed in making it possible to produce gold as cheaply as iron. This, he argued, would merely "take gold and silver out of the number of the things that are scarce, and therefore precious" (*Della Moneta*, Book I, Chap. IV; pp. 132 ff. of the Custodi edition). See also Galiani's very clear account of the effect of cost of production upon supply by way of its effect upon the profitability of working inferior mines in the face of the decline in the value of the metals consequent upon the increase in the supply of these metals from America (itself due to the fact that "with equal effort a greater quantity of metal is obtained"), in Book I, Chap. I (pp. 48 ff. of the Custodi edition).

[59] See, for example, *Della Moneta*, Book I, Chap. II, on the increase in the arts demand as a result of the fall in the value of the precious metals, which in turn resulted from the increase in the supplies coming from America. Also, see *ibid.*, on the effect of this increase in the arts demand in preventing the value of the precious metals "from falling as much as their abundance [would otherwise have] required" (p. 89 of the Custodi edition; p. 297 of Monroe's *Early Economic Thought*). This conclusion with respect to the relation between the arts demand and the value of the money-metal was itself only a special application of Galiani's "general" proposition that "whatever is cheaper is more readily taken for consumption; and thus price, which arises from scarcity, regulates consumption" (see above, p. 20, n. 49).

[60] See Rist, *Histoire*, 90; and cf. pp. 113 f. of the Custodi edition of Galiani's *Della Moneta*. It is, of course, true that Galiani regarded the

themselves of substantive significance for the problem of the determination of money prices; and they would have such significance even if Galiani had not deduced them from, or associated them with, his general "Principles which Govern the Value of all Things."

8. *Cantillon.* It is idle to speculate as to what might have happened to the "general" Theory of Value if later economists had taken as a starting point the statement of that theory by Galiani, who, as we have seen, was prepared to do full justice to the element of cost of production at the same time that he insisted upon the fundamental importance of the elements of Utility and Scarcity. For the statement of the theory which came, by way of its effect upon Adam Smith, to be accepted as the framework for discussion by economists of the "classical tradition" was, not that of Galiani, but that of Cantillon, whose distinction between "the Intrinsic Value of a Thing in General," as affected by its cost of production, and the "Market Price" of such a "Thing," as determined by "the quantity of Produce or of Merchandise offered for sale, in proportion to the demand or number of Buyers," has usually been regarded as the first which "attempted to harmonize the two points of view" represented by the "supply-and-demand theory," on the one hand, and the emphasis upon cost of production, on the other.[61] There can be still less doubt as to the importance of Cantillon in the history of monetary theory. Again, therefore, it is to be observed that Cantillon's formal discussion of the problem of the Value of Money left no doubt as to his belief that it was to be regarded as "simply a case of value in general." [62] On the contrary, he was as explicit as one could wish in his insistence that the principles involved in the determination of the Value of Money were exactly the same as those affecting the value of other "Merchandise or Produce" and, indeed, of "everything" ("de

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arts demand as *historically prior* to the monetary demand; and it is likewise true that he regarded the former as quantitatively more important, in his own day, than the latter (*Della Moneta*, pp. 72, 101 f. of the Custodi edition). It is obvious, however, that neither proposition can be taken to indicate that Galiani regarded the monetary demand as of no importance.

[61] So, for example, Monroe, *Monetary Theory before Adam Smith,* 207. The principal passages in Cantillon's *Essai sur la Nature du Commerce en Général* which justify this description are, of course, those in Part I, Chap. X (on "Intrinsic Value"), and Part II, Chap. II (on "Market Prices"). Cf. Jevons, "Richard Cantillon and the Nationality of Political Economy," *Contemporary Review,* January, 1881: "These few pages contain not only the whole doctrine of market value as contrasted to cost value, or, as the late Professor Cairnes called it, normal value, but there are allusions to difficulties which Ricardo, Mill and many others have ignored" (p. 167 of the essay as reprinted in Jevons's *The Principles of Economics . . . and Other Papers* [1905]).

[62] Cf. Monroe, *Monetary Theory before Adam Smith,* 209, and Hoffmann, *Kritische Dogmengeschichte,* 57. For Cantillon's formal application of his general Theory of Value to the problem of the Value of Money, see especially his *Essai,* Part I, Chap. XVII.

même que toutes les marchandises ou denrées," "comme de toutes choses").[63]

Once more, however, there could be no greater error than that of supposing that it was this type of formal "assimilation" by Cantillon of his theory of the Value of Money to his "general" Theory of Value that justifies Jevons's characterization of his discussion of the problems of monetary theory as "almost beyond praise," a "complete little treatise on currency" which is "probably more profound than anything of the same size since published on the subject."[64] What justifies such a characterization is rather such things as the analysis which Jevons, in an often quoted passage, called "one of the most marvellous things in the book": namely, Cantillon's description of "the successive effects of a discovery of gold or silver mines on the rates of wages and prices of commodities."[65] The determination of "rates of wages and prices of commodities" is certainly a problem with which the "general" Theory of Value is concerned; and, as we shall see, there is a fundamental sense in which it can be said that the model set by Cantillon's discussion on this head is one that might well have been followed by all subsequent writers on the subject of the relation between the Theory of Money and Prices, on the one hand, and the "general" Theory of Value, on the other.[66] The point made here is that the type of analysis involved is of a very different kind from that involved in Cantillon's formal "assimilation" of the two bodies of theory, upon which most historians of monetary theory have commented; just as it is very different from the type of analysis that has been most frequently invoked by some of those, among contemporary writers, who have been most critical of the failure of earlier "economists" to effect a satisfactory union between the Theory of Money and Prices, on the one hand, and the "general" Theory of Value, on the other. The matter involved, as we shall see, is of the utmost importance; and we shall have more than one occasion to recur to it in the pages which follow.

III

FROM ADAM SMITH TO J. S. MILL

It is easy to imagine grounds on which an attempt may be made to discredit the demonstration, provided in the preceding section of this chapter, of the falsity of the sug-

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[63] See, for example, Cantillon's *Essai*, 127 f., 232 (97, 175 of Higgs's edition).

[64] Jevons, "Richard Cantillon and the Nationality of Political Economy," *loc. cit.*, (p. 164 of Jevons's *Principles of Economics . . . and Other Papers*).

[65] See p. 171 of Jevons's *Principles*, etc.

[66] See especially, in this connection, below, pp. 137 ff., 304 ff., 308 ff., 523 ff.

gestion that economists in general have been guilty of allowing a serious hiatus to exist between their theories of Money and Prices, on the one hand, and their "general" theories of Value, on the other. It might be argued, for example, that all the instances presented thus far belong to prehistory, in the sense that they are typical of the pre-"classical" era. Mr. Keynes's assault, on the other hand, is directed specifically against the "classical" economists; and it might be imagined that the sharp cleavage alleged to exist between the Theory of Money and Prices, on the one hand, and the general Theory of Value, on the other, is to be regarded as the particular "creation" of the "classical school," just as Mr. Keynes has argued that the "cleavage" in other respects between the "conclusions of economic theory" and what he calls the conclusions of "common sense" may be regarded as a "creation" of the "classical school." [67]

Unfortunately for this suggestion, however, it happens that the "classical" economists were not less explicit than their predecessors in treating the problem of the Value of Money as a special case of their "general" Theory of Value. It is one of the purposes of this section, as well as of the following chapter, to demonstrate that this is so by considering the case of each of the writers who may be regarded as "classical" economists in Mr. Keynes's sense of the term: namely, those writers who have been generally regarded as the "founders of the theory which culminated in the Ricardian economics," on the one hand, and, on the other, those writers, from J. S. Mill to Alfred Marshall, who have been generally regarded as having "adopted and perfected the theory of the Ricardian economics." [68]

Again, however, it must be pointed out that the principal result of this demonstration should not be taken to be merely a proof of the fact that Mr. Keynes has been careless in his treatment of the writers whom he lumps together under the heading of the "classical economists." The principal result must be seen rather in the demonstration of two propositions which are much more important than the mere fact

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[67] Cf. the *General Theory*, 350.
[68] *General Theory*, 3 n.

that a given writer "assimilated" his Theory of Money and Prices to his "general" Theory of Value. These two propositions are (1) that the substantive results for the future development of an adequate Theory of Money and Prices varied greatly in the cases of writers each of whom insisted with equal explicitness upon establishing a connection between his version of that theory, on the one hand, and, on the other, the particular version of the "general" Theory of Value to which he acknowledged allegiance; and (2) that, as often as not, the concern of later writers with the alleged "failure" of the "classical" writers to establish such a connection has resulted only in a transference of interest from issues of substance to issues that are entirely factitious in nature.

1. *Adam Smith.* If, as has been recently suggested, "it is not easy to give a summary account" of what has been characterized as "Adam Smith's ambiguous and confused theory of value," there is no difficulty in identifying the one aspect of his "general Theory of Value" which, more than any other, may be said to justify the characterization of Smith as one of the "founders of the theory" that not only "culminated in the Ricardian economics" but also continued to serve as a framework for the "general" Theory of Value developed by Marshall, the greatest of those who, in the words of Mr. Keynes, "adopted and perfected the theory of the Ricardian economics." [69] This aspect, of course, is Smith's "combination," in the manner of Cantillon, of the theories which stressed the short-term determinants of value, on the one hand ("supply and demand," in the special "classical" sense of the terms), with the long-term determinant "cost of production," on the other—the former being related to "market" price and the latter to "natural" price.[70]

With this established, it is easy to point to the passages in the *Wealth of Nations* which provide complete justification for the statement by historians of monetary theory that Smith's discussion of the problem of the Value of Money represented nothing more nor less than a direct

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[69] The characterization of Adam Smith's "theory of value" first cited is from Roll, *History*, 158.

[70] The *locus classicus* in the *Wealth of Nations* is, of course, Book I, Chap. VII ("Of the natural and market Price of Commodities"). So far as the relation of Marshall's framework to that of Smith is concerned, it should be sufficient to cite, in this connection, the celebrated passage in Marshall's *Principles* (p. 347 of the eighth edition) in which Marshall adduced Smith's use of the concept of "natural" value in connection with his own emphasis upon "the great importance of the element of time in relation to demand and supply."

application, to this special problem, of the conclusions he had reached with respect to the "general" Theory of Value.[71]   So far as the "natural" value of the money-metals was concerned, for example, Smith laid down the proposition that, as in the case of "every other commodity," this "natural" value would be determined by the amount of "labour" which it "cost . . . to bring those metals from the mine to the market": "the proportion between the value of gold and silver and that of goods of any other kind, depends in all cases . . . upon the quantity of labour which is necessary in order to bring a certain quantity of gold and silver to market, and that which is necessary in order to bring thither a certain quantity of any other sort of goods." [72]   As with "every other commodity," also, the *market* value of silver, for example, would be determined in all cases by the demand for and the supply (or "quantity") of the metal, and by their mutual interplay.[73]   And finally, as in the case of "every other commodity," a market "price" might temporarily be established for the money metals which would differ from their "natural price"; but in all cases it would be the "natural price" that would be established after the lapse of a period of "time sufficient to produce . . . [the] full effect" of changes, say, in the conditions of production, since this "natural price" would represent "the lowest price . . . for which it is possible to bring . . . [the money metals] to market for any considerable time together." [74]

In order, however, to realize how little importance the formal "assimilation" of the problem of the Value of Money to the "general" Theory of Value may have for the substantive development of our understanding of the forces determining money prices, one has only to ask in what ways this particular "assimilation" of the two bodies of theory can be said to have advanced our knowledge beyond what was already available in the Theory of Money and Prices at the time the *Wealth of Nations* was published.   Judged from the standpoint of a formal "assimilation" of the two bodies of doctrine, Smith's statement of the problem was virtually identical with that of Cantillon.   Yet, if anything is clear from a comparison between Cantillon's and Smith's descriptions of the

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[71] See, for example, Hoffmann, *Kritische Dogmengeschichte,* 69 f.; and cf. also J. L. Laughlin, *The Principles of Money,* 237.

[72] *Wealth of Nations,* Book I, Chap. V, and Book II, Chap. II (pp. 32 and 313 of the Modern Library edition, to which all page citations refer unless otherwise indicated).

[73] On the market value of silver as being determined by the "demand" for and the "supply" of the money-metal, see especially the *Wealth of Nations,* Book I, Chap. XI, part III, including the famous "Digression concerning the Variations in the Value of Silver during the Course of the Four last Centuries" (pp. 175 f., 181, 191, 202, 210).   On the effect of the interplay of the "quantity" of and the "effectual demand" for the money-metal within a particular country, see the *Wealth of Nations,* Book IV, Chap. I (p. 404).

[74] *Wealth of Nations,* Book I, Chap. XI, part III (201, 213).

processes by which money prices are determined, it is the nature of the
consequences which followed from Smith's failure to provide anything
remotely resembling Cantillon's insistence that, while "through what-
ever hands the money which is introduced may pass it will naturally
increase the consumption," the money "will be directed more or less to
certain kinds of products or merchandise according to the idea of those
who acquire the money." [75] For it will be clear, from our discussion
in Parts Two and Three of the present volume, that in failing to see
the significance of this proposition for the problem in hand, Smith
missed what is from many points of view at once the most unforced, the
most inescapable, and the most fruitful of all the methods by which
the Theory of Money and Prices can be brought into "close contact
with the theory of value." [76] As we shall see, also, Smith's example in
this respect has been followed by a number of the writers of our own
day who have been most insistent upon ending the "double life" which

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[75] Cf., in this connection, Volume I, 307 ff., of this work, where Cantillon's
contribution is contrasted with much that, under the head of the "income
theory of prices," has been hailed as being, among other things, partic-
ularly significant from the standpoint of an assimilation of the Theory of
Money and Prices to the "general" Theory of Value.

[76] The nature of the first of the two principal sets of issues involved
is best seen by including under "the idea of those who acquire the money"
the particular type of "idea" which is dealt with by the Theory of Demand
as the latter appears within the "general" Theory of Value. The symbol
of this type of "idea" may be taken to be the demand curves for specific
commodities, other than money, of the "general" Theory of Value, and
the body of analysis with respect to the "theory of choice" which may be
said to lie behind these demand curves. The particular property of these
demand curves which in turn may be taken as a symbol for the purposes
of testing the usefulness of the curves in accounting for changes in the
structure of money prices (and therefore for all the consequences that may
follow from such changes) is the property of "elasticity," in the Marshallian
sense of the term, since it is this property which is chiefly important in
determining the conformation of derivatives of the ordinary demand curve
(such as the marginal revenue curve, for example). It is obvious, therefore,
that the chapters of the present volume which bear most directly on the
issues indicated are those included in Part Two—the first of these (Chapter
Four) being concerned precisely with "Elasticity of Demand and the
Structure of Money Prices." The nature of the second set of issues in-
volved, on the other hand, is best seen by observing (1) that the structure
of money prices is what it is not only because of the "idea of those who
acquire the money" but also because of the fact that one group of indi-
viduals may "acquire the money" rather than another group; (2) that it
is chiefly the task of *monetary* theory to explain why one group of indi-
viduals "acquires the money" rather than another; but (3) that the ele-
ments thus contributed by monetary theory are in all cases capable of
translation into movements in the demand curves of the "general" Theory
of Value or in the factors on which these demand curves depend. See
below, pp. 305 ff., 315 ff.

is alleged to have existed between the two bodies of theory.[77]   Once more, therefore, it may be said that a failure to heed the lessons of doctrinal history has resulted in an insistence upon treading again the paths that had already been shown to be circuitous bypaths, while the highroad to genuine progress lay untrodden before us.

2. *J. B. Say.* It is a commonplace, in doctrinal histories concerned with the development of "classical" economic theory, to regard Adam Smith as the common ancestor of two separate branches of "classical" doctrine, the one stemming from Ricardo, and the other, less compactly organized but nevertheless continuous in its influence, stemming from J. B. Say. It is therefore of some interest to observe that Say has been described by historians of monetary theory as having acted in this respect, as in others, as the "godfather of Adam Smith's doctrines on the continent," in that he, like Smith, insisted that money was subject to the "general laws of value." [78]   And indeed there can be no question that Say himself would have assented without the slightest hesitation to the latter proposition. "Money," he insisted, "is a commodity whose value is determined by the same general laws as that of all other commodities; that is to say, it rises and falls in proportion to demand and supply." [79]   It is likewise a commonplace of doctrinal history that Say's treatment of "demand and supply" differed from that of both Smith and Ricardo in the amount of emphasis placed upon "utility" as the factor lying behind "demand." [80]   It is worth asking, therefore, whether Say applied his "general" ideas on the subject of "utility" to the problem of the Value of Money.

Again the answer is unequivocal. For, in the first place, Say incorporated into his own argument virtually all the substantive contributions to the theory of the Value of Money that had been made by earlier writers who, like himself, have been regarded as "anticipators," or protagonists, of the "theory of subjective value." Like Law and Turgot, for example, he insisted that the selection of a commodity as the money commodity means that a "new use" has been "discovered for the commodity" and that this must affect the demand for it and,

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[77] This is true, for example, of Mr. Keynes, by virtue of his explicit rejection, in the *General Theory,* of the demand curves of the "general" Theory of Value as devices helpful in accounting for movements in "Output as a Whole," despite the considerations that can be adduced in support of the contention that the structure of money prices is precisely one of the elements on which the level of "Output as a Whole" depends. See especially Chapter Four, below.

[78] Cf. Hoffmann, *Kritische Dogmengeschichte,* 73.

[79] See Say's *Treatise on Political Economy,* Book I, Chap. XXI, sec. 3 (pp. 226 ff. of the English version cited above, p. 22, n. 55). Cf. also Book II, Chap. IV (pp. 307 ff.) of the same work.

[80] Cf., for example, Schumpeter, "Epochen der Dogmen- und Methodengeschichte," *loc. cit.,* 84; Cannan, *A Review of Economic Theory,* 197; Gray, *The Development of Economic Doctrine,* 271 f.; Bowley, *Nassau Senior and Classical Economics,* 76 ff.; Roll, *History,* 317 ff., 367.

therefore, its value.[81]   Like Law, he insisted that this monetary demand might be so "intense" as to "make paper, employed as money, equal in value to gold of the same denomination." [82]   At the same time, he insisted not only upon calling attention to the arts demand, but also, in the manner of Galiani, on pointing to the effect *upon* this arts demand of changes in the value of the metals.[83]   And like Galiani, finally, he held that the cost of production of the money metal affects its value by way of its effect upon the supply of the metal, as determined by the profitability of working given mines in the face of such changes in the value of money as might have already occurred for other reasons, including changes in the monetary and the arts demands.[84]

In view, however, of the claims that have been made in our own day for the novelty of the application of the principles of "utility analysis" to the demand for money, it is particularly worth pointing out that Say's utterances upon this head have at least as much claim to be regarded as an "anticipation" of the relevant propositions of modern monetary theory as his utterances with respect to "utility" in general have to be regarded as an "anticipation" of the relevant propositions of "modern"

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[81] See Say's *Treatise*, Book I, Chap. XXI, sec. 3 (p. 224). On Law's argument in this connection, see above, p. 19, and especially n. 44 thereto. For the statement of the same point by Turgot, who was mentioned by Say himself in this connection, see Turgot's *Reflexions sur la Formation et la Distribution des Richesses*, sec. XLV (p. 40 of the English version edited by W. J. Ashley). Of Turgot's right to a place in the history of the development of the "theory of subjective value," there can be no question (see, for example, R. Zuckerkandl, *Zur Theorie des Preises* [1889], 53 ff.). A complete history of monetary theory and its relation to "general" value theory, therefore, would certainly have to take Turgot into account. It would be very easy, however, to demonstrate—though the demonstration will not be undertaken here—that Turgot's case provides only another example of the absurdity of the suggestion that "economists" in general have allowed a serious hiatus to develop between monetary theory, on the one hand, and the "general" Theory of Value, on the other. In the present instance, moreover, such a demonstration would have the effect only of showing again that writers who are often regarded as having held widely differing "general" theories of value, and who were equally explicit in insisting that their theory of the Value of Money was only on application of their "general" Theory of Value, nevertheless managed to come to an identical conclusion when they confined their discussion to the issues of substance involved—in this case, the effect of the monetary demand for the money metal upon its value. See, for example, what was said on this matter by Ricardo—not usually cited as a protagonist of the "theory of subjective value"!—in his *Letters to Malthus*, 9 f.

[82] Say, *Treatise*, 226; cf. also Book I, Chap. XXII (p. 281) of the same work. Cf. Law, *Money and Trade Considered*, Chap. VII (I, 120 ff. of Harsin's edition of Law's *Oeuvres Complètes*).

[83] Say, *Treatise*, 225. For Galiani's argument on this point, as well as on the point indicated in the following sentence of the text, see above, p. 23, and n. 59 thereto.

[84] Say, *Treatise*, 225 n., and also 310 n.

value theory.   For Say started, as the corresponding group of "modern" monetary theorists have started, from the proposition that the "nation . . . is but an aggregate of many individuals," and that therefore any discussion of the social "demand" for money must begin with an examination of individuals' "demand" for money.[85]   And long before futile discussions with respect to the applicability of the concept of "utility" to the demand for money had diverted the attention of economists from questions of substance to questions purely factitious in nature, Say protested against the suggestion (advanced, in his own day, by G. Garnier) that the question of the "utility" of money, as such, could be disposed of by the proposition that money, as such, "does not directly and immediately satisfy a want or procure an enjoyment." On the contrary, Say insisted, the "fitness" of the precious metals, for example, to "act as money" is "part of the utility . . . wherein originates their value." [86]   Clearly, therefore, if there was a "classical" economist who was guilty of allowing a hiatus to exist between his formal theory of the Value of Money, on the one hand, and his "general" Theory of Value, on the other, that economist was not J. B. Say.[87]

3.   *Ricardo*.   If, however, we are to believe Professor Cannan, this is precisely what must be said of Ricardo, the arch-"classical" economist, in Mr. Keynes's understanding of the term.   "It seems impossible," wrote Professor Cannan, "to avoid the impression that he [Ricardo] did in fact keep his theories of the value of currency so to speak in a different side of his head from that occupied by his general theory of value." [88]   Yet if anything is certain it is that this is precisely *not* the "impression" which Ricardo himself intended to convey.   As early as 1811, for example, in the Appendix to his *High Price of Bullion*, Ricardo characterized the view "which considers coin and bullion as things essentially differing in all their operations from other commodities" as merely a "deep-rooted . . . prejudice." [89]   Both on this occasion and in a later letter to Malthus, moreover, he chided those who, "after having requested their readers to consider money and bullion merely as commodities subject to 'the same general principle[s] of supply and demand which are unquestionably the foundation on which the whole superstructure of political economy is built,' " proceeded "to forget this

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[85] Say, *Treatise*, 228. For "modern" examples of the same position, see the references to Walras and Menger given on p. 418 of Volume I of the present work. It may be noted also, however—as a partial commentary upon the significance of the relation between the theory of the Value of Money and the "general" Theory of Value—that more than a century and a half earlier Petty had made a comment virtually identical in *substance* with that quoted here from Say, without benefit of the idea of applying notions of "utility" to the demand for money. Cf. Vol. I, 418.

[86] Say, *Treatise*, 228.

[87] On the suggestion that such a "hiatus" is represented by Say's Law of Markets, see below, pp. 95 f., and nn. 15 and 16 thereto.

[88] Cannan, *A Review of Economic Theory*, 182.

[89] See Ricardo's *Economic Essays*, edited by Gonner, 45.

recommendation themselves," and, instead of considering "money only as a commodity, and subject to the same laws of variation in value from demand and supply as other commodities, seldom proceed far in their reasoning about money without showing that they really consider money as something peculiar, varying from causes totally different from those which affect other commodities." [90]   It is therefore of some importance to consider the nature of the reasoning which led Professor Cannan (as it had led earlier commentators on Ricardo's theory of the Value of Money) to the conclusion quoted above.[91]

When this is done, however, what emerges is, not a bill of indictment against Ricardo on the ground of inconsistency, but a basis for arguing that he showed a degree of good sense in the application of his "general Theory of Value" to the problem of the "value of the currency," which a number of later writers would have done well to emulate.   Ricardo did believe, as did so many of his predecessors, that cost of production is a factor of very great importance in the determination of the value of the money metals *when those metals are freely produced under conditions involving computation of profit and loss, and, being subject to free coinage, are added without limitation to the stock of money of ultimate redemption;* just as he believed that cost of production is a factor of very great importance in the determination of the value of "other commodities" than the money metals, whenever these "other commodities" belong to the class of commodities "on the production of which competition operates without restraint." [92]   Both propositions, it will be observed, are propositions of substantive content, the accuracy of which, when stated as they have just been stated, no one can deny.

On the other hand, it is a tribute to Ricardo's good sense that he made no attempt, in his "general" Theory of Value, to insist upon the importance of "cost of production" as a factor determining the value of those "ordinary" commodities which are not in fact being "produced" at all, or are being produced under conditions in which the importance of cost of production is overshadowed by the importance of other factors which, as Marshall argued, Ricardo included, explicitly or implicitly, in his analysis but which he ordinarily subordinated to "cost of production," since he regarded the latter as of more importance over

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[90] Ricardo's *Economic Essays,* 45; *Letters to Malthus,* 72 f.   Cf. also pp. 9 f. of the *Letters to Malthus.*   It may be observed that Cannan, in the passage cited above, p. 32, n. 88, makes no reference to any of these utterances.

[91] For an earlier example of the accusation that Ricardo's theory of the Value of Money and his "general" Theory of Value were "inconsistent," see Laughlin, *The Principles of Money,* 240.   Cannan's interpretation of Malthus as having advanced the same charge of "inconsistency" against Ricardo (Cannan, *Review,* 181 f.) is not unfounded, though Malthus himself did not argue in precisely these terms.   Cf. Malthus's *Principles of Political Economy,* Chap. II, sec. 3 (p. 73 of the second [1836] edition).

[92] Cf. Cannan, *Review of Economic Theory,* 182, and the reference to Ricardo's *Principles* given in n. 2 thereto; and see also the following note.

longer periods.[93]   And it is equally a tribute to the good sense of
Ricardo as a monetary theorist that, unlike later and more "consistent"
supporters of cost-of-production theories of the Value of Money, he
explicitly refrained from falling back on forced constructions for which
little could be said other than that they provided a spurious "con-
sistency" which did more to conceal than to reveal the true nature
of the factors involved.[94]

For Ricardo introduced the element of cost of production as a factor
affecting the Value of Money in the one case in which it has undoubted
validity—namely, that in which cost of production can be shown to
affect the supply of metallic money of ultimate redemption—and turned
to other factors whenever observation and common sense showed that
"cost of production" could not be a principal factor affecting the supply
of "money," or even necessarily an operative factor altogether.   In so
doing, he may be said to have established a principle which, as we shall
see, has unfortunately not always been honored by later writers on the
relation between monetary theory and the "general" Theory of Value:
namely, the principle that a desire for formal symmetry must never
be allowed to obscure the true nature of economic processes, or to lead
one to confuse the provision of mere elegance in the restatement of
results already familiar, with a definitive advance in the substance of
our knowledge of the processes by which money prices are determined.
Under the circumstances, surely, to adduce Ricardo's theory of the
"Value of the Currency" as a revelation of the alleged internal "incon-
sistencies" in his "general" Theory of Value, without asking whether

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[93] It may be observed here that Cannan did not do justice to Ricardo's
admittedly brief treatment of the cases in question by suggesting (*Review,*
182) that the only cases that Ricardo had in mind were those involving
"monopolized things."   In the second of the passages cited by Cannan, for
example (*Review,* 182, n. 3), Ricardo was quite explicit in saying that the
cases in which prices could be said to depend "solely on the proportion of
supply to demand" would include not only the case of "monopolized com-
modities" but also that *"of all other commodities for a limited period"*
(p. 376 of the Gonner edition of Ricardo's *Principles* [italics mine]).   It
will be recalled, moreover, that the condition that the commodities whose
value was held to be determined by their cost of production are only
such commodities "as can be increased in quantity by the exertion of human
industry and on the production of which competition operates without
restraint," was a condition laid down by Ricardo as a limit to his enquiry
at the very outset.   See Chap. I, sec. 1 of Ricardo's *Principles* (p. 7 of the
Gonner edition).

[94] The only case known to me in Ricardo's writings in which there is
even an appearance of a striving after such a spurious "consistency" is that
in Chap. XXVII of Ricardo's *Principles,* in connection with the matter of
seigniorage.   It is, however, perfectly possible to interpret the passage
otherwise than as an "attempt to bring [other than full-valued metallic]
currency under the quantity of labour theory" (Cannan, *Review,* 181); and
it may be observed that Cannan himself regards the passage, even on his
own interpretation of it, as an "aberration" from Ricardo's general prac-
tice.

his use of devices taken over from the "general" Theory of Value resulted in propositions of sufficient substantive accuracy to stand on their own feet as descriptions of the processes involved in the determination of money prices, is to provide merely another instance in which a concern with the "assimilation" of the two bodies of theory has succeeded only in drawing interest away from issues of substance to issues of whose essentially factitious nature there should never have been any doubt.

There is, however, another matter which should be touched upon before leaving Ricardo. In earlier parts of this chapter it was pointed out that, while our formal discussion has proceeded on the basis of an attempt to discover the extent to which economists of standing have applied to the problem of the Value of Money the analytical apparatus represented by their "general" Theory of Value, this is by no means the only, or necessarily the most important, way in which a *modus vivendi* may be, or has been, established between the Theory of Money and Prices, on the one hand, and "general" economic theory, on the other.[95] There is, after all, the broader and in every respect more

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[95] See above, pp. 5 ff., 8, and 25. It may, indeed, be suggested that much of the material presented in the present chapter and the one following is made irrelevant to current controversy by the fact that Mr. Keynes's own interest in establishing a *modus vivendi* between the two bodies of theory was directed toward issues other than those raised by the application to the problem of the Value of Money of the analytical apparatus represented by the "general" Theory of Value. By way of answer, however, it may be pointed out (1) that the methodological issues involved are essentially the same in all cases, in the sense that the test to be applied to a given "assimilation" of the two bodies of theory is that of determining how far such an "assimilation" represents a genuinely substantive advance over what was already available for our understanding of the forces determining money prices; (2) that supporters of the claim of the *General Theory* to have effected an "assimilation" have not made the distinction indicated above—as is evidenced, for example, by their characterization of Mr. Hicks, who has been concerned primarily with the application to the problem of the Value of Money of the apparatus of the general Theory of Value, as a writer who, with Mr. Keynes, has been able to "impart a new unity to the theory of value and the theory of money" (*Economic Journal*, XLIX [1939], 204); (3) that Mr. Keynes himself has not made the distinction in question, as is evidenced by his inclusion, in the *et hoc genus omne* of concepts alleged to indicate a hiatus between the Theory of Money and Prices and the "general" Theory of Value (*General Theory*, 292), of concepts such as "the quantity of money" and "the velocity of circulation of money relatively to the volume of transactions"—in other words, those very concepts which had been regarded by some earlier writers as superseded by these writers' application of the conceptual apparatus of the "general" Theory of Value to the theory of the Value of Money; and (4) that certain aspects of Mr. Keynes's alleged "assimilation" of the two bodies of theory involve just such an application—as when he applies concepts such as "elasticity of substitution" and "elasticity of production" to money (see below, pp. 628 ff., and 663 ff.).

fundamental question of the rôle assigned by the economists in question to money as a factor *affecting the functioning of the economic system*. And since Ricardo is the fountainhead of the "Ricardian" economics which Mr. Keynes identifies with "classical" economics, it would be well to comment briefly on the question whether, and to what extent, Ricardo may be said to have lived the kind of "double life" in this respect which Mr. Keynes has accused economists in general of having lived.

As it happens, it is possible to quote in this connection the judgment of a writer of our own day who certainly cannot be accused of a blind adoration of Ricardo and all his works. "Ricardo," Wesley Mitchell has insisted, was "acutely sensitive" to certain of the "complications" that "the use of money introduces into economic problems. . . . He did not abstract from the use of money." [96] As long, to be sure, as Ricardo was "focusing his attention upon other subjects, he supposed that money was invariable in value, that all changes in prices came from the commodity side of the equation"; but it is to be observed that "with unwonted care, Ricardo several times recalled this supposition to the attention of his readers." [97] It was through the use of this device that "he kept his problems simple enough to be managed, and yet let his capitalists, laborers, and landlords behave like real men and calculate in money. A very large portion of Ricardo's general theory runs thus on the pecuniary level. . . . In short, Ricardo treated 'the money surface of things' not as a distorting veil to be pushed aside, but as part of the subject to be investigated." [98]

There are those, undoubtedly, to whom this will seem much too generous a judgment.[99] That Ricardo did have his blind spots in

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[96] W. C. Mitchell, "Postulates and Preconceptions of Ricardian Economics (1929); pp. 216 ff. of the version reprinted in the same author's *The Backward Art of Spending Money and Other Essays* (1937).

[97] See Mitchell, "Postulates, etc.," *loc. cit.,* 217, and especially n. 45 thereto. The list of passages in which Ricardo called attention to what he referred to as his "supposition of a medium [of exchange] which shall itself be invariable" could be considerably extended. In addition, for example, to the passages in Chap. One, secs. VI and VII and in Chap. Six of Ricardo's *Principles* (pp. 38, 40, 87 n. of the Gonner edition), some of which are cited by Professor Mitchell, see the *Letters of David Ricardo to John Ramsay McCulloch,* 64, 168 ff., and Ricardo's *Notes on Malthus,* 35, 142.

[98] Mitchell, "Postulates, etc.," *loc. cit.,* 217 f.

[99] It may be pointed out that one of the reasons for this conclusion may well be the fact that Professor Mitchell, following the example set in his earlier essay on "The Rôle of Money in Economic Theory" (likewise reprinted in *The Backward Art of Spending Money,* 149 ff.), includes, under the head of analysis running in the "pecuniary" level, analysis concerned with the incentive to make "money," in the sense of making *profits* (cf. also Mitchell's *Business Cycles: The Problem and its Setting* [1927], 106); whereas it can certainly be argued that, while there are both historical and logical connections between the two types of "pecuniary"

dealing with the effect of money on *certain* economic processes, there can be no question—the most noteworthy example, in this connection, being his treatment of the effect of monetary expansion and contraction upon the level and the structure of output as a whole.[100]   This, however, is a very different thing from suggesting that Ricardo lived the kind of "double life" which classical-"Ricardian" economists in general have been charged with living.   No one could suggest, for example, that Ricardo's theory of international trade and international prices was developed in complete disregard of the effect of the working of the monetary mechanism upon the relevant economic processes.[101]   And

fact, the area covered in the two cases is by no means necessarily coextensive. Cf., in this connection, what is said below, p. 73, n. 54.   On the other hand, it is only fair to point out that Professor Mitchell's citations of Ricardo in this connection include much more than illustrations of Ricardo's emphasis upon "pecuniary" *motives,* in the sense indicated; and it may be pointed out further that Professor Mitchell might have included more instances of Ricardo's "sensitiveness" to the importance of money, of a kind which would make them directly relevant to the present discussion. See, for example, the references to Ricardo's treatment of the relation between money and interest, given below, p. 38, n. 102.

[100] An extended discussion of Ricardo's shortcomings in this respect (cf. J. Viner, *Studies in the Theory of International Trade* [1937], 195 ff.) must be left for another occasion.   For another occasion, also, must be left a demonstration of a further proposition: namely, that the mere fact that Ricardo was blind to the importance of *monetary* factors for fluctuations in "output as a whole" does not mean that he was unwilling to consider either the possibility and the reality of fluctuations in output as a whole, and that he made no attempt to provide an explanation for such fluctuations on *non*monetary grounds.   This in itself provides a commentary on the usage, by Mr. Keynes (see, for example, the *General Theory,* p. vi), which would seem to suggest that the area covered by the "theory of output as a whole" is coextensive with that covered by "monetary theory"—or at least with that part of "monetary theory" which is concerned with the effect of monetary expansion and contraction upon output —instead of overlapping it at several points, in such wise that writers who may not have contributed to the construction of an adequate "theory of a Monetary Economy" (*General Theory,* 293) may nevertheless be regarded as having contributed to the "theory of output as a whole," and vice versa.

[101] Ricardo's contributions to this sector of the Theory of Prices are too well known to require further specification.   In view, however, of (1) the greatly lessened emphasis, in Keynes's *General Theory,* as contrasted with that in his *Treatise,* on the necessity for working with a "plurality of price levels" (cf. below, pp. 155 ff.); and (2) the fact that, as we shall see in Parts Two and Three of the present volume [cf. especially pp. 320 ff. and 601 ff.], the concept of a "plurality of price levels" represents one of the most promising, as well as one of the most natural, bridges between the Theory of Money and Prices, on the one hand, and the "general" Theory of Value, on the other, it would be well to call attention to what is said in Volume I of the present work (p. 503, and especially n. 53 thereto) with respect to Ricardo's treatment of the "plurality of price levels" in the theory of international trade.

it is something of a commentary on the carelessness with which the sponsors of the myth of a "double life" have treated the facts of doctrinal history that, in his treatment of the relation of monetary phenomena to the *rate of interest,* Ricardo adopted explicitly the substance of those propositions which have made Mr. Keynes regard Wicksell and Fisher, respectively, as his intellectual ancestors in regarding money as a "real" factor affecting the determination of, and the consequences of changes in, the rate of interest.[102]

The finding, therefore, that Ricardo's treatment of the effect upon output of monetary expansion and contraction was unsatisfactory cannot be assigned any significance beyond the fact that he arrived at a series of wrong conclusions with respect to this specific problem. He certainly did not reach these conclusions on the basis of a separation, on methodological grounds, of "monetary" theory, on the one hand, and "general" economic theory, on the other. That the conclusions in question did not follow from certain essential "postulates" of the Ricardian system is demonstrated, indeed, by a further significant fact: namely, that no less a person than J. R. McCulloch, the most intransigently Ricardian of the "Ricardians" in so many other respects, adopted without substantial reservation the position of Hume concerning the effect of monetary expansion upon output, with its crucial emphasis on "money demand" as a factor in the problem.[103] Surely this fact is

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[102] For Keynes's comment on Wicksell and Fisher in this connection, see above, p. 7, and the reference given in n. 11 thereto; also below, pp. 97 ff. and 109 f. On Ricardo as an "anticipator" of what is in many respects the heart of Wicksell's doctrine with respect to the relation between the rate of interest and monetary phenomena, see Volume I of the present work, 173 ff., 191, and the references there given. For an example of Ricardo's use of a distinction analogous to Fisher's "distinction between the money rate of interest and the real rate of interest where the latter is equal to the former after correction for changes in the value of money" (Keynes, *General Theory,* 142), see Ricardo's "Letter to *The Morning Chronicle* on the Bullion Report" (1810), as reprinted in Ricardo's *Minor Papers on the Currency Question,* 69, where Ricardo drew a distinction between changes in the (money) "rate of interest," on the one hand, and, on the other hand, changes in "the value of that interest" as a result, say, of a "fall in the prices of . . . commodities."

[103] See, for example, McCulloch's *Treatise on the Principles and Practical Influence of Taxation and the Funding System* (1845), 362; also his article on the "Precious Metals" in the eighth (1859) edition of the *Encyclopædia Britannica,* XVIII, 476. That McCulloch himself did not believe that his argument with respect to the effect, upon output as a whole, of monetary expansion and contraction was in conflict with any central "postulate" of the Ricardian "system," as such, is sufficiently evidenced by the fact that McCulloch must have been aware of Ricardo's slighting treatment of Hume's argument in the *Essay on the Influence of a Low Price of Corn on the Profits of Stock* (387 f. of McCulloch's edition of Ricardo's *Works;* 248 f. of Gonner's edition of Ricardo's *Economic Essays*). The episode is therefore something of a commentary on that variety of doctrinal history which undertakes to summarize the position

worthy of particular note when one considers that it was precisely Hume's position on this matter which has led Mr. Keynes to confer upon Hume the accolade implied by the suggestion that Hume was in this respect "enough of a mercantilist" to refrain from adopting certain conclusions of the "classical school" which Mr. Keynes himself regards as having created "a cleavage between the conclusions of economic theory and those of common sense." [104]

4. *Senior*. There has been some discussion, in recent years, as to the sense in which a writer such as Nassau Senior can be regarded as a "classical" economist.[105] That he was a "classical" economist in Mr. Keynes's sense of the term there can be no doubt, just as there can be no doubt that he would have to be regarded as a "classical" economist under any definition of the "classical school" which would "include all those economists before Jevons who drew inspiration directly or indirectly from Adam Smith." [106] And even if the criterion of the "classicism" of a given writer were the extent to which he followed literally what *Ricardo* had to say on a given subject, then Senior was more Ricardian than even a "Ricardian" as supposedly orthodox as James Mill in protesting, as Ricardo had protested, against the suggestion that "the value of money is decided by causes differing from those which decide the value of other commodities." [107] "Mr. Mill," Senior admitted, "does not say in so many words that the value of money is decided by causes differing from those which decide the value of other commodities"; but, he insisted, "such is, in fact, the result" of the way in which Mill had stated the problem of the Value of Money, when this statement is compared with Mill's discussion of the forces determining "Exchangeable Value" in general. According to Senior, there could be no question of drawing a sharp contrast between the two cases: the value of metallic money, for example, he insisted, "is governed by the same rules as those which govern the value of all other commodities." [108]

Senior, of course, was not a strict Ricardian in the amount of emphasis

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of a given "school" of economists on a specific subject, on the basis of a set of "postulates" attributed to (though not explicitly stated by) the school as a whole, instead of on the basis of a careful study of the writings of the individual members of the "school" in question. See, in this connection, the sensible remarks on "the search for postulates," in Mitchell's "Postulates and Preconceptions of Ricardian Economics" (*loc. cit.*, 203 ff.), also what is said on this matter below, p. 65, n. 31; and contrast, for example, the remarks on the "Ricardian" position with respect to the effect of monetary expansion and contraction upon output in V. Wagner, *Geschichte der Kredittheorien* (1937), 33 f.

[104] Cf. the *General Theory*, 343, n. 3, and 350.

[105] See, for example, Bowley, *Nassau Senior and Classical Economics*, 16 ff.

[106] So Bowley, *Nassau Senior*, 17.

[107] See Senior's *Three Lectures on the Value of Money* (1840), 8. For the protest of Ricardo to which reference is made in the text, see above, pp. 32 f., and the references given in nn. 89 and 90 thereto.

[108] Senior, *Three Lectures on the Value of Money*, 9.

that he placed upon "utility" among the factors determining value.[109]
The question that arises, therefore, is whether Senior applied this same
kind of emphasis in his discussion of the forces determining the Value
of Money. Again the answer is unequivocally that Senior was, if any-
thing, more explicit in his application of the emphasis in question than
Say had been. Like Say, and like others who have been regarded as
"anticipators" of the "theory of subjective value," Senior insisted upon
the fact that the "use as money" of gold, for example, is a "cause of
the utility of gold" over and above its "utility" in the arts, and that
an adequate discussion of the demand for the money metals must do
justice to the relative magnitude and the mutual interaction of the two
"demands," by way of their capacity to effect, and to respond to,
changes in the value of the money metal.[110] Like Say, moreover, and
indeed like many of the modern "cash-balance" theorists who have
consciously undertaken to apply the methodological principles of
"modern" value theory to the special problem of the Value of Money,
Senior insisted upon referring the problem of the demand for money
back to a study of the actions of economizing individuals: for, he wrote,
"it is obvious . . . that the whole quantity of money in a community
must consist of the aggregate of all the different sums possessed by the
different individuals of whom it is constituted." [111]   And like Galiani,
Say, and others among the abler "anticipators" of the "theory of subjec-
tive value," Senior, instead of arguing that the cost of producing a money
metal such as gold is of no significance for the determination of its
value, followed strictly the "classical" pattern in arguing that it was
precisely upon the "cost of its production" that the value of gold would
"depend *permanently*," by way of the effect of cost of production upon
the supply of gold, through its determination of the profitability of
working given mines in the face of whatever changes in the value of
gold might have already occurred for other reasons, such as "the joint
[that is, "composite"] demand for plate and money." [112]
    It should be clear, from this summary account, that Senior may be

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[109] See again the familiar comments by Jevons, on this aspect of
Senior's work, in the former's *Theory of Political Economy* (pp. 43, 53 f.
of the fourth [1924] edition); in his *Primer of Political Economy* (1878),
17; and in his *Principles of Economics*, 1 ff.; and cf. Zuckerkandl, *Zur
Theorie des Preises*, 75 f.; Bowley, *Nassau Senior*, 66, 95 ff.; and Roll,
History, 343 ff.

[110] See Senior's *Three Lectures on the Value of Money*, 23 ff., 51 ff.,
76 ff. Cf., in this connection, the references given to Say and others on
pp. 31 f., above; but see also the reference to Ricardo's treatment of the
effect of the monetary demand for the money metal upon its value given
above, p. 31, n. 81.

[111] Senior, *Three Lectures on the Value of Money*, 11 ff. Cf. the refer-
ence to Say given above, p. 32, n. 85.

[112] Senior, *Three Lectures on the Value of Money*, 30, 33 ff., 49, 55 ff.
Cf. the references to Galiani and Say in this connection, given above, pp.
23 f. and 30 f.

regarded as one of the writers on the relation between the problem of
the Value of Money, on the one hand, and the "general" Theory of
Value, on the other, who actually succeeded in deriving, from their
attempts to "assimilate" the two bodies of doctrine, a series of proposi-
tions which, though they were not all of an equal degree of novelty, can
nevertheless be said to stand on their own feet as contributions to our
understanding of the forces determining money prices.[113]   Yet it is
of the utmost importance to emphasize that what gives positive signifi-
cance to Senior's treatment of the problem of the Value of Money is
precisely the fact that these propositions *can* stand on their own feet,
instead of having no claim to our attention other than that they
represent an "assimilation" of the problem of the Value of Money to the
"general" Theory of Value. For, if it is true that Senior, as a result
of his desire to effect such an "assimilation," reached results of whose
substantive correctness there can be no doubt, it is also true that, in the
attempt to pursue this desire in the solution of other problems, he
reached results which are extremely dubious. This much must be said,
for example, of that part of his treatment in which he argued, in the
manner of so many other writers who have insisted that money is a
"commodity" whose value is "decided" by the same causes which "decide

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[113] Among the features of Senior's treatment, apart from those already
indicated, which may be regarded as having a claim to comparative
"novelty" by reason of the freshness and the articulation of the exposition,
may be mentioned particularly: (1) his discussion of "the causes which
determine what proportion of the value of his income each individual
shall habitually retain in money," with its clear realization that this in-
cludes the problem of "hoards," which in turn was related to the phenome-
non of monetary velocity; and (2) his discussion of the "causes which
actually decide the cost at which" a given money metal (say, silver)
"shall be produced." See Senior's *Three Lectures on the Value of Money,*
11 ff., 57 ff. Even in these matters, however, it would be easy to exag-
gerate the uniqueness of Senior's discussion, even in its own day. On the
first point, for example, see Henry Vethake, *The Principles of Political
Economy* (Philadelphia, 1838), 141 f., in which the author not only pre-
sented the proposition that "a diminished demand for money implies an
augmented rapidity in the rate of its circulation, and, on the other hand,
that an augmented demand for it implies a diminished rate of circulation"
(the "demand for money" being "said to have become greater, when
people generally are more disposed than they previously were to retain
it in their possession for future use"), but in which he also presented it as
a proposition so "obvious" as hardly to merit extended discussion. It may
be noted also that Vethake believed that in so arguing he was using "the
term demand, in reference to money, in a sense . . . perfectly analogous
to its ordinary acceptation when we speak of commodities other than
money" (p. 141). As in the case of Senior, Vethake also believed that he
was providing a further confirmation of the proposition which he himself
believed to be "of so much importance": namely, that there should be
no hesitation in applying to the special case of the Value of Money "the
established principles concerning the exchangeable values of commodities
in general" (p. 133).

the value of other commodities," that "the value of the precious metals, as money, must depend ultimately on their value as materials of jewellery and plate; *since, if they were not used as [*"ordinary"*] commodities,* they could not circulate as money." [114]

Similarly, Senior's criticism of James Mill's formulation, as implying that "the value of money is decided by causes differing from those which decide the value of other commodities," becomes something more than a mere bit of formalism by virtue of the specific content which was added to the elder Mill's formulation by Senior's own insistence upon describing the nature of (1) the *forces determining* the "quantity" of metallic money (cost of production), and (2) the *forces determining* the "demand" for the money metal (the emphasis on the arts demand and the "cash-balance approach" to the problem of the monetary demand). But Senior was less explicit than he might have been in making clear that what he was doing was *adding* to a formulation such as that of the elder Mill, which ran in terms of the "quantity" of money, its "velocity," and so on, instead of *contradicting* such a formulation.[115] To Senior's credit, it must be said that his exposition in this respect is much less objectionable than that of many later "assimilators" of the two bodies of doctrine.[116]   Yet there can be little doubt that one must

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[114] Senior, *Three Lectures on the Value of Money,* 17 (italics mine). Cf. also the passage from Senior's lectures of 1826–1830 quoted by Bowley, *Nassau Senior,* 205. In this respect, of course, Senior was merely providing a further example of the blight that has been associated with discussions of the "commodity" character of money from the days of Aristotle to the present. See above, pp. 11 ff. and 19.

[115] See, for example, *Three Lectures on the Value of Money,* 55, in which Senior's implied disapproval of "the opinion that the value of money depends on its quantity" turns out to amount to no more than an insistence upon considering the nature of the forces *affecting* the "quantity" of money over longer periods—or, as Senior put it, "the comparative force of the obstacles by which the supply is limited." See also what is said in the following note with respect to Senior's treatment of "rapidity of circulation."

[116] It may be observed, for example, that Senior did not—at any rate, in any of his *published* writings known to me—characterize his criticism of Mill as a "criticism of the *quantity theory* of Money" (contrast Bowley, *Nassau Senior,* 213 f.). He avoided, therefore, the disastrous confusion that has followed from an identification of the "quantity theory" with that type of "quantity *equation*" of which Mill's exposition may be regarded as a nonalgebraic rendering. Nor can I find any evidence in Senior's published writings of an alleged "refusal" by Senior "to consider the velocity of circulation as one of the determinants of the value of money" (Bowley, *Nassau Senior,* 215), in a sense of the word "determinant" which would permit anything but "cost of production" to be regarded as a "determinant" (cf. Senior's *Three Lectures on the Value of Money,* 30). Indeed, there is no clear evidence that Senior even wished "to eliminate from the problem the . . . element of the rapidity of circulation" on the ground that it is "extraneous" (Bowley, p. 214). He did, to be sure, point out that there was nothing in James Mill's "general"

take into account the particular aspect of Senior's argument to which attention has been called in any attempt to obtain a judgment of the extent to which the desire to effect such an "assimilation" has been an unmixed blessing, when viewed from the standpoint of its effect in aiding the substantive development of monetary theory.

5. *J. S. Mill.* Mr. Keynes has followed the usual practice of putting John Stuart Mill at the head of the list of those who are to be regarded as having "adopted and perfected the theory of the Ricardian economics" which Mr. Keynes himself identifies with "classical" economics, and against which the argument of the *General Theory* is supposed to be directed. More than a little interest, therefore, attaches to the question whether the younger Mill was guilty of arguing, as Senior had accused James Mill of arguing by implication, that "the value of money is decided by causes differing from those which decide the value of other commodities." As it happens, the evidence with respect to the younger Mill's explicit intentions in the matter is so unequivocal that it has not been ignored even by those writers who have otherwise shown no hesitation in advancing propositions with respect to the existence of a "hiatus" between the two bodies of theory which, even upon the basis of the material presented thus far, must be regarded as completely without foundation.[117] "How the Exchange Value of money . . . is determined," said Mill, "is not a question of any difficulty, when the illusion is dispelled, which caused money to be looked upon as a peculiar thing, not governed by the same laws as other things. Money is a commodity, and its value is determined like that of other commodities,

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Theory of Value corresponding to "rapidity of circulation"; but Senior's own subsequent argument is such as to suggest that he wished here merely to argue, in the manner of the ablest among later "cash balance" theorists, that it is necessary to provide a type of analysis which will indicate the *"causes that govern* the rapidity of circulation of the currency" (see Volume I, 418 f. of the present work). For, in his positive analysis of the forces determining the "demand for money," Senior repeatedly referred to "rapidity of circulation"—or, as he sometimes put it, the number of times money "changes hands"—as one of the factors *affecting* this demand (Senior, *loc. cit.,* 14 f., 21 ff., 26). The other factors affecting the demand for money, according to Senior, were either those which might be included under the equivalent of the Fisherine T (as Senior put it, those affecting the extent of the "use of money in exchange," Senior, *loc. cit.,* 12 ff., 16, 20, 26 f.); or, in the case of the "demand" for money of ultimate redemption, those summarized, in the notation suggested in Volume I of the present work, by the ratio M'/M_r (Senior, *loc. cit.,* 26 ff.). The very fact, therefore, that Senior *distinguished* the factors affecting the monetary "demand for gold" which are summed up under the head of "rapidity of circulation" from *other* factors affecting this monetary demand (see, for example, p. 26 of the work cited) may be taken as showing that while he regarded the framework thus provided as *only* a framework, he did not regard the framework itself as "extraneous."

[117] See, for example, Anderson, *The Value of Money,* 46 f., 61; and cf. above, p. 4, n. 4.

temporarily by demand and supply, permanently and on the average by cost of production." [118]

The mere fact, to be sure, that the younger Mill insisted upon thus "assimilating" his theory of the Value of Money to his "general" Theory of Value is no proof that the substantive *details* of his analysis of the forces determining the Value of Money were identical with those of, say, Senior, who had likewise insisted upon such an "assimilation." Indeed, as we have seen, this is precisely one of the reasons why so little significance is to be attached to the mere fact that a given writer did or did not undertake to "assimilate" the two bodies of doctrine.[119] One ought, therefore, to have welcomed such discussion of the details of the younger Mill's analysis as would have brought out clearly the differences between them and the details of the positive argument of a writer such as Senior, as well as the points of agreement between them. Unfortunately, however, most of the discussion of this matter that has heretofore taken place, instead of emphasizing the points of substantive agreement between the younger Mill and other writers who have insisted upon "assimilating" the theory of the Value of Money to the "general" Theory of Value, has exaggerated the points of difference between them; and, instead of showing how such genuine differences as existed resulted in different types of analysis each of which can be regarded as supplementing the other, this later discussion has succeeded only in strengthening the conclusion that Senior's criticism of the formulation of James Mill encouraged a tendency toward an exclusive formalism the consequences of which were anything but happy.

That this discussion, in its desire to emphasize the differences between the details of Senior's argument, on the one hand, and that of the younger Mill, on the other, has in fact underemphasized the points of agreement between the two writers, is clear from a direct examination of their respective arguments. Mill, for example, not only quoted with approval what may be regarded as Senior's summary statement of the forces which, in the absence of barter and credit, would determine the community's demand for money (the "quantity wanted" by a community), but regarded it as so unquestionable as to stand in no "need of any further illustration." [120] Similarly, Senior, as we have seen, so far from

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[118] Mill, *Principles,* Book III, Chap. VII, sec. 3 (p. 488 of the Ashley edition). Cf. also Book III, Chaps. VIII and IX of the same work (especially pp. 490, 498 ff., and 504 ff. of the Ashley edition).

[119] See, for example, what is said above, pp. 19 f. and 28 ff.

[120] See Mill's *Principles,* Book III, Chap. IX, sec. 3 (p. 505 of the Ashley edition); and cf. Senior's *Three Lectures on the Value of Money* (obviously the "printed, but not published, Lectures of Mr. Senior" to which Mill refers), 21. The statement to which both Senior and Mill thus assented—namely, that "the quantity wanted would depend partly on the cost of production, and partly on the rapidity of its circulation"—will, of course, seem to modern eyes to be very quaintly phrased; but when it is remembered that both Senior and Mill were agreed that "cost of production" will affect prices by affecting the relative quantities of gold

having regarded the framework provided by concepts such as "rapidity of circulation" and the equivalent of the Fisherine T as being "extraneous" to the problem of the "demand" for money, actually made use of just this framework; and it is worth noting that the younger Mill went out of his way to praise the relevant parts of Senior's exposition precisely in terms of the framework which thus served as a common basis for the discussion presented by both writers.[121]   One of the chief lessons, indeed, which can be drawn from this part of the discussion between Senior and the younger Mill is a lesson that, if it had been learned in time, might have avoided an almost unbelievable amount of confusion in later discussion: namely, that, given a willingness on the part of users of different "approaches" to the problem of the Value of Money to translate their respective arguments into a common language other than one running in terms of concepts suggested by the "general" Theory of Value, the common language which lends itself more easily to the purpose is precisely that represented by the now familiar Quantity Equations, or their nonalgebraic equivalent.

No one, to be sure, could deny that there were important differences of both emphasis and substance in the respective arguments of Senior and the younger Mill. Of these differences, the one that matters most for our present purpose is that Senior was a protagonist of what has

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and of *other commodities* that will be produced, it is seen that the formulation really amounts to a rough statement of what was called, in Volume I of the present work, the "absolute" demand for money, in one of its senses (see Volume I, pp. 444 ff., and especially p. 445, n. 86)—that is, $(PT)/\ V$. See also the following note.

[121] See the footnote to sec. 3 of Book II, Chap. IX, of Mill's *Principles* (p. 505 of the Ashley edition), and cf. above, p. 42, n. 116. It is clear, therefore, that Miss Bowley's account of Mill's argument in this connection (*Nassau Senior*, 215, n. 2) is considerably less than fair. If, for example, Mill would have been prepared to say (though he did not say) that "the demand for money equals the quantity of money multiplied by the velocity of circulation," he could have meant by such a statement only that the *"absolute"* demand for money, in one sense of the term, would be determined by the magnitude of PT, after allowance for the effect upon this "absolute" demand of changes in the "relative" demand—that is, of changes that would be reflected in V. In truth, of course, Mill did not, in this instance, mean, by "demand," "quantity demanded" (cf. J. E. Cairnes, *Some Leading Principles of Political Economy Newly Expounded* [1874], 28 f.). And when his argument is translated, as it should be, into terms which make it relevant to "demand" in the latter sense, there is nothing absurd in suggesting that the magnitude of this "demand" will be in part an "effect" of that *element* in "demand" whose own magnitude will be reflected in the "rapidity of circulation" of money. For all that this amounts to is the proposition that what has been called in this work the "relative" demand for cash balances is a factor that will affect the "absolute" demand (cf. Volume I, 445, of the present work). It will be observed that there was no significant difference whatever between Mill and Senior in this respect.

been called "the holding theory of money," whereas Mill was not.[122] By "the holding theory of money," of course, is meant what has been called, in the present work, the "cash-balance approach." To readers of Volume I of the present work, it must be clear that this "approach," properly utilized, must remain an essential part of any adequate Theory of Money and Prices. Mill's failure to recognize its significance represents, therefore, a definite gap in his positive analysis; and the most that can be said against those who have criticized this part of his discussion is that they have failed to make clear that what was involved *was* a "gap," which was capable of being filled by taking over the body of cash-balance analysis outlined by Senior's predecessors and successors, and not a structural defect which made nonsense of the whole of Mill's discussion.

For it may be said, with equal justice, that Mill's comparative lack of interest in the cash-balance approach was in some degree compensated by his greater interest in what must likewise be regarded as an essential part of any adequate Theory of Money and Prices: namely, an emphasis upon the consequences of what has been referred to, in our own day, as "the mutual impact of the relevant flow of money and the relevant flow of goods," which appeared in Mill's formulation under the general heading of the money "demand for goods." [123] The question of the relation between the two notions of "demand" that are thus involved, when viewed from the standpoint of the "general" Theory of Value, is one that was raised not long after the appearance of Mill's *Principles* by J. E. Cairnes, who insisted upon contrasting what he referred to as "the quantity demanded," on the one hand, and, on the other, "the quantity of purchasing power offered in support of the desire for commodities." [124] It was Alfred Marshall who undertook to

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[122] Cf. Bowley, *Nassau Senior*, 215. This aspect of Senior's analysis had already been pointed out by a number of writers. See, for example, the reference to R. Opie in my "Léon Walras and the 'Cash-Balance Approach' to the Problem of the Value of Money," *Journal of Political Economy*, XXXIX (1931), 571, n. 4; also B. P. Whale in *Economica*, XII (1932), 473. That Mill, on the other hand, was not a "cash-balance theorist," despite the fact that, like so many other "motion-theorists," he often evidenced at least the elements of an understanding of the forces leading to the holding of money altogether (see, for example, the "Preliminary Remarks" to his *Principles* [pp. 4 f. of the Ashley edition]), is established, not so much by statements to the effect that "money, as money, satisfies no want" ("Preliminary Remarks," p. 6 of the Ashley edition), as by his unsatisfactory treatment of the distinction between money "in circulation" and money "out of circulation" (*Principles*, 490, 494; cf. Volume I, 460 ff. of the present work), and his essential lack of interest in the *forces determining* the "rapidity of circulation" of money.

[123] Cf. Mill's *Principles*, Book III, Chap. VIII, sec. 2 (pp. 491 ff. of the Ashley edition). The expression with respect to the "mutual impact of the relevant flows" is Robertson's. Cf. Volume I, 260, of the present work, and the reference to Robertson given in n. 70 thereto.

[124] See Cairnes, *Some Leading Principles*, 27 ff.

show that, within the range of problems suggested by the "general" Theory of Value, each concept of "demand" might be regarded as having its place, depending upon the nature of the problem in which we happen to be interested.[125] For our present purpose, therefore, it is of very great importance to establish the fact that precisely the same thing may be said, within the field of monetary theory, of the two notions of "demand" which are presented by (1) the *demand for money for holding purposes,* and (2) that concept which, though it has had a very long history, has received its most articulate and emphatic statement at the hands of Mr. Hawtrey in the form of the concept of "general demand." [126]

The relation between the two is, in fact, one of extreme simplicity: the "demand for money for holding purposes" is one of the *factors affecting* the dimensions of "general demand." As we shall see, the nature of the causes and the consequences of changes in "general demand" is a problem which requires, at each step of the analysis, a simultaneous concern with the elements of *both* monetary theory and the "general" Theory of Value, as well as with specific elements within the field of monetary theory which in some cases may have been arrived at as a result of a desire to "assimilate" monetary theory with the "general" Theory of Value.[127] Yet we shall see also that the writers of modern times who have done most to stress the importance of the concept of "general" demand are not those who have been concerned mainly with the type of "assimilation" of the two bodies of doctrine represented by the treatment of the "demand for holding purposes" under the categories of the "general" Theory of Value. On the contrary, they are writers who have been either indifferent or hostile to such an assimilation.[128] In most cases, moreover, they have received their inspiration from precisely the type of "stream," or "impact," formulation which derives, not from the concept of "demand" involved in exclusive "holding" theories of money, but from the concept of a money "demand for goods" which represented the principal difference between Mill's formulation and that of Senior.[129]

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[125] See Marshall's paper on "Mr. Mill's Theory of Value" (1876) (cf. especially p. 129 of the *Memorials of Alfred Marshall* as edited by Pigou); and see also Marshall's *Principles,* 97, n. 1 of the eighth (1920) edition.

[126] Cf. what is said in this connection below, pp. 120 f.

[127] See below, pp. 202 ff., 263 ff., 546, 562, 624.

[128] See below, pp. 95 ff., 117, 120 ff.

[129] It is, of course, not suggested here that Senior himself would not have been prepared to incorporate into his analysis an emphasis upon what Mill called the "demand for goods." See, for example, pp. 24 f. of the *Three Lectures on the Value of Money,* in which Senior was concerned with the amount of money "offered on every purchase," and the effect of such "offers" on the further generation of what Senior himself called "monied incomes." The point made here is merely that the type of emphasis involved in the concept of a "demand for goods" was not Senior's major emphasis, whereas it was the major emphasis in Mill's argument.

In short, we have here a perfect illustration of the way in which an excessive concern with the formal application of the apparatus of the "general" Theory of Value to the problem of the Value of Money has tended to degenerate into an exclusive formalism that has impeded, rather than encouraged, the advance of a positive monetary theory on several fronts simultaneously. *This* is the lesson which should have been drawn from an examination of Mill's "assimilation" of his theory of the Value of Money to the categories of "supply" and "demand" of "general" value theory, as well as from an examination of the criticisms advanced against Mill's "assimilation." It is a lesson of a very different kind from that which—being based either upon a misreading of the clear facts of doctrinal history with respect to the alleged lack of interest in the relation between the two bodies of theory in the past, or upon issues entirely factitious in nature—has succeeded only in encouraging the conclusion that a concern with the "assimilation" in question has, as often as not, obstructed rather than advanced the substantive progress of monetary theory.

The special position assigned to the younger Mill by Mr. Keynes among those who may be regarded as having "adopted and perfected the theory of the Ricardian economics" justifies a final comment upon his treatment of the relation between monetary theory and "general" economic theory. The comment might itself be summarized under some such head as "The Futility of Slogans." [130]   For the younger Mill had a "slogan" with respect to the rôle of money in economic theory which ought to have served in almost every respect as a horrible example of how the problem ought *not* to be discussed.

Mill's "slogan" was represented by his proposition that "there cannot, in short, be intrinsically a more insignificant thing, in the economy of society, than money." [131]   Taken by itself, the statement is clearly either

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[130] It should hardly be necessary to labor the point that "The Futility of Slogans" might provide a suitable rubric for the discussion of whole chapters in the history of economic doctrine quite remote from that under discussion here. How much, for example, of the misrepresentation of the position of the older economists on matters of general social and economic policy would have occurred if, instead of contenting themselves with the statement that the older writers followed a slogan of "laissez-faire," commentators had undertaken to ascertain just what position these economists had taken on the specific social and political questions of their day?

[131] Mill, *Principles*, Book III, Chap. VII, sec. 3 (p. 488 of the Ashley edition). Mill's "slogan" has been often quoted—usually with marked disapproval—by later writers. See, for example, W. W. Carlile, *The Evolution of Modern Money* (1901), 325 (cf. the same author's *Economic Method and Economic Fallacies* [1904], 171, and *Monetary Economics* [1912], 8); also W. C. Mitchell, "The Rationality of Economic Activity" (*Journal of Political Economy*, XVIII [1910]), 206, and "The Rôle of Money in Economic Theory" (*The Backward Art of Spending Money*, 151, 168); *Business Cycles: The Problem and its Setting*, 106, n. 3; and Hayek, *Prices and Production* [1932], 110. It may be observed that these citations can themselves be regarded as illustrating "The Futility of Slogans,"

absurd or meaningless. It is absurd insofar as its literal acceptance would leave unexplained why economists from the very beginnings of the subject down to our own day have devoted so large a part of their writings to a description of the way in which this "insignificant" thing, money, operates in "society." It is meaningless insofar as the statement acquires whatever *correctness* it has only in a given context, in which a specific meaning is assigned to the qualifying word "intrinsically," and in which the details of the specific argument that is held to justify the slogan are contrasted with the details of a given argument that is held to prove that money may be of the utmost "significance." One of the chief lessons, therefore, that should have been drawn from Mill's practice in this respect is that slogans of this type, apart from their possible value as warnings against a complacent one-sidedness in analysis, are, at best, dangerously misleading and, at the worst, positively erroneous—whether they insist, as Mill's slogan insisted, upon the "insignificance" of money, or whether, in the modern manner, they insist upon just the opposite: as in the proposition that "money is the root of economic science," or that "the trade cycle is a purely monetary phenomenon." [132]

Here, however, it is sufficient to point out that, in Mill's case, the slogan itself provides no indication whatever as to the extent to which his discussion of specific problems was invalidated by an inadequate appreciation of the importance of money for each of these problems. There can be no doubt, for example, that the younger Mill followed both his own pernicious slogan and the equally pernicious example of Ricardo in failing to do justice to the effect of monetary expansion and contraction upon output as a whole.[133] There can be just as little doubt,

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by virtue of (1) the extremely uneven quality—to put it mildly—of the results obtained as the result of a professed desire to follow a contrary slogan; (2) the extreme diversity of opinion as to the range of problems with which a truly "monetary" economics would be concerned; and (3) the fact that, as often as not, the authors concerned went on to insist (a) that, in fact, "every line that he [Mill] wrote, from first to last, was permeated by monetary assumptions" (Carlile, *Monetary Economics,* 7), and (b) that Mill's preachment, as contrasted with his actual practice, merely represented an example of the "contradiction between the letter of the economic law and its spirit" which has been alleged to have been "part of the classical tradition" with respect to the importance of money for economic theory (Mitchell, "The Rôle of Money in Economic Theory," *loc. cit.,* 151).

[132] The first of the two slogans just cited is that of W. C. Mitchell, "The Rôle of Money in Economic Theory," *loc. cit.,* 171. (Cf. Carlile, *The Evolution of Modern Money,* 325: "Money is the pivot of everything in economics.") The second is, of course, the celebrated one of Hawtrey ("The Genoa Resolutions on Currency," *Economic Journal,* XXXII [1922], 298; p. 132 of the second [1926] edition of Hawtrey's *Monetary Reconstruction*).

[133] A detailed examination of the shortcomings of the argument of Mill in this respect, as of that of Ricardo, must be left for another occasion. It should be sufficient to call attention here to the celebrated passage in

however, that his discussion of other problems—for example, the relation of money to the *rate of interest*—was such as to entitle him to a place alongside some of those whom Mr. Keynes has recognized as his own intellectual ancestors in regarding money as a "real" factor in the determination of the rate of interest.

If, for example, Irving Fisher's treatment of the relation between the "money" and the "real" rate of interest entitles him to be regarded as an "ancestor" of Mr. Keynes in this respect, so does that of the younger Mill; for it was Professor Fisher himself who pointed to Mill as one of those who had anticipated him on the general point involved.[134] It is true that supporters of the theory of interest presented in Mr. Keynes's *General Theory* would hardly be satisfied by Mill's insistence, at the outset of his discussion "Of the Value of Money, as Dependent on Demand and Supply," on the proposition that what the "demand and supply" of money determine is, not the Value of Money" in the sense of the rate of interest, but the Value of Money in the sense of the "purchasing power" of money.[135] There is every reason to believe, however, that these supporters would have at least as serious objections to certain remarks by Professor Fisher with respect to what he has called the "money-theory" of interest.[136] And for those who would apply literally, to the treatment by "classical" writers of the determination of the interest rate, the statement of Mr. Keynes concerning what has appeared in "Volume One" and "Volume Two," respectively, of treatises on economics, it may come as a shock to be reminded that Mill's formal discussion "Of the Rate of Interest" appeared, not among the chapters on the "general" Theory of Value, but among the chapters on the *Currency*. This was designedly so; for, Mill wrote, "the two topics of Currency and Loans, though in themselves distinct, are so intimately blended in the phenomena of what is called the money

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Mill's *Principles* (Book III, Chap. XIII, sec. 4) which, like Mill's essay on "The Currency Juggle" (I, 68 ff., of Mill's *Dissertations and Discussions: Political, Philosophical and Historical*), is remarkable chiefly as an example of how completely Mill could, on occasion, both misunderstand and misrepresent an opponent—in this case, Thomas Attwood.

[134] See Fisher's *The Rate of Interest* (1907), 357, and the reference there given to Mill's *Principles* (the "single paragraph" to which Fisher refers is the fourth paragraph on p. 646 of the Ashley edition).

[135] See Mill's *Principles,* Book III, Chap. VIII, sec. 1 (pp. 489 f. of the Ashley edition).

[136] See, for example, Chap. XVI of Fisher's *The Rate of Interest* (1907); and cf. what is said on this matter below, p. 109, n. 46. It is only fair to Professor Fisher to point out that the treatment of the relation between Money and the Rate of Interest which is presented in Fisher's later *Theory of Interest,* while it is still very far removed from that in Keynes's *General Theory,* is much freer from extreme generalizations with respect to the essential lack of importance of certain types of pecuniary phenomena than was his earlier treatment. On the differences between the two versions in this respect, see my article, "Irving Fishers Theorie des Zinses," *Zeitschrift für Nationalökonomie,* II (1931).

market, that it is impossible to understand the one without the other." [137]

Mill's treatment of the determination of the rate of interest represented, indeed, what might now be called a "loan fund" approach to that problem: a characterization which is peculiarly appropriate in view of the fact that he seems to have been one of the earliest writers to use the term "the loan fund" in this connection.[138]   And it is something of a commentary on the extent to which Mill was prepared to admit the influence of pecuniary factors upon the determination of the rate of interest that both of the writers whom Mr. Keynes has claimed as intellectual ancestors on the point in question—Fisher and Wicksell—undertook to criticize Mill for having gone too far in this respect: Fisher on the ground that Mill had made unnecessary concessions to what the former characterized as "the money-theory of interest," and Wicksell on the ground that Mill had added to the "confusion" already existing with respect to the relation between "interest on capital" and "interest on money." [139]   If there could be a better example of the Futility of Slogans regarding the importance or unimportance of money, when the problem is that of judging the extent to which the "classical" writers allowed a "hiatus" to develop between monetary theory, on the one hand, and "general" economic theory, on the other, it would be interesting to know what it could be.

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[137] Mill, *Principles,* Book III, Chap. XXIII (pp. 637 ff., Ashley edition).

[138] See, for example, p. 643 of the Ashley edition of the *Principles.* The use of the expression "the Loan Fund" in connection with the problem of the determination of the rate of interest has, of course, been popularized in our own day chiefly by H. J. Davenport (see, for example, the latter's *Value and Distribution* [1908], Chap. XII [also pp. 211, 242 ff.], and his *Economics of Enterprise* [1913], Chaps. XVIII and XIX), although the substance of the argument which may be regarded as underlying something properly called a "loan-fund" approach is much older even than Mill. A consideration of the history of the "loan-fund approach," however, including its relation to the argument of the "classical" economists, as well as to that of Keynes's *General Theory,* must be left for another occasion.

[139] See Fisher, *The Rate of Interest,* 324, and Wicksell, *Interest and Prices,* page xxv. It may be observed here that this was one of the rare instances in which Wicksell was guilty of misrepresenting another writer. For the "two subjects" which, according to Mill, had come to be "mixed up in the most inextricable confusion" were not, as Wicksell implied, "money" and "real capital," but *"Currency"* and *"Loans."* The difference is not only vital in itself but is such as to make what Mill had to say on the subject much more directly relevant to recent controversy on the theory of the determination of the Rate of Interest than a large part of the argument of Wicksell's own *Interest and Prices* can be said to be. It will be observed also that this difference forbids any identification of the concept of a "natural rate" as used by Mill (p. 638 of the Ashley edition) with the "natural rate" as formally defined in Wicksell's *Interest and Prices* —namely, as the rate which would be set if "real capital" were lent *in natura.* It may be added that this fact is by no means to be regarded as demonstrating that it was only *Mill* whose general discussion of the relation between money and interest can be charged with having "added confusion" to the subject.

CHAPTER TWO

Monetary Theory and Value Theory in Modern Economic Literature

I

FROM THE "REVOLUTION" OF THE 1870's TO ALFRED MARSHALL

THERE ARE MANY STRANDS in the mythology that has grown up with respect to the treatment, in economic literature, of the relation between the theory of the Value of Money, on the one hand, and the "general" Theory of Value, on the other. According to one of these strands, the theory of the Value of Money may, indeed, be said to have involved, in certain cases, the application of *some* "general" theory of value to the special problem in hand; but—so the legend runs—it must be said to have involved only an extremely *retrograde* version of the "general" theory.[1] It may be observed that if this charge were true, it would in itself constitute a commentary on the suggestion that the mere application of *a* "general" theory of value to the theory of the Value of Money in itself represents a significant achievement.[2] As we have seen, however, it is precisely a characteristic of the history of the relation between the two bodies of theory, over the period discussed thus far, that there *was* no appreciable lag between the development of a given set of analytical tools intended primarily for use within the "general" Theory of Value, and

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[1] See, for example, F. von Wieser, "Der Geldwert und seine Veränderungen," *Schriften des Vereins für Sozialpolitik,* CXXXII (1909), 514 ff. (212 ff. of Wieser's *Gesammelte Abhandlungen,* edited by Hayek); A. Aftalion, *Monnaie, Prix, et Change,* 164; J. R. Hicks, "A Suggestion for Simplifying the Theory of Money," *Economica,* February, 1935, 2.

[2] See above, p. 11; also below, p. 125, and the forward references given in n. 85 thereto.

the application of these tools to the special problem of the Value of Money. It is of some interest, therefore, to ask whether such a lag was noticeable in the years commonly taken as dating the "revolution" in the "general" Theory of Value which the work of Jevons, Menger, and Walras in that field is regarded as having brought about.

That such a lag did in fact occur has been the contention of a number of writers, who have supported their statement by pointing to the supposed fact that while "well-known workers on the theory of value, such as Jevons, Walras, and Menger, entered fairly deeply into questions concerning money . . . their treatment of such questions runs, for the most part, in the old ruts." [3] If, however, this statement is taken to mean that the writers indicated made no attempt to apply their "new" theories of value to the problem of the Value of Money, it must be characterized at once as either inaccurate or irrelevant.

In the case of Jevons, for example, the statement is irrelevant for the simple reason that Jevons's discussion of the problem of the forces determining the Value of Money was itself extremely fragmentary.[4] In the case of Menger, recent reconsiderations of his treatment of the "demand" for money (which is certainly relevant to the problem of the *Value* of Money) have led to the conclusion that the earlier statements concerning this part of Menger's treatment were inaccurate; for in reality Menger was, in this instance, merely applying to the problem in hand the methodological principles underlying his "general" theory of value.[5] And in the case of Walras it has been possible

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[3] So Wicksell, *Interest and Prices,* 18 (see also the comment by Wicksell on Walras's monetary theory which is cited in my "Léon Walras and the 'Cash-Balance Approach,' " *loc. cit.,* 594, n. 55). Cf. the similar remarks by Anderson, *The Value of Money,* 48.

[4] See the discussion in the fine print section, below.

[5] See, in this connection, the references given to A. Nielsen and F. A. von Hayek in Volume I, 418 n., of the present work; and cf. the comments by E. Roll, "Menger on Money," *Economica* (New Series), III (1936), 455, 458 (also the same author's *History of Economic Thought,* 383). In the light of the statement in the text as to the relation between the "demand" for money and its value, it is clear that Mises's tribute (*Theory of Money and Credit,* 131 ff.) to Menger's treatment of the former problem itself provides a warning against attaching too much emphasis to Mises's statement else-

to show that he could not have been more explicit in insisting that one of his chief purposes was precisely the application, to the problem of the Value of Money, of his "general" theory of value—or, as he put it, his "system of pure economics" —and, in particular, his emphasis on the concept of *rareté*.[6]

References have already been given to more extended discussions of the positions of Menger and of Walras, respectively, with respect to the relation between the theory of the Value of Money and the "general" Theory of Value. It is therefore permissible to confine any further remarks on the position of the three writers indicated thus far to support of the proposition that *Jevons's* discussion of the problem of the forces determining the Value of Money was so fragmentary as to make it of very little relevance to the point under discussion.

When, for example, Jevons, in his paper entitled "A Serious Fall in the Value of Gold Ascertained," advanced the proposition "that an article tends to fall in value as it is supplied more abundantly and easily than before," he presented it, not as a statement of a "general" Theory of Value, but simply as "a most familiar fact."[7] And it was an outstanding characteristic of the paper cited that in it Jevons was concerned almost exclusively with the ascertainment of the "fact" that the "value" of gold, in the sense of its "ratio of exchange" with other commodities, had fallen, rather than with the "question how this fall of value is caused."[8] On the latter question, to be sure, Jevons did venture the hardly startling suggestion that money prices are at least partially "dependent" upon the quantity of "credit," and that, while "credit gives a certain latitude" to fluctuations in general prices, it does so "without rendering prices ultimately independent of gold."[9] Similarly, he made it clear that he regarded variations in the supply of and demand for gold as significant chiefly for what would now be called the secular *trend* of prices rather than for the fluctuations about this trend.[10] By far the larger part of the paper indicated, however, as well as of other papers devoted to the problem of the "value of gold," was concerned with

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where that Menger had not "even so much as attempted to solve the fundamental problem of the value of money" (*Theory of Money and Credit*, 116).

[6] See my "Léon Walras and the 'Cash-Balance Approach,' " *loc. cit.*, 592, and the references to Walras given in n. 52 thereto. On the criticisms that have been advanced by the few, among later writers, who were aware of Walras's efforts in the direction indicated, see my article "The Monetary Aspects of the Walrasian System," *Journal of Political Economy*, XLIII (1935), 156 ff.

[7] See p. 13 of the "new" (1909) edition of Jevons's *Investigations in Currency and Finance*.

[8] See, for example, Jevons's *Investigations*, p. 18, and especially p. 54; and cf. the similar comments in Jevons's later paper on "The Variation of Prices, and the Value of the Currency since 1782" (*Investigations*, 122, 128).

[9] *Investigations*, 27 f.

[10] See especially the *Investigations*, 30 ff.

establishing the *facts* with respect to these long-term variations rather than with the development of a formal analytical apparatus for dealing with the *causes* of changes in the "value of money." [11]

The comparative lack of interest thus evidenced by Jevons in the problem of the forces determining the Value of Money is therefore sufficient to account for the fact that one does not find, in his writings, any detailed analytical apparatus for dealing with the problem, and, therefore, any apparatus which could be compared, or contrasted, with the apparatus represented by Jevon's "general" Theory of Value. Yet on the few occasions on which Jevons did touch upon the problem of the forces determining the Value of Money, his usage was such as to indicate that if he had undertaken to develop a formal apparatus for dealing with the problem, it would have proceeded upon lines that would have been strictly consistent with his "general" theory. He regarded as axiomatic, for example, the proposition that "the theory of economy" would "naturally" remain "the same throughout its applications," in the sense that such a subject as "currency," for example, must be held to involve "the same ultimate laws . . . of supply and demand" that are provided by an adequate "theory of economy." [12]

Likewise, on more than one of the occasions on which he introduced into his discussion of the Value of Money considerations associated with the "general" Theory of Value, he referred to his *Theory of Political Economy* for support.[13] It is true that in the few applications of the "general" theory of "supply and demand" which Jevons made to the special problem of the Value of Money, he referred as often to the influence of the "cost of production" of the money metal as he did to

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[11] Jevons was, of course, concerned to establish the fact that the rise in money prices was "due" in large part to the "depreciation of gold"—that is, that it resulted from large increases in the supply of gold (*Investigations*, 44 ff., 46, 104 f., 130, 146, 148). An examination of the context in which even these passages appear, however, will show that Jevons's predominant concern was with the establishment of the *fact* that most prices had risen in terms of gold, rather than with an account of the mechanism, for example, by which changes in the quantity of gold would be expected to affect money prices.

[12] See Jevons's paper on "The Future of Political Economy" (1876; p. 200 of Jevons's *The Principles of Economics, etc.*). Attention may be called also to Jevons's repeated insistence upon the proposition that monetary gold and silver are "commodities" and are therefore subject to the same laws of value as are other commodities (*Investigations*, 57, 102, 279, 293, 295; *Primer of Political Economy*, 98). See, finally, Jevons's *Theory of Political Economy*, 138, where "money" is treated as one "commodity" among many.

[13] See, for example, Jevons's *Investigations*, 16, 75, 228 (on "value" as a "ratio," or "relation"), and 58 (on the "equivalence of commodities"). Cf. also Jevons's *Money and the Mechanism of Exchange*, 10. Conversely, Jevons did not hesitate, in his *Theory of Political Economy*, to refer to his writings on currency for illustrations of his "general" Theory of Value. Cf., for example, p. 137 of the fourth edition of the *Theory*.

conditions affecting the "utility" of such metals.[14]   To argue from this, however, that Jevons thereby introduced a hiatus between his treatment of the problem of the Value of Money and his "general" Theory of Value is not only to misrepresent Jevons's treatment of the rôle of cost of production in determining "value" as seriously as Jevons himself has been charged with having misrepresented Ricardo, but also to provide an example of the pernicious encouragement to an analytical *exclusivism* which has so often resulted from attempts to "reconcile" the two bodies of theory.   Of Jevons's treatment, it can at least be said that, for all its fragmentary nature, it was at least free from this kind of factitious exclusivism.

Attention may be called, finally, to a further fact which may be regarded as demonstrating that it was Jevons's comparative lack of interest in the special problem of the forces determining the Value of Money that was chiefly responsible for his own failure to provide a formal and detailed "application" of his "general" Theory of Value to the problem of the Value of Money, rather than a conviction that his "general" Theory of Value could not be applied to the special problem of the Value of Money.   This is the fact that Jevons, unlike the Walras of the first edition of the *Eléments*, showed just as little interest in, and appreciation of, equations of exchange of the "Fisherine" type, so often regarded as a formulation opposed to those based upon "modern" theories of value, as he did in the latter type of formulation.[15]   Jevons

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[14] For examples of Jevons's introduction of an emphasis upon the cost of production of the money metals as an element affecting their value, see his *Investigations*, 62, 65 ff., 70, 293. On "utility" as a factor affecting the Value of Money, see especially Jevons's *Money and the Mechanism of Exchange*, 32 ff. It will be observed, from the latter passage, that Jevons was perfectly prepared to speak of the "utility" of the money metal as "depending" upon the services which it provides *as money*. It may be observed also that while it is true that in *The Theory of Political Economy*, Jevons's "utility of money" (p. 140) was primarily the "utility of money *income*," it is also true that he used this expression in connection with his discussion of the "Acquired Utility of Commodities" (pp. 137 ff.)—that is, in connection with his discussion of the fact that "the power of exchanging one commodity for another greatly extends the range of utility." He was perfectly prepared, moreover, to speak of the "utility" of "that quantity of money" which a man "will desire *not* to exchange" (p. 138). It is upon precisely such passages that one must base any surmise as to the form which Jevons would have given to his formal theory of the rôle of "utility" in the determination of the Value of Money if he had been sufficiently interested in developing such a theory. In this connection, see what is said below. p. 57, n. 15, concerning the relation of *Wicksteed's* analysis to the type of suggestion indicated here.

[15] On Walras's use of an equation of the "Fisherine" form, see my "Léon Walras and the 'Cash-Balance Approach,'" *loc. cit.*, 573 ff. On the suggestion that the use of such an equation is "inconsistent" with "modern" theories of value, see above, pp. 42 and 45, and also below, pp. 59 f., 87 f., 126, n. 85, and p. 652, n. 56. Wicksteed not only pointed out, but regarded as "noteworthy," the fact "that there is no mention" of a quantity equation

knew of Lubbock's *On Currency,* in which a "Fisherine" equation had appeared; and the "Fisherine" equation in K. H. Rau's *Grundsätze der Volkswirtschaftslehre* is one of the very few "symbolic statements" in Rau's work generally which would justify Jevons's listing of Rau among those who had "incidentally acknowledged" the value of a "mathematical treatment" of economic problems.[16] There seems to be no evidence that he attached any particular significance to either Lubbock's formulation or that of Rau; yet it is difficult to believe that he could have failed to do so had he been really interested in the problem of the forces determining the Value of Money. It would follow, therefore, that any test as to what the authors of the "revolution" of the 1870's in the general Theory of Value had to say with respect to the relation between the "general" Theory of Value, on the one hand, and the theory of the Value of Money, on the other, takes on significance only when applied to writers, such as Menger and Walras, who *were* interested in the

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of the general Fisherine form (referred to, in this instance, as "the quantity law"), "nor any implication direct or indirect of *its* existence, to be found from end to end of the numerous works on currency and finance of the late Professor Jevons" (see p. 611 of the 1933 edition of Wicksteed's *The Common Sense of Political Economy*); and he implied that this was proof of Jevons's superior insight into the issues involved. Others, however, will rejoice that Jevons himself was not guilty of the absurd comments on "the quantity law" which are to be found in Wicksteed (*Common Sense,* 611 ff.). For, despite the importance which Wicksteed assigned to his own discussion of "the quantity law" (cf. *Common Sense,* 8, 596, n.), he succeeded for the most part only in providing an early instance (of the kind of which we have since had so many) of how the "novice" in the field of monetary theory is "almost certain to be the victim of aggravated *vertigo*" in "these regions of discourse" in which "the most experienced scalers of the Alpine heights of speculation . . . have constantly to steady their heads" (*Common Sense,* 597). Nevertheless, it is interesting to observe in how many ways Wicksteed, in his *positive* discussion of the problem of the Value of Money, without giving specific references to Jevons, actually carried forward, to further construction, suggestions on the relation between monetary theory and the "general" Theory of Value which are to be found in Jevons. His development of an embryonic version of the "cash-balance approach," for example (*Common Sense,* 600 ff.), was a direct result of an application of his general concept of "marginal significance" to money as such. In this connection, cf. the references to Jevons given above, p. 56, n. 14; and compare also Wicksteed's discussion of "derivative value" as "not peculiar to the currency," and as providing the basis for removing "mistaken" ideas concerning the "difference between currency and other commodities" (*Common Sense,* 615 ff.), with Jevons's remarks on the "acquired utility of commodities" and on the essential "sameness" of "the theory of economy" in "all its applications," including its applications to the "currency" (see above, p. 55, n. 12, and p. 56, n. 14).

[16] On the "Fisherine" equations of Lubbock and Rau, respectively, see Volume I, 10 ff., and the references there given. For references to Lubbock by Jevons, see the Preface to the first edition of Jevons's *Theory of Political Economy* (p. viii of the fourth edition), and his *Investigations,* 116; and for Jevons's reference to Rau, see p. xxiv of the fourth edition of the *Theory.*

problem of the forces determining the Value of Money. The results of such a test are summarized above.

For dramatic completeness, therefore, only one further instance was needed in order to establish the fact that the appearance of new developments in the "general" Theory of Value has been followed almost immediately by an application of these developments to the theory of the Value of Money in virtually every generation of monetary theorists from the very beginning of economic science to our own day. It was Alfred Marshall who, in his own understanding as well as in the understanding of others, undertook to consolidate the advances that had been made in the field of the general Theory of Value by attempting to establish a *modus vivendi* between the "value theory" of the Ricardians and "value theory" of the Jevonian stamp. It is proper to point out, therefore, that it was Mr. Keynes himself who, a bare decade before the appearance of his *General Theory*, characterized as one of the merits of Marshall's treatment of the problem of the Value of Money the fact that the latter was expounded "as a part of the General Theory of Value." [17]

There is, therefore, no foundation whatever for the suggestion that the four great names in the development of the "general" Theory of Value after 1870 may be cited in support of the statement that economists in general, in dealing with the problem of the Value of Money, have contented themselves with a type of theory which bore no relation whatever to their own "general" Theory of Value. It is of very much greater importance for our present purpose, however, to establish a further proposition: namely, that the measure of the achievement thus represented is not given by the mere fact that the monetary theory of writers such as Menger, Walras, and Marshall was "completely coherent with" their "general theory of value." [18] For it is one of the principal lessons of the history of doctrine on the subject that the victory which is supposed to be represented

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[17] Cf. Volume I, 441 of the present work, and the reference to Keynes given in n. 77 thereto.

[18] Cf. the comment of Pigou on this aspect of Marshall's monetary theory which is cited in Volume I, 441, n. 77, of the present work.

by the attainment of such "coherence" has as often as not
been a Pyrrhic victory, by reason of the fact that the search
after such "coherence" has resulted, altogether too fre-
quently, in the substitution of purely factitious issues for
genuine issues of substance.[19]

In the case of Menger, Walras, and Marshall, what mat-
tered for the future substantive development of monetary
theory was not the mere attainment of "coherence," in the
sense of a formal application of the language and the tech-
niques of the "general" Theory of Value to the problem of
the Value of Money. It was rather the fact that in these
particular instances the search for such "coherence" re-
sulted in the development of a specific analytical device—
the so-called "cash-balance approach"—which is a truly
indispensable weapon for attacking certain problems of
monetary theory, and which can therefore stand on its
own feet quite apart from the historical fact that, in selected
instances, it happened to be associated with a desire of the
writers concerned to apply their "general" theories of value
to the special problem of the Value of Money.[20]   Moreover,
with the single possible exception of Menger, the writers
indicated refrained from arguing that a decent respect for
the principles of "modern" value theory required the setting
up of an antithesis between one analytical device (in their
case, the "cash-balance approach") and another analytical
device (in this case, the concept of "velocity") which is a
false antithesis, for the reason that the one concept can be
shown to *supplement*, rather than to invalidate, the other.[21]

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[19] See below, p. 126, and the backward and forward references given
in n. 86 thereto.

[20] It is of some importance to observe that, as a matter of doctrinal
history, the "cash-balance approach" has not *always* been so associated.
See below, pp. 85, 93, 120, 662 f., 666 f.

[21] The passage in Menger's article "Geld" which might be held to support
the suggestion that he was guilty of setting up the false antithesis indicated
is to be found on p. 109 of Volume IV of *The Collected Works of Carl
Menger*. (For characterizations of Menger as an "opponent" of the con-
cept of "velocity," see the references given in Volume I, 297, n. 18, of the
present work.)   Walras, on the other hand, refrained even from comparing
the two "approaches"; thus, he can hardly be charged with having set up
a false antithesis between them. It is significant, moreover, that he was
not guilty of the misunderstanding of the nature of the relation between
the two which has been evidenced by commentators on this aspect of his

And what matters finally is that none of the writers indi-
cated was led by his concern with the establishment of the
relation between "utility analysis" and the problem of the
Value of Money to raise that type of altogether factitious
issue which has led some commentators on the relation be-
tween the "general" Theory of Value and the problems of
monetary theory to regard the supposed "isolation of mone-
tary problems from the central problems of price formation"
as in part a blessing, on the ground that this supposed "isola-
tion" saved monetary theory from sharing "the abstruse
chaos of objective and subjective, individual and social,
marginal and total, utility and value" into which the "cen-
tral theory of price formation" is regarded as having fallen.[22]

Again, however, it is by no means certain that the only,

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monetary theory (see, for example, my "Léon Walras and the 'Cash-
Balance Approach,'" loc. cit., 589 and 599, and the references there given).
Marshall, on the other hand, provided a statement of the relation between
the two "approaches" which is unexceptionable from the standpoint in-
dicated in the text (cf. Volume I, 418 f., and the reference to Marshall
given on p. 419, n. 12); and in this respect he was followed faithfully by
Pigou and Robertson (cf. Volume I, 391, n. 7, and 417, n. 10). For con-
trary examples in the discussion of the problem by later writers, see below,
pages 87, 654 ff., and 577, n. 59, 653, n. 58, 670, n. 99, 673, n. 111, 728 f. It
may be pointed out that the second of these groups of examples, which is
represented by the failure of Mr. Keynes and his followers to treat satis-
factorily the relation between the concept of "liquidity preference," on
the one hand, and that of "velocity," on the other, takes on a particularly
striking degree of irony when it is recalled that Mr. Keynes himself, on an
earlier occasion (Memorials of Alfred Marshall, 29 f.), had called attention
to the fact that Marshall had been "able to show the exact logical connec-
tion between" the "conception of 'rapidity of circulation,'" on the one hand,
and his own version of the "cash-balance approach," on the other.

[22] The quotation is from G. Myrdal, "Der Gleichgewichtsbegriff als
Instrument der geldtheoretischen Analyse," loc. cit., 374 (Monetary Equi-
librium, 16). It must be observed that whatever one may think of Pro-
fessor Myrdal's characterization of the state of the "general" Theory of
Value as a result of economists' concern with "utility analysis," he is un-
fortunately wrong in his suggestion that monetary theory has been spared
the "abstruse chaos" which has followed from an excessive concern with
the rôle of "utility analysis" in the theory of the Value of Money. See
Volume I, 305, 337, 450 ff., and the references there given (contrast the
statement by G. Demaria ["La teoria dei prezzi," Giornale degli economisti
e Annali di economia, I (1939), 286] that nothing was said in Volume I of
this work concerning the "marginal utility of money"); and see also what is
said on this matter below, pp. 84 ff. What is true, on the other hand, is
that much of this "abstruse chaos" would have been avoided if later writers
had followed strictly the example set by Walras, Menger, and Marshall in
this respect.

or even the most fruitful, way of establishing a *modus vivendi* between the Theory of Money and Prices, on the one hand, and the "general" Theory of Value, on the other, is represented by the formal application, to the special problem of the Value of Money, of certain analytical weapons developed originally within the "general" Theory of Value.[23] There is, after all, the more inclusive task of establishing the precise *modus operandi* of money as a "real" factor affecting the processes of economic life in the world we know.   To what extent was justice done to this aspect of the problem by the four writers generally regarded as responsible for the "revolution" in value theory in the 1870's and the incorporation of the results of this "revolution" within the main corpus of generally accepted theory?

As it happens, the comparative specialization of interests manifested by Jevons and Menger in their more formal work in economic theory prevented their dealing with problems of this type in detail.   To argue from this fact, however, that they were committed to the proposition that money is a factor of no importance in either economic theory or economic life would be nothing short of absurd.   For, with very few exceptions, both Jevons and Menger, *whenever they discussed phenomena in which money can be shown to play an important rôle,* showed an awareness of the potential importance of monetary factors which was often superior to that shown by many of their contemporaries.

In the case of Jevons, for example, it is generally recognized that the fragmentary structure of his major analytical work—*The Theory of Political Economy*—was not such as to lead one to expect a systematic survey of the ways in which money might be expected to affect the functioning of the economic process.[24]   The famous chapter on "The

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[23] See above, p. 8; and cf. what is said on this matter below, pp. 89 ff., 92 f., 99 f., 119 f., 126 ff.

[24] On the fragmentary and unsystematic character of Jevons's principal theoretical work, see A. A. Young, "Jevons's 'Theory of Political Economy,'" *American Economic Review*, II (1912; pp. 219 f. of Young's *Economic Problems New and Old* [1927]); and cf. also the comments by Mr. Keynes in his memoir on Marshall (*Memorials of Alfred Marshall*, 23, 36).   This fragmentary character of Jevons's principal theoretical work must be borne in mind, moreover, in any attempt to evaluate the suggestion that Jevons's alleged pushing of "the use of money . . . into the background" was the "result" of certain postulates on which his system was

Theory of Capital," for example, is itself a fragment, which Jevons himself intended to supplement by a further discussion, the nature of which can only be guessed from another fragment that has come down to us.[25] No one could pretend that the concept of "capital" presented in these fragments, and the conception of the problem of the determination of the rate of interest which they may be held to imply, show a clear awareness of the importance of pecuniary factors for the latter problem. Yet it would be equally absurd to suggest that Jevons himself felt that it was either desirable or possible to develop a "complete"

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built—and, in particular, of "his explicit avowal of hedonism as the basis of economic theory" (so, Mitchell, "The Rôle of Money in Economic Theory," *loc. cit.*, 152). It is not necessary here to go into all aspects of the question whether a "hedonist" would or would not be justified in refusing to admit that there is any "fundamental inconsistency between the hedonic and the pecuniary calculus" (cf. Mitchell, *loc. cit.*, 152, n. 11); it is sufficient, rather, to point out that acceptance of the necessity for "the hedonic calculus" in *certain* economic problems does not mean that one would necessarily confine oneself to it in dealing with *all* problems within the field of economic theory. In this respect, the criticism of Jevons just cited is on a par with that leveled by the mathematician Bertrand against Walras: namely, that any attempt to explain the actions of men in the market in terms of calculations with respect to *rareté* "loses all significance when it is applied to business men" (see my "The Monetary Aspects of the Walrasian System," *loc. cit.*, 171 f.). For the truth is that the kind of "calculus" ascribed to "traders" by Jevons in connection with his discussion of "credit cycles" (see, for example, his *Primer of Political Economy,* 116 ff.) was a "calculus" with respect to *money profits.* The reason, of course, why one finds this in Jevons's *Primer* rather than in his *Theory of Political Economy* is that, *from the standpoint of the range of topics treated,* even the tiny *Primer* was less of a "fragment" than the far bulkier *Theory.* There is no reason for supposing that, if Jevons had been able to complete his much more comprehensively planned *Principles of Economics,* his "explicit avowal of hedonism as the basis of economic theory" would have led him to push "the use of money into the background" even when he was dealing with a problem for which money could be shown to be important. It may be observed, finally, that to say that Jevons regarded the effects of money upon the economic process as belonging to "the higher complications of the subject" (cf. the quotation from Jevons in Mitchell, *loc. cit.,* 153) is not to say that Jevons regarded these "higher complications" as superficial or unworthy of the attention of the "general" economic theorist.

[25] See the "Fragment on Capital" printed as Appendix II (pp. 294 ff.) to the fourth edition of Jevons's *Theory of Political Economy.* Not least interesting, for our present purpose, is the last paragraph of this "Fragment" (p. 302), in which Jevons laid down the proposition, prophetic of Marshall's later treatment of interest, that "capital expended ["invested"?] in a fixed form ceases to be subject to the laws of interest," and that "it is *free capital* which we have to treat . . . as *capital* properly speaking" (italics in the original). It is of course true that, as Jevons used the term "free capital," it was not usually conceived of as a sum of "general purchasing power" in the form of money. See, however, what is said on this matter below, p. 63, n. 27.

theory of "capital" and "interest" in which money would play no significant rôle.

I have been unable, for example, to find anything in Jevons's writings with respect to the supposedly "superficial" character of the relation of monetary phenomena to the rate of interest that one finds in the writings of Böhm-Bawerk.[26] What one finds in certain of Jevons's writings, on the contrary, is a series of isolated attempts to incorporate pecuniary factors into his analysis; and what one discovers is that these attempts involved, as often as not, so crude an identification of "capital" with the amount of *banking resources* that they evoked a protest from Wicksell, who otherwise regarded his own approach to the problem as being as much an attempt to relate the *Jevonian* "theory of capital" to pecuniary phenomena as an attempt to relate the Böhm-Bawerkian "theory of capital" to those phenomena.[27] What really matters here,

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[26] See, for example, Böhm-Bawerk's "Eine 'dynamische' Theorie des Kapitalzinses" (*Zeitschrift für Volkswirtschaft, Sozialpolitik und Verwaltung,* XXII [1913], 30 f. [p. 552 of Böhm-Bawerk's *Kleinere Abhandlungen über Kapital und Zins*]), where Schumpeter's discussion of the limits to which "productive credit can be granted," with its attribution of the "essential rôle to money and media of payment, instead of the real stocks of goods existing in the economy," was characterized as a "dangerous" and "genuinely mercantilist error of superficiality," which, according to Böhm-Bawerk, brought Schumpeter's position close to that of such writers as John Law. I have been unable, indeed, to find in Jevons's writings anything corresponding to the degree of minimization of pecuniary factors which one finds even in *The Rate of Interest* of Irving Fisher, whom, as we have seen, Mr. Keynes has accepted as one of his intellectual ancestors in recognizing that money may be a "real" factor in the determination of the rate of interest. (See above, p. 7, n. 11; and for the relevant passages in Fisher's *Rate of Interest,* together with a comparison of these passages with the corresponding treatment in Fisher's later *Theory of Interest,* see my "Irving Fishers Theorie des Zinses," *Zeitschrift für Nationalökonomie,* II [1931].)

[27] For Wicksell's criticism of Jevons, in this connection, see the former's *Interest and Prices,* 108 f.; and for Wicksell's attitude otherwise toward Jevons's "theory of capital," see pp. xxv, 122 ff., of the same work. As it happens, the passage cited by Wicksell from Jevons (pp. 27 f. of the latter's *Investigations*) by no means represented an isolated instance. See, for example, Jevons's *Investigations,* 19, 24–26, 155 ff., 162, 171. It must be added, however, that all these instances are relevant for a judgment as to the direction in which Jevons might have pushed any further analysis of the problem of the relation between his "theory of capital," on the one hand, and the phenomena of the money market, on the other. It will be observed, for one thing, that the "free capital" of which Jevons spoke in his *Investigations* (for example, pp. 28, 102) was not, as it usually was in *The Theory of Political Economy* (cf. pp. 243 ff. of the latter work), "free capital" in its *"real* form of food and other necessaries of life," but was rather "free capital" in what, in his *Theory* (p. 243), Jevons called "its transitory form of money." One observes also that, in the *Investigations,* the process of the *disinvestment* of "capital" was not regarded as complete until the "capital" was "returned as *ready money"* (p. 24; italics mine),

however, is that Jevons himself certainly did not pretend to have provided in his *Theory of Political Economy* a "complete" solution of the problems associated with "capital," such that it required at no point the introduction of a consideration of the influence of pecuniary phenomena before being applied to the facts of the real world. On the contrary, he could not have been more explicit in admitting that no solution of the problem of "capital" and "interest"—including his own—could be regarded as satisfactory until justice was done to these pecuniary phenomena. "I am far from supposing," he wrote, "that the exact relations in regard to prices, commodities, gold, and capital have been hit upon. I do not believe that any of our economists have yet untied this Gordian knot of economic science, although some cut it in a very unhesitating manner."[28] "This Gordian knot of economic science" is hardly an expression that would have been used by one for whom money was a factor of no "real" significance within the complex of problems associated with the concept of "capital"!

Jevons's concern, finally, with the causes of "periodic commercial fluctuations" is so well known that it is of some interest to ask what rôle, if any, he believed was played by money in the changes in the level of "output as a whole" which these "commercial fluctuations" usually represent. It is true that he sometimes protested against what he regarded as an overemphasis upon monetary factors.[29] This, indeed, is hardly surprising, in the light of his own conviction as to the crucial rôle played by agricultural harvests in causing these fluctuations.[30] Yet

It is therefore not unreasonable to suggest that if Jevons had lived to write the more nearly complete *Principles* he had planned, his treatment might very well have been such as to permit an interpretation of the statement quoted above on p. 62, n. 25 which would make it virtually the exact equivalent of the proposition of Marshall quoted below, p. 75, n. 60.

[28] Jevons, *Investigations*, 28 f.

[29] See, for example, the *Investigations*, 123, where Jevons could not "too fully concur" with Tooke in the latter's "protest against the common practice of attributing every evil to the monetary circulation, which . . . does not deserve to be made the scapegoat it has long been"; also p. 293 of the same work, where Jevons went so far as to agree with some of those who have been interpreted as arguing that "if the money in the world were suddenly doubled or halved, trade would go on as before, all prices being approximately doubled or halved." It is probably such passages as these which have led commentators on this aspect of Jevons's work to suggest that he had been led to "ignore the stimulating effect on trade of an increase in . . . [the] quantity [of money]" (so Foxwell, in his Introduction to the *Investigations*, p. xvi, n.). It is clear, however, that any interpretation of Jevons's position with respect to the effect upon output of monetary expansion and contraction would have to take account of the unequivocal utterances with respect to the reality of such effects which are cited below.

[30] Again, however, in justice to Jevons it must be pointed out that only a blind insistence upon the mechanical classification of "theories" of the business cycle could lead one to suggest that Jevons himself believed that agricultural harvests were the only factor involved, to the exclusion of other factors, of which the pecuniary factor would be one. In his earlier

it is also true that on the one occasion on which he ventured an un-
equivocal opinion as to the effect of monetary "depreciation" on the
"making" and "acquiring" of "wealth," and therefore upon the material
well-being of the "community as a whole," he did not hesitate to "agree
with Macculloch" that such "depreciation" must have "a most power-
fully beneficial effect" upon such well-being.[31]   Again, therefore, one
is forced to conclude that Jevons was very far indeed from arguing, or
implying, that it is possible to provide an essentially complete theory
of the functioning of the economic process without taking into account
the rôle played by money as a "real" factor conditioning that process.

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writings, for example, Jevons expressed the belief that the "remote cause"
of the "commercial tides" lies "in the *varying proportion which the capital
devoted to permanent and remote investment bears to that which is but
temporarily invested soon to reproduce itself*" (*Investigations,* 24 [italics
in the original]; cf. also p. 165 of the same work). Failures in harvests
were, indeed, regarded by Jevons as a factor by which "the arrival of . . .
[a] dearth ["of capital, or loanable money"] is generally accelerated"
(*Investigations,* 26), in the sense that they "strongly contribute to hasten
or retard the several periods of abundant capital and investment, and
again those of scarcity and revulsion" (*Investigations,* 44). No one, how-
ever, could have been more explicit than Jevons in regarding the truly
"salient fact" involved in the downturn as "a great dearth of . . . loanable
money," the "result" of this "dearth" being that "the stocks of commodities
cannot be sold against the stock of available ready money at the point to
which prices have advanced" (*Investigations,* 25 f.). That these early
convictions, moreover, were not modified by Jevons's later investigations
into the specific influence of the harvest factor is evidenced by the account
of "Credit Cycles" given in Jevons's *Primer of Political Economy,* in which,
although the book appeared in the same year (1878) as Jevons's two most
important papers on the influence of harvests, only one paragraph (p. 120)
in the whole chapter is devoted to the latter, whereas the influence of
pecuniary factors is stressed throughout.

[31] See Jevons's *Investigations,* 91. The passage cited is a commentary
not only on the indiscriminate lumping together of all "English economists"
as having been responsible for a "mechanical teaching . . . on the subject
of money," of such a kind that it led to an ignoring of "the stimulating
effect on trade" of an increase in the quantity of money (cf. the reference
to Foxwell given above, p. 64, n. 29); it is a commentary also on the
suggestion that the undoubted shortcomings in this respect of "English
economists" such as Ricardo and Mill followed directly from certain "postu-
lates" of the Ricardian "system" (see above, pp. 38 f.). It is difficult to
imagine a "system" more generally "Ricardian" than that of McCulloch,
or one less so than that of Jevons; yet it is a striking fact that on the one
occasion known to me (see Jevons, *Money and the Mechanism of Exchange,*
39) on which Jevons, in discussing the effects upon general well-being of a
"variation in the currency" and the related "incitement to industry and
commerce," invoked considerations arising out of that aspect of his
"system" which was least "Ricardian"—namely, his emphasis upon "utility"
—he reached a conclusion *different* from that which both McCulloch and he
reached on the basis of considerations having nothing directly to do with
their respective "general" theories of value.

Much the same may be said of Menger. His *Grundsätze* were, of course, as fragmentary in their way as Jevons's *Theory* had been in its way.[32] Yet, on the few later occasions on which Menger was concerned with specific questions of economic analysis (as opposed to questions of methodology), his discussion was such as to emphasize, rather than to minimize, the importance of money in the functioning of the economic process. Even in his *Grundsätze* he had gone out of his way to characterize certain suggestions with respect to the lack of any connection between money and "capital" as typical of those writers who had gone too far in their opposition to the ideas of mercantilism.[33] And one of the outstanding characteristics of his later essay "Zur Theorie des Kapitals" was its protest against the suggestion that nothing more was involved in the businessman's association of "money" with "capital" than a "confusion" between two entirely distinct sets of phenomena.[34] One wonders, also, what the subsequent history of the theory of the determination of the rate of interest might have been if, instead of following the example set by Böhm-Bawerk in the direction of "generalizing" the concept of "interest," more economists had heeded Menger's warning, in his later essay, against adopting just such a practice, and had gone on, as Menger suggested, to develop separate analysis for the explanation of the separate categories (such as the "rate of interest on sums [of money] advanced on loan," as contrasted with other forms of "return on property" [*Vermögensertrag*]), rather than to conceal the differences between these categories under the too inclusive, and therefore unmanageable, type of usage that would identify "interest" with the "return on property" generally.[35]

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[32] It will be remembered, for example, that the *Grundsätze*, as we now have them, were originally announced as the "First, General Part" of a broader treatise in no less than four "Parts," of which the second was to contain, among other things, material on Interest, Income, Credit, and Paper Money. See the Introduction (by Karl Menger, Jr.) to the second (1923) edition of the *Grundsätze*, p. vi.

[33] Menger, *Grundsätze*, 132 n. (89 n. of the second edition). Cf. also Menger's article "Geld," sec. ix (*The Collected Works of Carl Menger*, IV, 59): "Indeed, there is, in reality, along with the function of money as medium of exchange (as mediator on the commodity-market!) and its use as a preferred medium for hoarding and capitalization, no other one of its functions which requires such large quantities of money and has such great significance for the economy as the function of money as mediator of the trade in capital (on the 'money market'!)." The exclamation points are Menger's.

[34] Menger, "Zur Theorie des Kapitals," *Jahrbücher für Nationalökonomie und Statistik*, LXX (1888), 38, 44 (III, 172, 178 of the *Collected Works*).

[35] The passage, which appears on p. 47 of "Zur Theorie des Kapitals" (III, 181 of the *Collected Works*), is sufficiently striking to be quoted here in full: "Anyone with a practical knowledge of business knows that the rate of interest on sums of money advanced on loan *(Leihsummen)* is dependent upon causes essentially different from those upon which the net yield of rented houses or lands depends; that the yield on hired parks

The same awareness of the impossibility of developing a "complete" theory of the economic process in abstraction from the effects of money on the functioning of that process is shown, moreover, by Menger's further comments on the rôle played by money in those processes with which his main work in economic theory was concerned: namely, those described by the "general" theory of pricing. No one, indeed, could have been more explicit than he in recognizing that the type of precise adjustment in the setting of ratios of exchange which is described by the "general" theory of pricing would be impossible in any but a money economy.[36] He did not argue, either explicitly or by implication, that

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depends on other causes than does that on rented fields; and that the yield on industrial or commercial undertakings, in turn, is subject to other determining conditions than is that on the above-mentioned categories of income-bearing property. It is obvious that the phenomena of yield here in question need a separate explanation each according to its different nature and its different origin. The problem of return on property *(Vermögensertrag)* is, for practical purposes, an extremely complicated one; it is, in practical life, in no way synonymous with the problem of interest; it must not be so in our science, either." Cf. the comment on this aspect of Menger's argument by Hayek, "Carl Menger," *Economica*, November, 1934, 411; and contrast the closing pages of Böhm-Bawerk's *Positive Theory of Capital* (482 ff. of the fourth [1921] edition; 421 ff. of Smart's translation). It may be observed also that of the numerous later fragments by Menger on the theory of capital and interest, including a "criticism of Böhm-Bawerk's interest theory," whose publication had been promised (see the Editor's Introduction to the second edition of the *Grundsätze*, p. xiii), only the essay "Zur Theorie des Kapitals" appears in the *Collected Works* as published in the London series of reprints.

[36] See, for example, the section in Menger's article "Geld" entitled "The Effect of the Emergence of Generally Used Media of Exchange on Commodity Markets and on Price Formation" (*Collected Works,* IV, 18 ff.), especially what is said with respect to the relation between the emergence of money and "competition in the demand for and supply of commodities in trading in goods" on p. 21 (cf. also p. 63, n.; and on the rôle played by such "competition" in Menger's "general" theory of pricing, see his *Grundsätze*, 181 ff., 201 ff.). That Menger was aware, moreover, of the importance of money for the pricing process even at the time he published his *Grundsätze* is apparent even in the single chapter devoted to Money in that work (pp. 250 ff.). See, for example, his comments (p. 276) on the valuation of commodities in terms of each other as *presupposing* their valuation in terms of money; and cf. the comments by A. A. Young on "the notion of exchange value" as "a derivative of the phenomena of price" in Young's "Some Limitations of the Value Concept," *Quarterly Journal of Economics*, XXV (1911; pp. 203 f. of Young's *Economic Problems New and Old*). There can be little doubt, therefore, that in this respect Menger was much more alive to the implications of his analysis with respect to the use of money than was Jevons (cf. Young, "Jevons's 'Theory of Political Economy,'" *loc. cit.*, 224 ff.); though it is only fair to Jevons to point out that a commentary on the degree of pertinacity with which he would have clung to his alleged "refusal to assume a general medium of exchange" (Young, *loc. cit.*, 226) is provided by the fact that the list of occasions on

a satisfactory "general" theory of pricing should or could be constructed upon the basis of "barter" assumptions.[37]   And in refraining from so arguing, he set an example that might well have been followed by later writers for whom an understanding of the effect of money upon the structure of prices demands the preliminary conceptual construction of a "barter" economy, or a series of "barter" economies, in which the "neutrality" of money with respect to the process of price determination is guaranteed by the fact of its complete absence.[38]

This last point, indeed, is worthy of more than passing comment, in view of the twofold circumstance that (1) the concept of "neutral" money, in some of its embodiments, has been associated with the concept of a "barter" economy; and that (2) Menger has been regarded as a forerunner of the concept of "neutral money," by reason of his use of the contrast between changes in the "internal" exchange value of money, on the one hand, and the "external" exchange value of money, on the other.[39]   For what one discovers, upon reading the relevant pas-

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which he "slips into the vocabulary of the money economy" in expounding his "general" theory of value is much longer than one might suppose from the single quotation given by Young *(ibid.)*. See, for example, Jevons's *Theory,* 138 ff.; his *Primer,* 98 ff.; and his *Principles of Economics,* 57, 148.

[37] Contrast Mises, *The Theory of Money and Credit,* 162, where it is alleged that "the modern theory of prices has stated all its propositions with a view to the case of direct exchange" (that is, the case of exchange without the intermediacy of money). The statement is incorrect not only with respect to Menger, but also with respect to Walras and Marshall, who certainly deserve places of honor among the creators of "the modern theory of prices." On the suggestion that Walras's general "theory of equilibrium" was based on "barter" assumptions, see below, p. 70, n. 44; and on Marshall, see below, pp. 73 ff.

[38] The best-known construction of this type in recent years is that of J. G. Koopmans, "Zum Problem des 'Neutralen' Geldes," in *Beiträge zur Geldtheorie* (1933), 228 ff. On the value of such constructions in general, see, in addition to the references to A. Cabiati given in my "Monetary Aspects of the Walrasian System," *loc. cit.,* 168, n. 45, the terse comments by Pigou, *The Theory of Unemployment* (1933), 188 n.

[39] For examples of the association of Menger's distinction with the concept of "neutral money," see W. G. Behrens, *Das Geldschöpfungsproblem* (1928), 229; Hayek, *Preise und Produktion* (1931), 30, n., and *Monetary Theory and the Trade Cycle* (1932), 117 n.; Koopmans, "Zum Problem des neutralen Geldes," *loc. cit.,* 222, 241 n.; and Roll, "Menger on Money," *loc. cit.,* 457 f. It may be remarked that not all these writers have associated the concept of "neutral money" with the construction of a "barter economy" in the manner, say, of Koopmans (cf. the preceding note). Unfortunately, however, one of the lessons to be learned from the discussion growing out of momentarily fashionable concepts such as that of "neutral money" is that the ambiguity of connotation which was characteristic of the concept from the very beginning has been enhanced by the diversity of usage evidenced by subsequent writers who have employed the concept. There is all the more reason, therefore, for distinguishing that part of the argument attributed to Menger which was in fact his own, and that part which has been saddled upon him by later "interpreters."

sages in Menger's writings, is not only that he himself did not associate the latter distinction with the distinction between a "barter" economy, on the one hand, and a "money" economy, on the other, but that the whole point of his own distinction was to emphasize the fact that both "monetary" and "non-monetary" factors are of such far-reaching importance for price formation that one must be continually on one's guard against specious attempts to explain a given set of price movements in terms of either "monetary" or "non-monetary" factors alone.[40] He argued, in short, that any attempt to explain the "movement of commodity prices" requires, at every step in the process, not only the weapons of monetary theory, in the narrower sense of the term, but also the whole of the apparatus of "general" pricing theory.[41] And in so arguing he provided a further proof that, although he himself did not go so far as one might have hoped toward a final solution of the problem of relating the substance of the "general" Theory of Value to those parts of monetary theory which are relevant to the problem of the determination of the structure of money prices, he nevertheless showed an awareness of the nature of the required solution which later writers might well have emulated.

Of the four authors with whom we are here concerned, however, it is Léon Walras and Alfred Marshall who have the clearest claims to being regarded as "classical" authors, in the sense in which Marshall himself defined a "classical author": namely, one who "either by the form or the matter of his words or deeds . . . has stated or indicated architectonic ideas in thought or sentiment, which are in some degree his own, and which, once created, can never die but are an existing yeast ceaselessly working in the Cosmos."[42] It is therefore worth asking whether the two authors indicated

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[40] See Menger's "Geld" (*Collected Works,* IV, 73 ff.). It may be pointed out that Menger, so far from presenting this conclusion as a discovery of his own, took pains to point out (p. 83) that it went back at least as far as Bodin. It is not surprising, therefore, to discover precisely the same kind of emphasis in Jevons (see, for example, the latter's *Investigations,* 15 ff., 43 ff., 49 ff., 120, 123). The difference between Menger and Jevons was that Jevons proposed to "solve" the age-old problem by "trusting to probabilities," and by constructing a *general average* of all prices, such that this average could, "in all reasonable probability," be taken to "represent some single influence acting on all the commodities" (*Investigations,* 147); whereas Menger resolutely refused to accept any such "solution" of the problem. See especially Menger's "Geld" (*Collected Works,* IV, 89 ff.). On the significance of this difference, and the nature of the argument by which it can be resolved, see below, pp. 278 ff., 333 ff.

[41] See especially, in this connection, the last two paragraphs on p. 91 of Volume IV of Menger's *Collected Works.*

[42] See Marshall's letter to J. Bonar, in *Memorials of Alfred Marshall,* 374.

developed their analytical structures either in complete abstraction from money or in such wise as to suggest that money is of no importance for an understanding of the functioning of the economic process. And again the answer must be, unequivocally, that they did neither.

In the case of Walras, point is given to the suggestion that an intensive concern with the establishment of a *modus vivendi* between monetary theory, on the one hand, and the "general" Theory of Value, on the other, has by no means always led to beneficial results, by the fact that a number of recent writers who have evidenced such a concern have also made themselves guilty of what can be described only as a complete failure to understand the essential aspects of Walras's treatment of the rôle of money in the economic process. I have discussed elsewhere the suggestion, for example, that Walras was guilty of a grave "inconsistency" in introducing money into his general system of "equilibrium," and I need not repeat that discussion here.[43] Nor need I repeat here the reasons for refusing to accept the contradictory accusation that Walras's "theory of equilibrium" was based throughout upon "barter" assumptions.[44] The fact, as I have tried to show, is rather that Walras, instead of "abstracting" from money in the construction of his general "system," included it as an integral part of that "system" in virtually all his major writings, from the earliest to the latest; and that in this, as in so many other respects, he deserves the place of honor that has been accorded to him among those who have succeeded in making "monetary theory a part of the general theory of the economic process." [45]

The thing to be said of this aspect of Walras's work, indeed, is that it is not only free, in virtually every significant respect, from the obsta-

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[43] See my "The Monetary Aspects of the Walrasian System," *loc. cit.*, 158 ff., and the references there given to J. R. Hicks. In Professor Hicks's more recent *Value and Capital* (1939) there is, to be sure, no formal recantation of the position which has been summarized by others under the slogan of the "Incompatibility of Money and Static Equilibrium" (so, for example, P. N. Rosenstein-Rodan, "The Coordination of the General Theories of Money and Price," *Economica*, August, 1936, 269 ff.). On the contrary, the earlier paper is cited (p. 4, n. 1) as representing one of the "earlier stages" of the work presented in *Value and Capital* itself. In fairness to Professor Hicks, however, it may be pointed out that the position adopted in the earlier paper with respect to the "incompatibility" of money with "equilibrium" not only is not formally reasserted in the later work, but would seem to have been tacitly abandoned. See, for example, *Value and Capital*, 59 and 249, where the holding of cash balances in a "static" system is taken for granted.

[44] See my "The Monetary Aspects of the Walrasian System," *loc. cit.*, 164, 168, n. 47, and 169 ff.; and cf. the comment in Rist, *Histoire*, 328, n. 2.

[45] So J. Schumpeter, "Das Sozialprodukt und die Rechenpfennige," *Archiv für Sozialwissenschaft und Sozialpolitik*, XLIV (1917), 631.

cles of both form and substance which later writers on the relation between the two bodies of theory have succeeded in introducing, in the form of factitious discussions with respect to the relation between "barter" assumptions and the concept of "equilibrium," but was so far in advance of later discussion in a number of other respects that the full significance of all aspects of Walras's discussion cannot yet be said to have been sufficiently appreciated by most workers in the field. It is only recently, for example, that comment has been made on that aspect of Walras's discussion which was concerned precisely with the effect of injections of "additional" money-spending power upon the structure of money prices and money incomes—and therefore upon the "structure" of output—and which thus anticipated much of the later discussion that came to be summarized under some such concept as that of "forced saving." [46] And it is only still more recently that comment has been made on the significance of his general analytical structure for an adequate treatment of the relation between money, on the one hand, and the rate of interest, on the other.[47] There are reasons, indeed, for believing that economists have by no means taken full advantage, even yet, of the suggestions for further development along these lines (as well as along others involving an understanding of the rôle played by money in the economic process) which can be found in Walras's work.[48] The one thing that can be said here is that in this respect

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[46] See my "The Monetary Aspects of the Walrasian System," *loc. cit.*, 150, and the references given in n. 12 thereto.

[47] It is of some importance, for an estimate of the significance of what Walras had to say on this subject, to recognize that the comments to which reference is made in the text have concerned only those parts of the Walrasian structure which bear upon the particular aspect of the problem of the relation between money and the rate of interest in which the commentators happen to have been interested. See, however, the comments on "Walras' Theory of Capitalization" by C. Bresciani-Turroni, "The Theory of Saving," in *Economica*, February, 1936, 3 ff.; and on the relation of Walras's analysis to what has been characterized by O. Lange as "the general theory of interest," see Lange's "The Rate of Interest and the Optimum Propensity to Consume," *Economica*, February, 1938, 20 ff.

[48] In addition to the suggestions touched upon in Volume I, pp. 406, n. 46, and 505, n. 57 of the present work, I might call attention to the following elements in the analytical structure presented in this work, virtually all of which can be related to suggestions to be found in Walras: (1) an explicit recognition of the fact that all realized money "prices" are the resultant of an impact of a stream of money, on the one hand, and of objects sold for money, on the other (see below, pp. 266 f., 364 f.), whether these "prices" are the prices of separate goods or *groups* of such goods (cf. Volume I, p. 428, and below, pp. 320 ff.); (2) an equally explicit recognition of the fact that these money prices may be summed in such a way as to represent the total of money receipts, of an income- or nonincome character, respectively, accruing to different groups within the economy, as well as the total of money payments out of these receipts or incomes, respectively (see Volume I, 383 f., and below, pp. 365 f.); (3) the provision of time-period subscripts, in such a way as to make the Walrasian "circuit" of payments

(as in so many others) the "architectonic ideas" that were "stated or indicated" by Léon Walras provide a commentary at once on (1) the suggestion that economists in general have allowed a serious gap to exist between their "general" economic theory, on the one hand, and their monetary theory, on the other, and on (2) much of the recent discussion which, in attempting to bridge this "gap," has succeeded only in creating a series of factitious difficulties from which the exposition of a writer such as Walras was, in fact, completely free.

The failure to do justice to Walras's treatment of the rôle played by money in the functioning of the economic process is understandable, if not forgivable, because of the peculiar position of Walras as a member of that select group of writers on economics who are more frequently honored by being referred to than by being read. Fortunately for economics, Alfred Marshall has not yet fallen into that category. It is therefore worth noting that writers really anxious to discover what "economists" have said concerning the rôle of money in the economic process have been able to report that it was precisely one of the characteristics of Marshall's work that it "puts money conspicuously into the foreground from the start." [49]

This is a judgment which no one familiar with Marshall's writings could be prepared for a moment to contradict. What it is really important to affirm, however, is that Marshall's putting of "money" "into the foreground" was represented by very much more than the adoption of a slogan which may be regarded as the antithesis of Mill's proposition that "there cannot, in short, be intrinsically a more insignificant thing, in the economy of society, than money." [50] Marshall did, indeed, say, and has often been quoted as having said, that "money" is "the center around which economic science clusters." [51] Nothing, however, could

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explicitly applicable to a process of payments evidencing *changes in time* (see Volume I, pp. 382, n. 85, and 383, n. 88, and below, pp. 351 f., 357 ff., 416 ff.), with a corresponding transference of emphasis from the concept of general economic *equilibrium* to the concept of general economic *interdependence* as the "essence" of the Walrasian system (see below, pp. 412 ff., 416 ff.); and (4) the application of the apparatus thus outlined to all problems involving a thesis with respect to (*a*) the generation and utilization of money income, (*b*) changes, over time, in the structure of prices and output, and, indeed, (*c*) any phenomenon which can be shown to be related to the magnitude and the direction of money payments.

[49] So Mitchell, "The Rôle of Money in Economic Theory," *loc. cit.,* 164. Contrast the considerably less generous comments by the same writer in his earlier paper, "The Rationality of Economic Activity," *loc. cit.,* 207 f.

[50] See above, p. 48.

[51] Marshall, *Principles,* p. 22 of the eighth (1920) edition. It is presumably this passage, as it appeared in earlier editions of the *Principles,* to which reference is made by W. W. Carlile in his *Economic Method and Economic Fallacies,* 171 n., when he attributes to Marshall the "opinion that money is the pivot of everything in economics." Cf. also Mitchell, "The Rôle of Money," *loc. cit.,* 164, where the passage quoted in the text is accurately reproduced.

more thoroughly misrepresent the substance of Marshall's position with respect to the rôle of money in economic theory than the suggestion that he regarded the slogan itself as anything more than what he himself characterized as at best "only the starting-point of economics." [52]

For, in the first place, he was himself prepared to provide a slogan with the contrary emphasis whenever he felt that such emphasis was necessary: as when he insisted that he was "never weary of preaching in the wilderness 'the only very important thing to be said about currency is that it is not nearly as important as it looks.' " [53] In the second place, it is perfectly clear, from a study of the context in which the slogan first quoted appears, that the "money" to which so much importance was thus assigned was itself a symbol for a complex of considerations, including that of the *motives* to economic action, which are only remotely connected with the questions raised when we ask in what specific ways the presence of a concrete medium of exchange affects the nature and the direction of economic processes.[54] And in the third place, no one could have been more explicit than Marshall in granting not only that "a world can be *conceived* in which there is a science of economics very much like our own, but in it there is no money of any sort," but also that the "playful excursions" represented by such constructions may "throw side lights on real problems" and are "often suggestive in unexpected ways." [55] What really matters, therefore, for a judgment of Marshall's treatment of the rôle of money in economic theory is not his utterance of the slogan itself. What matters is the fact that he himself insisted that any economic theory constructed in abstraction from the existence of money involved a shutting of our eyes to "realities" which "in serious work must be closely followed"; and what matters even more is that he himself provided an important series of examples of the way in which realism could be imparted to

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[52] Marshall, *Principles,* 17.

[53] Cf. Marshall's letter to J. Bonar, March 6, 1899 (*Memorials of Alfred Marshall,* 375).

[54] It would, indeed, conduce to much greater clarity, in discussion of the "rôle of money in economic theory," if, whenever what is in issue is the "making of money," one would refer to a "profit" economy, or (as Professor Mitchell does in his *Business Cycles: The Problem and its Setting,* 63 ff.), to a "business" economy, rather than to a "money economy." Cf., in this connection, what is said above, p. 36, n. 99. Again no one would deny that there have been important *historical* connections between the rise of a "profit" or "business economy" and the "money" economy, in the sense of an economy opposed to one based upon *barter.* Nor can it be denied that it is difficult to conceive of a "profit" or "business" economy as elaborate as our own in which all operations would be carried on by "barter." It is, however, quite easy to conceive of a nonbarter economy which would *not* be dominated by the desire to "make money." Everything, therefore, would argue for adopting a terminological usage which would keep the two sets of connotations quite distinct.

[55] Marshall, *Principles,* 22, n. 2, and 782.

economic analysis by an effort to do justice to those aspects of money which can be shown to be part of the "realities" of economic life.[56]

No one, for example, who has read what Marshall had to say concerning the rôle played by money in the *pricing process* could imagine his arguing, in the manner of more recent writers on the rôle of money in "general" economic theory, that the existence of money is "incompatible" with the kind of "equilibrium" which was envisaged in Marshall's account of the pricing process.[57] Nor, in view of Marshall's repeated warnings that his "neglect," in the "introductory" volume which his *Principles* was intended to be, of "possible changes in the general purchasing power of money" was merely a device designed to facilitate the exposition of one part of the subject, could anyone suppose that he believed that a theory of "pricing" could be in any sense "complete" until it took account of those changes in money prices and their mutual relations which can be attributed to the functioning of the monetary mechanism.[58] It must be remembered, finally, that Marshall promised repeatedly to deal with the interrelations between "Currency," "Credit," and "Employment" in later publications. It is therefore nothing but sheer misrepresentation to suggest that the alleged fact that nothing properly called a "Monetary Theory of Production" appears in Marshall's *Principles* shows that Marshall himself saw no need

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[56] For an example of Marshall's insistence in the direction indicated, see his *Principles,* 782 f., including the marginal headings. It should be obvious that the examples which are indicated in the text, immediately following, provide a commentary on Mr. Keynes's statement that Marshall's *Principles* is a "treatise which takes no account of money" *(General Theory,* 189).

[57] For examples of the type of argument indicated, see above, p. 68, n. 38, and p. 70, n. 43, and the references there given. Contrast, on the other hand, the comments of Marshall *(Principles,* 118) on the "urgent need for the free use of money, or general purchasing power," which "alone can be applied easily in an unlimited variety of purchases," if we are to have the kind of adjustment implied by the ordinary statements with respect to the utilization of income in such a way as to maximize utility; and see especially his comments on the influence of money as a factor tending to "steady the market," in contrast with what would occur under barter *(Principles,* 793; see also p. 336 and the famous Appendix F on "Barter"). These passages would certainly have to be taken into account in any attempt to evaluate the characterization of Marshall as one of those who, though their "theory of exchange has been couched in terms of price," adopted this procedure "only because it simplified the task of exposition," even though they "felt that the procedure involved some sacrifice of scientific rigor" (so Young, "Some Limitations of the Value Concept," *loc. cit.,* 199 n.).

[58] For examples of Marshall's warnings in the direction indicated, see his *Principles,* 62, 109, 355, n., 593 ff., 709 ff. It may be remarked that in this respect, as in so many others, Marshall was a strict Ricardian. See above, p. 36, and the references given in n. 97 thereto.

for, and would have been unsympathetic to, the construction of such a theory.[59]

It is, however, in connection with the relation between money and *the rate of interest* that Marshall's awareness of the rôle of money in the economic process was most clearly evidenced. It was Marshall himself, for example, who pointed out that it was made clear, as early as the Preface to the first edition of his *Principles*, that one of the "chief purposes" of the work was "to insist that the term 'interest' is properly applicable only to 'free' or 'floating' capital" in the form of "money or general purchasing power." [60] This proposition, indeed, is one whose

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[59] In this connection, see the remarks by Mr. Keynes in his contribution to *Der Stand und die nächste Zukunft der Konjunkturforschung (Festschrift für Arthur Spiethoff* [1933]), 123, where, in complaining of the "lack," in economic literature, "of what might be termed a *Monetary Theory of Production*," Mr. Keynes cited Marshall's *Principles* as a case in point. For examples of Marshall's announced intention to deal with such subjects as "Money," "Credit," and "Employment" in later volumes of the series of which the *Principles*, as we now have it, was intended to be only the first volume, see Marshall's preface to the fifth (1907) edition of the *Principles*, pp. v–vi; and cf. Keynes's memoir on Marshall, p. 60 of the *Memorials of Alfred Marshall*. See also the last paragraph of the main text of the *Principles* (p. 736 of the first edition, p. 722 of the eighth edition), where Marshall reminded the reader why he had been able, in his first volume, to reach "very few practical conclusions" was that "it is generally necessary to look at the whole of the economic, to say nothing of the moral and social, aspects of a practical problem before attempting to deal with it at all." He pointed out particularly that "in real life nearly every economic issue depends, more or less directly, on some complex actions and reactions of *Credit*." (Cf. p. 324 of the eighth edition, where Marshall reminded the reader that his "account of markets" was to be regarded only as "provisional," and he adduced, as a principal reason for this, the fact that "the organization of markets is intimately connected both as cause and effect with *money, credit,* and foreign trade," so that "a full study of it must therefore be deferred to a later volume, where it will be taken in connection with *commercial and industrial fluctuations*" [italics mine]. See also pp. 620 and 660 of the eighth edition, and Marshall's *Economics of Industry,* Book III, Chap. II, on "Market Fluctuations," with its discussion of the effect on the "demand for commodities" of "alternations of commercial prosperity and adversity" and related variations in "credit" and "purchasing power" [p. 163].) It was Marshall's firm "belief that each of these sets of conditions [including those associated with *"credit* and *employment"*] influences and is influenced by the others" (cf. the Preface to the fifth edition of the *Principles*, p. v); and, indeed, it was this belief that led him to incorporate even into the "introductory volume" that his *Principles* was supposed to be, passages taken from the account of fluctuations in "the state of trade" presented originally in his *Economics of Industry*, in which major emphasis was put upon the influence of pecuniary phenomena such as variations in the volume of "credit." See the *Principles,* 710 f.; and cf. the *Economics of Industry,* 152 ff.

[60] See, for example, the Preface to the fifth edition of the *Principles* (p. xi); and cf. the Preface to the first edition itself (p. viii of the eighth

full significance can be appreciated only after a consideration of all those sectors of so-called "interest theory" in which its implications are ignored; and it is anything but clear that its full implications are adequately appreciated by a large number of economists even today.[61] That this, however, is by no means all that Marshall had to say on the subject of the relation between interest and money is best seen by observing that Marshall himself emphasized, independently of Wicksell and Fisher, the two central doctrines which have led Mr. Keynes to assign to the two writers indicated places in the list of his intellectual ancestors in the treatment of money as a "real" factor in the determination of the rate of interest.[62]   Nor is the irony of the situation lessened

edition). For the revelant passages in the body of the *Principles,* see pp. 411 f. and 593 of the eighth edition; and cf. *Money, Credit, and Commerce,* 289. On the proposition itself, as stated in the text above, cf. what is said on Jevons, above, p. 62, n. 25, and p. 63, n. 27.

[61] Since a full demonstration of this proposition must be left for another occasion, I must content myself with pointing out that what is involved is nothing less than a complete revision of all versions of the theory of the determination of the "rate of interest" running in "real capital" terms, which fail to take account of the fact that, as Wicksell put it in his *Interest and Prices* (though it is anything but clear that Wicksell himself was aware of the full implications of his proposition), "in modern communities" "real" capital goods are "never lent—they are never given and taken by way of borrowing—they are simply *bought or sold*" for money (*Interest and Prices,* 102). Of Mr. Keynes's comments, in his *General Theory* (pp. 186 ff.) on Marshall's utterances in this connection, it need be remarked only that it is a strange criticism of Marshall's *Principles* to characterize the latter as "a treatise which takes no account of money" and at the same time to suggest that a discussion of the monetary aspects of the "interest" problem "has really no business to turn up at all" in that treatise, on the ground that such a discussion belongs to "another branch of the subject" (Keynes, *General Theory,* 189)—particularly when the criticism comes from one who has protested against an alleged lack of connection between monetary theory, on the one hand, and "general" economic theory, on the other!

[62] On Marshall's position in the history of what may be regarded as the heart of the "classical" doctrine with respect to the *modus operandi* of bank rate, of which Wicksell regarded himself as an exponent, see Volume I, pp. 184, n. 74 and 191 f. of the present work. On Marshall's position with respect to the doctrine discussed by Fisher under the head of "Appreciation and Interest," it should be sufficient to observe that, although Mr. Keynes makes no mention of the fact in his *General Theory* (see, in this connection, the comment by Mr. Robertson in his "Notes on Mr. Keynes's General Theory of Employment," *Quarterly Journal of Economics,* LI [1936], 179, n. 2), it was Mr. Keynes himself, in his memoir on Marshall, who not only called attention to the relation of Marshall's analysis to that of Fisher (*Memorials of Alfred Marshall,* 27, n. 2, and especially 30, n. 3), but also characterized Marshall's "distinction between the 'real' rate of interest and the 'money' rate of interest, and the relevance of this to the credit cycle, when the value of money is fluctuating" as one of "the most important and characteristic of Marshall's original contributions" to economics (*Memorials,* 29 f.). It should be observed especially that this particular "contribution"

by the fact that in both cases, as in so many others, Marshall was merely restating doctrines for which authority can be found in Ricardo and Mill, whom Mr. Keynes has specifically designated as representatives of that *"classical* theory of the subject" with which his own work is to be regarded as having broken completely.[63]

II

THE "SCHOOLS"

The material presented in the preceding section of this chapter, when taken in conjunction with that presented in Chapter One, should dispose once and for all of two suggestions that have been repeatedly made. The first is that "the theory of money has been for a long time a more or less isolated discipline," in the sense that there has been a general "lack of connection" between the theory of the Value of Money, on the one hand, and the "general" Theory of Value, on the other; and the second is that this supposed "lack of connection" is partly attributable to the alleged "fact that the writers who have developed the general theory of value have not been, in general, the writers who have most elaborated the theory of the value of money." [64]

It has been demonstrated that the greatest names in the development of the "general" Theory of Value after 1870 were also, with very few exceptions, the names that would have to be invoked in any account of the attempts to apply the "general" Theory of Value to the special problem of the Value of Money. This, however, does not in itself amount to a demonstration that this aspect of the work of the writers indicated was clearly appreciated at the time, say, that Keynes's *General Theory* was published, in such wise that one could regard it as a dominant aspect of contempo-

of Marshall follows immediately, in the *Principles* (pp. 593 ff. of the eighth edition), upon one of Marshall's most emphatic statements to the effect that "it cannot be repeated too often that the phrase 'the rate of interest' is applicable to old investments of capital only in a very limited sense." This fact itself constitutes a further commentary on Mr. Keynes's characterization of the *Principles* as a treatise which "takes no account of money," and in which, therefore, a discussion of the forces determining the rate of interest "has really no business to turn up at all" (see the preceding note).

[63] See, in this connection, the comments on Ricardo and Mill, respectively, above, p. 7, n. 11, and the references there given.

[64] The quotations are from Anderson, *The Value of Money*, 47.

rary monetary theory. There was, after all, the case of Pareto, who showed himself to be so blind to the significance of this aspect of the work of Walras that the Walrasian influence was completely lost upon those members of the "school of Lausanne" who derived their inspiration from Pareto, instead of from Walras directly.[65] It is of some interest, therefore, to ask whether, in the "schools" of economic theory, other than the school of Lausanne, dominant at the time the *General Theory* was published, there was evidenced a concern with the relations between the "general" Theory of Value, on the one hand, and the theory of the Value of Money, on the other.[66]

On this point, the evidence is unequivocal. For indeed it was precisely a characteristic of the monetary theory of the 1920's, say, that in every one of the leading "schools" of economic theory, outside the school of Lausanne, there was at least one outstanding writer who showed, for the problem indicated, a degree of concern much more articulate and intense, if anything, than the concern evidenced by the founders of the "school" to which the writer in question acknowledged allegiance.

In the case of the "older" Cambridge school, for example, the treatment of the problem by Pigou, Lavington, and Robertson (all of whom acknowledged their indebtedness in this connection to Marshall) was such that, to those for whom "orthodox" economics is the economics of "old" Cambridge, it has seemed proper to characterize "the orthodox Theory of Money" as "an attempt to apply the supply-and-demand tool to the analysis of the purchasing power of money," in the sense that "just as, in the Theory of Value, the supply-and-demand mechanism is used to analyse the

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[65] See my "Léon Walras and the 'Cash-Balance Approach,'" *loc. cit.,* 596 ff., and my "The Monetary Aspects of the Walrasian System," *loc. cit.,* 152 ff.

[66] It may be pointed out that no account of the extent to which such a concern was evidenced in the decades preceding the publication of the *General Theory* would be complete if it failed to mention the discussion of the matter by a number of writers outside the "schools" indicated. See, for example, the references given in my "Léon Walras and the 'Cash-Balance Approach,'" *loc. cit.,* 571, n. 5, and the reference to T. N. Carver on p. 592, n. 53, of the same article, as well as Carver's later article on "The Demand for Money," *Economic Journal,* XLIV (1934), 188 ff.

forces determining the value of a single commodity, so in the traditional Theory of Money the supply-and-demand mechanism, with some necessary modifications, is used to analyse the forces determining the value of money." [67]   In the case of the "Austrian" school, the degree of concern with this problem evidenced by Wieser, among the members of the older generation, and by Mises, among the members of the "middle" generation, was so articulate that the relevant writings of both were seized upon for intensive discussion, if not always for enthusiastic comment, by other writers concerned with the relation between the two bodies of theory. [68]   And in the case of the "school" of London, the

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[67] So Joan Robinson, "The Theory of Money and the Analysis of Output," *Review of Economic Studies,* I (1933), 22 (cf. also the comment by J. Schumpeter in the *Journal of the American Statistical Association,* XXXI [1936], 793, where the "extension of the 'Marshallian cross' to the case of money" is characterized as "a besetting sin of the Cambridge group"). Mrs. Robinson's further conclusions with respect to "the traditional Theory of Money" are commented on below, pp. 82 ff. Here it is sufficient to point out that the very fact that Mrs. Robinson could, in 1933, characterize "the traditional Theory of Money" in the terms quoted in the text, itself provides a commentary on Mr. Keynes's statement, in 1936, as to the rôle of the "homely but intelligible concepts" of "supply and demand" in the "Theory of Money and Prices." It will be recalled, for example, (1) that the very framework of Pigou's well-known paper on "The Value of Money" (*Quarterly Journal of Economics,* XXXII [1917], 38 ff.) was built upon the concepts of "Demand" and "Supply" (176 ff., 189 ff., 191 ff. of the revised version of the paper published in Pigou's *Essays in Applied Economics*); (2) that the "Outline of the Theory of Money" presented in Lavington's *The English Capital Market* (1921), 22 ff., is based precisely upon the concepts of "The Demand for Money" (Chap. VI) and "The Supply of Money" (Chaps. VII–IX), respectively; and (3) that D. H. Robertson's account of "How the Value of Money is Determined" (*Money,* p. 28 of the first edition; cf. p. 30 of the second edition) is based on the proposition that the value of money "is primarily determined by exactly the same two factors as determine the value of any other thing, namely, the conditions of demand for it, and the quantity of it available." Cf. also the comment of H. D. Henderson, *Supply and Demand* (1922), 33 f., on the bearing of "our general laws" of "supply and demand" on "monetary and allied questions."

[68] For examples of extended discussion of this aspect of Wieser's work, see, in addition to Anderson, *The Value of Money,* 83 ff., the references to Wieser's writings on money in F. X. Weiss, "Die moderne Tendenz in der Lehre vom Geldwert," in the *Zeitschrift für Volkswirtschaft, Sozialpolitik, und Verwaltung* (XIX), 1910, and W. Hirsch, *Grenznutzentheorie und Geldwerttheorie* (Jena, 1928). For similar examples of discussion of, or comment on, the corresponding aspect of Mises's work, see, in addition

explicit emphasis which was provided by the very title of
Professor Cannan's chief paper on the subject—"The Appli-
cation of the Theoretical Apparatus of Supply and Demand
to Units of Currency"—made it inevitable that his dis-
ciples, in listing Cannan's contributions to contemporary
monetary theory, should have emphasized particularly "the
added significance" which is provided "when the laws regu-
lating the value of money are fully subsumed under the
General Law of Value." [69]   Thus, the concern with the
establishment of a *modus vivendi* between the "general"
Theory of Value, on the one hand, and the theory of the
Value of Money, on the other, had been so marked in the
years preceding 1936 that the chief interest attaching to
current suggestions that the problem had been "neglected"
is that they again show how easy it is for those anxious to
establish a claim for originality in the posing of a given
problem to be considerably less than generous to those who
have anticipated them.

In the case of "old" Cambridge, for example, it was surely gratuitous
to suggest that while "Marshall and his followers were aware that money
ought to be subjected to marginal utility analysis," their "invocation of
marginal utility remained little more than a pious hope" as the result
of their insistence upon adopting a "real-balance" version of the so-
called "cash-balance approach." [70]   For if anything is clear from such
discussions as we have had of the relation between "utility analysis," on
the one hand, and the concept of "real balances," on the other, it is that
virtually all of it is concerned with issues that can be shown to be en-
tirely factitious.[71]   Nor, in the light of a work such as Robertson's

to the works just cited, the comments by Professor Robbins in his Intro-
duction to Mises's *The Theory of Money and Credit*, 12, and in his *Essay
on the Nature and Significance of Economic Science* (1932), 82.

[69] So T. E. Gregory, "Professor Cannan and Contemporary Monetary
Theory," in *London Essays in Economics in Honor of Edwin Cannan*, 40.
Cf. also Robbins, *Essay*, 82. It may be added that the very title of the
article by Cannan cited in the text (*Economic Journal*, XXI [1921]) itself
provides a further commentary on the suggestion that when economists
have passed from the general Theory of Value to the Theory of Money and
Prices, we have heard "no more of homely but intelligible concepts" such
as "supply" and "demand."

[70] So J. R. Hicks, "A Suggestion for Simplifying the Theory of Money,"
loc. cit., 2 n.

[71] It is, indeed, something of a commentary on the factitious nature of
the issues involved that, whereas Hicks argues that the use of a "real-
balances" variant of the cash-balance approach *prevents* the application
of the concept of "marginal utility" to the problem of cash-balance admin-

Banking Policy and the Price Level, with its emphasis on the relation between "Applied Lacking" and "Abortive Lacking, or Hoarding," on the one hand, and the Marshallian version of the cash-balance approach, on the other, can it be regarded as anything more than either an empty bit of formalism or sheer misrepresentation to suggest that the adherence to "real-balance" variants of the cash-balance approach made the Marshallians "unable to distinguish, on marginal utility lines, between the desire to save and the desire to hoard." [72] Nor, finally, in the light of the possibility of demonstrating that the better articulated versions of the "real-balance" approach reduce, in all essentials, to the better articulated versions of the "cash-balance" approach which were not expressed in "real" terms, can it be regarded as anything but gratuitous to suggest that an element of "indeterminateness" is introduced into the "real-balance" versions, in a sense in which it is not introduced into other variants of the "cash-balance approach," whenever "the prices of consumption goods are expected to change." [73]

The evidence, indeed, is so clear that the Marshallians were as explicit as one could wish in applying the concepts of their "general" Theory of Value to the special problem of the Value of Money, that other commentators, instead of attempting to demonstrate that the Marshallians did not so apply these concepts, have argued that the vice of their treatment resided precisely in the fact that they did; for, it is suggested, their very concern with the problem blinded them to the significance of other aspects of monetary theory which are at least as important as those concerned directly with the application of the categories of the "general" Theory of Value to the special problem of the

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istration, other writers have argued that it is only through the use of a "real-balances" variant that such an application becomes possible. In fact, however, neither proposition can be defended. See what is said on this matter in Volume I, 450 ff., of the present work.

[72] So Hicks, "A Suggestion," *loc. cit.,* 2 n. Contrast Robertson, *Banking Policy and the Price Level,* 45 f.

[73] Hicks, "A Suggestion," 2 n. On the formal identity of the "real-balance" variants of the "cash-balance approach" with other variants of that approach when the better-articulated variants of both are used, see, for example, Volume I, pp. 449, n. 96, and 455, n. 112. On the rôle played by expectations with respect to changes in prices in each type of variant, see what is said, for example, in Volume I, 446, n. 88; and, on the relation of expectations concerning price movements to the "price-level" involved in "cash-balance" equations generally, see Volume I, 429 ff. As a final commentary on the absence of fundamental differences between the better-articulated versions of both the "real" and the "monetary" variants of the cash-balance approach, it may be observed that Hicks himself, despite his criticisms of "Marshall and his followers" for their use of the concept of "real balances," is forced to "admit that some versions of the Marshallian theory come very close" to what he is "driving at"; and he cites, in this connection, Chapter Six of Lavington's *English Capital Market,* despite the fact that the argument of most of this chapter is couched in terms of the concept of "real balances."

Value of Money.[74] That a concern with the latter type of problem
has had the effect, in many cases, of encouraging a vicious exclusivism
in the statement of the purposes and the results of monetary theory,
there cannot be the slightest doubt.[75] Yet common fairness demands
that the charges leveled in this connection by members of the "new"
Cambridge group against the "traditional Theory of Money," in general,
and the Theory of Money of "old" Cambridge, in particular, as well as
the claims made in the same connection on behalf of the monetary
theory of "new" Cambridge, be reduced to something like their proper
proportions.

In the light, for example, of the very extensive literature on the sub-
ject of the effect of monetary expansion and contraction upon "output
as a whole," and of the rôle assigned in that literature to the concept
which has come to be called "general ["moneyed"] demand," it is little
short of grotesque to say that the suggestion, imputed to Keynes's
Treatise, that "progress can be made by thinking in terms of the de-
mand for output as a whole, and its cost of production," effected a
veritable "revolution" in monetary theory.[76] What is worth noting
here is rather that (1) the writers who have put most emphasis on
the concept of the money "demand for output as a whole" ("general

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[74] This is one of the principal contentions advanced in the article by
Joan Robinson cited above, p. 79, n. 67. Cf. also the references to J.
Schumpeter given in Volume I, 441, n. 78 of the present work, and the
further comments below, pp. 110 f.

[75] See, for example, what is said on this matter in Volume I, 449 f. of
the present work; cf. also below, pp. 128 f., and the references there given.
In the light of the fact that the work of J. R. Hicks has since been grouped
with that of Mr. Keynes as having imparted "a new unity to the theory
of value and the theory of money" (see above, p. 35, n. 95, and below,
page 83), it may be pointed out here that Hicks's often cited article
(cf. above, p. 52, n. 1, and p. 80, n. 70) represents a particularly striking
instance of the type of exclusivism indicated in the text. See below, p.
83, n. 78, and p. 98, n. 21.

[76] Cf. Joan Robinson, "The Theory of Money and the Analysis of Out-
put," loc. cit., 22, 24. Mrs. Robinson adds, to be sure, that Mr. Keynes
himself, at the time he wrote the Treatise, failed "to realise the nature of
the revolution he was carrying through." In view of the fact that, so far
as one is able to discover, the very expression "demand for output as a
whole" does not appear in the Treatise, this must be regarded as a really
extraordinary example of understatement. Contrast what is said below,
p. 204, n. 132, and p. 686, n. 13, with respect to the use of the concept of a
money "demand for output as a whole" in economic literature prior to the
appearance of the Treatise. Mrs. Robinson is on firmer ground when she
points to the use, in the Treatise, of the concept of the "cost of production"
of "output as a whole" (the E/O of the Fundamental Equations of the
Treatise). See, however, what is said below, pp. 539 ff., with respect to the
usefulness of this concept, as compared with other methods of treating
the element of "cost of production," even when the problem is that of
establishing the nature of the forces determining the level of "output as a
whole."

demand") have been precisely those who have been least sympathetic
to suggestions, such as that in the *General Theory*, that the Theory of
Money and Prices has suffered from the failure to apply the "homely
but intelligible concepts" of the general Theory of Value to the problem
of the Value of Money; whereas (2) a number of these writers have
been very sympathetic to the concept of money prices as "governed by
the quantity of money, but its income-velocity, the velocity of circula-
tion relative to the volume of transactions, . . . *et hoc genus omne"*—
that is, the type of concept which, according to the Keynes of the
General Theory, is typical of those used by writers who have been willing
to tolerate the existence of a serious gap between the two bodies of
theory.[77] As far as "old" Cambridge is concerned, moreover, it is worth
noting that if one of its sins was an *excessive* emphasis on the applica-
tion to the special problem of the Value of Money of the categories of
the "general" Theory of Value, this is a sin which is much less fairly
chargeable against "old" Cambridge than it is against certain writers
who have been grouped by admirers of Mr. Keynes's later work with
Mr. Keynes himself as having been able to "impart a new unity to the
theory of value and the theory of money."[78] And finally, in the light
of productions, by members of the "old" Cambridge group, such as
Robertson's *Banking Policy and the Price Level* and the chapters in
Pigou's *Industrial Fluctuations* devoted to the effects of monetary ex-
pansion and contraction upon the level of output as a whole, it is
nothing less than sheer misrepresentation of the substance of the mone-
tary theory of "old" Cambridge to suggest that, for it, the whole of the
Theory of Money is adequately "described as an attempt to apply the
supply-and-demand tool to the analysis of the purchasing power of
money," in a degree which would permit it to be completely uncon-
cerned with the rôle of money in any "Analysis of Output."[79]

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[77] See again Keynes's *General Theory*, 292. It is characteristic that Mrs.
Robinson, in the article cited (pp. 23 f.), as well as in her later writings
(see, for example, her *Introduction to the Theory of Employment* [1937],
94 ff.), evidences the same desire to disparage the familiar Quantity
Equations as did the Keynes of the *General Theory*. Contrast, on the
other hand, what is said below, pp. 104 f.. with respect to the use, by de-
fenders of the Quantity Equations, of the concept of "general demand."
[78] In this connection, see the comment on the work of J. R. Hicks, cited
above, p. 35, n. 95. As an example of the degree of exclusivism spon-
sored by Hicks, attention may be called to p. 3 of his "A Suggestion,"
where it was contended that the only thing in Keynes's *Treatise* which "to
a value theorist looks sensible and interesting," by virtue of its emphasis
upon "a choice at the margin," was Keynes's discussion of the "relative
preference of the investor—to hold bank-deposits or to hold securities,"
and that it is therefore from this "that we ought to start in constructing the
theory of money." I have been unable to find any such drastic limitation
of the scope of "the theory of money" in any of the writings of the mem-
bers of the "old" Cambridge group.
[79] The quotation is from Joan Robinson, "The Theory of Money and
the Analysis of Output," *loc. cit.*, 22. Cf. also what is said by Mr. Keynes

The same lack of generosity must be charged, likewise, against those who, in their anxiety to signalize the importance of their own contributions, have done considerably less than justice to those contributions made by "schools" other than that of "old" Cambridge. Consider, for example, the statement that monetary theory has "done without marginal utility altogether."[80] The examples to the contrary provided by writers such as Walras, Carver, and Cannan (to go no further) are enough in themselves to create some astonishment at such a statement.[81] And, indeed, Professor Hicks, who is responsible for the generalization, has himself attempted to moderate that astonishment by indicating that what he really meant was not that the concept of "marginal uility" had not been applied to the theory of the Value of Money, but that it had not been applied correctly.[82] In fact, however, his own statement of the problem shows that the very writers he criticizes were in agreement with him in the *substantive* part of his analysis, and that his criticisms were either concerned with aspects of their analysis which are subject to reservations on grounds quite apart from the question of the applicability of the concept of "marginal utility" to the problem of the Value of Money, or are purely formalistic in character.

For, as Professor Hicks himself reminds us, "marginal utility analysis is nothing less than a general theory of choice." It follows, therefore, that the very fact that "people do choose to have money rather than other things" means that "money must have a marginal utility."[83]

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himself on the absence, in the writings of the members of the "old" Cambridge group, of a "monetary theory of production," or even of a theory of "production as a whole," in his contribution to the *Festschrift für Arthur Spiethoff* (cf. above, p. 75, n. 59), and in the introduction to the German version of his *General Theory,* p. vii. Of the chapters in Pigou's *Industrial Fluctuations* which bear upon the justice of these generalizations, particular attention may be called to Chaps. VIII and XII–XVII, in Part One, and Chaps. III–VIII, in Part Two, of that work.

[80] So Hicks, "A Suggestion," 2.

[81] On Walras's treatment of the relation of the theory of the Value of Money to "the theory of *rareté*," see above, p. 54, and the references given in n. 6 thereto. For Carver's attempt to show how money can be "brought under the law of decreasing utility, or of 'final utility,' " see the *Publications of the American Economic Association,* third series, VI (1905), 129 ff. In view, however, of Professor Hicks's earlier association with the "school" of London, Cannan's utterances in this connection are of greatest interest. See, for example, his discussion of the relation of the concept of "marginal utility" to the problem of the Value of Money, in the quotation from his *Money* given by Gregory, "Professor Cannan and Contemporary Monetary Theory," *loc. cit.,* 40 f.; and cf. Robbins, *Essay,* 82, on Cannan as one of the writers whose theory of the Value of Money was constructed "on the basis of the general Law of Diminishing Marginal Significance." It may be observed also that since Cannan's statement of the problem was not couched in terms of the concept of "real balances," the objections raised by Hicks on this account (cf. above, pp. 80 f.) would not apply to his presentation.

[82] Hicks, "A Suggestion," 2 f.

[83] Hicks, "A Suggestion," 3.

What this means, however, if it means anything, is that monetary theory has made use of what amounts to "utility analysis" (even if it has not in all cases used the *term* "marginal utility") whenever it has made use of *cash-balance analysis;* for the latter is concerned precisely with the considerations that lead individuals to "choose to have money rather than other things." [84]

The actual use of the *term* "marginal utility" is, therefore, a bit of formalism; and so are the criticisms which Professor Hicks makes against particular cash-balance theorists on the ground that they have introduced, in *addition* to their emphasis upon the "choice" between the holding of money and of "other things," details of which he disapproves. We have seen, for example, that it becomes a matter of quite minor importance, from the standpoint of one interested in the central methodological implications of the "cash-balance approach," that the Cambridge sponsors of "cash-balance" analysis have used a "real-balance" variant thereof, in the light of the fact that they were most certainly concerned with the "choices" of individuals with respect to the holding of money rather than of "other things." [85] Similarly, it is a matter of quite minor importance that Mises, in his discussion of the application of the concept of marginal utility to the special problem of the Value of Money, should have come to a conclusion which Professor Hicks translates into the proposition that "money is a ghost of gold." [86] For what really matters is that Mises did relate his "cash-balance" analysis to the "theory of choice"; indeed, his emphasis upon "subjective considerations" was concerned with nothing else.[87] It is reflections

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[84] This is made perfectly clear by Professor Hicks's own statement: "The essence of the method I am proposing is that we should take the position of an individual at a particular point of time, and enquire what determines the precise quantity of money which he will desire to hold" ("A Suggestion," 4). What is this, if it is not the very "essence" of cash-balance analysis?

[85] On the relation of the concept of "marginal utility" to "real-balance" variants of the cash-balance approach, see above, pp. 80 f., and the references given in nn. 70–72 thereto. It may be pointed out also that Professor Hicks's more recent characterization (*Value and Capital*, 56, n. 3) of the "demand for money" in the sense of a "demand" for "all commodities" other than the one taken for examination as the "Marshallian sense" of the expression "the demand for money," is curiously inaccurate and misleading. This is so not only because the phrase itself is hardly adequate as a summary of the implications of Marshall's use of the concept of "the marginal utility of money [income]," but also because it might suggest to some readers that the "demand for money" in Professor Hicks's second, and more common, sense of the term is a concept which cannot be characterized as "Marshallian."

[86] Hicks, "A Suggestion," 2.

[87] See especially Mises's *Theory of Money and Credit*, 131 ff.; and cf. what is said with respect to the element of "choice," *ibid.*, 122. It is, of course, true that a substantive issue would be involved if Mises really argued that the "choices" involved proceeded on the assumption that the monetary unit is always regarded, in the valuation process, as a nugget

of this kind which lead one to the conclusion that the chief effect of
episodes of the type indicated has not been to further the substantive
development of monetary theory. The chief effect, on the contrary, has
been to provide further illustrations of the degree to which a concern
with the formal application to the special problem of the Value of
Money of categories developed within the "general" Theory of Value
may not only impede, instead of furthering, an appreciation of what is
truly relevant for the heuristic purposes of monetary theory, but may
also lead to a systematic undervaluation of the substantive results ob-
tained by earlier writers on the subject.

To stop, however, with a demonstration that the alleged
"neglect" by contemporary monetary theorists of the prob-
lem of establishing a *modus vivendi* between the theory of
the Value of Money, on the one hand, and the "general"
Theory of Value, on the other, is largely a myth would be to
miss one of the chief lessons to be learned from the history
of doctrine on the subject. It is true, to be sure, that the
intensification of interest in the problem which was evi-
denced by the leaders of the "schools" in the first three
decades of this century brought light on some issues of sub-
stance within the field of monetary theory. But if any-
thing is certain, it is that this same intensification of inter-
est brought obfuscation of other issues within that field.
It should, therefore, have been the first task of writers on
the relation between the two bodies of theory to indicate
precisely the points at which a concern with these relations
had brought light, on the one hand, and those, on the other,
to which it had brought little but obscurity.

We have already had occasion to observe, for example,
how such a concern could lead to the kind of *exclusivism*
which saw nothing in Ricardo's *partial* application of the

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of gold, or as a claim whose value depends primarily upon its prospects of
conversion into such a nugget, and on the assumption that the elements
of value inhering in the "monetary function" are of altogether minor im-
portance, if they are important at all. How far Mises is from supporting
such a view, however, may be judged from his discussion of Laughlin, in
this connection, on pp. 124 ff. of *The Theory of Money and Credit,* as well
as from his positive discussion on pp. 139 ff. of the same work, where the
concept of the "diminishing marginal utility of the monetary unit" is ex-
plicitly applied to the case of an "increase in the amount of money" *re-
gardless* of whether the "economic agents" holding the new "money" are
"the *issuers of fiat or credit money* or the producers of the substance of
which commodity money is made" (italics mine).

element of "cost of production" to the problem of the Value of Money but an "inconsistency" between his theory of the Value of Money, on the one hand, and his "general" Theory of Value, on the other.[88] It may therefore be observed here that an equal degree of obfuscation followed from the equally blameworthy exclusivism which led a writer such as Cannan to argue not only positively on behalf of his own variant of what has been called in this work "the cash-balance approach," but also negatively against the very use of concepts such as "velocity of circulation" and the type of "Quantity Equation" in which such concepts are included.[89]

The examples of confusion which can be cited, however, by no means end here. Nothing but confusion, surely, is involved when a given application of a concept developed within the "general" Theory of Value to the problem of the Value of Money results only in a *rewriting in unfamiliar terms of results already familiar within the field of the Theory of Money and Prices;* yet there can be little doubt that this was all that was accomplished by Professor Cannan's discovery, for example, that the "elasticity of demand for money" is not necessarily equal to unity.[90]

Similarly, it is difficult to see anything more than an obfuscation of the real issues in any encouragement of the suggestion that a problem is advanced further toward solution by the use of categories developed within the "general" Theory of Value, whenever all that such use accomplishes is a *restatement of the problem to be solved;* yet this was all that was accomplished by the statement, for example, of the forces determining the size of cash balances in terms of a weighing of the "utilities" or "disutilities" involved in the holding of such balances.[91]

Nor can there be any doubt, finally, that obfuscation sets in as soon as a discussion of the relation between the two

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[88] See above, pp. 32 ff., and the references given in nn. 88 and 91 thereto.

[89] See, for example, Cannan, *Money*, 73, and *An Economist's Protest*, 385. Attention should be called to the contrary practice, in this respect, of Marshall and the Marshallians. See above, p. 60, and the references to Marshall, Pigou, and Robertson given in n. 21 thereto.

[90] See below, pp. 652 ff., 658 ff.

[91] Cf., in this connection, what is said in Volume I, 480 f.

bodies of theory passes from an examination of the ways
in which either body of theory or both bodies of theory can
be used to explain processes occurring in the real world, and
degenerates into a discussion of issues which are *purely
factitious, in the sense that their "solution" advances us not
a step further toward an explanation of these "real" proc-
esses.* Such degeneration was involved in much of the
discussion, for example, of the contributions to monetary
theory by Friedrich von Wieser: as when the commentators
passed over his emphasis on the rôle of *money income* in the
processes of price formation in order to devote major atten-
tion to those parts of his argument with respect to the rela-
tion of "utility analysis" to the problem of the Value of
Money which a cynic might characterize as the problem of
squaring the "Austrian circle." [92]

It is precisely such episodes as these which ought to have
tempered the admiration of so many for an intensive con-
cern with the problem of the relation between the "general"
Theory of Value, on the one hand, and the theory of the
Value of Money, on the other; and it is such episodes which
must be taken into account not only by one who would
write the history of the achievement that has resulted from
a concern with this problem in the past, but also by anyone
who would insist that the monetary theory of the future
must necessarily start from a concern with precisely this
problem.

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[92] In this connection, see, for example, Volume I, 450 ff., and the refer-
ences to discussions of this aspect of Wieser's writings given in the works
cited above, p. 79, n. 68. It should be clear, from the references to Volume
I just given (cf. also p. 443 of the same volume) that I am not of the
opinion that the application of "utility analysis" to the problem of the
Value of Money represents an attempt to "square the circle" in the sense
that it represents the posing of a problem which is incapable of formal
"solution." On the contrary, when once the specific utility of the *cash
balance* is put in place of the utility of money *income,* the problem seems
to me perfectly capable of formal solution without relapse into "circular"
reasoning. The point is merely (1) that even a "solution" in these terms,
despite its formal correctness, represents a very small advance, if any, in
terms of *substantive* analysis; and (2) that the very facts (*a*) that so much
energy has been wasted on the problem of the relation of the marginal
utility of money *income* to the Value of Money; and (*b*) that this problem
itself has so often been posed in such a way as to deprive even a "solution"
of it of heuristic value for the explanation of the determination of money
prices, themselves provide support for the contention that in this case
the issues discussed were factitious rather than substantive in nature.

CHAPTER THREE

The Dissent, and Its Lessons

I

FROM WICKSELL TO HAWTREY

THE ACCOUNT presented in the preceding two chapters should have revealed the essential lack of foundation for the charge that economists, in passing from their discussion of the "general" Theory of Value to the Theory of Money and Prices, have made no attempt to apply to the problem of the Value of Money certain "homely but intelligible concepts" developed originally within the "general" Theory of Value. It should be clear, on the contrary, that economists have done so with such persistence in the past that the particular writers who failed to do so must be regarded as exceptions to a general rule applying to the major figures in the development of both the Theory of Money and Prices and the "general" Theory of Value. It is against these exceptions to the general rule, therefore, that criticism must be directed by those who would regard the particular type of "assimilation" of the two bodies of theory indicated as a touchstone for testing the superiority of a given Theory of Money.

It is of the first importance, however, to realize just what such criticism must involve. The list of "exceptions," despite its comparative brevity, includes names as distinguished among the economists of our generation as those of Wicksell, Fisher, Schumpeter, and Hawtrey; for each of these writers has protested in no uncertain terms against the suggestion that it is either necessary or wise to state the substance of the Theory of Money and Prices in such a way as to make it merely an application to the special problem of the Value of Money of categories developed

originally within the "general" Theory of Value.[1]   Surely
one need not be accused of a vicious subservience to the
"noxious influence of authority" if one suggests that the
very eminence of these names ought to have been sufficient
to give pause to those who have announced unhesitatingly
that if it were really true that "it is impossible to use the
modern theory [of value] to explain the Value of Money,"
then this "modern theory" would be "shaken in its founda-
tions."[2]

For the central point of the present argument is not that
just as high authority can be cited *against* the view that the
problem of the Value of Money is best approached by apply-
ing to that problem the categories of the "general" Theory
of Value as can be cited in favor of that view.   The central
point is rather that which emerges from a consideration of
the implications of the following propositions:

1.   The writers cited as having dissented from the view
that a "scientific" approach to monetary theory must neces-
sarily take the form of a specific application to the problem
of the Value of Money of categories developed originally
within the general Theory of Value, have themselves in all
cases provided a substantive theory of the forces determin-
ing money prices which does not take such a form.

2.   The very fact that they presented their respective
theories in full awareness of the claims made on behalf of
the opposing view provided a challenge which the more

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[1] See, for example, the references to Wicksell and Hawtrey given on
p. 442, n. 80 of Volume I of the present work. For Fisher's position with
respect to the inappropriateness of the concept of a "specific desirability"
of money, in the sense in which "all other goods" may be said to have a
"specific desirability," and with respect, therefore, to the impossibility of
using it in explaining how the Value of Money is determined, see *The
Purchasing Power of Money,* 32. For the attitude of Schumpeter, see, in
addition to the references given on p. 441, n. 78 of Volume I, the argument
on pp. 646 ff. of Schumpeter's "Das Sozialprodukt, etc.," against any attempt
to regard the problem of the Value of Money as "a special case of the
general phenomenon of exchange value," and particularly against the ap-
plication to that problem of analysis running in terms of the "subjective
valuations" by individuals of money and other commodities, respectively;
and cf. also Schumpeter's more recent *Business Cycles,* 453, 544, 547.

[2] So K. Maier, *Goldwanderungen* (1935), 73, in commenting on the
statement of Fisher cited in the preceding note.

recent sponsors of the latter view should have undertaken to meet.

3. What this challenge amounts to is an invitation to these sponsors to show that the *substance* of their own theories of the forces determining the Value of Money is superior, or even equal, in usefulness to the substance of the theories presented by the "dissenters."

4. The test of such usefulness is the adequacy of the respective theories when judged as realistic accounts of the forces actually involved in the determination of money prices, and the processes through which these forces operate.[3]

Clearly, these propositions bear directly upon the determination of the responsibility for whatever "haze" now surrounds the Theory of Money and Prices as a result of an alleged failure to establish the precise nature of the connections between this theory and the "general" Theory of Value. For one can hardly expect this "haze" to be lifted until the challenge indicated under (3) has been met. Yet it is precisely this challenge which has not been met by the sponsors of the view that there has been too much of a "hiatus" between the two bodies of theory in the past.

In what follows, therefore, an attempt is made to indicate the nature of the considerations which would have to be raised in any attempt to meet this challenge. It is proposed to do so by reconsidering the contributions to the Theory of Money and Prices of each of the four dissenters cited as having protested against the suggestion that it is either necessary or wise to state the Theory of Money and

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[3] It should be obvious, from this statement of the issues, that the challenge indicated under (3) cannot be said to have been met by the mere demonstration that writers such as Wicksell, Fisher, and Schumpeter were wrong, for example, in denying a "specific utility" or "specific desirability" to money. That on this point they were wrong, in a formal sense, may be admitted. What is not admitted is that this fact in and of itself means that the *substance* of their monetary theory, when tested by the criterion indicated under (4), was necessarily affected adversely as a result of this formal "error." The answer to the latter question, on the contrary, can be provided only by an investigation designed to determine, in each case, whether the particular writer involved did or did not make use of the type of concrete analysis which alone gives heuristic value to an emphasis on the concept of a "specific utility" of money. See what is said on this matter in the cases of Wicksell, Fisher, Schumpeter, and Hawtrey, respectively, on pp. 92, 101, 116 f., and 120, below.

Prices in such a way as to make it merely an application of
the categories of the "general" Theory of Value to the
special problem of the Value of Money.

More specifically, the purpose of such a reconsideration
is (1) to determine the precise extent, if any, to which their
failure to do so actually impaired the substance, and there-
fore the heuristic value, of their respective theories of the
determination of money prices; (2) to determine the ex-
tent, on the other hand, to which their critics' insistence
upon such applications resulted only in further examples
of the twin sins of *formalism* and *exclusivism* to which at-
tention was called in our discussion of the contribution to
the problem at hand by contemporary "schools" of economic
theory; and (3) to establish the nature of the *lessons* which
may be drawn from these findings, as well as from those
presented in Chapters One and Two, by anyone who wishes
to explore the possibilities, for the future development of
monetary theory, which are offered by a consideration of
the relations between the Theory of Money and Prices, on
the one hand, and the "general" Theory of Value, on the
other.   The reader interested only in these lessons, rather
than in the developments from which the lessons themselves
are drawn, should turn at once to section II of this chapter.
Others may be interested in a summary account, such as
that which follows, of the contributions of the four "dis-
senters" listed above, when these contributions are judged
in accordance with the criteria indicated under propositions
(1) and (2) of this paragraph.

1. *Wicksell.* (*a*) If one were to take literally some of the comments
on the fact that Wicksell, after having considered the possibility of
applying "utility analysis" to the problem of the Value of Money, failed
to produce a "marginal utility theory of money," one would imagine
that his "failure" in this respect resulted in a serious gap of substance
in his own analytical apparatus for dealing with the forces determining
money prices.[4] We have already seen, however, that to object to the
failure of a given writer to use the *term* "marginal utility analysis" as a
description of the "theory of choice" involved in decisions as to the
holding of cash, on the one hand, or of non-cash assets, on the other,
may, under certain circumstances, become a mere bit of formalism.

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[4] Cf. in this connection, the comment on Wicksell by Hicks, "A Sug-
gestion," *loc. cit.,* 2.

This will be so, we saw, whenever it can be shown that the particular writer involved did make use of such a "theory of choice" in his treatment of the forces determining the size of cash balances held relative to outlay.[5] To demonstrate, therefore, that Wicksell himself made explicit and emphatic use of "cash-balance analysis" in his discussion of the problems for which such analysis can be shown to be relevant is simultaneously to demonstrate that his alleged "failure" is at the most purely one of form, and leaves the issues of substance entirely untouched.[6]

(b) Much the same thing may be said of Wicksell's treatment of the rôle of *money income* in the determination of money prices. There is nothing in Wicksell's discussion corresponding to Wieser's intensive concern with the relation between what has been called the "income approach" to the Theory of Prices, on the one hand, and "utility analysis," on the other.[7] As was pointed out in Volume I of the present work, Wicksell's concern with the rôle of money income in the determination of money prices was derived, not from a concern with the implications of "utility analysis," but from the straightforward comments of Thomas Tooke, whose own discussion of the problem was likewise in no degree concerned with the formal implications of anything that could be called "utility analysis." [8] Again, therefore, one would be simply mistaking what is after all a mere matter of form for a genuine gap in substance if one were to blame Wicksell for having failed to follow the practice of those later "income theorists" who reached their emphasis on the importance, for the determination of money prices, of changes in the level of money income by starting from an emphasis on the implications of "utility analysis."

(c) Insofar as the relation of "utility analysis" to changes in the level of money income involves genuine questions of substance, it is concerned primarily with the bearing of such analysis upon changes in the *structure* of money prices.[9] It is true that Wicksell himself did

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[5] See above, pp. 84 ff.

[6] For examples of Wicksell's use of "cash-balance analysis," see his *Interest and Prices,* 39 ff., 52 ff.; *Lectures,* II, 21 ff., 61 ff., 142 f., 150.

[7] On this aspect of Wieser's "income theory of prices," see the references to Wieser given in Volume I, p. 309, n. 20, of the present work; and for examples of discussion of it by later writers, see the references given above, p. 79, n. 68. The only aspect of Wicksell's analysis, on the other hand, which could possibly be regarded as undertaking to establish a similar connection between the "income approach," on the one hand, and "utility analysis," on the other, is his brief (and not altogether happy) discussion of the relation of "marginal utility" to the argument for "confining the calculation" of the "general price level" to "objects of (direct) consumption." On this matter, see Volume I, 490 ff.

[8] On the relation of Wicksell's version of the "income approach" to that of Tooke, see Volume I, 324 ff.

[9] The connection is, of course, established by the facts (1) that the level of income is an essential element affecting those "choices" between commodities which "utility analysis" is intended to describe; and (2) that

not make so clear as he might have the precise relation between "utility analysis," in the sense of a "theory of choice" applied to the problem of relative price change, and changes in the level of money incomes. Neither, however, did some of the writers who have been most emphatic in insisting upon the connection between "utility analysis," on the one hand, and the element of money income, on the other. What matters here is whether there is anything to indicate that Wicksell himself argued, either explicitly or by implication, that a theory of the determination of "prices" could lay claim to completeness if it ignored what *either* the "Theory of Money and Prices" *or* the "general" Theory of Value had to say with respect to the nature of the forces determining the structure of relative prices. The answer to this question is given with sufficient explicitness by Wicksell's own treatment of the rôle played in the determination of the structure of money prices by (1) that element of the "general" Theory of Value which is represented by the concept of "capitalization," and (2) the effect of "capitalization" upon the dimensions and the orientation of the money streams directed against different parts of the price structure.[10] For since Wicksell showed himself willing to draw upon *both* the "general" Theory of Value *and* the Theory of Money and Prices in this case, there is every reason to suppose that he would have done likewise whenever it could be shown that elements discussed in the "general" Theory of Value other than the factor of "capitalization" could be shown to be relevant to the purpose in hand.[11]

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both the level of income **and the type** of choice described by most versions of "utility analysis" **are essential** for the determination and interpretation of the *demand curves* **of the "general"** Theory of Value. On this matter, see below, pp. 202 ff., **296 ff.**; and cf. what is said on the relation between the "income approach," "utility analysis," and the demand and supply curves of "modern value theory," in Volume I, 491 ff.

[10] In this connection, see Volume I, 248 ff., and the references to Wicksell's writings there given; and cf. Myrdal, "Der Gleichgewichtsbegriff," *loc. cit.*, 380 f. (*Monetary Equilibrium*, 24 f.), on the bearing of this aspect of Wicksell's analysis upon the assertion that he transferred the emphasis in monetary theory from "the superficial level of the mechanism of payment" to "the central theory of pricing." See also what is said on this matter below, pp. 304 ff.

[11] Wicksell's treatment of the element of "capitalization" as affecting the structure of money prices is chosen for purposes of illustration here only because it was the particular element of the "general" Theory of Value relevant to the problem of the determination of the structure of money prices of which Wicksell himself made most explicit and repeated use. It was, of course, by no means the only element of the "general" Theory of Value of which he made use. See, for example, what is said below, page 145, concerning Wicksell's treatment of the equivalent of the Marshallian "elasticity of demand" as an element affecting the structure of money prices; and, more generally, cf. his comment, in his *Lectures* (II, 132), to the effect that "the internal exchange value of goods [in Menger's sense of the term; cf. what is said above, pp. 68 f.] will repeatedly undergo changes which will find direct expression in fluctuations in their money

(d) Closely associated with Wicksell's treatment of the rôle played in the determination of money prices by the element of money income was his treatment of that concept which he himself referred to as the "moneyed demand." [12] It is, indeed, this aspect of his argument which has been regarded by certain of the younger Swedish economists as demonstrating that Wicksell was in fact much less "orthodox" in his treatment of the relation between money and the "general" Theory of Value than he believed himself to be. The suggestion, in this connection, is that this part of Wicksell's argument in reality ran counter to that supposed pillar of "orthodox" economics, Say's Law.[13] In reality, however, there was nothing whatever that deserves to be characterized as "obscure" in Wicksell's refusal to reject *in toto* the substance of Say's Law.[14] On the contrary, what is, and must remain, "obscure" is why contemporary "heretics" in economic theory have been unwilling to follow Wicksell's example of refusing to take out of its context a proposition the original ambiguity of which has been still further accentuated by the heterogeneity of contexts in which it has been applied—or misapplied.[15]

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prices." Typical, indeed, of Wicksell's readiness to make use of *both* bodies of theory whenever either could be shown to be relevant to the problem of the structure of money prices was his acceptance, simultaneously with the proposition just quoted, of the further proposition that a "change in the value of money" will "to some extent" occasion "a change in the relative prices of other goods" (*Lectures*, II, 132). It should hardly be necessary to emphasize the fact that acceptance of the latter proposition underlay Wicksell's use of the concept of "forced saving," with all that this concept implies with respect to the differential impact of changes in the dimensions of the money stream upon the structure of money prices and money incomes. In this connection, cf. Volume I, 249, n. 43, of the present work, and the references there given.

[12] See Volume I, 327, and the references to Wicksell's writings given in n. 75 thereto. Like Hawtrey in his more recent writings, Wicksell also made use of the term "general demand." See, for example, his *Lectures*, II, 159.

[13] See, for example, the comments on this aspect of Wicksell's argument by Myrdal, "Der Gleichgewichtsbegriff," *loc. cit.*, 378 (*Monetary Equilibrium*, 21); and cf. also Lerner, "Some Swedish Stepping Stones in Economic Theory," *loc. cit.*, 581.

[14] Contrast Myrdal, *loc. cit.* The passage to which Myrdal refers is to be found in Wicksell's *Lectures*, II, 159. An even more explicit indication, however, of Wicksell's refusal to reject entirely the substance of Say's Law is to be found in his article "The Monetary Problem of the Scandinavian Countries," published as late as 1925, where Wicksell quoted one form of the Law with unqualified approval as applying to the particular problem with which he was there concerned (p. 213 of the version of the article published in the English translation of Wicksell's *Interest and Prices*).

[15] The essential wisdom of Wicksell's attitude toward the Law of Markets can be appreciated only by one who has gone sufficiently deeply into the history of discussion of the "Law" to realize how small a part, relatively speaking, has been played in that discussion by its use as a device for minimizing the importance of the rôle of money in economic theory,

What was "unorthodox" in Wicksell, therefore, was not his use of the concept of a "moneyed demand." [16]   It was his refusal to accept as

and particularly in the theory of output as a whole; and also how difficult it is to pass judgment on either the validity or usefulness of the Law of Markets until certainty is established with respect to (1) *which* of the very numerous, and by no means necessarily identical, statements of the "Law" is under discussion; and (2) the precise content of the position in whose support or refutation the Law of Markets has been used (or misused). These are matters that must be left for another occasion. It may be pointed out here, however, that the example set by Wicksell in refusing to argue that acceptance of a concept such as his "moneyed demand" necessarily means a rejection of the Law of Markets in all its formulations and in all contexts in which it has appeared, has been followed by other writers who have made use of the concept of a "moneyed demand." This was true, for example, of Tooke, whose emphasis on the element of "moneyed demand" (or, as he sometimes put it, the element of the "pecuniary means of the consumer" as limiting "demand," cf. Tooke's *History of Prices,* IV, 462) was commented upon in Volume I (p. 314). See, for example, Tooke's comments on Say's Law in his *Thoughts and Details on the High and Low Prices of the Last Thirty Years* (1823), IV, 5 n.; and cf. also II, 45 f. of the same work. It is true also of Schumpeter, on whose use of the concept of an "aggregate [money] demand" *(Gesamtnachfrage)* see below, page 117. Cf., for example, Schumpeter's temperate comments on the "Law" in his "Epochen der Dogmen- und Methodengeschichte," *loc. cit.,* 97. It is true, finally, of Hawtrey, who can hardly be accused of hostility to the concept of an "aggregate [money] demand," or, as Hawtrey himself has called it in his later works, the concept of "general" demand. See, for example, Hawtrey's *Monetary Reconstruction,* 161, on "every producer" as "a purchaser," and "supply" as "itself demand"; his *Art of Central Banking,* 323, on every "increment of production" as bringing with it "an equal increment of demand," "so long as there is no obstacle interposed to the expansion of credit"; his *Trade Depression and the Way Out,* 1, on "the total consuming power of the community" as "equal to its output"; and cf. also the characterization of Hawtrey's argument by Lambert, *La Théorie quantitative de la Monnaie,* 204, as resting on the proposition that "production creates its own markets." It would be easy to provide a fairly extended list of similar instances from other writers who have laid considerable stress on the importance of the concept of a "moneyed demand."

    [16] It is sufficient here to call attention to the discussion, by so "orthodox" an economist as J. E. Cairnes, of the effect of the introduction of a "medium of exchange" in making it possible "to distinguish Demand and Supply, not merely in reference to particular persons and products, but as general ideas"; so that "under our actual *régime* we speak of Demand and Supply, not merely as of this or of that person, but as of a whole community, and not merely with reference to this or that product, but with reference to all products." The result is that "aggregate Demand or aggregate Supply become possible ideas," and, Cairnes argued, "where we have a medium of exchange, we can form the conception of general Demand as distinct from general Supply" (Cairnes, *Some Leading Principles of Political Economy Newly Expounded,* 24 f., 31. In the light of these passages, it is an obvious misrepresentation of Cairnes's position to cite, *against the validity of the*

a dogma precisely that proposition which has been hailed as having imparted "a new unity to the theory of value and the theory of money," but which was formally accepted by virtually all the "classical" economists: namely, the proposition that the theory of the determination of money prices must run in terms of an application to the problem of the Value of Money of categories developed originally within the "general" Theory of Value.[17]

(e) It is for the range of issues raised by the problem of the relation between money and the *rate of interest* that Wicksell's work has usually been regarded as having been most significant, so far as the interconnections between monetary theory and the "general" Theory of Value are concerned.[18] Here, therefore, it is necessary to add only two comments, both of which are strictly relevant to any attempt to evaluate the significance of a discussion, such as that of Wicksell, for an understanding of the relations that have existed historically between the Theory of Money, on the one hand, and the "general" Theory of Value,

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concept of a "general" money demand for goods, those passages in which, having in mind *other* problems than those under discussion here, Cairnes argued in terms that would suggest that "general demand and general supply are identical phenomena, seen only from different sides" [cf. Laughlin, *Principles of Money,* 324 n.]). In addition, cf. what is said below, pp. 106, n. 37, and 269 ff., concerning Cairnes's usage elsewhere with respect to the concept of a "general [money] demand"; also what is said below, p. 104, n. 36, with respect to the rôle played by the concept of "general demand" in the monetary theory of an "orthodox" economist like John Stuart Mill, in contrast with the attitude toward the concept expressed by certain of Mill's critics.

[17] It is only fair to Wicksell, moreover, to point out that he himself did not press his methodological position on the matter under discussion to the point of refusing to accept those applications of the "general" Theory of Value made by the classical economists which can be shown to have heuristic value in their own right, so far as the problem of the Value of Money is concerned. See, for example, Wicksell's comments on the "effect on the purchasing power of money" of "the conditions of production of the precious metals," in *Interest and Prices,* 32, and on "the cost of production theory . . . as constituting *an element* in the Quantity Theory," in his *Lectures,* II, 149; and cf. the comment on Davanzati (cited above, p. 14, n. 25) by Schumpeter (himself, like Wicksell, unsympathetic to much that has been done in the way of applying categories of the "general" Theory of Value to the special problem of the Value of Money). In this respect, as in so many others, the treatment of the issues by Wicksell and Schumpeter is to be contrasted with the exclusivism of later sponsors of the general methodological proposition which both Wicksell and Schumpeter rejected.

[18] In this connection, cf. Volume I, 176 f., of the present work. See also the comment on this aspect of Wicksell's work by Myrdal, "Der Gleichgewichtsbegriff, etc.," *loc. cit.,* 391 (cf. also pp. 377 and 410 of the same work [*Monetary Equilibrium,* 20, 49, 86]), and by the authors cited in Volume I, 177, n. 57, of the present work, as well as Lerner, "Some Swedish Stepping Stones in Economic Theory," *loc. cit.,* 581; and contrast the judgment of Hicks, as summarized in the passage quoted below, p. 98, n. 21.

on the other. The first comment is a reminder that not only in fact, but also in Wicksell's own understanding, the heart of his doctrine with respect to the relation between money and the rate of interest was identical with the heart of Ricardian doctrine on the subject, a circumstance which itself provides a commentary on easy generalizations with respect to the degree to which "orthodox" economics has or has not concerned itself with establishing a *modus vivendi* between monetary theory and the "general" Theory of Value.[19] The second comment is that, in accepting and developing the doctrine of Thornton and Ricardo with respect to the relation between the rate of interest and changes in the *quantity of bank money,* Wicksell set a noteworthy example of a willingness to use segments of *either* monetary theory *or* the "general" Theory of Value, whenever either, or both, could be shown to explain the processes of the real world.[20] It is this example that should have been followed (but unfortunately has not always been followed) by later writers, some of whom have not only been particularly emphatic in condemning their predecessors for having failed to establish a connection between the two, but, in their own desire to provide a "simplified" solution of the problem, have evidenced a degree of exclusivism from which many of the older writers were, in fact, free.[21]

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[19] See again what is said in this connection above, pp. 7 and 38.

[20] On the rôle played by changes in the quantity of money in Wicksell's argument with respect to the effect of changes in the rate of interest, see Volume I, 183 ff., of the present work.

[21] There is, for example, no reference to the problem of the nature of the forces controlling the quantity of bank money in Hicks's "Suggestion for Simplifying the Theory of Money" (on which see again the comments in Volume I, p. 177, and above, pp. 82 f., nn. 75 and 78), nor indeed to any of the problems of monetary theory which cannot be forced into a strait jacket compounded of a combination of "the equation which states that the relative value of two commodities depends upon their relative marginal utilities" with the proposition that "people do choose to have money rather than other things, and therefore, in the relevant sense, money must have a marginal utility" (pp. 2 f.). This would not in itself be a serious matter if it were not for the fact that the article rejects the type of analytical device represented by a "Wicksellian natural rate theory" on the ground that such a device does not seem "sensible and interesting" to a "value theorist"; and, therefore, while it may "have a use in particular applications of monetary theory," it is "a nuisance in monetary theory itself," since it offers no help in "elucidating the general principles of the working of money" (*op. cit.,* 2, 5 n.). What this amounts to saying, obviously, is that an adequate account of the nature of the forces determining the quantity of bank money, and particularly of the effect of changes in the rate of interest upon that quantity, is of no importance in "elucidating the principles of the working of money"; for it was precisely with this problem, among others, that the type of device represented by the "Wicksellian natural rate theory" was supposed to deal. In his more recent *Value and Capital* (p. 159), Professor Hicks objects that while "the monetary specialist, intent upon the determination of the price-level by means of the money equation . . . cannot help stumbling upon interest, for example in the form

2. *Fisher.* It is against Irving Fisher, the second of the leading dissenters from the proposition that it is either possible or desirable to state the problem of the forces determining the Value of Money in terms of the categories of the "general" Theory of Value (and in particular in terms of a specific "utility" of money) that the sponsors of the view that it should be so stated have particularly directed their attacks. Indeed, Fisher's *Purchasing Power of Money* may well be regarded as the type case which Mr. Keynes must have had in mind when he charged that "so long as economists are concerned with what is called the Theory of Value, they have been accustomed to teach that prices are governed by the conditions of supply and demand, . . . but when they pass, in volume II, or more often in a separate *Treatise,* to the Theory of Money and Prices, we hear no more of these homely but intelligible concepts and move into a world where prices are governed by the quantity of money, by the velocity of circulation relatively to the volume of transactions, . . . *et hoc genus omne.*"

No evaluation of the historic importance of Fisher's work on the subject is possible, however, unless it is remembered that *The Purchasing Power of Money* was itself *a conscious attack upon what Fisher characterized as "the fallacious idea that the price level cannot be determined by other factors in the equation of exchange because it is already determined by other causes, usually alluded to as 'supply and demand.'"* [22] This fact, taken in conjunction with Fisher's explicit refusal to regard the problem of the Value of Money as capable of solution by the application of "utility analysis" to money as such, justifies the statement that Fisher's work constituted the most vigorous challenge that had been directed against the suggestion that there have been too few attempts, rather than too many attempts *of a mistaken kind,* to "assimilate" the Theory of Money and Prices to the "general" Theory of Value.

That Fisher's challenge was in some respects too strongly stated may be admitted. What cannot be admitted is that his challenge was met with an adequate degree of fairness and understanding of the importance

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of bank rate," such a "monetary specialist" "regards this interest as a factor controlling the quantity of money . . . and may not relate it to the general interest problem." To this it may be replied (1) that if the "monetary specialist" fails to "relate it to the general interest problem," he thereby illustrates merely the vice of excessive specialization, and not the vice of a concern with the nature of the forces determining the quantity of bank money; and (2) that it is equally a vice of excessive specialization for any writer on either "the general interest problem" or the relation between "the (relative) value of commodities and the value of money" to continue to evidence a lack of interest both in the nature of the forces controlling the quantity of bank money and in the rôle played by changes in the rate of interest in determining that quantity. Cf. also, in this connection, what is said below, pp. 581 f., 643 f., concerning the treatment of the forces determining the quantity of bank money in Keynes's *General Theory* and the writings of its supporters.

[22] *Purchasing Power of Money,* 174.

of the issues it raised, or with a degree of imagination which would make it possible to show that, for all the incompleteness of his own solution, and its crudity in a number of important respects, it represented a solution which was in other respects *more* complete and *more* pregnant with possibilities for further development than the exclusivist solutions which a number of his critics have wished to set up in its place. This should be clear from the following considerations:

(*a*) Nothing but a crude exclusivism, for example, could have led to the suggestion that anyone committed to the use of Quantity Equations of the Fisherine type necessarily stands committed to a "mechanical" treatment of price determination and price change, in the sense of a treatment which leaves no room for the play of the conscious decisions of "economizing" individuals.[23] Actually, there is not a single variable in the Fisherine equation—whether it be "velocity," the "quantity of money," or the "volume of transactions"—which is not capable of treatment such that the movements of these variables are in all cases referred to the actions of economizing individuals, as those individuals operate in a given institutional setting.[24]

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[23] In this connection, see Volume I, 160, 173, 178, and 493, n. 20.

[24] In the case of V, for example, the proof of this proposition is provided by an adequate appreciation of the relation of V to the "Marshallian K" and to the "cash-balance approach" generally (Volume I, 415 ff.). In the case of M', it is provided by an understanding of the relation between movements in M' and the calculations of business borrowers with respect to the possibilities of profitable borrowing at a given bank rate (Volume I, 171 ff.). In the case of $(M + M_r)$, when M and M_r are made up of "commodity money," the proof is provided on the supply side by an understanding of the rôle played by calculations with respect to the profitability of producing the money commodity under varying conditions of cost (Volume I, 154, and pp. 15, 23, 24, 27 f., 31, 33 f., 40, 44, 55, 87, 97, n. 17 and 631 ff., in the present volume), and on the demand side by calculations, on the part of one set of individuals, with respect to the "utility" of the money commodity in the arts uses, on the one hand (Volume I, 154, and pp. 23, 31, 40 f., and 639, 660, n. 78, 665, n. 89 in the present volume), and, on the other, by calculations, on the part of another set of individuals, with respect to the absolute demand for hand-to-hand currency—the internal drain, for example (Volume I, 151 f., 159, n. 2, 209, n. 9). In the case of T, the type of "calculations" undertaken depends upon which component of T is involved. In the case of the volume of Output, for example, the calculations involved are those of entrepreneurs with respect to profit and loss, on the basis of the type of data represented by the cost curves and the demand curves of the "general" Theory of Value (Volume I, 439, 449, and Parts Two and Three of the present volume). In the case of the "rate of sale" of goods, the calculations involved are partly those involved in the administration of cash balances, as when a change in the "rate of sale" of goods operates in a manner "accompanying and intensifying" changes in the velocity of circulation of money (Volume I, 454 ff.), and partly those involved in the concept of "reservation prices" of the "general" Theory of Value (p. 555 of the present volume and the references given in

(*b*) There is a sense in which any theory of the determination of money prices *must* be "mechanical," if it is to provide an adequate account of the successive steps in the economic processes involved in the determination of money prices: it must provide an adequate *mechanics* of these processes, in the sense that it must leave out no steps at which the operations of economizing individuals, or changes in the institutions under which these economizing individuals operate, can be shown to affect the final result. And the evidence thus far is that in virtually every case in which alternative devices for tracing the mechanics of price change have been proposed, they have shown themselves to be less adequate for tracing the specific steps involved than have equations of the general Fisherine form, or variants thereof.[25]

(*c*) It is true that, in denying the formal possibility of applying "utility analysis" to the problem of the Value of Money, Fisher overlooked the possibilities inherent in that "theory of choice" with respect to the holding of assets in the form of cash or in other forms, which constitutes the essence of the "cash-balance approach."[26] Yet the history of the latter approach has shown that, in cases in which it did not *also* make use of the framework provided by equations of the general Fisherine form, it has resulted in confusions that have had consequences which can be regarded only as unfortunate in the extreme. The confusion of the nature of the forces determining what has been called in this work the "relative" demand for cash balances with the nature of

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n. 8 thereto). It will be observed that it is also claimed, for the more inclusive variants of the Fisherine equation, that they make it possible to deal with the "actions of economizing individuals, as those individuals operate *in a given institutional setting.*" See below, pp. 102 f., and 464 ff.

[25] See, for example, what is said in Volume I concerning the relative merits, in this respect, of the more highly developed equations of the general Fisherine form, on the one hand, and, on the other, the Fundamental Equations of Keynes's *Treatise* (Volume I, 178 ff., 211 f., 214, 265, 268 ff., 283 ff., 411 ff.) and "income equations" of the Aftalion type (Volume I, 344 ff.); and see also what is said below, pp. 104, 114, n. 59, 365 ff., and 728, n. 124, with respect to the rôle of equations of the general Fisherine form in the mechanics of the generation and utilization of money income, when these Fisherine equations are subjected to certain simple, although crucial, processes of elaboration.

[26] It is only fair to Fisher to point out that there are aspects of his treatment of "velocity"—such as his use of what he calls the "person-turnover" concept of velocity, as opposed to the "coin-transfer" concept (*Purchasing Power of Money*, 352 ff., 362 f.)—which have led some historians of doctrine on the subject of "velocity" to characterize him as a "cash-balance theorist." See, for example, the references to Holtrop, in this connection, given in my "Léon Walras and the 'Cash-Balance Approach,'" *loc. cit.*, 572, n. 7. It can hardly be denied, however, that the respects in which Fisher's treatment of "velocity" is characteristic of the so-called "motion theory" (Holtrop) or "money on the wing" (Robertson) approach to the problem are very much more marked than those in which it is characteristic of the "cash-balance" or "money-sitting" approach.

the forces determining what has been called the "absolute" demand may be regarded as a case in point.[27]

(d) It has been a vice of precisely those theories of the determination of money prices which have stressed the necessity for a "theory of choice" as the basis for an explanation of such determination, that they have tended, all too often, to confine themselves to a *single type* of economic "choice," with the result that large areas which can be shown both to be such as to give room for the economic "choices" of individuals and to be relevant to the determination of money prices have been left unexplored.[28]   Of Fisher's formulation, it can be said at least that it did not lend itself to such exclusivism.[29]

(e) An emphasis on the necessity for providing a framework for the study of the forces determining money prices in which adequate place will be given to the effect of choices of economizing individuals, does not justify neglect of the study of the effect upon prices of changes in the *institutional setting* in which these choices are exercised.[30]   Yet there can be little doubt that it is formulations of the Fisherine type, or variants which are direct outgrowths of such formulations, which have thus far provided the most nearly adequate framework for the study of the effects of such institutional changes.[31]

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[27] On the distinction between the "absolute" and the "relative" demands for cash balances, and their relation to the variables of the Fisherine equation, see Volume I, 209, n. 10, 370, 437, 444 ff., 534 ff., 554, 570, 575.   For examples of the type of confusion which has often arisen as the result of a failure to bear this distinction in mind, it should be sufficient to point to those aspects of the concept of "liquidity preference" which are discussed below, pp. 653, 709 ff., 717 ff., 724 ff., 729 f., and particularly to the recent discussion with respect to the possibility of increased "hoarding" in the absence of an increase in the quantity of money (below, p. 653, n. 58). In the light of these confusions, it should be clear that very much more is involved in the distinction between the two types of "demand" for cash balances than "a mere restatement of the well-known fact that the total volume of those balances does not depend on the volume of monetary outlay alone" (so M. Palyi, in *Journal of the American Statistical Association*, XXXIV [1939], 193).

[28] In this connection, see what is said in Volume I, 439 and 449 f., of the present work, and also what is said. above, pp. 82 ff., especially nn. 75 and 78, and p. 98, especially n. 21, concerning the exclusivist character of the "simplified" Theory of Money proposed by writers such as J. R. Hicks.

[29] Cf., for example, what is said above, p. 100, n. 24, with respect to the type of "calculation" and "choices" which are summed up by the variables of the Fisher equation.

[30] See again, in this connection, Volume I, 449 ff., of the present work, and below, pp. 464 ff.

[31] Attention may be called, for example, to the facts (1) that the inclusion of a specific term for the "quantity of money of ultimate redemption" (and the possibility of subdividing this term into as many subterms as may be required in order to distinguish the various types of money of ultimate redemption [see Volume I, 147], whenever there is more than one type) makes it possible to deal in all necessary detail with the effect of

(*f*) It is true that Fisher himself did not go far enough in exploring the possibilities suggested by the conception of a *series* of "partial" equations of the general Fisherine form.[32] It is only fair to point out,

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such institutional changes as a change in the monetary standard; (2) that the possibility of using an expression such as $M' = cM_r$ (see Volume I, 145, 150 ff.) makes it possible to deal with institutional changes affecting the magnitude of bank reserves; (3) that the use of a term *(v)* to represent the "velocity of circulation of goods," of which the "number of middlemen's sales" is a component (Volume I, 554; cf. also *Economica* for November, 1939, 450 ff.), makes it possible to deal with the effect of institutional changes such as a change in the degree of integration of industry; (4) that the same thing may be said with respect to the term ω, in the expression $G = \omega O$, in which ω is "a coefficient establishing the relation between Output (*O*) and the volume of goods intended for sale" (Volume I, 599, n. 58; cf. also pp. 544 ff.); and (5) that the inclusion of a special term for transactions in *securities* (cf. Volume I, 599, n. 58, and also 576 ff.) makes it possible to take account of the effects of institutional changes in matters affecting stock market practice which can be shown to affect the "absolute" demand for cash balances; and so on.

[32] Even here, however, it may be noted that Fisher himself has displayed a much clearer understanding of the methodological principles involved than have many of his critics. In this connection, see Volume I, 512, and the references to Fisher given in n. 76 thereto. Cf. also the discussion of Fisher's proposed "modification" of his equation of exchange, which he regarded as "required by international trade," in Volume I, 513, 56 f. In the light of these facts, and of the further precedents that can be cited for a break-up of the Fisherine equation into as many parts as are appropriate to a given set of problems (cf. Volume I, 512 ff.), it is somewhat strange to be told that, in defending equations of the general Fisherine form against the charge that they necessarily lead to a "hotch-potch price level" (cf. Volume I, 514, and the reference to Keynes's *Treatise* there given), one is "thereby defending the individual parts and not the original whole" (so A. F. W. Plumptre, in the *Canadian Journal of Economics and Political Science*, V [1939], 265). For a defense, moreover, of the "original whole" when regarded as a weapon to be used *along with* "partial" equations which are nevertheless of the general Fisherine form, see the reference to Volume I in the following note. The answer, finally, to the question whether these "partial" equations "are the best available means of dealing with the problems for which they are designed" (cf. Plumptre, *loc. cit.*)— as contrasted, say, with the alternative formulations proposed by Mr. Keynes—may be answered by pointing out (1) that whatever else was claimed for the successive formulations presented by Mr. Keynes, they were presented also as specific alternatives to equations of the Fisherine type for "explaining the exchange value of a monetary unit" (Plumptre, *loc. cit.*); (2) that the relative advantages and disadvantages, for this purpose, of the Keynesian alternatives, as compared with formulations of the general Fisherine type, are discussed in Chapters Five and Ten of Volume I of the present work (in connection with the *Treatise* formulation) and in Chapter Fourteen of the present volume (in connection with the formulation of the *General Theory*); and (3) that the relative merits of the apparatus here presented, as compared with that presented in the *General Theory,* for dealing with those "relative alterations in prices"

however, that the very fact that Fisher's own "equation of exchange" was an equation of the "total transactions" type had a significance, even for writers who have insisted upon working with "partial" equations of the general Fisherine form, which these writers have by no means always sufficiently appreciated.[33]

(g) The most serious shortcoming of Fisher's own treatment was his failure to place sufficient emphasis on the rôle of money *income* in the process of price determination.[34] The very fact, however, that it was possible for Schumpeter, a bare six years after the publication of *The Purchasing Power of Money*, to present an "income equation" which Schumpeter himself characterized as being, in its "external" aspects, "completely identical" with Fisher's, shows that, where constructive imagination was not wanting, the Fisherine presentation lent itself perfectly to further construction of a highly significant kind.[35]

(h) The suggestion that use of equations of the general Fisherine form is inconsistent with an emphasis upon concepts such as that of a "general [money] demand" is patently absurd in view of one simple historical fact. This fact is that it is precisely writers who have made use of "stream" equations of the Fisherine type that have been most articulate in speaking of "general [money] demand"; whereas it has been the *opponents* of the use of such equations who have been most emphatic in rejecting the concept of "general demand," and, on occasion, have insisted that the concept of "general demand" is totally unfitted to implement the proposition that all prices are determined by "supply" and "demand." [36] The point takes on a particularly striking

which are rightly held to be of the greatest importance "in explaining the trade cycle" (Plumptre, *loc. cit.*) may be judged particularly on the basis of the argument presented in Parts Two and Three of the present volume.

[33] See especially, in this connection, Volume I, 518 ff.

[34] It should hardly be necessary to emphasize to a generation familiar with Fisher's writings, over a long period of scientific activity, on the element of Income, that this criticism can apply only to the treatment of the element of Income which is to be found in Fisher's *Purchasing Power of Money*. It may not be unnecessary, however, to remind the reader of the central rôle played by the element of money income and changes therein in Fisher's earlier *Mathematical Investigations in the Theory of Value and Prices* (1892). See especially pp. 44 ff. of the latter work. Cf. also Fisher's *Elementary Principles of Economics*, 408 f., where, by way of "review" of "the theory of prices," changes in the size and type of *income* are listed (under I, B, and II, B, 4, b) among "possible causes which might decrease the price of, let us say, pig iron in New York."

[35] Cf. Volume I, 409; and see what is said below, p. 114, including n. 59 thereto, with respect to the possibility of supplementing Schumpeter's formulation by still further developments running essentially in "Fisherine" terms.

[36] See Laughlin, *Principles of Money*, 324 (cf. also pp. 322 f., 240, 276), for an example of such a rejection of the "stream" type of analysis summarized by equations of the general Fisherine form (though the attack was directed against nonalgebraic versions of those equations, such as that found in John Stuart Mill), precisely on the ground that such analysis

degree of irony, moreover, in view of the fact that among the users of the concept of a "general [money] demand" is a writer such as Newcomb, Fisher's best-known predecessor in the development of equations of the "stream" type; whereas it is precisely in the writings of those who have concerned themselves exclusively with such matters as the construction of a "theory of choice" with respect to the distribution of

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involves the concept of "a general demand for goods arising from the side of money," whereas a "general demand," as so conceived, is "only a phantom demand, a figment of the imagination." Actually, the practice of objecting to the implications of what would now be regarded as "stream" formulations of the general Fisherine form on grounds of the type indicated, is by no means a phenomenon of only our own day. See, for example, Tooke's objection to James Mill's formulation, which Tooke reduced to the syllogism that since "*given* the supply, prices depend upon the demand," and since "money is the instrument of demand," therefore "an increase of bank notes must increase the demand for, and raise the price of, commodities" (Tooke, *Inquiry into the Currency Principle* [1844], 135). Tooke himself, to be sure, reintroduced the concept of "general demand" in the form of "the quantity of money constituting the revenues . . . of the different orders of the state under the head of rents, profits, salaries and wages, destined for current expenditure" (cf. Volume I, 314, of the present work, and the references given in n. 33 thereto), just as he reintroduced a relation between the "quantity of money" and this "demand" by introducing that segment of "the quantity of money . . . which was in the pockets or hands of the consumers, going to market to supply their immediate wants" (*Inquiry*, 136; cf., however, what is said on this matter below, pp. 149 ff.). It remains true, nevertheless, that in the earlier period it was sponsors of "stream" formulations of the Mill-Newcomb-Fisher type who were under attack for having failed to do justice to elements borrowed from the "general" Theory of Value, such as "the cost of production of the precious metals" (cf. Tooke, *Inquiry*, 136, where Senior's attack on James Mill in this connection is quoted with approval), and that one of the reasons for these attacks was precisely the emphasis on a money "demand for goods" which is to be found in the writers indicated. For examples of J. S. Mill's emphasis on such a money "demand for goods," see pp. 491 f., 524 ff. of Ashley's edition of Mill's *Principles,* as well as the reference to an earlier paper of Mill given in Volume I, 473, n. 39; and cf. Laughlin, *Principles of Money,* 276, where the "conception of a [money] demand for goods" is characterized as "the centre of Mr. Mill's theory of price," and is regarded as constituting the chief reason for rejecting the latter. Contrast, in this connection, J. Viner, *Studies in the Theory of International Trade,* 199, where, in support of the statement that "it was . . . the two Attwoods, and especially Thomas Attwood, who first explained in a reasonably satisfactory fashion the dependence of the 'demand and supply' of ["general"] price theory on the state of the currency," a passage is cited from Thomas Attwood's *The Scotch Banker* (1828), the heart of which is the proposition later advanced almost verbatim by John Stuart Mill: namely, that "the supply of commodities is the demand for money, and the supply of money is the demand for commodities" (cf. the first sentence on p. 491 of the Ashley edition of Mill's *Principles*).

wealth holdings between cash and noncash assets that no such concept as that of "general demand" appears.[37]

(i) The progress of monetary theory would have been greatly furthered if all our leading theorists had been as explicit as was Fisher in establishing the nature of the relation between *the demand and supply curves for particular commodities* of the "general" Theory of Value, on the one hand, and, on the other, *"stream" analysis* of the type which his own equation of exchange was intended to represent.[38]   As we shall see,

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[37] For Newcomb's general statement of the problem, see his *Principles of Political Economy,* 342. Newcomb started from the proposition that "in the social organism demand is exercised only through the instrumentality of the currency," so that "we may consider money as in some sort the *instrument of demand*" (a phrase, by the way, which occurs in the writings of so "orthodox" an economist as J. E. Cairnes [see, for example, the latter's *Leading Principles of ¡Political Economy Newly Expounded,* 208]); and he then announced his intention of discussing "the effect upon demand, price, and supply produced of changes in the amount of money in circulation." For the details of his discussion, which was concerned directly with the concept of a "market demand for things in general," and with the relation of "this demand for things in general" to "the flow of the currency," see especially pp. 351 ff., 371 ff., and 380 ff., of Newcomb's *Principles.* It may be added that while Newcomb's presentation, like Fisher's, can hardly be said to have been sufficiently explicit in establishing the relation between a money "demand for things in general" and money *income,* Newcomb himself was quite aware that, with some individuals, "income may be only a very small fraction of their transactions" (*Principles,* 359); and indeed the whole of Book IV, Chap. V of his *Principles,* which was entitled "Of Individual Income and Expenditure" (and which was inserted in the center of the group of chapters devoted to the relations between the money "demand for things in general" and "the flow of the currency"), gives evidence of Newcomb's awareness of the problem to be solved, even though he himself can hardly be said to have presented an articulate solution. For Fisher's use of the concept of "general demand," see, for example, *The Purchasing Power of Money,* 180. It may be remarked that the apparent conflict between Fisher's statement (*op. cit.,* 181) that "a general increase in demand, *resulting in an increase in trade,* tends to decrease and not to increase the general level of prices," and more common statements, such as that of Newcomb, to the effect that an "increase in general [money] demand" may, in fact, "increase the general level of prices," disappears (1) when note is taken of the phrase italicized in the quotation from Fisher, and (2) when one observes the emphasis, throughout Newcomb's discussion, on the interpretation of the magnitude of the money "demand for things in general" in the light of whatever "scale of prices" happens to be prevailing at the time.

[38] The particular aspect of the more general problem in which Fisher himself was most interested was, of course, associated with the proposition that, regardless of what may be said with respect to the usefulness of the supply and demand curves of the "general" Theory of Value in explaining the *structure* of money prices, the *absolute* "scale" of these prices can be explained only by the use of the type of apparatus represented by equations of the general Fisherine form, or their analytical equivalents. This matter

it is this relation which constitutes at one and the same time (1) the most obvious and ineluctable channel for the establishment of a satisfactory *modus vivendi* between monetary theory and "general" Theory of Value; and (2) the one which has been neglected most egregiously by some of those who have insisted most emphatically upon establishing such a *modus vivendi*.[39] Here, therefore, it is necessary only to call attention to Fisher's emphatic insistence that "the 'supply and demand' or the 'cost of production' of goods in terms of money do not and cannot completely determine prices," since "each phrase, fully expressed, already implies *money*," and therefore "the money side of each exchange must never be forgotten." [40] For what this amounted to was an insistence (which might well have been emulated by many who have complained of an alleged "hiatus" between the Theory of Money, on the one hand, and the "general" Theory of Value, on the other) upon *supplementing* the "general" Theory of Value by the whole of the Theory of Money, and *vice versa*, whenever it is desired to obtain an adequate account of the processes by which money prices are actually determined.[41]

(*j*) Many of the misunderstandings that have been associated with Fisher's treatment of the problem of the determination of money prices might have been avoided if, instead of using the concept of a "general price level," he had used the phrase of earlier writers such as Cairnes and Newcomb: namely, "the [absolute] *scale* of prices." [42] It is also

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is discussed in more detail below, pp. 280 ff., 319 ff., 330 ff. Attention may be called here, however, to the significant passages on pp. 177 f., 192, 194, 197, and 382 ff. of *The Purchasing Power of Money*. It may be observed, incidentally, that it is passages such as these, particularly when they are interpreted in the manner suggested below, which provide the answer to the suggestion that an emphasis on the necessity for making explicit use of the individual "demand and supply curves" of "modern value theory" has been peculiar to the "income approach" to a theory of the determination of money prices. See Volume I, 492, of the present work, and the references there given.

[39] See below, Parts Two and Three, and the references to Keynes's *General Theory* given throughout, especially in Chapters Four and Nine.

[40] *The Purchasing Power of Money*, 176 f.

[41] In this connection, see, on the one hand, Fisher's *The Purchasing Power of Money*, 175: "It is amazing how tenaciously many people cling to the mistaken idea that an individual price, though expressed in money, may be determined wholly without reference to money"; and see, on the other hand, the elaborate "classification of price influences" to which reference is made above, p. 104, n. 34—a classification including elements from *both* the Theory of Money and the "general" Theory of Value, presented, by way of a "review" of "the theory of prices," in the form of a list of "the various possible causes which might decrease the price of, let us say, pig iron in New York."

[42] See Cairnes, *Essays in Political Economy*, 3, 6, 13, and Newcomb, *Principles of Political Economy*, 207 ff. The expression "the *scale* of prices" itself, of course, instead of being original with either writer, was in fairly common use in the early nineteenth century. See, for example,

true, however, that, for the explanation of the determination of the absolute height of this "scale of prices," as well as of the absolute magnitude of the "*sum* of prices" *(Preissumme)*, to which the "scale" of prices is related, but with which it is not identical, no adequate substitute has yet been found for "stream" equations of the type of those for whose popularization Fisher deserves more credit than any other writer on monetary theory.[43]

(*k*) A concern with the problem of the determination of the absolute level of the "scale of prices" is in no way inconsistent with a concern with the *internal structure* of this "scale of prices." [44]   It is true that Fisher's own work was not primarily concerned with the factors deter-

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the Editor's Note on p. 391 of the second (1836) edition of Malthus's *Principles of Political Economy.* Its use by Cairnes and Newcomb, however, is particularly significant for two reasons. The first of these is represented by the very fact that a need for some such concept as the "*scale* of prices" in the description of what Cairnes himself referred to elsewhere as changes in "general prices" or "general" movements in prices (see, for example, Cairnes's *Essays,* 3, 4 n., 5, 7, 10, 26, 54, 57, 64, 83) was felt by such a writer as Cairnes, who certainly cannot be accused of a blindness to the importance of studying changes in the internal *structure* of money prices during processes of monetary expansion and contraction (cf. Volume I, 502 f., of the present work, as well as below, pp. 313 ff., 523 f.; and see especially Cairnes's *Essays,* 55 ff., and his *Some Leading Principles of Political Economy Newly Expounded,* 208). The second fact of significance is that Newcomb actually *defined* the "scale of prices" as "a general *average* of prices of all goods bought and sold" (*Principles,* 207; italics mine). He meant by the "scale of prices," that is, precisely what other writers, such as Fisher, have meant by "the general price level"; so that in a sense his usage may be taken as providing a kind of dividing line between the earlier usage, involving some such term as the "scale of prices" (or the "range of prices"; cf. R. Giffen, *The Case Against Bimetallism,* [1898] 89 f., 92, 217 ff.) and the modern practice of speaking of changes in the "general price level." On the rôle of the concept of a "scale" of "general prices" in monetary and general economic theory, see below, pp. 330 ff.

[43] For examples of a use of the expression "the sum of prices," see the references given below to Schumpeter (p. 118, n. 67) and Tooke (p. 151, n. 20). It will be remembered that Schumpeter regarded his "sum of prices" as given by an equation which he himself characterized as being, in its "external" aspects, "completely identical" with that of Fisher. See above, p. 104, and the references given in n. 35 thereto. On the rôle of the concept of the "sum of prices" in monetary and general economic theory, and its relation to the concept of a general "scale" of prices, see below, pp. 341 ff.

[44] It may be recalled that it was precisely one of the avowed purposes of Fisher to demonstrate "the compatibility of the equation of exchange with the equations which have to deal with prices individually" (*The Purchasing Power of Money,* 175). On the aspects of Fisher's argument in this respect which are capable of considerable development, as well as on those aspects which have since proved to be misleading, see below, pp. 336 ff.

mining this internal structure of money prices. Yet only the least generous canons of criticism could justify those interpretations of Fisher's position which have been concerned only with what he had to say with respect to the "general level of prices," and have ignored what he had to say concerning what he called "the *dispersion* of prices." [45]

(*l*) No summary of Fisher's contributions toward an "integration" of monetary theory with "general" economics would be complete if it failed to point out that he has been a pioneer in insisting upon the importance of two of the cardinal problems of substance which are necessarily involved in any such attempt at integration, and which have bulked so large in economic discussion in recent years: namely, (1) the relation of money to the determination of the *rate of interest,* and (2) the effect of monetary expansion and contraction upon the level of "*output as a whole.*" It would be difficult to argue that all he has said on these problems over a long period of scientific activity is of equal merit, or can even be regarded, in all instances, as altogether sound. [46]

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[45] In this connection, see especially Fisher's remarks on "The Dispersion of Prices," in Chap. IX of *The Purchasing Power of Money.* It can hardly be doubted that it was misleading to argue, as Fisher argued, that it is precisely this "dispersion of prices" which "makes necessary an index of purchasing power"—that is, an index of the "general level of prices." It would be much more reasonable, on the contrary, to argue that, with all possible recognition of the necessity for both a concept such as that of an absolute "scale of prices," and for a "total transactions equation" for the purpose of dealing with the "composite demand for cash balances" (see Volume I, 521 ff.), the "dispersion of prices" is precisely what would recommend the use of the concept of a *"plurality* of price levels." The point here is merely that it is nothing less than a libel on Fisher's work to suggest that he was unaware that "practically prices never do move in perfect unison"; that indeed they *"cannot* all move up and down in perfect unison," since "only by extremely violent hypotheses could we imagine perfect adjustability in all prices"; that "a further dispersion is produced by the fact that the special forces of supply and demand are playing on each individual price, and causing relative variations among them"; that "among the special factors working through supply and demand, changes in the rate of interest should be particularly mentioned," and that this will be so whether the change in the rate of interest is or is not "due to monetary changes"; and, finally, that one of the most important reasons why it "is difficult to conceive even in theory" that "all the *Q*'s change uniformly in one direction and all the *p*'s uniformly in the other" is that "a doubling in the quantities of all commodities sold, or . . . a doubling of the quantities consumed, would change their relative desirabilities and therefore their relative prices"; so that it is, in fact, "well-nigh useless to speak of uniform changes in prices (*p*'s) or of uniform changes in quantities exchanged (*Q*'s)." See Fisher's *The Purchasing Power of Money,* 184, 186, 192 f., 194 f.

[46] Common fairness, however, requires that attention be called, in this connection, to the admirable scientific candor which Professor Fisher has shown in being prepared to retract those statements, in his earlier publications, which must be regarded as doing considerably less than justice to

What one can argue is that in this respect, as in so many others, Fisher —the dissenter from the view that a theory of the Value of Money must run, from first to last, in terms of the categories of the "general" Theory of Value, and the *bête noir* of all those "reconcilers" of the two bodies of theory for whom it is anathema to conceive of "prices" as "governed by the quantity of money, by the velocity of circulation of money relative to the volume of transactions, . . . *et hoc genus omne*"—has done far more to bring about a substantive synthesis between the two bodies of doctrine, and with vastly less submission to what were characterized above as the twin vices of exclusivism and formalism, than have many of those who have claimed to have effected, or who have been credited with having effected, just such a synthesis in recent years.

3. *Schumpeter*.[47]   Of all the dissenters from the proposition that some special merit attaches to the application of the formal apparatus of "supply" and "demand" to the special case of the Value of Money, as the former appears in the "general" Theory of Value, none has been more vigorous in his dissent than Joseph Schumpeter.[48]   There could

the importance of pecuniary factors for the solution of both of the problems indicated. See, for example, what is said above, p. 50, n. 136, concerning the relation, in this respect, between Fisher's *The Theory of Interest*, on the one hand, and, on the other, his earlier *The Rate of Interest*. In the light, also, of Professor Fisher's well-known position in recent years with respect to the effect of monetary contraction upon output and employment during the worst years of the Great Depression, it is difficult to believe that he would continue to accept such propositions with respect to the relation between "the volume of trade" and the "quantity of money" as appear on p. 155 of his *The Purchasing Power of Money*.

[47] It should be pointed out that, in one sense, the interpretation of Professor Schumpeter's argument which follows is subject to correction on the basis of the new treatise on Money which he promises. Insofar as indications of the nature of the argument to be presented therein are to be gleaned from Schumpeter's recent *Business Cycles*, account is taken of these indications in the footnotes to this chapter. It should be added, however, that, in another sense, any correction that is to be made of the summary in the text above of the *ways in which Professor Schumpeter's argument may be regarded as capable of further development* must be made solely on the basis of the soundness of the suggested lines of development themselves. I hope that the specific references given to Professor Schumpeter's own works will be sufficient to indicate where his personal responsibility begins, and where it ends.

[48] See again the references given above, p. 90, n. 1. It may be noted, in passing, that in one of the passages there cited (*Journal of the American Statistical Association*, XXXI [1936], 792 f.), Professor Schumpeter's objection to the application of the "Marshallian cross" to the "case of money" was coupled with an equally emphatic protest against what Professor Schumpeter regards as Mr. Keynes's failure, in his *General Theory*, to realize that "the old supply and demand apparatus renders its very limited service only if applied to individual commodities . . . and that it either loses or changes its meaning if applied to comprehensive social aggregates," as in the case of Keynes's "Aggregate Demand" and "Aggregate Supply." On the latter point, see what is said below, pp. 204, 539 ff.

be no better test of the nature of the issues involved, therefore, than that provided by a comparison of the heuristic value of the results obtained by Schumpeter, on the one hand, and, on the other, by those who have insisted that unless one accepts a synthesis of precisely the type rejected by Schumpeter, the whole of the "modern" Theory of Value is "shaken in its foundations."

(a) Schumpeter did, to be sure, reject the suggestion that the most promising avenues for cross-fertilization as between the "Theory of Money," on the one hand, and the "Theory of Value and Price," on the other, lay in treating money as a "commodity," subject to the general laws of "supply" and "demand" which govern the value of "commodities," or subject to the laws included under that aspect of general value theory which is summed up by the concept of marginal utility. The case of Schumpeter himself, however, provides as good an illustration as one could wish of a proposition which is fundamental for our present purpose: namely, that the mere fact that a given writer rejects particular proposals of this type does not necessarily mean that he regards as undesirable *all* attempts to establish a *modus vivendi* between "general" economic theory, on the one hand, and the "Theory of Money," on the other. On the contrary, Schumpeter argued, as early as 1917, precisely that "the greatest advance in monetary theory has lain in freeing the problem of money from the isolation in which it once stood, as an element separate from the Theory of Value and Price, and in allowing the solution of the problem to grow out of the Theory of Value and Price. . . ." [49] This fact in itself should have been regarded as providing a challenge to other "reconcilers" of the two bodies of doctrine to prove that their own substantive results were superior, or even equal, to those obtained by Schumpeter from his own attempt to "synthesize" the two bodies of doctrine.

(b) The central point of Schumpeter's argument was that the problem consists of studying the rôle played by money in the "circular flow" *(Kreislauf)* pictured by that theory of the general interdependence of economic magnitudes which we owe above all to Walras.[50] Taken even as it stands, this proposition is one whose importance cannot be overestimated; for what it amounts to (in Schumpeter's own words) is an insistence that monetary theory must necessarily and in all cases be "part of the general theory of the economic process." [51] More

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[49] See Schumpeter's "Das Sozialprodukt und die Rechenpfennige," *loc. cit.*, 630.

[50] Schumpeter, "Das Sozialprodukt," *loc. cit.*, 631. It is worth contrasting this emphasis upon the rôle played by money in the Walrasian system with those generalizations which have gained wide currency in recent years (but for which no basis exists in fact) with respect to the "barter assumptions" supposedly underlying "equilibrium theory" as developed by the "Lausanne school." See above, pp. 70 f.

[51] *Ibid.*, 631. See also Schumpeter's comment on the consequences, for economic theory, of "the fact that most of our quantities are either monetary expressions or corrected monetary expressions," in the *Journal of*

specifically, it means (1) that the Theory of Money must necessarily be part of any body of analysis which claims to account for the determination of money prices in the world we know; and (2) that, conversely, the whole body of "general" pricing theory necessarily retains its full validity, alongside that of the whole Theory of Money, in any adequate study of the forces determining these prices.[52]

(c) The proposition becomes much more significant, however, when one realizes that emphasis upon the Walrasian "circular flow" does not necessarily mean that we are concerned solely with the "static" aspects of the determination of money prices. Walras, to be sure, was concerned primarily with the "static" aspects of his system, in the sense that he was concerned essentially with the determination of the conditions necessary for the *equilibrium* of the "system." The very fact, however, that Schumpeter chose to emphasize the *flow* aspect of the Walrasian system shows that the latter is not to be conceived of as "static" in any meaning of the term which would make it *"timeless"*—at any rate if, by "timeless," one means that the actions with which it is concerned are conceived of as *instantaneous*.[53] For the very concept of a "flow" necessarily implies the concept of a time *period*.[54]

Political Economy, XLII (1934), 256; and cf. what is said in the same author's *Business Cycles*, 548, on the proper treatment of "the 'veil' of money" in any attempt "to describe the process of the production and consumption of wealth."

[52] It is of considerable importance to emphasize that the second of these propositions is quite as significant as the first; and that the successful integration of the whole body of "general" pricing theory into a theory purporting to account for the *structure*, as well as the absolute "scale," of money prices, is one of the chief problems with which any attempt to "synthesize" monetary theory and "general" pricing theory must be prepared to deal. Contrast what is said below in Chapters Four and Ten, respectively, concerning the relevant parts of the arguments of Keynes's *General Theory*.

[53] It should be observed, therefore, that when, in restating in his *Business Cycles* the concept of the "circular flow" (or, as he sometimes calls it, the "stationary flow," or "the stationary circuit flow"), Professor Schumpeter suggests (p. 41) that "for some purposes it is more convenient to eliminate the time factor and to speak of absolute quantities," he does not suggest that this so-called "elimination" of time involves the assumption of "instantaneous" action. On the contrary, the "elimination of time" thus indicated is specifically regarded by Professor Schumpeter as referring to the fact that "if flows are constant, . . . any *period of account* may be arbitrarily chosen, or if they are strictly periodic, . . . the *period of account* would have to be a common multiple of all the periods." (The italics, which are intended to call attention to the fact that a "period" is involved in all cases, are mine. The reader will note the importance of this fact for any attempt to interpret the frequent suggestion that in "the Walrasian system . . . no process in time is involved" [so, for example, P. M. Sweezy, "Expectations and the Scope of Economics," *Review of Economic Studies*, V (1938), 234]). Contrast Schumpeter's *Business Cycles*, 53; and see also the following note.

[54] It is not surprising, therefore, to observe that Schumpeter's own description of the "circular flow" ran throughout in terms of an "economic

Nor is there anything in the concept of a *"circular"* flow which would support the suggestion that the "flows" involved in the Walrasian system do not move *forward* in time; given the irreversibility of time itself, the very concept of a "flow" in time means that the adjective "circular" can be intended only to describe forces which act and interact in the *same way* through time or to describe the *mutual interaction* of different types of expenditure.[55] From this, however, it follows that one requires only the addition of a series of analytical devices of extreme simplicity to effect the transition from a system, running in terms of "flow" analysis, in which the *same kinds* of actions and interactions reproduce themselves, to one in which, without necessarily introducing the problem of the "equilibrium" of the *system,* it becomes possible to trace the forces making for *change* in these actions and interactions, whether, on the one hand, this change has to do with the absolute magnitude of the factors involved and the direction of their impact, or whether, on the other hand, it is conceived of as cumulative or self-destroying in nature.[56]

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period" (see, for example, "Das Sozialprodukt," *loc. cit.,* 631 ff.; and cf. the use of the concept of a "period of account" in the passage quoted from the same author's *Business Cycles* in the preceding note). It is not necessary to raise here the question whether such an "economic period" is to be thought of in terms of "clock" time or of "operational" time (though cf., in this connection, the use in Schumpeter's *Business Cycles* of the distinction between "historic time" [p. 72] and "theoretic time" [p. 138 n.]). Actually, of course, a very large number of those actions which, in certain branches of "static" theory, are assumed to be "instantaneous," could equally well be described in terms of time periods the "clock" length of which will be determined by "operational" considerations. In any case, regardless of what may be held to be necessary for "static" analysis, there is certainly much to be said for the development of an apparatus which runs throughout in terms of "clock-time" periods, but which is made usable for "operational" analysis through the subdivision or grouping of the initially chosen "clock-time" periods into other "clock-time" periods of whatever length is required to give play to the particular "operational" processes which are chosen for study. On this matter, see especially what is said below, pp. 366 ff.

[55] In this connection, cf. the statements as to the meaning of the concept of the "circuit flow" in Schumpeter's *Business Cycles,* 35 f., 37 f., 41.

[56] See below, pp. 361 ff., 427 ff., 489 ff., 496 f. Readers of Professor Schumpeter's works, and particularly of his recent *Business Cycles* (see especially pp. 68 ff.) will be aware that he would almost certainly protest with considerable vigor against the suggestion that it is not "necessary," in "system" analysis, to introduce the concept of an *equilibrium* of the "system." It may be observed here, therefore, that the comment in the text *is designed to point out only that the case for developing the Walras-Schumpeter apparatus along the lines indicated* (on the nature of the supplementary devices to which reference is made in the text, see below, p. 114, n. 59, in addition to what is said above, p. 71, n. 48) is *independent* of acceptance or rejection of Professor Schumpeter's argument with respect to the rôle of the concept of the "equilibrium of the system" in "system analysis." It should be added, moreover, that the nature of the considerations which must be invoked in order to settle the question of the "neces-

(d) The most important of these analytical devices are represented by those designed to aid in the explanation of the generation and utilization of *money income.*[57] It is of considerable importance, therefore, to observe that it was precisely an outstanding characteristic of Schumpeter's treatment that it stressed the central importance, for the problem of the determination of money prices, of the element of *money income.*[58] It would be difficult, to be sure, to argue that Schumpeter's own treatment of the problem of the generation of money income could in any true sense be regarded as definitive.[59] The fact

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sity" for the concept of the equilibrium of the *system* is not, at all points, the same as the nature of the considerations that must be raised in order to settle the question of the necessity for the concepts of the equilibrium of the *individual* (consumer) and of the *firm,* respectively, in any attempt to describe the process by which the "system" (or "structure") of money prices is determined. On this matter, see below, pp. 407 ff.

[57] From what is said under (c), it should be clear that there is no suggestion here of a denial that incomes are generated and utilized also under the conditions assumed in that type of "circular flow" in which whatever "motion" is involved is like the motion—to use an example quoted by Marshall (*Memorials of Alfred Marshall,* 315)—of a spinning top. The point for our present purpose is merely that an adequate account of the forces determining the generation and utilization of money income is precisely one of the devices which become of particular importance in any attempt to provide a picture of the types of "change" suggested at the end of the preceding paragraph of the text.

[58] On Schumpeter's place in the history of the Income Approach to the Theory of Prices, see Volume I, 338, 343.

[59] The reader is again reminded that the judgment of Professor Schumpeter's analysis which is implied in this statement has reference only to that part of his work which can be judged on the basis of his publications up to date. The reader is reminded also that the suggestions which follow for further development of that analysis are my own, and that Professor Schumpeter bears no responsibility of any kind for them. On the assumption that the reader will bear these warnings in mind, I venture to suggest that the principal respects in which Schumpeter's treatment of the problem of the generation of money income may be regarded as capable of further development have to do with the necessity for supplementing a concept such as that of "income velocity" by analysis designed to trace the successive steps by which money (1) *enters* income, and (2) *is disbursed* out of income. (It may be recalled, in passing, that while Schumpeter himself did not undertake, in his earlier publications, to deal explicitly with this distinction, so vital for the purpose in hand, his own exposition was one of the few making use of a concept of "income velocity" which can be said to be free of the charge of having been *inconsistent* in its treatment of the distinction in question. See, for example, what is said on this matter in Volume I, 360, n. 33, and 379 f. It may be added that in Professor Schumpeter's more recent *Business Cycles* the distinction between the two problems just indicated is made sharper by the use of the concept of "consumers' *expenditure*" [see, for example, *Business Cycles,* 545, 558, 561] *in addition to* the concept of "the sum total of *incomes*" [see, for example, pp. 467, 489 of the same work]). The twofold problem thus outlined suggests, in

remains, nevertheless, that in at least two respects Schumpeter's treatment set an example which other writers might well have followed.

In the first place, his treatment was notable for the fact that, at the same time that it accepted Wieser's "income equation" as a starting point, it avoided almost entirely the bog of irrelevancies represented by Wieser's own association of his "income equation" with the implications of the theory of "marginal utility." [60] In the second place—and more important—by insisting that his own income equation was, in its "external" aspects, "completely identical" with that of Newcomb and Fisher, Schumpeter established a precedent which, unhappily, has been followed all too infrequently: the precedent, namely, of arguing that an emphasis on the importance of money *income* not only does not necessarily mean an abandonment of equations of the general Fisherine form, but, on the contrary, *requires* the use of equations of precisely that general form, developed and adapted for the special purpose in hand.[61] Given this posing of the problem, it should have been regarded

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turn, the use of an apparatus such as that sketched in Volume I (see, for example, pp. 382 f.) and referred to repeatedly in the present volume. The essential features of this apparatus are: (1) the development of a notation to distinguish between payments which do, and those payments which do not, enter money income, on the one hand, and between payments *out of* and payments *into* income, on the other; (2) the use of time-period subscripts, in combination with this notation, to indicate the successive steps in the payment process, as this process unfolds itself in time; (3) the emphasis on cash-balance administration ("velocity," in the strict sense of the term) as the principal link between the streams of payments *into* income or traders' receipts, on the one hand, and the subsequent stream of payments *out of* income or traders' receipts, on the other (see, for example, Volume I, 382, n. 85, and 383, n. 88), the whole being conceived as a series of successive steps in time; (4) the statement of the argument throughout in terms of "Fisherine" equations, which make it possible to use a type of "period analysis" involving "clock" time rather than merely "operational" time (see above, p. 113, n. 54, and below, pp. 366 ff.); and (5) the use throughout of "Fisherine" equations of a *"partial"* type (see Volume I, 509 ff.), which make it possible to show at all points (*a*) the effect of the changes in the dimensions of the separate money streams upon the price *structure,* and (*b*) the effect of the actions of economizing individuals upon the dimensions of these individual streams and upon the components of the correlative "goods" streams (and therefore upon the structure of prices and output), as the actions of these individuals are described by both the "general" Theory of Value and the relevant parts of the Theory of Money and Prices. On the last point, see especially below, pp. 320 ff.

[60] On the rôle played by discussion of "utility analysis" in the history of the Income Approach, see Volume I, 308 f. On the relation of Schumpeter's "income equation" to the nonalgebraic "income equation" presented by Wieser, see Volume I, 339, n. 111. And on Schumpeter's own position with respect to the rôle of "utility analysis" in the theory of the Value of Money, see above, p. 90, n. 1.

[61] See above, p. 104, and especially n. 35 thereto.

as axiomatic that any further development along the lines indicated ought to begin with a critique of Schumpeter's concept of income velocity (or, as he called it, the "efficiency" of money), with a view to supplementing it by other analytical devices—all of them running in "Fisherine" terms—designed to establish more clearly the precise steps involved in the generation and utilization of money income.[62]

(e) Given this point of view, it is easy to forgive what is perhaps the most important gap in Schumpeter's positive analysis: namely, the absence of a sharply articulated version of what has since been called the "cash-balance approach." [63]   For, as was pointed out in Volume I of the present work, it is precisely the necessity for the use of such an

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[62] Cf. above, p. 114, n. 59.

[63] From Professor Schumpeter's entirely unsympathetic discussion of what amounts to the "cash-balance approach" in his *Business Cycles* (547 f.), it is clear that he would reject without hesitation the suggestion that his lack of interest in, and indeed positive distaste for, the "cash-balance approach" is something for which he must be "forgiven." It is for the reader to decide, on the basis of the argument for the "cash-balance approach" presented in Volume I of this work, and the use made of it in the present volume, whether the arguments advanced against it on pp. 547 f. of Professor Schumpeter's *Business Cycles* are convincing (cf., however, his discussion of the "individual's demand for money" in "Das Sozialprodukt," *loc. cit.*, 650 f.). It may be observed here only that no basis for a minimization of the importance of Professor Schumpeter's positive analysis is provided by either (1) a rejection of his argument against the use of the cash-balance approach; or (2) the conviction that in rejecting the cash-balance approach he is rejecting an analytical device which provides a necessary *complement*, rather than an alternative, to his own analytical structure. In connection with the second point, indeed, attention should be called to certain indications, in Professor Schumpeter's latest work, that he may himself do much to provide a "complement" of the type indicated. See, for example, his discussion, in *Business Cycles*, 578 ff., of "the subject of balances": a discussion which grants the essential methodological principles of the "cash-balance approach" by (1) its insistence upon looking at "financing *from the standpoint of firms and households*" (that is, from the standpoint of the individual *administrators* of cash balances), and by (2) its desire to "avoid any implications about mechanical effects being exerted on the pulse of business by the 'flow of funds.'" Regardless, moreover, of what one may think of Professor Schumpeter's rejection of the "cash-balance" aspect of Walras's monetary theory (see my "Léon Walras and the 'Cash-Balance Approach,'" *loc. cit.*, 597 f., and cf. Schumpeter's *Business Cycles*, 547), it should be obvious, in the light of what is said above, p. 111, with respect to the relation between Schumpeter's analysis and the Walrasian "circular flow," that the specific point mentioned in n. 68 to p. 598 of my earlier article (namely, the appearance of the concept of "forced saving" in both Walras and Schumpeter) hardly does justice to Professor Schumpeter's insight in seizing upon the broader aspects of the Walrasian system, in its monetary aspects, as the starting point for further constructive work in monetary theory.

approach, among other things, which is established by the kind of closely critical examination of the concept of "income velocity" to which Schumpeter's work should have led.[64]

(f) The appearance, in Schumpeter's argument, of the concept of a "general [money] demand" (or, as he called it, the "aggregate demand" [Gesamtnachfrage]) demonstrates again the close connection of this concept with equations of the general Fisherine form, and particularly with the "income" variants of these equations.[65] It is equally important to observe, moreover, that Schumpeter's use of the concept of "Aggregate Demand" in this context was not the result of a conscious transfer to the Theory of Money and Prices of the "homely but intelligible concepts" of the "general" Theory of Value. On the contrary, he went out of his way then, as he has gone out of his way since, to protest against what he has regarded as an unwarranted extension, to such a concept as that of Aggregate Demand, of certain connotations associated with the concepts of supply and demand, when the latter are applied to individual commodities.[66]

(g) The appearance, in Schumpeter's argument, of the concept of "the sum of prices" (Preissumme), along with the concept of "the price level," may be taken as evidence of Schumpeter's awareness of the perfect consistency of the use of "stream" equations of the general Fisherine form with an interest in a "plurality of price levels," and

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[64] See, in this connection, Volume I, 368, 389 ff., 420.

[65] See especially "Das Sozialprodukt," loc. cit., 675 (the same page on which appears the Quantity Equation described by Schumpeter as "completely identical," in its "external" aspects, with the "Newcomb-Fisher equation"), where Schumpeter, in laying down the proposition that "the sum of incomes [Einkommensumme] is the monetary expression of the 'Aggregate Demand,'" not only took pains to point out that "the sum of incomes is . . . equal to the product: quantity of money multiplied by its efficiency," but also went out of his way to pay respects to those who, in substituting "the sum of incomes" for "the quantity of money," believed that in so doing they had made obsolete "the basic idea underlying the Quantity Theory." The whole passage is strikingly relevant for an evaluation not only of the argument of certain sponsors of the "income approach" (see again Volume I, 349 ff.), but also of a concept such as Mr. Keynes's "elasticity of effective demand," particularly when the latter is considered in conjunction with Mr. Keynes's treatment of the concept of "income velocity" in his General Theory. On this matter, see below, Chapter Thirteen.

[66] See "Das Sozialprodukt," loc. cit., 678 f.; and cf. the similar comments by Schumpeter on certain aspects of the concepts of Aggregate Demand and Aggregate Supply as they appear in Keynes's General Theory, in the Journal of the American Statistical Association, XXXI (1936), 792 f. On the specific point involved in the criticism of Keynes, see below, pp. 204 ff.; and on the true nature of the relation of the demand curves of the "general" Theory of Value to the concept of Aggregate Demand, see below, pp. 263 ff., 285 ff.

therefore in the internal *structure* of money prices.[67] Much more
direct evidence of such an awareness, however, is provided by that part
of his positive analysis which was concerned with the relation of the
monetary process of "forced saving" to events within the "circular flow"
as the latter may be supposed to function before that change in the

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[67] For Schumpeter's use of the concept of a "sum of prices," see, for
example, "Das Sozialprodukt," *loc. cit.*, 634 f. (cf. the similar expressions
used by Tooke which are cited below, p. 151, n. 20). Cf. also Schumpeter's
use of the term "the sum of products" *(Produktensumme)*—that is, "the
sum of the products [in the mathematical sense] of prices and quantities"
—in "Das Sozialprodukt," *loc. cit.*, 654, 676 ff.; and see what is said on this
matter below, pp. 341 ff. The reason, of course, why the concept of a *sum*
of prices is more immediately reconcilable with an emphasis on the *struc-
ture* of prices than is the concept of a "general price level" is that the
"sum of prices" may be represented literally as the "sum" of an array of
individual prices, or as the "sum" of a series of price "groups"; whereas
the concept of a "general price level" suggests to many an attempt to
obscure the differences in the movements of individual prices or price
groups through a process of *averaging*. This is not to say, however, that
the concept of a *"sum* of prices" is capable of satisfying all the purposes
for which the concept of a "general" price level was devised. On this
matter, see what is said below, pp. 341 ff. The point made here is merely
that Schumpeter's usage has been consistently such as to do justice to
the problems involved in a changing *structure* of money prices, as well as
to those involved in the concept of a "general price *level*," with all that
the former emphasis implies with respect to the concept of a *"plurality* of
prices levels." On the lack of foundation for the suggestion that Schumpe-
ter's concern with the prices of *consumers'* goods (cf., in this connection,
Schumpeter's *Business Cycles*, 457) blinded him to the necessity for dealing
with a "plurality of price levels" which would include also a "price level"
of *producers'* goods, see Volume I, 497 f., of the present work (and cf.
Business Cycles, loc. cit.). It may be added here that Schumpeter's dis-
cussion of the concept of a "price level" of *consumers'* goods was itself
characterized by an awareness of the necessity for taking account of changes
in the *structure* of prices even within the general category of "consumers'
goods." Attention may be called, for example, to his comments on the
concept of "the purchasing power of money" and its relation to the use
of index numbers, in "Das Sozialprodukt," *loc. cit.*, 652 ff., and also to the
fact that in his "consumers' goods" equation (p. 675), the "prices" are
expressed as a series of *individual* prices multiplied by the quantities sold
at these prices, instead of in the form of a symbol representing "average"
prices. For an understanding of Professor Schumpeter's position with re-
spect to what amounts to the concept of a "plurality of price levels," see
also the use, in his *Business Cycles*, of concepts such as the "price *struc-
ture,"* the *"system"* of prices, *"sectional* price levels," and *"group* prices"
(Business Cycles, 3, 128, 137, 453 ff., 476 ff.), as well as his emphasis on "the
relations between prices" (cf., for example, *Business Cycles*, 27, 123). It
should be observed, however, that in all this there was nothing to suggest
that there are no purposes for which the type of concept called by other
writers "the price *level"* is required. See "Das Sozialprodukt," *loc. cit.*,
653, and his *Business Cycles, passim,* but especially 452 ff.; and cf. what is
said on this matter below, pp. 280 ff., 330 ff.

structure of prices, incomes, and output which may be induced by the stream of "additional" money-spending power.[68] For it is precisely such analysis which, by stressing the necessity for examining the effect of monetary changes upon the *structure of money incomes and prices,* provides one of those complements to the "general" theory of pricing which can be provided by the body of analysis found within the Theory of Money, and found there alone.[69]

(h) The whole of Schumpeter's analysis, instead of evidencing the vices of formalism and exclusivism that have characterized so many attempts to establish a "synthesis" between the "general" Theory of Value, on the one hand, and the Theory of Money and Prices, on the other, was concerned with discovering the specific ways in which money could be shown to *affect all elements involved in the actual functioning of the economic process.* One of these elements, obviously, is the *rate of interest;* and Schumpeter's concern with the relations between monetary phenomena and the phenomenon of interest, itself provides a further commentary on the suggestion that a recognition of the importance of these relations is, apart from Fisher's discussion of the problem, in a peculiar sense a contribution of Mr. Keynes, alone among contemporary writers.[70]

4. *Hawtrey.* If Mr. Hawtrey has not been so vigorous as the other three "dissenters" discussed above in his opposition to attempts to apply to the problem of the Value of Money the categories developed

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[68] See the well-known passages in Schumpeter's *The Theory of Economic Development,* 61 ff., 71 ff., 108 ff., 121 ff. (cf. the same author's *Business Cycles,* 111 ff.). On the use of the concept of "forced saving," in particular, see "Das Sozialprodukt," *loc. cit.,* 691 ff. (though see also *Business Cycles,* 112 n., on the use of the *expression* "forced savings").

[69] See also, in this connection, Schumpeter's comment ("Das Sozialprodukt," *loc. cit.,* 652) to the effect that not only do "individual prices reflect, along with the factors affecting all commodities, those which are peculiar to the commodities concerned," but also that *"even those causes which affect all commodity prices* affect individual prices *with very different force"* (italics mine). Cf. what is said on this matter below, pp. 304 ff.

[70] For Schumpeter's argument with respect to the rate of interest—an argument which he himself has characterized as presenting a "monetary theory of interest" (*Journal of the American Statistical Association,* XXXI [1936], 794; cf. also Schumpeter's *Business Cycles,* 127 n., 129)—see especially his *Theory of Economic Development,* Chap. Five, and cf. his *Business Cycles,* 123 ff., 602 ff. See also Schumpeter's own comment, in the passage first cited, on Keynes's "monetary" theory of interest and his own "monetary" theory of interest, when both are judged from the standpoint of the degree to which they succeed in relating the "surface phenomena" involved to "the economic processes that lie behind" these "surface phenomena." It may be remarked also, in passing, that any account of what "contemporary" writers have had to say with respect to the relations between monetary factors and the rate of interest would certainly have to include the name of H. J. Davenport, whose theory of the determination of the rate of interest was in many respects *more* of a "monetary theory of interest" than is that of Fisher. Cf. above, p. 51, n. 138.

within the "general" Theory of Value, he has nevertheless been as explicit as one could wish in insisting that the very posing of the problem in these terms raises a series of issues that must be regarded as either factitious or of altogether subsidiary importance.[71]   Again, therefore, it is proper to compare the substantive content of his Theory of Prices with that of the writers who have been most explicit in their insistence that a hiatus has existed as between the "general" Theory of Value, on the one hand, and the Theory of Money and Prices, on the other, as the result of a general failure to apply to the latter certain "homely but intelligible concepts" developed originally within the former.

(a) Like Wicksell, Hawtrey has made explicit and consistent use of the two sets of analytical devices discussed, in this work, under the heads of the "cash-balance approach" and the "income approach," respectively.   Indeed, from the standpoint of both articulation and comprehensiveness, there can be little doubt that Hawtrey's combination of the two "approaches" is superior to that of Wicksell.[72]   Like Wicksell, moreover, Hawtrey reached his results directly, and not through the intermediacy of an intensive concern with the formal application to the Theory of Money and Prices of concepts developed originally within the "general" Theory of Value.   The case of Hawtrey, therefore, like that of Wicksell, should have been taken as an illustration of the propositions laid down so often in these pages: namely, (1) that what matters in all cases is whether a given writer did or did not emerge from his attempt to "synthesize" the two bodies of theory with an analytical equipment that can stand on its own feet as a set of heuristic devices for the explanation of the determination of money prices; and (2) that the fact that these devices may have been discovered, in some cases, as a result of a desire to "synthesize" the two bodies of doctrine is of no more importance than the historical fact that a concern with the problems of alchemy in some cases led to the attaining of results which can stand on their own feet as parts of a scientific chemistry whose ultimate validity rests solely upon its ability to explain the phenomena of the real world.

(b) Like Wicksell and Schumpeter, Hawtrey is to be grouped with those sponsors of an "income approach" who made explicit use of the concept of a "moneyed demand," or, as it is called in Hawtrey's later works, "general" demand.[73]   It is of some importance, however, to

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[71] See again the quotations from Hawtrey given in Volume I, 442, n. 80, of the present work.

[72] See, in this connection, the comments on Hawtrey in Volume I, 340 f.

[73] The emphasis on money "demand" and its association with outlay from money income has appeared throughout Hawtrey's published writings, from the earliest to the latest. See, for example, *Good and Bad Trade* (1913), 6, 78, 224 f., and especially the two closing sentences on p. 272; *Currency and Credit*, 42 f. of the first (1919) edition (48 f., 59 of the third [1928] edition); *Monetary Reconstruction* (second edition, 1926), 130, 132; *The Gold Standard in Theory and Practice* (1927), 10 ff., 41, 80; *Trade and Credit* (1928), 83, 106, 117 f.; *The Art of Central Banking* (1932), 96 f., 100,

observe that Hawtrey, like Schumpeter, but unlike Wicksell, did not present his concept of "general" or "total" demand in such a way as to suggest that, in making use of this concept, he was making use of concepts developed originally within the "general" Theory of Value.[74] On the contrary, he made use of the concept of "general demand" because there are problems in *monetary* theory for which the concept is indispensable, and for the solution of which no substitute is available.[75]

There can be little doubt, moreover, that Hawtrey's use of the concept of "general demand" is not only more articulate than that of Wicksell but is also more inclusive, by virtue of his use of it in dealing with the effect of monetary expansion and contraction upon the level of output as a whole, as well as upon the level of money prices, whereas Wicksell himself was almost exclusively concerned with the latter problem.[76] This aspect of Hawtrey's work provides a further confirmation,

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103, 145, 205, 207, 218, 311, 321; *Trade Depression and the Way Out* ("New" edition, 1933), 1 ff., 13, 19, 25 f., 39, 44 f., 69, 71, 82, 97 f., 101 f., 117, 127, 174; *Capital and Employment* (1937), 69, 71, 73, 85, 96, 98, 127, 129, 132; *A Century of Bank Rate* (1938), 11, 38, 62 f., 241.

[74] It is only fair to Wicksell to point out not only that he himself refrained from making claims to novelty, in this particular connection, of the extravagant kind made on his behalf by later writers (see, for example, Ohlin's Introduction to *Interest and Prices*, p. xiii), but also that he was careful to say only that since "every rise or fall in the price of a particular commodity presupposes a disturbance between the equilibrium between the supply of and the demand for that commodity," "what is true *in this respect* of each commodity separately must doubtless be true of all commodities collectively" (*Lectures*, II, 159; it should be noted that the italics are Wicksell's). It is true, on the other hand, that Wicksell, in presenting his concept of a "moneyed demand," did refer to the analogy of the apparatus of the "general" Theory of Value for dealing with "a particular commodity," whereas Hawtrey did not. See also the following note.

[75] It may be observed further that just as Schumpeter associated his concept of "aggregate demand" with a device as strikingly characteristic of *monetary* theory (rather than the "general" Theory of Value) as an income variant of "stream" equations of the Fisherine type (see above, pp. 104, 115), so Hawtrey has shown himself ready to recognize that "Professor Fisher's version of the quantity theory" (by which, from the context, only "quantity equation" could be meant) "in a sense may be regarded" as describing the impact of "the total of demand" upon "the total of supply," the only improvement suggested by Hawtrey being precisely that type of recognition of the "qualitative difference between the purchase of a thing out of income, . . . and the purchase of a thing with a view to resale" which is represented by Schumpeter's conversion of Fisher's equation into an "income equation." See Hawtrey's *The Art of Central Banking*, 106.

[76] Wicksell was, of course, concerned with the effect of the "moneyed demand," conceived in the broadest sense, upon the *structure* of money prices, and particularly that aspect of the structure of money prices which is represented by the relation between the prices of producers' and consumers' goods, respectively (see, in this connection, Volume I, 496 f., of the present work, and especially nn. 28 and 29 thereto); and of course the *structure* of money prices is a matter that is closely related to the explana-

therefore, of the proposition that, in weighing the contributions of any two writers to the Theory of Prices, what matters is not the degree of explicitness with which the respective writers announced their intention of "synthesizing" the two bodies of doctrine, but the range and the solidity of the specific results they obtained in attempting to explain the facts of the real world.

(c) Hawtrey has, on the whole, evidenced much less interest than a writer such as Wicksell in the problem of tracing those effects upon the *structure* of money prices which can be attributed to the impact of different segments of the aggregate money stream on the structure of money incomes and other forms of money receipts, and therefore on outlay from such income or receipts.[77] He has been even more explicit than Wicksell, however, in making clear that the structure of money prices will at all times be what it is as the result of the conformation, as well as the position, of the demand and supply curves of the "general" Theory of Value—as the result, for example, of the different elasticities which can be shown to characterize the demand curves for specific commodities.[78] In so arguing, he not only provided a welcome complement

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tion of movements in the level of output as a whole. The fact remains, however, that Wicksell himself did not stress this relation, his own emphasis being primarily on the effect of changes in the structure of money prices upon (1) the expected profit rate in the system as a whole, and, therefore, upon (2) the total amount of borrowing from banks and the level of money prices (see Volume I, Chap. Nine, and especially pp. 248 ff.). In order, indeed, to appreciate the difference of emphasis in the two writers, one has only to compare passages from Wicksell such as those cited in Volume I, 327, n. 75, with the treatment of demand in relation to *output* which runs throughout Hawtrey's work.

[77] In this connection, cf. the criticism of Hawtrey by Saulnier, *Contemporary Monetary Theory*, 47, on the ground that the former's emphasis on "general demand and the total of consumers' outlay almost to the exclusion of the demand for specific kinds or groups of goods" has led to a failure "to take account of disturbances which may grow out of changes in the distribution of demand." This is by no means to say, of course, that Hawtrey has been unaware that "great inequality of price movements may arise from the action of monetary causes themselves" (so, for example, *The Art of Central Banking*, 308). Indeed, the mere fact that passages can be cited in which Mr. Hawtrey has taken account of the possibility that a monetary expansion may result in a particularly intensified "demand for *capital goods*" (see, for example, *Trade Depression and the Way Out*, 35, 44) is sufficient to provide a warning against attaching too narrow an interpretation to those passages in which he has argued that "monetary theory is constantly concerned with tendencies which affect *all* prices equally, or at any rate impartially, at the same time and in the same direction" (*The Art of Central Banking*, 304 [italics Hawtrey's]; cf. also p. 330 of the same work). The very fact, however, that statements such as that just quoted do appear in Mr. Hawtrey's writings is itself an indication of where his chief *emphasis* has lain; and it is solely with the matter of emphasis that the statement in the text is concerned.

[78] This, again, has been a characteristic feature of Mr. Hawtrey's writings from the very beginning. See, for example, *Good and Bad Trade*, 85 f.,

to an emphasis such as that of Wicksell, but also indicated a path leading toward an adequate "synthesis" of the two bodies of theory which has been either deliberately renounced or ignored by some of those who have been most extreme in thir claims for having effected just such a "synthesis."[79]

(d) No one in our own generation has been more insistent than Hawtrey on the point that it is dangerous to talk of tearing aside the "monetary veil" in order to study the "realities" of economic life which that "veil" is supposed to hide. No one, by both precept and example, has started more explicitly from the proposition that, if it be granted that the function of "general" economic theory is to explain the working of the economic process in the world we know, then no description of economic processes in the world we know can be regarded as satisfactory if it abstracts from the effects upon these processes of the working of the monetary mechanism.

Not everyone, to be sure, would be willing to accept at their face value all of Mr. Hawtrey's statements with respect, for example, to the relative importance of monetary and nonmonetary factors for the explanation of movements in output as a whole. It is only fair to add, however, that no writer who has been as emphatic as Mr. Hawtrey in stressing the importance of monetary factors has been so moderate as he, both in his attitude toward his predecessors and in his own formal statements with respect to the relation between monetary theory and "general" economic theory. Enough has been said, for example, of the treatment, by such "classical" writers as Ricardo and J. S. Mill, of the effect of monetary expansion and contraction on the level of output as a whole to make it clear that their analysis on this head is open to very severe criticism.[80] It is interesting, therefore, to find Mr. Hawtrey summarizing the position of such writers by no more violent a judgment than that while "economists of the classical school do not leave the monetary factor out altogether, . . . they regard it as subsidiary, and as merely modifying and perhaps intensifying tendencies otherwise accounted for."[81] And in dealing with the rôle of money in any "general" theory of the economic process, one could certainly do worse than take as a motto Hawtrey's remarks upon the supposed necessity for tearing aside the "distorting veil of money": "The distorting veil of money cannot be put aside. As well . . . play lawn tennis without the distorting veil of the net. All the skill and all the energy emanate

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140 f., 205, 235; *Currency and Credit*, 164 f. of the third edition (cf. p. 137 of the first edition); *The Gold Standard in Theory and Practice*, 10, 80; *The Art of Central Banking*, 179, 309, 322; *Trade Depression and the Way Out*, 38; *Capital and Employment*, 310.

[79] See especially, in this connection, what is said below, pp. 154 ff., concerning Mr. Keynes's treatment of the problem in his *General Theory*.

[80] See above, pp. 37, 49.

[81] Hawtrey, *Trade and Credit*, 86. Cf. also the moderate statements with respect to what "economists" generally have argued with respect to "the influence of money in economic phenomena," in *Good and Bad Trade*, 5.

from the players and are transmitted through the rackets to the balls. The net does nothing; it is a mere limiting condition. So is money." [82]

II
THE LESSONS OF DOCTRINAL HISTORY

In the light of what has preceded, there should be no doubt as to the nature of the argument advanced in this work on behalf of a reasonably close acquaintance with that body of received doctrine against which it is proposed to instigate a "revolution." This argument, again, is not primarily that such an acquaintance is likely to inhibit tendencies to advance claims for having effected such a "revolution"—although, one may add, this likelihood becomes a virtual certainty when one not only *discovers* earlier claims to "revolutionary" accomplishment, but goes on to *test* these earlier claims in the light of still earlier doctrinal history. The real argument for a study of the earlier instances is that from such study one may acquire not only humility but also wisdom—in the sense of an understanding of both the limitations attaching to, and the pitfalls surrounding, formulations of the kind that one might otherwise believe to be in some fundamental sense "new."

This, of course, amounts merely to saying that, as historians who would wish to be regarded as "prophets looking backward," we are interested, above all, in the *lessons* that can be derived from past attempts to effect a "synthesis" between the "general" Theory of Value, on the one hand, and the Theory of Money and Prices, on the other, *in order that the lessons thus learned may be applied to future attempts to perfect such a synthesis.* In brief outline, these "lessons" may be stated as follows:

1. The very fact that the problem of "reconciling" the two bodies of theory has been posed from the very earliest times means that no merit whatever attaches to the mere *posing* of the problem. On the contrary, it must be shown that each new posing of the problem leads to specific substantive results which leave the subject in a more advanced

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[82] *Trade and Credit,* 105 f.

state than it was in before the problem was posed anew.[83]

2. So far from its being true that in each successive case the posing of the problem has led to such results, it has happened, as often as not, that the results obtained were inferior to those already available.[84]

3. In some cases the reason for this inferiority was that the particular author concerned made use of a "general" Theory of Value which was itself retrograde, when judged either from the standpoint of later developments within the "general" Theory of Value or from the standpoint of the developments within that field already available at the time the synthesis was undertaken.[85]

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[83] Cf. what is said above: (1) pp. 18 f., concerning the relative importance, for the further development of monetary theory, of John Law's proposition, on the one hand, that "the value of money obeys the same laws as other goods," and, on the other, his emphasis on the rôle of the demand for money, as money, as a factor affecting its value; and (2) p. 28, concerning the lack of importance attaching to Adam Smith's "assimilation" of his theory of the Value of Money to his "general" Theory of Value, in terms which would make this "assimilation" almost identical in its formal aspects with that of Cantillon, as compared with the importance of Smith's failure to take over certain other aspects of Cantillon's argument, with the result that Smith left the theory of the determination of money prices in a state that was definitely retrograde as compared with what was available in the earlier literature. For applications of this "lesson" to the later literature, including what have been regarded as the specific contributions of Keynes's *General Theory,* see below, pp. 154 ff., 452, 458 f., 474 ff., 500 ff., 533 ff., 539 ff., 549 ff., 553 ff., 562 ff., 573 ff., 583 ff., 606 ff., 620 ff., 633 ff., 648 ff., 664 ff., 681 ff., 740 ff.

[84] In addition to the reference given in the preceding note to the discussion of the relative merits, in this respect, of Smith and Cantillon, see what is said above, p. 68, concerning the superiority of Menger's use of his distinction between changes in the "internal" and "external" value of money to both (1) the uses of this distinction which have associated it, *via* the concept of "neutral money," with the construction of "barter" economies, and (2) those treatments of the Theory of Prices which have failed to carry through the positive implications of the distinction in question. See also what is said above, pp. 70 ff., on the superiority of Walras's positive treatment of the issues involved in any attempt to "synthesize" the two bodies of doctrine, as compared with the treatment of these issues by Walras's critics. For applications of this lesson to the later literature, including the argument of Keynes's *General Theory,* see the forward references given in the preceding note.

[85] It is a striking fact of doctrinal history, despite repeated statements to the contrary in recent times, that in most cases the "retrograde" character of the specific "general" Theory of Value underlying a given Theory of Money and Prices did not derive from an appreciable lag in the application of the newer developments within the Theory of Value to the

4. This, however, is by no means the only, or even the chief reason for the disappointing nature of the results obtained from earlier attempts at "synthesis." A much more frequent source of disappointment has been that the supposed "synthesis" has resulted only in the posing of problems that are purely factitious, in the sense that even the "solution" of these problems would throw very little light on the issues of substance involved.[86]

theory of the Value of Money. See, for example, what is said, in this connection, concerning the impact of the "revolution" of the 1870's in value theory upon the theory of the Value of Money (above, pp. 52 ff.). On the contrary, the retrograde character of the "general" Theory of Value involved derived more commonly from an arbitrary insistence upon *first* rejecting the newer developments *within the "general" Theory of Value itself,* and *then* applying the consequently retrograde "general" theory to the problem of the determination of the Value of Money. For applications of the "lesson" thus involved to the later literature, and especially to the argument of the *General Theory,* see below, pp. 533 f., 539 ff., 574 ff., 583 ff., 636 f. It also should be pointed out, however, that the charge that a given device used in monetary theory represents nothing more than a carry-over into monetary theory of a type of device long since abandoned within the "general" Theory of Value, has often derived either from (*a*) an unjustified *exclusivism,* of the kind indicated below under "lesson" 8, or from (*b*) a simple failure to understand the implications of the particular weapon of monetary theory that happens to be involved. In this connection, see the comment at the end of n. 49 to p. 20, and p. 21, n. 51, above; and cf. what is said below, pp. 280 ff., 319 ff., 323 ff., 330 ff., 364 ff., 464 ff., 570 ff., 591 ff., 601 ff., 622 ff., concerning the rôle, in any adequate "synthesis" of the two bodies of doctrine, of equations of the Fisherine type, sometimes characterized as an example of a carry-over from a "retrograde" Theory of Value (cf., for example, Hicks, "A Suggestion for Simplifying the Theory of Money," *loc. cit.,* 2). The "lesson" indicated under (3) above is therefore one that must be very carefully applied if it is not to be abused. There is no doubt, however, that there are cases to which this lesson is applicable; and it is one of the contentions of this work that it is applicable to a number of aspects of the argument of Keynes's *General Theory.* On the "lesson" itself, see what is said above, page 11, on Aristotle, and pp. 20 f., on John Law; and on the application of the "lesson" to certain aspects of the argument of the *General Theory,* see the references given earlier in this note.

[86] For examples, see, in addition to those cited in the following note, the comments above: (1) pp. 11 ff., and 16 ff., (in connection with Aristotle and Locke, respectively), on the perennially recurring dispute as to whether money is to be regarded as a "commodity," subject to the same Laws of Supply and Demand as "other commodities"; (2) p. 32 ff. (in connection with Ricardo), on the alleged "inconsistency" of regarding the cost of production of the money material as a factor affecting its value in the case of metallic money but not in the case of a paper currency; and (3) pp. 59 and 88, on the applicability of the concept of "marginal utility" to money as such, as well as pp. 80 f., on the relation of the concept of "real balances"

5. On the contrary, in many cases these factitious problems have been stated in such a way as to result in an actual *obscuring* of the nature of the substantive issues in dispute; whereas the modes of stating the problem that allow the substantive issues to appear most clearly have been, as often as not, precisely those which had been rejected by the "synthesizers" on the ground that they do not make use of the concepts of the "general" Theory of Value.[87]

6. In other cases, the reason for the disappointing nature of the results obtained has been an excessive concern with matters which are purely *formal* in nature.[88] One of

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to the latter problem. Contrast what is said above, pp. 59, with respect to the practice of Walras, Menger, and Marshall in connection with point (3); and for examples in more recent literature of the introduction of issues as factitious as those indicated above, see the forward references given in nn. 87 and 88 immediately following.

[87] For examples of an obscuring of the substantive issues, as a result of the procedure indicated in the text, see what is said above: (1) p. 12 (in connection with Aristotle), on the so-called "commodity" character of money, on the one hand, and the true nature of the issues involved in the bimetallic dispute, on the other; (2) pp. 16 f. (in connection with Locke), on the question whether the "same" laws of value apply to money as to other commodities, on the one hand, and, on the other, the question as to the relative importance of changes in the supply of and demand for money in the determination of its value; (3) page 19 (in connection with John Law), on the fitness of the proposition that the "value of money obeys the same laws as other goods, rising or falling in proportion to changes in supply and demand" to deal with the substantive issues involved in the dispute between John Law and his opponents, on the one hand, and the "quantity theorists" and "anti-quantity theorists," on the other; (4) pp. 41 ff. (in connection with Senior), on the fitness of the proposition that money is a "commodity" whose value is "decided" by the same causes which "decide the value of other commodities," to deal with questions such as the theoretical possibility of fiat money or the relation between "velocity" and the cash-balance approach; and (5) p. 87 (in connection with Cannan), on the proposition that the "elasticity of demand for money" is not necessarily equal to unity. See also what is said above, pp. 48 ff. and 72 ff. on the usefulness of slogans, such as those of J. S. Mill and other writers, with respect to the importance or lack of importance of money in economic theory, when these slogans are judged as guides to the nature of the issues involved. For similar instances of an obfuscation of the issues by later writers, including Mr. Keynes, as the result of a desire to use the categories of the "general" Theory of Value in describing the working of the monetary mechanism, see below, pp. 652 ff., 668 ff., 693 ff.

[88] For examples, see what is said above: (1) pp. 84 ff. and 92 (in connection with Wicksell and certain of his critics) on the degree of importance attaching to the fact that writers who made use of a "theory of choice" in discussing the forces determining the size of cash balances relative to outlay may not have used the *terminology* of "utility analysis"; and (2)

the consequences of this excessive formalism has been that, instead of leading to results which are new in *substance*, the alleged "synthesis" has amounted only to a restatement in unfamiliar terms of substantive results already perfectly familiar within the Theory of Money and Prices.[89]

7. A further consequence of this excessive formalism has been a premature complacency that has led writers to suppose that they have actually provided a substantive solution of a given problem, whereas in fact they have merely restated in other terms the problem to be solved.[90]

8. In still other cases, the reason for the disappointing nature of the results obtained has been an unreasonable *exclusivism*. In some instances, this exclusivism has taken the form of a failure to see that two approaches to a given problem, instead of being contradictory, are mutually *complementary*.[91] In other instances, it is represented by

p. 90, in connection with the proposition, advanced by certain contemporary writers, that if it were really true that "it is impossible to use the modern theory [of value] to explain the Value of Money," this "modern theory" would be "shaken in its foundations." For examples of a similar type of formalism in later economic literature, see the forward references given in nn. 89 and 90 immediately following.

[89] See, for example, what is said above: (1) pp. 21 f., on the relation of the statement that the "demand" for money is a factor affecting its value, to statements respecting the effect on the value of money of changes in monetåry "velocity" and the "volume of·trade"; and (2) p. 87, on Cannan's proposition that the elasticity of demand for money is not necessarily equal to unity (cf. also the forward references there given). For similar examples from the later literature, including Keynes's *General Theory*, see below, pp. 658 ff., 674 ff., 686 ff., 740 ff.

[90] See, for example, what is said above: (1) pp. 20 ff. (in connection with John Law), on the difference between the mere statement that the value of money is determined by the "proportion between supply and demand," on the one hand, and the nature of the *forces determining* both "supply and demand"; and (2) p. 87, as well as in the parts of Volume I cited in n. 91 thereto, on the degree of achievement represented by the statement of the forces determining the size of cash balances in terms of a weighing of the "utilities" or "disutilities" involved in the holding of such balances, as compared with a detailed description of the factors affecting the degree of "utility" or "disutility" involved in the holding of a cash balance. For examples from the later literature, see below, pp. 659 ff., 681 ff.

[91] For examples, see (1) the comment at the end of n. 49 to p. 20, above, on the essentially complementary nature of a formulation such as $D = F(p)$, on the one hand, and a formulation such as $P = D/S$, on the other, when the second formulation is translated, in the manner indicated in n. 51 to p. 21, into an equation of the Fisherine type, and is thereby

a failure to see that the mere fact that a given analytical device borrowed from the "general" Theory of Value is helpful for solving *certain* problems within the field of the Theory of Money and Prices does not mean that it is helpful for solving other problems within that field.[92]   And in still other instances, it has resulted in a failure to face problems which not only are of the utmost importance in themselves, but which also provide opportunities for a genuinely useful

related to the concept of a *"general"* ["moneyed"] demand" (cf. pp. 46 ff., and the forward references there given); (2) the comment on pp. 44 ff. on the alleged contradiction between the formulation of Senior and that of J. S. Mill; (3) the comment on p 82 with respect to the failure of certain critics of the familiar Quantity Equations to appreciate either the historical or the logical connection between formulations of this type and that concept of "general demand" of which they themselves approve; (4) the comment on p. 87 with respect to the failure of a writer such as Professor Cannan to appreciate the essentially complementary nature of the relation between concepts employed by the "cash-balance approach" and concepts such as "velocity" (cf. also the remarks on the contrary, and superior, example in this respect set by Marshall and others, above, pp. 59 f.); and (5) the comment on pp. 100 ff. on the exclusivist character of certain common criticisms of formulations of the type represented by the Fisher equation of exchange, all of which derive from a failure to appreciate the extent to which these formulations complement, as they themselves are complemented by, other devices in monetary theory.   For examples from the later literature, see below, pp. 240, 280 ff., 285 ff., 319 ff., 330 ff., 364 ff., 464 ff., 471 ff., 591 ff., 601 ff., 652 ff., 661 ff.

[92] For examples, see the comments above: (1) pp. 14 f., on criticisms of Davanzati on the ground that his monetary theory was characterized by an objectionable duality, whereas in fact it represented an altogether sensible application of different devices to different problems in accordance with the requirements of the particular problems involved; (2) pp. 33 ff., on the charge of "inconsistency" leveled against Ricardo, whereas in fact he was following the sensible practice just indicated; (3) p. 55, on the equally sensible practice of Jevons in connection with the rôle of "utility" and "cost of production," respectively, in the determination of the Value of Money; (4) n. 24 to p. 62, on Jevons's varying emphasis on the importance of money in economic theory generally, in accordance with the nature of the particular problem he happened to be discussing; (5) pp. 82 ff., on the degree to which "old" Cambridge, on the one hand, and certain of its critics, on the other, can be charged with an exclusivist addiction to the use of the same types of analytical device in all problems of monetary theory, regardless of the nature of the particular problem taken for examination; (6) pp. 97 ff., on certain criticisms of Wicksell which have the effect only of emphasizing the inclusiveness of his own analytical apparatus, and its flexibility in accordance with the peculiarities of the problem chosen for examination, in contrast with the analytical exclusivism of certain of his critics; and (7) pp. 102 f., on the inclusiveness of Fisher's formulation in contrast with that of certain of his critics.   For examples from the later literature, including Keynes's *General Theory,* see below, pp. 633 ff., 726 ff.

application of the categories of the "general" Theory of Value to the problem of the forces determining money prices and the channels through which these forces operate.[93]

9.   There are, to be sure, instances in which a conscious effort to "synthesize" the two bodies of doctrine by carrying over to the problem of the Value of Money the categories of the "general" Theory of Value has led to substantive results which can stand on their own feet as contributions to our understanding of the nature of the forces determining money prices and the processes through which these forces make themselves felt; and this fact in itself would argue against a refusal to encourage further attempts at "synthesis."[94]   In a very large number of cases, however, pre-

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[93] For examples, see what is said above: (1) pp. 25 and 28 ff., concerning the failure, by many later writers, to appreciate the full significance in this respect of certain aspects of the argument of Cantillon; (2) pp. 68 f., concerning Menger's emphasis on the necessity for making full use of the substance of *both* "monetary" theory and "general" economic theory in explaining the determination of the prices of specific commodities; and see (3) the similar remarks, on pp. 93, 106 f., 118 ff., and 122 f., concerning the treatment by Wicksell, Fisher, Schumpeter, and Hawtrey, respectively, of the forces determining the *structure* of relative prices. For examples of a neglect of these models by later writers, particularly the Keynes of the *General Theory,* see especially below, Chapters Four and Ten.

[94] For examples of such substantive contributions, see above: (1) p. 13, on Bodin's use of his general proposition that *"c'est . . . l'abondance qui cause le mépris"* to establish the importance of an increase in the quantity of the precious metals as a factor leading to their depreciation; (2) p. 16, on Petty's application, to the case of the money metals, of his general position with respect to the influence of cost of production upon value; (3) pp. 18 f., on Law's use of his rudimentary "theory of subjective value" to establish the importance of the monetary demand for the value of the money material; (4) pp. 23 ff., on Galiani's application of his principle of "scarcity" to the question of the way in which cost of production affects the value of the precious metals, as well as his application of his principle of "utility" to the arts and the monetary demands for the money material; (5) pp. 30 ff., on Say's use of his general emphasis upon "utility" to establish propositions similar to those of Law and Galiani; (6) pp. 40 f. and 46, on Senior's use of a similar emphasis to establish similar propositions, and particularly his use of it to establish the foundations of the "cash-balance approach"; and (7) p. 59, on the use of the principles of "modern" value theory by Walras, Menger, and Marshall to lay the foundations of this approach more firmly. The association of the "income approach" with "utility analysis," in the cases of Wieser and Zwiedineck, may be taken as a further case in point; though in these instances the force of the claim to have established genuinely substantive results is considerably weakened not only by the fact that the "income approach" had already had a fairly extensive history prior to the publication of the essays of Wieser and

cisely the same substantive results were obtained directly, and without reference to the necessity for overcoming a "hiatus" alleged to exist between the two bodies of doctrine.[95] In all cases in which this has been so, it is clear that the fact that a given writer may have obtained his results in connection with his desire to overcome such a "hiatus" is of as much importance as, but of no more importance than, the fact that certain results were obtained in chemistry as the result of a search for a solution to problems originally posed by the alchemists.[96]

10. The most fruitful results of attempts at "synthesis" have come about when, instead of using the devices of the "general" Theory of Value to restate results already familiar within the Theory of Money and Prices, these devices have been introduced for the solution of problems *to which they alone can provide an answer*.[97] Or they have come about, conversely, when results obtained within the Theory of

Zwiedineck, but also by the number of extraneous and essentially factitious issues introduced by these authors' mode of stating the problem. See Volume I, 305, 309, and 491 f.; and also what is said above, p. 88.

[95] For examples, see what is said, above: (1) p. 32, n. 85, concerning Petty and the "cash-balance approach"; (2) p. 29, and n. 75 thereto, on Cantillon and contributions which have sometimes been regarded as peculiar to the "value theory" aspects of certain variants of the "income approach"; (3) p. 31, n. 81, on Ricardo and the effect of the monetary demand for the money material on the value of the latter. See especially, however, what is said above, pp. 92 ff., and 120, concerning the relation of Wicksell and Hawtrey, respectively, to both the "income" and the "cash-balance" approaches. For applications of this "lesson" to certain aspects of current monetary theory, see below, pp. 662 ff., 685 ff.

[96] Cf. above, p. 120. The effect of this conclusion is obviously to put much of the discussion of the relation between the theory of the Value of Money, on the one hand, and the "general" Theory of Value, on the other, on a par with most of what has been written concerning the "nature" of money, and other questions which I have elsewhere characterized as problems of "monetary metaphysics." In this connection, see the *Quarterly Journal of Economics*, XLII (1927), 144 f., 148 f., 151.

[97] For examples, see above: (1) p. 29, and especially n. 76 thereto, on the methodological implications of Cantillon's emphasis on "the idea of those who acquire the money" as a factor affecting the structure of money prices; (2) p. 69, on the really important implications of Menger's distinction between changes in the "external" and the "internal" Value of Money; and (3) pp. 94, 106 f., and 122 f. on the use by Wicksell, Fisher, and Hawtrey, respectively, of the apparatus of the "general" Theory of Value in explaining the structure of money prices. For applications of this "lesson" to current controversy, and particularly to Keynes's *General Theory*, see especially Chapters Four and Ten, below.

Money and Prices can be shown to provide a necessary *supplement to those obtained within the "general" Theory of Value,* in the sense that they deal with phenomena which, for methodological or other reasons, have been either inadequately treated or ignored altogether by "general" economic theory.[98]   Yet what the history of doctrine on the subject shows is that some of the most valuable contributions of both types have come from writers either indifferent or actively unsympathetic to much that has been done in the way of "synthesizing" the two bodies of doctrine, whereas it is precisely some of the writers who have complained most insistently of the existence of a "gap" between the two bodies of theory who have been most blind to the possibilities in one or the other of the directions indicated.[99]

III

The Lessons of Doctrinal History and Keynes's *General Theory*

It is these "lessons," then, that must be applied in any attempt to meet that challenge to received doctrine on the subject of the relation between the "general" Theory of Value, on the one hand, and the Theory of Money and Prices, on the other, which, on the basis of what was said in the first section of Chapter One of the present volume, must be regarded as having been provided by Keynes's *General Theory.*   Specifically, it will be recalled that Mr. Keynes has objected to the alleged fact that when "economists" pass from the general Theory of Value "to the Theory of Money and Prices . . . little or no attempt is made to relate" the "vaguer" phrases of the second body of theory "to our former notions of the *elasticities of supply and*

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[98] For examples, in addition to the references given in the preceding note, see above: pp. 61 ff., on Jevons and Menger; pp. 70 ff., on Walras and Marshall; pp. 109 f., on Fisher; pp. 118 f., on Schumpeter; and pp. 120 ff., on Hawtrey.

[99] See the references given to Wicksell, Fisher, Schumpeter, and Hawtrey in nn. 97 and 98, immediately preceding; and contrast what is said concerning certain aspects of the argument of J. R. Hicks, above, p. 83, n. 78, and those aspects of the argument of Keynes's *General Theory* which are discussed below in Chapters Four and Ten, respectively.

demand.'' [100]   It will be recalled, also, that the type of Theory of Money and Prices with which Mr. Keynes wishes his own Theory of Prices to be contrasted is that in which prices are alleged to be "governed by the quantity of money, . . . by the velocity of circulation relatively to the volume of transactions . . . *et hoc genus omne''*—in other words, by the type of formulation represented by those Quantity Equations whose meaning and purpose were expounded in Volume I of the present work.   The issues raised by these specific challenges are therefore among the issues with which the remaining chapters of this volume are concerned.

That these are not the *only* issues with which the remainder of this volume is concerned is, however, only to be expected in the light of the central purpose of the work as a whole.   That purpose, again, is the *constructive* one of presenting an apparatus for accounting for the determination of realized money prices (and of the amount of realized sales at these prices) which will make full use of all that is offered by *both* the "general" Theory of Value, on the one hand, and the Theory of Money and Prices, on the other. It is this constructive task which must be regarded as setting the ultimate goal of the analysis which follows.

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[100] *General Theory,* 292.

PART TWO

DEMAND CURVES, STREAM EQUATIONS, AND A MOVING
SYSTEM OF ECONOMIC QUANTITIES

CHAPTER FOUR

Elasticity of Demand and the Structure
of Money Prices

I

A S WE HAVE SEEN, "elasticity of demand" is one of
the "homely but intelligible concepts" of the "general"
Theory of Value which Mr. Keynes has charged economists
generally with abandoning as soon as they pass to the The-
ory of Money and Prices. In fact, however, the concept of
"elasticity of demand" has played, not one, but at least
two quite distinct rôles in the body of monetary theory as
it had developed prior to the appearance of Keynes's *Gen-
eral Theory*. It is necessary, therefore, to consider the two
rôles separately: not only because the analytical connection
between the two is an extremely tenuous one, but also be-
cause, as we shall see, the results obtained in the two cases
are of greatly different degrees of significance from the stand-
point of the relative amount of light they have thrown
upon the problem toward whose solution it is hoped that the
present work is a contribution: namely, that of establishing
the nature of the forces determining money prices and the
quantity of objects sold at these prices.

In the present chapter we shall be concerned with the
bearing of the concept of "elasticity of demand" upon the
problem of explaining the *structure* of money prices. This,
surely, is the particular application of the concept of "elas-
ticity of demand" which ought to suggest itself as the most
natural one to all who are interested in establishing a satis-
factory *modus vivendi* between the "general" Theory of
Value, on the one hand, and the Theory of Money and
Prices, on the other. For the very subject matter of that

part of the "general" Theory of Value which is summed up,
for example, in Marshallian demand and supply curves for
individual commodities is itself part of a "general" theory
of the determination of *relative* prices.   More specifically,
it is that part of a "general" theory of the determination
of money prices which is concerned with the relation of the
prices of individual commodities other than money to *one
another,* without immediate regard to the problem of their
relation to the "value" of a "money" which may itself be
evidencing the effects of changes in its "supply" or "de-
mand," or to those changes in the position or conformation
of the demand and supply curves for individual commodities
which can be shown to result from the working of the mone-
tary system.[1]

"Elasticity of demand," in the Marshallian sense of the
term, is a property of these demand curves for individual
commodities.   One may insist, if one wishes, that nothing
but failure has thus far been yielded by all of that part of
the "general" Theory of Value which is summed up by these
Marshallian demand curves and their property of "elas-
ticity."   What one may not do is to pretend that the gap
left by this presumed failure can be filled by anything called
monetary theory, in any conventional sense of the latter
term.   Monetary theory can *add* to the theory of the de-
termination of relative prices which is presented by the

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[1] It will be observed that this description of the subject matter of the
"general" Theory of Value avoids completely the suggestion that the latter
is constructed on "barter" assumptions.   To be sure, it comes closer to what
has been meant by many of the writers who have suggested that in the
"general" Theory of Value money is assumed to act only as a *numéraire,*
or to be "neutral" with respect to the structure of prices.   Unfortunately,
however, both the concept of a *numéraire* and the concept of "neutral
money" have sometimes been associated with the use of "barter" assump-
tions (see my "The Monetary Aspects of the Walrasian System," *loc. cit.,*
166 ff., 172 ff.) ; and in any case both statements are likely to convey a mis-
leading impression as to the degree of separation between "monetary
theory," on the one hand, and the "general Theory of Value," on the other,
which has actually occurred in the treatment of the "Theory of Prices" by
the ablest writers in the past.   On both grounds, as well as in the light of
the diversity of the connotations that have come to be associated with the
concept of "neutral money," in particular, it seems to me preferable to use
the terms indicated in the text to describe the subject matter of that part
of the theory of "pricing" which is represented by the "general" Theory
of Value.

"general" Theory of Value; *it cannot displace that theory.* On the contrary, any adequate theory of the determination of money prices must be prepared, at the very least, to *incorporate* those elements of the "general" Theory of Value which can be shown to have specific heuristic value for the explanation of the determination of prices, but with which monetary theory, in the narrower sense of the term, is not fitted—and in the nature of the case cannot be expected to be fitted—to deal. It will be observed at once that this particular type of *modus vivendi* between the two bodies of theory is the very antithesis of the two types of result that have so often emerged from a desire to "synthesize" the two bodies of doctrine in the past: namely, (1) the introduction of issues which are in themselves entirely factitious; and (2) the futile translation of results already perfectly familiar within the field of monetary theory in terms of the formal categories of the "general" Theory of Value. On the contrary, it represents the establishment of a type of *modus vivendi* which is *inevitable,* for the simple reason that there is no reasonable alternative to the particular type of synthesis which it undertakes to effect.

These simple methodological considerations, which ought to be regarded as being in the nature of axioms, are reinforced by an argument based on certain simple observed facts. The upshot of this argument is that it is only by the use of analytical devices such as the concept of "elasticity of demand," in the Marshallian sense of the term, that we are able to explain certain changes within the *structure* of money prices that have been known to occur during periods of "general" price change.[2] It is a common feature of periods of drastic monetary contraction, for example, that commodities heretofore held as inventories are rapidly unloaded on the market. It is an equally common feature of such periods that the falls in the respective prices of the commodities thus unloaded often show quantitative differences which cannot be explained *solely* in terms of (1) differences in the relative amounts offered for sale, (2) dif-

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[2] On the meaning of the concept of "general" price change, and its relation to changes in the *structure* of money prices, see below, pp. 330 ff.

ferences in the degree of change evidenced by the money incomes of the particular groups in the community who happen to be the principal purchasers of each commodity, or (3) differences in the rigidity of the institutional controls to which the particular prices are subjected. Under the circumstances, the only type of explanation which accounts completely for the differences in the degree of price change shown by different commodities during such a period of monetary contraction (and of course the same kind of argument would apply to a period of monetary expansion whenever such expansion involves phenomena similar to those indicated above in the case of monetary contraction) is one which is prepared to make use *also* of that type of analytical device, within the "general" Theory of Value, which is represented by particular demand schedules of the Marshallian type and their property of "elasticity." For it is such devices that enable us to account for the sharper rises or falls of particular prices in terms of differing responses of prospective purchasers to quantitatively equal changes in the amount of different commodities thrown on the market, or to quantitatively equal *initial* changes in price, however induced, with all that this must mean for the emergence of further spreads between the different members of the family of individual prices.

If these simple propositions are accepted, the first question to be raised is whether writers on the Theory of Money and Prices have or have not been aware of the fact that a concept such as the Marshallian "elasticity of demand" must necessarily be introduced in any attempt to account fully for changes in the structure of money prices, during periods of "general" falls or "general" rises in prices as well as during periods of comparative stability in "general" prices. To this the answer must be that the argument, as stated thus far, has been explicitly accepted not only by monetary theorists of the standing of Wicksell, Fisher, and Hawtrey, but also by the authors of textbooks and of works of so popular a nature as to forbid the suggestion that the author concerned was pretending to break new ground in the subject.[3]

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[3] For an example of an explicit acceptance, in current textbooks, of the proposition indicated, see L. D. Edie, *Money, Bank-Credit, and Prices*

Clearly, therefore, if "economists" generally are to be charged with having failed to appreciate the rôle of a concept such as the *Marshallian* "elasticity of demand" in that part of the Theory of Money and Prices which is concerned with the problem of the forces determining the *structure* of relative prices and of changes in that structure, the charge must be supported by specific evidence of a type that has not yet been forthcoming.

In one sense, indeed, the evidence that "economists" generally have in fact been prepared to accept without question the argument stated above with respect to the rôle played by the Marshallian "elasticity of demand" in the determination of the structure of money prices, during periods of "general" price change as well as during periods of "general" price stability, is so vast that one hardly knows where to begin in order to summarize it. For, to my knowledge, no economist of standing prior to the appearance of Keynes's *General Theory* had ever challenged the proposition that the principles of "general" value theory, with all that they have to say with respect to the nature of the forces causing differences in the "demand" for specific commodities apart from influences specifically attributable to the working of the monetary system, continue to be relevant in any attempt to explain why a given money price is what it is. On the contrary, as we have seen, a number of the most eminent writers have gone out of their way to insist upon precisely this proposition.[4] To say this, however, is to say simultaneously that the relevance of the Marshallian "elasticity of demand" to the explanation of changes in the structure of money prices has been recognized implicitly ever since the concept, or its equivalent, was incorporated into the "general" Theory of Value itself. At the most, therefore, all that can be expected is a series of instances taken from the writings of monetary theorists of unquestioned standing, in which the point was made *explicitly* with reference to the particular concept of "elasticity of demand," in the Marshallian sense of the term.

It was only natural, for example, that the concept should have appeared in the writings of the representatives of "old" Cambridge on the problem of explaining variations in output as a whole—a problem for

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(1928), 82. For an example in popular writings on the subject, see P. H. Douglas, *Controlling Depressions* (1935), 38 f., 73 f., 206 ff., 231 n. And for an example taken from a work which, while neither a textbook nor a book addressed primarily to a popular audience, explicitly disclaims any intention of bringing a *new* "contribution to the complete explanation" of the determination of money prices, see Lambert, *La Théorie quantitative de la Monnaie*, 183, 186. On Hawtrey, Wicksell, and Fisher, in this connection, see the references given below, pp. 144 ff., nn. 8–11.

[4] Cf., for example, what is said in this connection above, pp. 69, 94, and 122 f., concerning Menger, Wicksell, and Fisher, respectively; and see also what is said below, pp. 274 f. (Proposition VI),

which the question of the *structure* of money prices is, of course, crucial.[5]

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[5] It will be recalled that Mr. D. H. Robertson announced, at the outset (p. 11) of his *A Study of Industrial Fluctuation* (1915), that he was "deliberately of the opinion that one cause of the obscurity which still surrounds this problem [of "Industrial Fluctuation"] is that in the attack upon it full use has never hitherto been made of the weapons" supplied by the "particular intellectual armoury" associated "chiefly with the name of Dr. Marshall." For examples, from Mr. Robertson's work, of an application, to the problem, of the particular weapon from the Marshallian "armoury" which is represented by the concept of elasticity of demand, see Robertson's comments regarding (1) the effect of the differing elasticities of demand for different agricultural products upon the variability of the receipts accruing to the producers of these products, and therefore upon the "demand for constructional goods" exercised by these producers (*A Study of Industrial Fluctuation,* 91 f.); (2) the effect of the comparatively inelastic demand for wheat upon the "prosperity of the consumptive trades" generally and upon "certain other kinds of food consumption" in particular (*ibid.,* 110, 117); (3) the effect upon "aggregate industrial production" of the "elasticity of . . . [consumers'] demand for corn in terms of effort"—an "elasticity" in whose determination a vital rôle will, of course, be played by the *monetary* elasticity of demand for "corn," since the latter "elasticity" will affect not only the extent to which increased production of "corn" will lower its exchange value (and therefore the amount of money received by the producers of corn), but also the amount by which *consumers* of "corn" will expand or contract their expenditure on things other than "corn" when the price of "corn" changes (*ibid.,* 131 ff., 165 ff.; cf. also the following note); and (4) the differing elasticities of demand for different industrial products as a factor which, in combination with conditions with respect to cost, will determine whether the particular industries in question will or will not find it advantageous to maintain rather than to restrict production, with inevitable effects, for good or ill, upon the profit position (and therefore the level of activity) of "other trades as well." (*Ibid.,* 201 ff.; for a series of uses of the concept of "elasticity of demand" similar to those cited, see, in addition, Robertson's essay, "Economic Incentive" [1921; pp. 2 f., 5 of his *Economic Fragments* (1931)], and his *Banking Policy and the Price Level,* 13 ff., 26 ff.) See also the comments of Professor Pigou regarding (1) the effect of an inelastic demand for agricultural produce upon the "amount of industrial activity" (*Industrial Fluctuations,* 36 f., 53 n., 55 f.); (2) the possible effect, in initiating a "general industrial disturbance," of an inelastic demand for a given industrial product whose cost of production has been lowered by invention or other technical innovation (*ibid.,* 41, 52); (3) the rôle of elasticity of demand in determining the effect, upon "aggregate industrial activity," of a given change "of taste or fashion" (*ibid.,* 47 f.); (4) the bearing of the elasticity of demand for a given product upon the advisability of attempting to stimulate recovery by granting "bounties" to selected industries (*ibid.,* 315), or by price reduction by individual firms (*ibid.,* 298 f.); and (5) the effect of elasticity of demand in determining the degree of offset to increased purchases by one group of the consumers of a given commodity during a depression as a result of possible decreased purchases by another group (*ibid.,* 300 f.). In the light of these passages, it should hardly be necessary to comment at length on the statement, by a representative of "new" Cambridge con-

Indeed, the only point calling for comment here is that the occasional statement of the argument, by the authors concerned, in terms of a "real" elasticity of demand in response to a change in "*real* prices" is merely an "old" Cambridge way of indicating that what is involved is precisely the problem of changes in the *structure* of money prices. It should hardly be necessary to labor the point that recognition of this fact does not necessarily imply *approval* of the method indicated as a means of dealing with the causes and consequences of those changes in the structure of money prices which involve the Marshallian concept of "elasticity of demand." The point made here is merely that a distaste for the method should not lead to a misrepresentation of its substance; and, specifically, that such misrepresentation would be involved if one were to ignore the fact that, despite the statement of the argument in "real" terms, the argument itself necessarily implies consideration of the effects of differing elasticities of demand, in the Marshallian sense of the term, on the structure of *money* prices.[6]

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vinced that Mr. Keynes's work has effected a "violent revolution" in our subject, that "until recently [that is, until the advent of Mr. Keynes and his group] no economist appears to have attacked this problem [that is, the problem of the forces affecting the "amount of employment and the wealth of the community"] directly," by "setting the *supply-and-demand apparatus* to work on the question in which he was really interested—the forces determining the volume of output" (Joan Robinson, "The Theory of Money and the Analysis of Output," *loc. cit.*, 22 [italics mine]). The *manner* of "setting the supply-and-demand apparatus to work" on this question is, of course, another matter; and the reader must be left to decide, on the basis of the chapters that follow, whether Mr. Keynes's "manner" is in fact the proper manner.

[6] It is true that the exposition of the members of the "old" Cambridge group has itself on occasion been such as to encourage such misrepresentation or misunderstanding. When, for example, Mr. Robertson announced his intention, in his *Banking Policy and the Price Level* (p. 8), of reasoning "as though the processes of exchange were conducted without the aid of money by direct barter," he may have encouraged the belief that the argument which followed was either (1) *valid* only on the assumption of "direct barter," or (2) incapable of translation into money terms. Actually, of course, neither proposition is true. It is perfectly *valid*, for example, to discuss the effects, in the world we know, of an inelasticity of the "real" demand of one group in the community for the product of another group, when all that is meant by the concept of an "inelasticity" of "real" demand is that we must observe (1) the effect of the *monetary* elasticity of demand for the product of the first group on the *money receipts* of the producers of this first commodity; and (2) the amount of commodities (as indicated by the existing structure of money prices) which these money receipts would command if expended by the recipients upon the particular commodities they happen to desire. And it is equally valid to discuss the effects of the inelasticity of "real" demand, "in terms of effort," of one group in the community for the product of another group, when what is meant is that we must take into account not only elements (1) and (2), as already indicated, but also (3) the *effect* of (1) and (2) "upon the most

The case of Hawtrey, in any event, provides an instance of a writer who can hardly be regarded as having shown excessive sympathy for "real" concepts of the type dear to the "old" Cambridge group.[7] He provides, therefore, an example of what may be expected when the problem is stated explicitly in terms indicating that what is involved is the effect of differences in elasticity of demand, in the Marshallian sense, upon the structure of *money* prices. And indeed, as we have seen, Mr. Hawtrey has made the point so often and with such explicitness in precisely these terms that the relevant passages in his writings may be regarded as being in some respects the *loci classici* for the point at issue.[8]

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profitable level of production" for others than members of the first group (cf. Robertson, *A Study of Industrial Fluctuation,* 204). The important point, for our present purpose, is that the answer to these questions (and particularly the questions indicated by [2] and [3]) can be provided only by studying the contours of the relevant sector of the general structure of *money* prices. The examples given may thus be taken as illustrating not only the *possibility* of translating the respective "real" concepts in terms of elements associated with the structure of money prices, but also the *necessity* for doing so, if we are to make further progress toward a "visualization in detail" of "the monetary routes by which these results are reached" (cf. Robertson, *Banking Policy and the Price Level,* 28)—a visualization which may be complicated, to be sure, but which is emphatically not to be regarded as impossible. There is no reason, in any case, for believing that the principal sponsors of the "real" concepts involved would be prepared to reject, on grounds of principle, translations of the type indicated. See, for example, Robertson, *Banking Policy and the Price Level,* 26, 28, and the reference to Pigou on p. 23, n. 1, of the same work; and on the general necessity for translations of the "real" concepts involved, in terms of money and monetary processes, cf. Haberler, *Prosperity and Depression* (second edition, 1939), 158.

[7] See, for example, Hawtrey's incisive discussion of Pigou's concept of "wage-goods" (a concept, it should be observed, which, as Hawtrey's critique makes clear, necessarily involves a series of assumptions with respect to the structure of money prices and money costs) in *Economica* for May, 1934, 152 ff. (reproduced, with revisions and additions, in Hawtrey's *Capital and Employment,* 276 ff.).

[8] See above, p. 122, and the references given in n. 78 thereto. The immediate "point at issue," it may be observed, is the effect of differing elasticities of demand, in the Marshallian sense of the term, upon the *structure of money prices.* When, to be sure, one passes to the problem of tracing the effects of the changes in the structure of money prices thus accounted for upon the level of *output as a whole,* there are reasons for arguing that Mr. Hawtrey has on some occasions failed to do full justice to the intricacies of the problem, even if one starts from his proposition that these changes in the structure of money prices work out their effects upon output through an intermediate effect upon "general [money] demand." See, for example, Hawtrey's *Good and Bad Trade,* 83 ff., 141 f., and *The Gold Standard in Theory and Practice,* 80. The issue, in this case, is in one sense part of the broader question whether Mr. Hawtrey, in his anxiety to support his contention that the trade cycle is a "purely monetary phenomenon," has not underestimated the importance of certain elements

There are other instances in modern economic literature, however, in which the point may be regarded as having been made with an equal degree of explicitness, even if the actual *term* "elasticity of demand" was not used. Wicksell, for example, certainly cannot be interpreted as having argued in any other way when—in protesting against the suggestion that an increased money demand, of the kind that would be expected to characterize a period of monetary expansion, would result in a rise in all prices proportional to this increase in money demand—he pointed to the case of "indispensable necessities" as one in which, "in accordance with the well known so-called Law of Gregory King, even a slight increase in [aggregate money] demand might bring about a much greater increase in the price" of these "necessities." [9] And precisely the same thing must be said of the argument presented by Irving Fisher *against* assuming, in any attempt to show "the effect of a change in the volume of business upon the level of prices," that "all the Q's change uniformly in one direction and all the p's uniformly in the other." On the contrary, Fisher argued, "if the first set change uniformly, the second cannot change uniformly"; for "a doubling in the quantities of all commodities sold, or . . . a doubling of the quantities consumed, would change their relative desirabilities and therefore their relative prices." Thus, "to

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which are themselves relevant not only to the explanation of the trade cycle generally, but also to an adequate explanation of the way in which the monetary factor itself (and, specifically, the factor of "general demand") operates in the course of a given cycle. On this matter, see the comments by Saulnier, *Contemporary Monetary Theory*, 56 f., 66, 83, 103 f., 106. All that this means in the present instance, however, is that Mr. Hawtrey has failed to reap the full benefits of his own argument with respect to the effect of differing elasticities of demand, in the Marshallian sense of the term, upon the structure of money prices; it can hardly be taken as implying any derogation of Mr. Hawtrey's treatment of the first, and vital, stage in the argument, which must consist of an *explanation* of changes in the structure of money prices before one can go on to trace the consequences of these changes.

[9] See Wicksell's "Svar till ["Reply to"] kand. Åkerman," *Ekonomisk Tidskrift*, XXIV (1922), 11. It should be pointed out that Wicksell's own statement of the argument, like that of others who have introduced the concept of elasticity of demand, in the Marshallian sense, as an element affecting the structure of money prices during periods of monetary expansion or contraction, is elliptical, in that it assumes that the increase in "aggregate" money demand will *first* be accompanied by changes in the structure of money incomes or prices as a result of either (1) a differential impact of the new money upon these structures as the result of the particular way in which the new money is injected into the system; or (2) changes in the *supplies* of particular commodities. For it can be demonstrated (although the demonstration need not be provided here) that the intensifying effect upon the price structure of differing elasticities of demand can operate only if the conditions indicated by such assumptions are present. This does not alter the fact, however, that this intensifying effect is a very real one; and Wicksell must be given credit for having been one of those who pointed it out.

double the quantity of salt might make its marginal desirability zero, while to double the quantity of roses might scarcely lower their marginal desirability at all." [10]　Clearly, the question of the relation between a given relative change in the quantity of a given commodity offered for sale and the relative change in the price of that commodity, by way of the effect of the change in quantity on the "marginal desirability" of the commodity, is nothing if it is not a question of "elasticity of demand," in the Marshallian sense of the term.　Fisher's discussion of the point here, therefore, represents what is in all essentials an application of the Marshallian concept of elasticity of demand to the problem of changes in the structure of money prices; and the same thing must be said of his discussion, elsewhere in his *The Purchasing Power of Money*, of the question as to the relation between "the decrease in the price of any particular commodity" and "the increase in the amount of it exchanged" and therefore upon the size of the product (in the mathematical sense) of the price multiplied by quantity sold.[11]

Once, indeed, it is recognized that a given writer might have used what amounts to the Marshallian *concept* of elasticity of demand, even if he did not use the Marshallian *term*, the way is opened to adducing a series of examples in which the authors concerned accounted for changes in the structure of money prices during periods of "general" price change as well as during periods of "general" price stability by the use of the concept in question, long before it was given the name by which it is now known to beginners in the subject.　For, as it happens, one of the striking characteristics of the history of earlier attempts to describe that property of the "demand" for specific commodities which is indicated by the Marshallian concept of "elasticity of demand" is that, as often as not, these attempts were precisely parts of broader attempts to account for the different degrees of price change evidenced by different types of commodities during periods of "general" price change.

Consider, for example, the case of Locke.　Locke did not, to be sure, as did his contemporary Charles Davenant, make use of a table such as that supposedly borrowed by the latter from Gregory King, on the basis of which Davenant is usually cited as an early user of a crude approximation to the later Marshallian "elasticity of demand."　Locke was, however, concerned with the differences in the quantity of specific commodities demanded as the result of a given change in the price of those commodities.　More specifically, he was concerned with the different degrees of change in the quantity demanded of "whatsoever is absolutely necessary," on the one hand, and of things which are merely "more or less convenient," on the other; and he pointed out that whereas "men give any portion of money, for whatsoever is absolutely necessary, rather than go without it," "things convenient will be had only as they stand in preference with other conveniences," so that the

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[10] See *The Purchasing Power of Money*, 194 f.; and cf. Fisher's *Mathematical Investigations in the Theory of Value and Prices*, 50.

[11] In addition to the passage cited in the preceding note, see, for example, *The Purchasing Power of Money*, 178 f., 382 ff.

demand for (or "vent of") any one of these "conveniences" "depends upon its being preferred to other things, in its consumption." [12] These, surely, are among the phenomena with which the Marshallian "elasticity of demand" was intended to deal. It is therefore worth emphasizing that Locke's own use of his proposition was such as to make it part of an argument designed to show why *monetary debasement* would not necessarily "raise the value of all other things in proportion": why, for example, given an initial factor tending to raise the price of a commodity such as wheat, it might happen that the subsequent effect on the price structure, as a result of substitutions in and eliminations from the budgets of individual consumers, would be such that "silver, in respect of wheat," might be "nine tenths less worth than it was, in respect of oats two thirds less worth, and in respect of lead as much worth as before." [13]

The same type of introduction of a discussion of the effect, on the structure of money prices, of what amounts to a crude equivalent of the Marshallian "elasticity of demand," in a setting in which the principal topic discussed was the phenomenon of "general" price change, is to be found in a number of those writers of the early nineteenth century whose discussion of the phenomena summarized by the Marshallian "elasticity of demand" was avowedly based on the table supposedly borrowed by Davenant from Gregory King.

This was certainly true, for example, in the case of Henry Thornton, who made use of the point as part of an argument designed to refute certain erroneous conclusions that might be drawn from the mere statement that "the price of grain in London will by no means be found to have been high in proportion as the number of Bank of England notes have been great, and low in proportion as it has been small." [14] Such a statement, Thornton contended, does not disprove the existence of a "tendency of the fluctuations of the quantity of paper to produce correspondent variations in the price of commodities." For, he argued, "even a small reduction of the supply of grain can hardly fail to lead to a rise in its value when exchanged for paper, so great as to forbid all comparison between the effects of an alteration of the quantity of the one article and of an alteration of the quantity of the other." Thus, while "paper has been spoken of as raising the cost of commodities, at the most, only in proportion to its increased quantity," "in the case of a diminished supply of corn, the price rises according to a very different ratio," since we are dealing here with a "necessary of life." [15]

And it was equally true of Ricardo, whose use of the example of bread

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[12] See pp. 239 f. of the Ward, Lock and Company edition of Locke's *Consequences of the Lowering of Interest, and Raising the Value of Money.*

[13] Locke, *Considerations,* 238, 240.

[14] Thornton, *An Inquiry into the Nature and Effects of the Paper Credit of Great Britain,* pp. 211 ff. of the Philadelphia edition of 1807 (pp. 231 ff. of the 1939 reprint edited by F. A. von Hayek).

[15] Thornton, *Inquiry,* 227 f. (p. 243 of Hayek's edition). Thornton then reproduced, in a footnote, the passage from Davenant containing the table attributed to Gregory King.

as a commodity the "demand" for which "would not greatly increase" despite a fall of 50 per cent in its price shows that he was aware of the phenomenon now discussed under the head of "elasticity of demand," even if he did not make use of the table attributed to King. For Ricardo advanced this proposition in a context which shows clearly that he believed the argument to be applicable not only when "the value of money" continued "invariable," but also when it varied.[16]

The case of Tooke is particularly interesting, in this connection, by virtue of his reference to Davenant and to the latter's use of the table attributed to Gregory King, on the one hand, and the use of Tooke's own discussion by John Stuart Mill, on the other; for this means that in a sense Tooke's discussion may be regarded as representing the most important single link in the chain of pre-Marshallian doctrinal development with respect to the type of analysis which later came to be associated with the concept of "elasticity of demand." [17]   It is well known, of

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[16] See Ricardo's *Principles,* Chap. XXX (p. 376 of the Gonner edition). The particular point in the context which is relevant in this connection is the fact that Ricardo accepted as valid, for the purpose in hand, Lauderdale's proposition that "the *value* of any commodity . . . may vary at one period from what it is at another" not only as a result of particular "contingencies" affecting the supply of and demand for the particular "commodity of which we mean to express the value," but also as a result of those "contingencies" which affect the supply of and demand for "the commodity we have adopted as a measure of value"—that is, *money* (375 f.). Ricardo's citation of Lauderdale is interesting in this connection also because, although Ricardo himself presented his example of the elasticity of the demand for bread without using the table attributed to Gregory King, Lauderdale's own use of that table was such as to make him an important figure in the list of pre-Marshallian users of what amounts to the Marshallian elasticity of demand. In this connection, see Marshall's own comments on this aspect of Lauderdale's work in the former's *Principles,* 106, n. 2.

[17] For Tooke's citation of Davenant and the table attributed to King, see his *Thoughts and Details on the High and Low Prices of the Last Thirty Years,* III, 90 (reproduced in Tooke's *History of Prices,* I, 11 f.). It may be observed that this is not the only passage in Tooke's writings in which he made some use of what has been regarded as an adumbration of the Marshallian "elasticity of demand," though it is the one most commonly cited. See, for example, (1) Tooke's *Thoughts and Details,* I, 92, where he pointed out that "when the rise in price, from scarcity, has attained a certain height," one of the effects which "necessarily follow" is "a diminution, *greater or less,* of the consumption, *according as the article is more or less necessary, or admits of substitutes*" (italics mine); (2) II, 24 of the same work, where he suggested that the response to a rise in the price of a commodity regarded as a necessity, such as tea, would be that "rather than forego their usual quantity of tea, . . . a poor family may abridge its indulgences in an occasional pudding or pie" (cf. Cournot, *Researches,* 47, where the example of firewood is used in place of tea); (3) Tooke's *History of Prices,* I, 250, where he argued that the prices of articles "of *first necessity*" would be expected to show greater resistance to

course, that Tooke's own principal use of the concept was such as to make it part of an argument designed to refute the suggestion that "if the variations in price [of a given commodity] do not correspond with the variations in quantity [of that commodity] in exact proportion, . . . there must be something in the currency . . . to account for what appears . . . so anomalous an effect." [18] It is not necessary here to go into the merits and demerits of Tooke's specific arguments on this head, or into a discussion of the later misuses of his arguments to which

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monetary contraction than "articles not so circumstanced" (italics Tooke's); and (4) V, 325 of the same work, where, in illustration of "the difference of principle according to which the demand for Wheat and other articles of necessary food is determined, as compared with the demand for articles of produce not of the same necessity," Tooke argued that "there could be no such diminution of the consumption of Bread in consequence of an advance of the price, as there was of the consumption of Sugar," and he contended that "a still greater difference might be pointed out in other articles of still less necessity than Sugar." No one, to be sure, could deny that Tooke's treatment of the phenomenon later discussed by Marshall under the head of "elasticity of demand" was faulty in some respects: as when, for example, he implied that the elasticity of demand may be expected to be less than unity not only in the case of commodities such as "corn," but in the case of "commodities generally"—the demand for "corn" being regarded only as "more especially" inelastic than that for "commodities generally" (see, for example, *Thoughts and Details*, III, 98, 142, and IV, 4, and contrast Cournot, *Researches*, 46 f.). Yet there can be little doubt that Tooke's discussion of the phenomena involved was sufficiently enlightened to justify the current practice of referring to this discussion, and that of Mill which was based upon it, as examples of pre-Marshallian adumbrations of the Marshallian "elasticity of demand." For Mill's reference to Tooke in this connection, see the former's *Principles*, Book III, Chap. II, sec. 4 (p. 447 n. of the Ashley edition); and cf. the comments of Edgeworth in his article on "Elasticity" in Palgrave's *Dictionary of Political Economy*, I, 691, and the comments of Mr. Keynes in his memoir on Marshall (*Memorials of Alfred Marshall*, 45, n. 4). It may be pointed out that the statement in the text with respect to the chain of doctrinal development refers to the chain of *acknowledged* influences. In terms of substance, of course, the most nearly exact precedent for the Marshallian concept of "elasticity" was provided by Cournot (*Researches*, 54). As is well known, however, Marshall did not refer to Cournot in this connection (cf. H. Schultz, *Statistical Laws of Demand and Supply* [1928], 7, n. 6), any more than Cournot had referred to earlier writers, such as Lauderdale, whom Marshall later cited in his discussion of the concept of elasticity of demand (cf. the preceding note), even though he did not cite Jevons, who himself had cited Lauderdale, Thornton, Tooke, and others in connection with the phenomena with which the concept of "elasticity of demand" was intended to deal (or, as Jevons put it, "the relation between a change in the supply of a commodity and the consequent rise in price"; see Jevons, *Theory of Political Economy*, 148 f., 153 ff.).

[10] *Thoughts and Details*, III, 87; *History of Prices*, I, 10. Cf. the similar comment on the practice of "not a few economists" by Schultz, *Statistical Laws of Demand and Supply*, 13 n.

his exposition gave rise.[19]   What matters, for our present purpose, is that Tooke was at no time prepared to deny either the *possibility* or

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[19] The principal fault of Tooke's exposition, of course, was that it lent itself readily to the interpretation that what he was trying to prove was not only that, *in certain specific cases,* the price movements generally attributed to "an alteration in the amount of the Bank circulation" were capable of explanation on other grounds, but also that in *all conceivable cases* changes in prices commonly attributed to monetary expansion or contraction could be explained entirely in terms of changes in "supply" and "demand" which were themselves unrelated to the functioning of the monetary mechanism. At one stage in his career, indeed, Tooke himself was aware of what he characterized as an "error in the mode of the statement" of his arguments as they had appeared in his earlier writings, and he admitted that what he should have done was to have "stated, more distinctly than I did, the division of the argument into two branches, viz., that of alterations in the value of the currency arising from the suspension and resumption of cash payments, and that of alterations in the value of the currency from circumstances which would have affected it independently of those measures" (*Considerations on the State of the Currency* [1826] 2 n.; see also below, p. 152, n. 21). Unfortunately, however, as time went on, Tooke's reaction against the practice of attributing changes in prices to monetary expansion and contraction became so violent that it culminated in his celebrated "twelfth thesis": "The prices of commodities do not depend upon the quantity of money indicated by the amount of bank notes, nor upon the amount of the whole of the circulating medium; . . . on the contrary, the amount of the circulating medium is the consequence of prices" (*Inquiry into the Currency Principle,* 123 f.). Unfortunately, also, the effect of this reaction, as summarized in the "twelfth thesis" just quoted, was to impair not only the general validity of Tooke's explanation of the movements of money prices in the particular periods he took for study, but to impair even the usefulness of those parts of his positive analytical apparatus which were otherwise of abiding value: as when, for example, having progressed to the point of seeing that the "demand" for *all* commodities, and therefore, in some degree, the demand for any *one* commodity, is related to the level of money income (cf. Volume I, 314, of the present work), he failed to relate the level of money income in any significant way to the "amount of the circulating medium."   (Cf. Volume I, 346, and the reference given in n. 4 thereto.   See also Tooke's statements with respect to the absence of any significant effect upon the amount of "the pecuniary means of the consumer limiting the demand" from "variations in the quantity of the circulating medium," in his Evidence before the Select Committee of the House of Commons on Banks of Issue, 1840, qq. 3297 and 3298 [reproduced in his *History of Prices,* IV, 462].)   An appreciation of the degree of deterioration represented by Tooke's later position (though it is certain that Tooke himself would not have regarded it as a deterioration) may be obtained by comparing the implications of his twelfth and thirteenth theses, respectively, when taken in combination, or the implications of statements such as those in his Evidence of 1840, just cited, with the implications of a statement with respect to the relation between "demand" and the "quantity of money" such as is found, for example, in Tooke's earlier *Thoughts and Details,* II, 47 (cf. also II, 9 of the same work).

the *fact* of changes in what he himself called "general" prices.[20] For
what this means is that Tooke's argument with respect to the effect
upon the *structure* of money prices of what Marshall was later to call
"elasticity of demand" thereby becomes relevant to *any* discussion of
changes in that structure during periods of "general" price change,
whether this "general" price change is or is not alleged to be due to
monetary expansion or contraction.[21]

[20] For examples of Tooke's use of the expression "general prices," see
his *Thoughts and Details,* I, 7, 25; II, 2, 9–11, 13, 47; *Considerations on the
State of the Currency,* 2 n., 115; *Inquiry into the Currency Principle,* 68,
124; *History of Prices,* I, 127; II, 58, 323 n.; III, 276; IV, 174, 465. He
also spoke without hesitation of a "general rise of prices," a "general
improvement of prices," or a rise of "prices generally," on the one hand,
and, on the other, of a "general fall," a "general depression" of prices, a
"general tendency" to "lower prices" or "to a decline of prices," or of
prices falling "generally" (see, for example, *Thoughts,* II, 60; IV, 74, 77 f.;
Considerations, 51 f.; *History,* I, 119, 178, 188 f., 197, 362, 367; II, 10 n.,
12, 32, 193, 256, 267, 343; V, 341; cf. also Tooke's answers to questions 4111
and 5435 in his Evidence before the Committee on the Bank of England
Charter, 1832). To be sure, he did not, to my knowledge, speak of the
"general price level"—the nearest he came to such an expression being
"the general *range* of prices." (See, for example, his answer to question
3618 in his Evidence before the Select Committee of the House of Com-
mons on Banks of Issue, 1840. It may be added that while Tooke often
used the expressions "the range of prices" or "the scale of prices" in
contexts indicating that he had in mind what he called elsewhere "general"
prices [see, for example, *Thoughts and Details,* I, 4, 118, 178, 196; II, 6, 49;
III, 18; IV, 75 ff., 82; *Considerations,* 52 f.; *History,* I, 1, 3, 7, 329, 368;
II, 7, 116, 170, 183, 206, 214, 250, 252; V, 263, 341], he also spoke of the
"range" of the prices of individual commodities in such a way as to
divest the expression of the connotations associated with a "general" move-
ment in prices [see, for example, *Thoughts and Details,* I, 164; III, 35, 93,
133; IV, 62; *Considerations,* 30, 37; *History,* II, 152, 195, 200, 209, 226, 261;
V, 195, 219].) There can be no doubt, however, that Tooke, despite his
by no means altogether unwholesome distrust of index numbers (on which
see the Introduction by T. E. Gregory to Tooke's *History,* 14, 42), meant
by "general prices" what other writers have meant by the "general price
level." Evidence, moreover, that he had at least a glimpse of the issues
involved in the problem of the relation of movements in what he called
"general" prices to those movements of *individual* prices in which he was
so deeply interested—though he can hardly be said to have reached an
articulate and consistent solution of the problem—is provided by his use
of concepts such as "the *sum* of general prices" or "the aggregate of prices"
(*Thoughts and Details,* II, 10 f., 13; III, 6), and his relating of these con-
cepts to the "total of demand for commodities" (*ibid.,* II, 12), the latter
magnitude being itself related, by Tooke's "thirteenth thesis," to "the
aggregate of money prices" ("the only prices that can properly come under
the designation of general prices") through the link of "the aggregate of
money incomes" (*Inquiry into the Currency Principle,* 124; *History of
Prices,* III, 276). On the rôle of the concept of "the sum of prices" in
dealing with the problem indicated, see below, pp. 341 ff.

[21] It is of some importance to observe that at the time Tooke first

Indeed, the proof that this is so was provided, very shortly after the publication of the first volume of Tooke's *History,* by Sir John Lubbock.[22]

made his suggestion with respect to the effect upon the structure of money prices of what amounts to the factor of "elasticity of demand," he was quite explicit in recognizing that a given rise or fall in "general" prices *might* be due to monetary expansion or contraction. This was true even of his argument in the *Thoughts and Details,* which he himself, as we have seen (cf. above, p. 150, n. 19), later admitted was in some respects misleading; for he was at that time quite prepared to admit that, whatever the original cause of a rise in prices, an "increase in the amount of the circulating medium is . . . the occasion of accelerating the rate, and extending the range of the rise in prices," whereas a "contraction" of the circulation would "aggravate the fall of prices" (I, 89, 95; cf. also I, 156; III, 17, and IV, 76, of the same work). The emphasis in question was much more marked in his *Considerations on the State of the Currency,* which was characterized by Tooke himself as emphasizing "the degree in which the *currency* has been connected, in the relation of cause and effect, with the violent transitions between high and low prices" (p. 4 [italics Tooke's]; cf. also 31 f., 36, 44, 50 f., 53 f., 55, 62, 64, 67, 96, 101 f., 128 of the same work). It was still more striking in his Evidence before the Committee on the Bank of England Charter in 1832, in which Tooke insisted quite emphatically that "as a general proposition" he was "quite prepared to admit," and in fact had "never denied, that all other things being the same, an increase of the circulating medium would tend to produce a rise of prices, and *vice versa*" (q. 4019; cf. Tooke's answers to qq. 5439 and 5449, and also his answers to qq. 3818, 3821, 3825, 3832, 3845, 3849, 3881, 4012, 4013, 4090, 4091, 4102, 5454, 5469). In fact, even as late as 1838, the year in which the first two volumes of his *History of Prices* appeared, Tooke was willing to include in his argument passages in which the effect upon prices of monetary expansion and contraction was freely admitted. See, for example, his *History,* I, 250; II, 28, 164, 178 f., 202 n., 222, 235, 266 n., 268. Only two years later, however, Tooke was prepared not only to "doubt" whether changes in "the circulating medium" "operate upon prices at all," but even to repudiate formally the concessions he had made in his Evidence of 1832 to "the opinion . . . that there is a connection between the amount of bank notes in the hands of the public and the state of prices" (see Tooke's answers to qq. 3295, 3296, 3298, 3303, 3621, 3622 in his Evidence before the Select Committee of the House of Commons on Banks of Issue, 1840; reproduced in his *History of Prices,* IV, 462 f., 470).

[22] The case of Lubbock is cited because of his explicit references to Tooke in this connection (cf. the two notes immediately following). Lubbock, however, was not the only writer of Tooke's day who made use of an argument involving what amounts to the Marshallian elasticity of demand in connection with a discussion of the consequences of monetary expansion and contraction. See, for example, R. Torrens, *An Inquiry into the Practical Working of the Proposed Arrangements for the Renewal of the Charter of the Bank of England* (1844), 41, where he discussed the consequences of an expansion in the "circulation" in the light of the fact that "when the supply of an article so indispensably necessary as corn becomes deficient, its price is raised in a considerably greater proportion than that in which its quantity is diminished." It may be observed that

For Lubbock did not merely take over Tooke's use of the example, based on the table supposedly provided by Gregory King, which Tooke himself had borrowed from Davenant.[23] On the contrary, he went on to provide a formulation of which it must be said that, whatever its crudity otherwise, it made clear that the money price of any given commodity is what it is not only because of the special conditions of demand for and supply of that commodity, as these conditions might present themselves if monetary expansion and contraction were not taking place, but also because of what is happening in the way of monetary expansion and contraction.[24] Clearly, therefore, there is nothing new in the particular

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Torrens made no reference to Tooke on this particular point, despite the fact that the pamphlet as a whole was intended to be "A Refutation of the Fallacies Advanced by Mr. Tooke."

[23] Cf. Lubbock's *On Currency* (1840), 39, where Lubbock cited the page of Tooke's *History* on which reference was made to Davenant and King, in support of his own suggestion that the function associating a given change in the price (α) of a commodity with the relative increments of the "supply" (S) and the "consumption" (C) of the commodity (or, as Lubbock wrote it, "a function $\Delta S/S$, $\Delta C/C$") is "probably not linear." It is, of course, not to be supposed that Lubbock, a mathematician, identified linearity of the functions in question with the case of unitary elasticity of demand, or, as Tooke put it, with the case evidencing "a strict rule of proportion between a given defect of the harvest, and the probable rise of price" (*History*, I, 12). One must suppose, rather, that the point of Lubbock's citation of Tooke was to call attention to the phenomenon of elasticity of demand in general, and that he added the comment with respect to the nonlinearity of the function involved merely as an illustration of the possible complexity of the actual relations existing between changes in price and changes in the quantities supplied and "consumed." It is clear, at any rate, that while Lubbock's statement of the problem did not run explicitly in terms of a Cournot-Marshall demand function registering a given degree of "elasticity," the concept of such a function is necessarily implied in Lubbock's suggestion that the share in a given price change which is specifically attributable to a change, for example, in the supply of the commodity priced would depend upon the form of the function connecting the relative increments of supply ($\Delta S/S$) with the resultant change in the price of the commodity.

[24] For Lubbock's full formulation, see his *On Currency*, 39. The chief weakness of this formulation, quite apart from the unnecessary cumbersomeness of its representation of the simple fact of a change in the quantity of the circulating medium, is, of course, that it fails to make clear that the effect upon prices of a change in the quantity of money must itself be translated in all cases into changes in the conditions of "demand" and "supply" for particular commodities, and particularly into changes in the "money demand" for these commodities, whether or not these changes in the "money demand" result in a change in the *structure* of money prices (see below, pp. 304 ff.). As it stands, indeed, Lubbock's formulation tends to encourage the interpretation that it implies agreement with the host of writers who have suggested that the conditions of "demand" for a given commodity are unrelated to changes in the amount of the circulating medium. This interpretation was further encouraged by Lub-

modus vivendi between the "general" Theory of Value, on the one hand, and the Theory of Money and Prices, on the other, which is typified by the argument with respect to the effect upon the structure of money prices of the Marshallian elasticity of demand, in cases of monetary expansion and contraction and of "general" price change as well as in cases of monetary and "general" price stability. On the contrary, the examples here cited may be taken as evidence that this particular *modus vivendi* had been adopted in economic literature even before the position of the concept of "elasticity of demand" was itself consolidated within the corpus of "general" economic theory as a result of Marshall's own work on the subject.

II

KEYNES ON MARSHALLIAN ELASTICITY OF DEMAND AND THE STRUCTURE OF MONEY PRICES

There is perhaps no more striking feature of the "synthesis" of the "general" Theory of Value and the Theory of

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bock's statement that "the price α of any article may in fact be considered as a function of the supply and demand, of the quantity of money, etc." (*On Currency*, 38)—as if the "supply and demand" for commodities were quite unconnected with changes in the "quantity of money"! Yet if Lubbock is to be blamed for his exposition on this head, as Cairnes rightly blamed Newmarch, when the latter used a similar method of exposition (see Cairnes's *Essays in Political Economy*, 57 n.; cf. also pp. 5 f. of the same work, and see below, p. 271, n. 108), it is only fair to point out that a major purpose of Lubbock's formulation was to emphasize the fact that a complete explanation of the movements in money prices requires attention to *both* the effects of monetary expansion and contraction, on the one hand, and, on the other, to nonmonetary factors affecting "demand" and "supply," respectively. This much must be said also on behalf of the otherwise extremely naïve attempt of Italian economists of the eighteenth and early nineteenth centuries to combine the crude "quantity equation" of Henry Lloyd (cf. Volume I, 10) with an equally crude equation representing price as determined by "demand" and "supply." (See the extracts from Frisi [1772] and Fuoco [1827] reprinted by Fasiani in his "Note sui saggi economici di Francesco Fuoco," *loc. cit.*, 175, 273; though it should be added that Lubbock's formulation is certainly superior to that of these earlier writers, particularly by virtue of Lubbock's explicit, though hardly detailed, recognition of the existence of different functional relations between changes in "supply" and "demand," on the one hand, and changes in price, on the other, in the case of different commodities.) In further defense of Lubbock, moreover, it must be remembered that his pamphlet appeared in the very year in which Tooke began to express himself most dogmatically with respect to the absence of *any* "connection between the amount of bank notes in the hands of the public and the state of prices." In this connection, see Lubbock's comments on what "Mr. Tooke admits," in the former's *On Currency*, 37 f.; and on Tooke's recantation of these "admissions" in 1840, the year that Lubbock's pamphlet appeared, see above, p. 152, n. 21.

Money and Prices presented in Keynes's *General Theory* than the fact that nowhere in that work does there appear explicitly the particular use of the concept of "elasticity of demand" just discussed: namely, the use of the *Marshallian* "elasticity of demand," or its equivalent, as a device helpful in accounting for changes in the structure of money prices. This fact would call for comment in any case, in view of Mr. Keynes's charge that received monetary theory has made virtually no use of the "homely but intelligible concept" of elasticity of demand, *as that concept appears in the "general" Theory of Value.*[25] It calls for particular comment, however, by reason of the light which the omission throws upon the heuristic value of the particular "synthesis" of the two bodies of doctrine presented in the *General Theory,* as compared with those attempts at synthesis which were already available in economic literature.

In one sense, the omission calls attention to a broader characteristic of the argument of the *General Theory* which would justify the suggestion that the Theory of Prices presented in that work is in this respect inferior not only to what was available in the writings of authors other than Mr. Keynes at the time the *General Theory* was published, but also to the Theory of Prices presented in Mr. Keynes's earlier *Treatise on Money.* For whatever else may be said of the latter, it did contain a discussion of the forces affecting the *structure* of money prices, in the form of a discussion of the meaning and the consequences of the concept which at that time Mr. Keynes designated as a "Plurality of Price Levels." [26]

In the *General Theory,* on the other hand, we find, to be sure, a preliminary denunciation of "the concept of the general price level" as a concept that is "very unsatisfactory for the purposes of a causal analysis, which ought to be exact," and as one whose "proper place" lies, at best, "within the field of historical and statistical description"; and we find also a pronouncement to the effect that "our precision will be a mock precision if we try to use such partly vague and non-quantitative concepts as the basis of a quantitative analysis." [27]   Actually, however, the

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[25] On Mr. Keynes's "elasticity of effective demand," and its relation to the "general" Theory of Value, on the one hand, and the devices of monetary theory, on the other, see below, Chapter Thirteen (pp. 676 ff.).

[26] See especially Chaps. V and VII of the *Treatise;* and cf. Volume I, Chapter Seventeen (especially pp. 500 ff.), of the present work.

[27] *General Theory,* 39 f.; cf. also p. 43 of the same work, where it is proposed to limit "the use of vague concepts, such as . . . the general level of prices, to the occasions when we are attempting some historical comparison which is within certain (perhaps fairly wide) limits avowedly unprecise and approximate."

"money prices" that appear in the chapter of the *General Theory* specifically entitled "The Theory of Prices" are "money prices" in a sense which at best would make the latter expression equivalent to what, in his *Treatise*, Mr. Keynes designated as the "price level of output as a whole"—in other words, what a very large number of earlier writers had understood by "the general price level," the very concept that Mr. Keynes himself had rejected as "very unsatisfactory for the purposes of a causal analysis." [28] It is clear, therefore, that so far as this part of the argument of the *General Theory* is concerned, Mr. Keynes has not fulfilled his promise to show that "one can get on much better without" a concept such as that of the "general price level." [29] It is equally clear, however, that he has not done justice to the nature of the problems with which the concept of a "plurality of price levels" was intended to deal.[30]

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[28] As it happens, Mr. Keynes actually makes use of the *expression* "the general price-level" in this connection. See, for example, the *General Theory*, 294 f., 300. It is not surprising, therefore, that more than one critic of the *General Theory* should have commented upon Mr. Keynes's primary concern, in that work, with the *"general* price level" at the expense of a concern with the *structure* of money prices, despite his formal rejection of the concept of a "general price level" in the passages cited in the preceding note. See, for example, Lauchlin Currie, "Some Theoretical and Practical Implications of J. M. Keynes' General Theory," in *The Economic Doctrines of John Maynard Keynes, A Series of Papers Presented at a Symposium Conducted by the National Industrial Conference Board* (1938), 17; Saulnier, *Contemporary Monetary Theory*, 355, 357 f.; E. Lederer, "Industrial Fluctuations and Wage Policy," *International Labour Review*, XXXIX (1939), 26; and contrast the type of defense of Mr. Keynes's treatment of the problem which is cited below, p. 157, n. 32.

[29] Cf. the *General Theory*, 39. It should be clear that a demonstration of the possibility of "getting along without" the concept of a "general price level" necessarily involves at least two separate steps: (1) a demonstration that there are no significant problems in monetary theory for which the concept of a "general" price level is both valid and useful; and (2) the provision of an analytical alternative to the concept of a "general" price level, in those cases in which it is permissible to conceive the problem in terms of "alternatives." From what is said in the text, it is clear that Mr. Keynes's own *practice* has not been such as to show that one "can get along without" the concept of a "general" price level; on the contrary, it has been to make repeated use of just such a concept (cf. the references given in the preceding note) without, at the same time, providing an explicit argument in defense of the concept. (On the nature of such an argument, see what is said below, pp. 330 ff.) From what is said in the text it should be clear, also, that in the *General Theory* (in contrast with the *Treatise*), Mr. Keynes has neither presented an "alternative" to the concept of a "general" price level (in the form, say, of the concept of a *"plurality* of price levels"), nor indicated any reasons for rejecting such an "alternative." On the true relation between the concept of a "general" price level, on the one hand, and the concept of a "plurality of price levels," on the other, see what is said below, pp. 319 ff., 330 ff.

[30] This is not to deny, of course, that there are isolated instances in

Surely it is not unfair to suggest that the effect of both omissions has been to leave Mr. Keynes's treatment of the relevant problems in a state inferior not only to that which may properly be said to have characterized the best of received doctrine upon the subject, but also to that which may be said to have characterized Mr. Keynes's earlier *Treatise*.[31] This is obviously relevant to a judgment of the adequacy of the argument of the *General Theory* when it is judged, as Mr. Keynes wishes it to be judged, as a Theory of Output as a Whole, for which the problem of the structure of money prices has rightly been regarded as a matter of crucial significance.[32] Yet it is of more importance, for our

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the *General Theory* of a type of analysis which can be shown to be *relevant to* the problem of the determination of the structure of money prices. See, for example, what is said on this matter below, p. 317, n. 207, and p. 547, n. 52. The point made here is merely that there is nothing in the *General Theory* corresponding to the frontal attack upon the problems envisaged by the concept of a "plurality of price levels" which one finds even in Keynes's *Treatise*. Cf. also the following note.

[31] To the possible suggestion that Mr. Keynes, having dealt with the problem of a "plurality of price levels" in his *Treatise*, found it unnecessary to repeat the argument in his *General Theory*, it may be replied: (1) The force of this suggestion is greatly weakened in the light of the confusion engendered by Mr. Keynes's failure to indicate with all possible explicitness just what parts of the argument of the *Treatise*, in both its critical and positive aspects, he regards as still valid, and which he does not (cf. Volume I, 30 ff., 138 f., of the present work); this confusion can hardly be said to have been lessened by the insistence of Mr. Keynes's followers that a detailed concern with the argument of the *Treatise*, rather than with that of the *General Theory*, must be regarded as "otiose," since Mr. Keynes is alleged to have "abandoned" the positions adopted in the *Treatise* (cf. B. P. Whale in *Economica* for February, 1940, p. 89, and N. Kaldor in the *Economic Journal* for September, 1939, p. 496). (2) Almost no use is made, in the *General Theory*, of certain parts of the argument of the *Treatise* which provided the basis for the particular set of "plural" price levels presented in that work—for example, its argument with respect to the effect of changes in the rate of interest upon the structure of money prices when interest is regarded as a "capitalization factor" (cf. Volume I, 232 ff., of the present work)—with the result that we are left in uncertainty as to whether Mr. Keynes would or would not continue to sponsor the particular set of price levels which he had sponsored in the *Treatise*. (3) The particular set of "plural" price levels presented in the *Treatise* had itself been under severe attack, not only on the ground of uncertainty as to the precise meaning of some of these "price levels," but also because of Mr. Keynes's failure to provide a genuinely satisfactory treatment of the nature of the relations of his individual "price levels" to *one another*, as well as to changes in the dimensions of the stream of *aggregate money expenditure* (see Volume I, 508 ff., 518 ff., 525 ff., of the present work; and on the relation of this argument to the problem with which we are here concerned, see below, pp. 323 ff., 595 ff., 601 ff.).

[32] It is of interest to observe that it has been so regarded precisely by defenders of Mr. Keynes's work. See, for example, A. F. W. Plumptre, in the *Canadian Journal of Economics and Political Science*, V (1939), 265.

present purpose, to stress a further fact: namely, that in *neither* the *Treatise* nor the *General Theory* was Mr. Keynes's statement of the problem such as to lead directly to the type of "synthesis" between the "general" Theory of Value, on the one hand, and the Theory of Money and Prices, on the other, which is outlined in the chapters that follow, and which is avowedly based upon elements already lying at hand in

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All that need be said here, therefore, is that the defense of Mr. Keynes would be much stronger (1) if a distinction were made between the argument of the *Treatise,* on the one hand, and that of the *General Theory,* on the other, with respect to the importance of *"relative* alterations of prices" in "explaining the trade cycle" (Plumptre, *loc. cit.*); and (2) if the defense of Mr. Keynes were not accompanied by a belaboring of those commentators on Keynes's *Treatise* who accepted wholeheartedly its emphasis on the necessity for dealing with the causes and consequences of those "relative alterations of prices" which will be reflected in the movements, with respect to one another, of the individual "price levels" making up the "plurality" of price levels—even if these commentators did not accept the particular devices proposed by Mr. Keynes in his *Treatise* for dealing with the problem (see, for example, Volume I, Chapter Seventeen of the present work, especially pp. 495 ff.). To the suggestion, moreover, that "no attempt" had been made to show that "piece-meal equations" of the general Fisherine form, each leading to one of a set of "plural" price levels, "are the best available means for dealing with the problems for which they are designed" (Plumptre, *loc. cit.*), it may be replied: (1) One of the purposes for which these "piece-meal" equations are designed (a purpose which is certainly relevant "to the problems of the world in which we live"; cf. Plumptre, *loc. cit.*) is precisely to account for changes in the structure of money prices (see Volume I, 485 ff., 496 ff., 571, and cf. below, pp. 319 ff., 562 ff., 601 ff., 623 ff.). (2) A definite attempt was made in Volume I of the present work to show that it is precisely the use of such equations, *in combination with* the use of a Fisherine equation of the "total transactions" type, that would have made unnecessary one of the most serious sources of misunderstanding concerning the issues involved in a particular problem with respect to the structure of money prices which was raised by Mr. Keynes in his *Treatise:* namely, that represented by his argument on the relation between his two principal "price levels"—themselves, it may be observed, implying the use of "piece-meal equations," whether of the Fisherine form or the form of the first Fundamental Equation of Keynes's *Treatise* (see Volume I, 525 ff., of the present work). (3) So far as the problem of the determination of the structure of money prices is concerned, a definite attempt was likewise made to show that Mr. Keynes had certainly not demonstrated that "his own alternative theoretical structure was better adapted to the world in which we live," since fatal limitations attach to any claim, on behalf of the Fundamental Equations of the *Treatise,* to account for the causes and consequences of those particular "relative alterations of prices" which are represented by alterations of *costs* relative to *selling prices,* whenever one claims simultaneously, as Mr. Keynes did in his *Treatise,* that these Fundamental Equations "exhibit the causal process by which the price-level is determined" (Keynes, *Treatise,* I, 133. See Volume I of the present work, 124 ff., 273 f., 279).

the best of received doctrine upon our subject. For, in his *Treatise*, Mr. Keynes made no attempt to relate his discussion of the concept of a "plurality of price levels" to the Marshallian demand and supply curves of the "general" Theory of Value.[33] And in his *General Theory*, such an attempt was ruled out in advance by Mr. Keynes's lack of serious concern with the concept of a "plurality of price levels," on the one hand, and, on the other, his failure to make explicit use of the Marshallian demand schedules for the products of particular industries, with their special property of "elasticity." The latter omission, in particular, it is here argued, is especially significant for any judgment as

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[33] The argument of the *Treatise* in this respect should be contrasted with the argument not only of those sponsors of an "income approach" to the Theory of Prices who had made much of its relation to the use of the "demand and supply curves" of the "general" Theory of Value (see Volume I, 491 f., of the present work), but also of writers such as Fisher, whose use of devices such as his Quantity Equations has been regarded by Mr. Keynes and his followers as typical of those who have allowed a serious gap to exist between their "general" Theories of Value, on the one hand, and their Theories of Money and Prices, on the other. In this connection, see above, pp. 106 f., and the references given in n. 38 thereto. It is of some importance to emphasize that the comment made in the text is not intended to be an argument against the validity and the usefulness of the substance of the argument of the *Treatise* with respect to the nature of the forces determining the structure of money prices, when this argument is regarded as containing materials to be *embodied in* an effective "synthesis" of the "general" Theory of Value, on the one hand, and monetary theory, on the other. We have seen, for example (cf. above, p. 94), that the introduction of the element of "capitalization" as a factor affecting money prices itself represents an attempt to effect a substantive synthesis between the two bodies of doctrine. It is also true, moreover—and it is of the utmost importance to observe that it *is* true—that *all* the specific cases adduced by Mr. Keynes in support of the argument for the use of a "plurality of price levels" (on which see Volume I, 501 ff., of the present work) are *capable* of translation in terms of concepts of the "general" Theory of Value, and, in particular, of that part of the "general" Theory of Value which is represented by demand and supply curves of the Marshallian type. One may, indeed, point out that the second group of causes for differential price change adduced by Mr. Keynes (namely, those due to elements of rigidity in the price structure) is certainly not exhausted by his reference to the "many kinds of money-contracts, money-customs and money-understandings fixed over periods of time" (cf. Volume I, 504). The element of monopoly, for example, introduces a special and highly complicated series of rigidities of its own. The point made here is merely that Mr. Keynes himself gave no indication, in his *Treatise*, of an awareness of the fact that a translation of these cases in terms of the "supply" and "demand" schedules of the general Theory of Value was either necessary or desirable; and the further point made here is that, as a result of this circumstance, the *Treatise* itself failed to point the way to the particular type of "synthesis" of the "general" Theory of Value with monetary theory which is outlined in the chapters that follow.

to the heuristic value of the particular "synthesis" between monetary theory, on the one hand, and "general" Value Theory, on the other, which is presented in Keynes's *General Theory*, as compared with those attempts at synthesis which were already available in economic literature.[34]

For Mr. Keynes's failure to introduce the concept of "elasticity of demand," in the Marshallian sense of the term, into the discussion of the problem of the determination of the level of Output as a Whole by way of its effect upon the structure of money prices was not due to mere oversight. On the contrary, the argument of the *General Theory* is such as to leave no doubt that Mr. Keynes has adopted his position as the result of a firm conviction that "the demand schedules for particular industries," in the Marshallian sense of a "demand schedule," are simply incapable of being applied to the problem in hand.

The "problem in hand," the reader is reminded, is that of establishing the nature of the rôle played by demand schedules for particular commodities of the Marshallian type, and by their degree of "elasticity," in the determination of the structure of money prices. It is unfortunately characteristic of the oblique treatment accorded to the latter problem in the *General Theory* that, instead of devoting to it the frontal attack which its importance clearly demands, Mr. Keynes attempted to dispose of it in the course of an argument with respect to the efficacy of cuts in wage rates in affecting the level of "employment as a whole." [35] If

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[34] It should be clear also that this conclusion bears directly upon the validity of the suggestion that "the Keynesian approach" is to be included among those whose superiority to formulations making explicit use of the framework provided by Quantity Equations of the Fisherine type lies precisely in the alleged fact that the latter formulations necessarily run "in mechanical terms," whereas the former run in terms of the "decisions" of economizing individuals and "all of the factors which underlie those decisions"—in other words, the type of analysis provided by the apparatus of demand and supply curves of modern value theory. Cf. A. H. Hansen, in the *American Economic Review*, XXVIII (1938), 752; and on the general question of the meaning of a "mechanical" approach both to the problem of the determination of money prices and to a description of a functioning economic process generally, see what is said below, pp. 471 ff.

[35] See the *General Theory*, 258 ff. Mr. Keynes's practice in this respect has been closely followed by others. See, for example, R. A. Lester, "Political Economy *versus* Individualistic Economics," *American Economic Review*, XXVIII (1938), 59 f. The title of the article just cited, indeed, with the implication of antithesis which is conveyed by its use of the term *"versus,"* is itself symptomatic of the breadth of the issues raised by the procedure so lightly adopted by Mr. Keynes. See what is said on this matter below, pp. 462 ff., 498 ff.

Mr. Keynes's exposition, and that of others who have accepted the part of his argument under discussion here, were such as to make clear that the protest was being directed against a *misuse* of the demand schedules of the "general" Theory of Value, the issues involved would obviously be quite different from what they are. Unfortunately, however, this is precisely what Mr. Keynes's argument, as he has stated it, does not make clear. On the contrary, his argument consists *not only* of a statement of the limitations to which "the demand schedules for particular industries" are subject, but also of the conclusion that these limitations mean that any variant of "classical theory" which makes use of these "demand schedules for particular industries" "has nothing to offer when it is applied to the problem of what determines the volume of actual employment as a whole." [36] It is clear that no answer to the latter question can be provided by simply assuming that the only way in which "classical theory" would bring the type of analytical device represented by the "demand schedules for particular industries" to bear upon the problem would be by means of a mechanical extension of "its conclusions in respect of a particular industry to industry as a whole." [37] On the contrary, the answer to the question indicated depends entirely upon whether or not the *structure of money prices* has anything to offer in the solution of the problem. For, as we have seen, it is precisely with the "structure" of *relative* prices that these demand schedules of the "general" Theory of Value are concerned.[38] As we have also seen, the relevance of the *structure* of money prices to the problem of variations in the level of output and employment "as a whole" is not only admitted, but insisted upon emphatically by some defenders of Mr. Keynes's work.[39] There need be no apology, therefore, for leaving the detailed demonstration and illustration of its relevance until a later work which will be concerned precisely with the rôle of money in the determination of the level of output and employment as a whole. The point made here is that, given the relevance of the structure of money prices to the latter problem, Mr. Keynes's combination of a sweeping insistence upon the supposedly fatal limitations upon the use of "demand schedules for particular industries" and his own failure to make any use of them in his analysis makes it necessary to conclude that he believes that the limitations which he stresses apply also to the problem with which we are here concerned: namely, the determination of the structure of money prices.

Clearly, this is a conclusion which, if it could be established, would certainly have to be regarded as "revolutionary"; for it would amount, in effect, to a condemnation of "general" value theory of the Marshallian type, including

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[36] *General Theory,* 260.

[37] *General Theory,* 260.

[38] Cf. above, pp. 137 f.

[39] Cf. above, p. 157, n. 32.

the Marshallian concept of "elasticity of demand," as completely sterile and useless for the purpose of explaining events in the real world. It is in one sense unfortunate, therefore, that Mr. Keynes himself has not stated this conclusion explicitly, instead of allowing it to be deduced from (1) his insistence upon the fatal limitations supposedly attaching to concepts such as the "demand schedules for particular industries" and (2) his refusal to make explicit use of such concepts in his own analysis. For it is difficult to believe that so genuinely "revolutionary" a proposition would not have attracted more notice than it has. In fact, of course, and despite Mr. Keynes's own precept and example, most of Mr. Keynes's followers continue to make use of these "demand schedules" in their analysis; indeed, at least one of them—Mr. R. F. Harrod—has attempted to make it a central element in his own apparatus for accounting for events alleged to be characteristic of the trade cycle.[40]

In this respect, therefore, Mr. Keynes's "revolution" has not been so destructive as it might have been. In other respects, however, the general neglect of what is in many ways the aspect of Mr. Keynes's argument which carries the most "revolutionary" implications has been unfortunate in the extreme. For one thing, it may be argued that, in some of the cases in which Mr. Keynes's followers have continued to make use of the concept of "elasticity of demand," their usage has been such as to raise the question whether they might not have profited from a careful consideration of those long-recognized limitations upon the use of the Marshallian concept which Mr. Keynes's argument does

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[40] See Harrod's discussion of the proposition which he has labeled "the Law of Diminishing Elasticity of Demand" in his *The Trade Cycle* (1936), 17 ff., 21 f., 30 f., 51, 76, 85 ff., 92, 115 (cf. also his earlier review article, "Professor Pigou's Theory of Unemployment," *Economic Journal*, XLIV [1934], 28 f.). For examples of a continued use of the Marshallian "elasticity of demand" by other members of the Keynesian group (though it is true that here the usage *antedates* the publication of the *General Theory*), see R. F. Kahn, "The Relation of Home Investment to Unemployment," *Economic Journal*, XLI (1931), 186 f.; J. Robinson, *The Economics of Imperfect Competition* (1933), 60 ff., 71, 73, 313 n.; and N. Kaldor, "Wage Subsidies as a Remedy for Unemployment," *Journal of Political Economy*, XLIV (1936), 724, n. 7.

little more than stress anew.[41] For our present purpose,
however, it is more important to stress a further fact:
namely, that Mr. Keynes's attack, though it can certainly
be shown to be without the consequences which he attributes
to it, itself points to the necessity for *constructive work in
other directions.*[42] Of these other directions, one, in par-
ticular, will be found to be crucial: namely, that which is
suggested when one undertakes to establish, with all pos-
sible precision, the exact nature of *the relation between the
demand curves of the "general" Theory of Value, with their
property of "elasticity," on the one hand,* and, on the other
hand, those *"stream equations" of the Theory of Money and
Prices,* with their *"velocities of circulation, . . . volumes
of transactions . . . et hoc genus omne,"* that have been
so cavalierly dismissed not only by Mr. Keynes, but also by
those of his followers who, unlike him, continue to cling
unrepentantly to the Marshallian "elasticity of demand"
as a weapon for accounting for the determination of money
prices.

It is the latter problem with which we shall deal in the
chapters that follow, and which will be found to have ramifi-

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[41] The usages in question represent applications of the concept of "elas-
ticity of demand" to *groups* of commodities or factors: for example, the
"elasticity of demand . . . for *consumption goods,*" in the case of Mr.
Kahn, and the "elasticity of demand for labor in *industry as a whole,*"
in the case of Mr. Kaldor (cf. the references given in the preceding note).
Such usages are to be regarded as analytically dangerous not only because
they serve to obscure significant *differences* in the elasticity of demand
for particular members of the groups in question, but also, and more im-
portantly, because they do not pay sufficient attention to the familiar
proposition, advanced anew by Mr. Keynes, that *intra*-group elasticities
may themselves change appreciably as the result of changes in the prices of
other members of the same group. On the general point involved, see the
comments by Schumpeter, *Business Cycles,* 475, n. 2.

[42] A necessary preliminary to this work of further construction is, of
course, the obtaining of clarity with respect to the *nature* of the demand
schedules of the "general" Theory of Value, and particularly, as we shall
see, a clear appreciation of their *"ex ante"* character. On this matter also
certain of Mr. Keynes's followers could have profited from a greater
willingness to examine the precise implications both of Mr. Keynes's at-
tack upon the usefulness of these demand schedules and of the nature of
the argument by which this attack can be met. See, for example, what
is said below, p. 194, nn. 107 and 108, and pp. 195 f., nn. 113 and 114, with
respect to the treatment of the demand schedules of the "general" Theory
of Value by Mrs. Robinson and Mr. Kaldor, respectively.

cations that were naturally hidden from those who have rejected either the Marshallian demand and supply schedules for the products of particular industries, or "stream" equations of the type indicated, or both. In the present chapter, our task is a different one. In section I of this chapter, it was shown that if, by the "homely but intelligible concept" of "elasticity of demand," we mean the *Marshallian* "elasticity of demand" for particular commodities, "economists" have emphatically not been guilty of ignoring this concept when they have passed from the "general" Theory of Value to the Theory of Money and Prices. We have now to show that in refusing to ignore it, as Mr. Keynes himself has in effect ignored it in his *General Theory,* they acted wisely. In particular, it will be shown, first, that the specific reasons advanced in Keynes's *General Theory* against demand schedules of the Marshallian type for the purpose in hand, instead of being new in substance, were explicitly taken into account by the principal sponsors of analytical devices of the type of the Marshallian demand schedules for particular commodities, with their special property of elasticity. It will be shown, second, that the limitations upon the use of these demand schedules pointed out by their principal sponsors, as well as by their earlier detractors, and now reintroduced by Mr. Keynes, are not such as to invalidate their use for the purpose of accounting for changes in the structure of money prices, and therefore in the level and structure of output as a whole.

III

MARSHALLIAN ELASTICITY OF DEMAND AND "THE DEMAND AND SUPPLY SCHEDULES OF OTHER INDUSTRIES"

Mr. Keynes's own argument for rejecting any argument making use of "the demand schedules for particular industries" in order to account for changes in the level of output as a whole by way of changes in the structure of money prices is summed up by his proposition that these "demand schedules for particular industries *can only be constructed upon some fixed assumptions [1] as to the nature of the demand and supply schedules of other industries and*

[2] *as to the amount of the aggregate effective demand."* [43]
The two difficulties thus indicated have sometimes been
discussed in the literature on the "general" Theory of Value
as if they were merely two facets of the same problem.[44]
We shall see, on the other hand, that much is to be said for
separating the two.[45]   In what follows, therefore, they will
be discussed separately in the first instance.   In both cases,
however, it will be shown that (1) the difficulty in question

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[43] *General Theory,* 259.

[44] The practice indicated is apparently to be traced to an interpretation
of Marshall's assumption of a constancy of the "marginal utility of money
income" as relating *not only* to the effects of changes in the prices of
commodities other than the particular commodity taken for examination,
but also to the effects of changes in the level of money income. See, for
example, Hicks, *Value and Capital,* 26 f.; also M. F. W. Joseph, "The Ex-
cess Burden of Indirect Taxation," *Review of Economic Studies,* VI (1939),
where (p. 226) "the assumption of constant marginal utility of money" is
identified with the condition "that income elasticity of demand is zero,"
and the latter proposition is in turn translated (p. 228) into the assump-
tion that "only a negligible proportion of income is spent on the [that is,
on any *one*] commodity." There can be little doubt that the use of the
concept of a "marginal utility of income" in connection with both types
of propositions has had a long history (see, for example, Jevons, *Theory
of Political Economy,* 148, 152, 159). It can be argued, to be sure, that
Marshall's own treatment of the problem was such as to keep the two
aspects of the concept of a "constant marginal utility of money" with
which we are here concerned more nearly separate than a number of sub-
sequent writers have succeeded in doing: and this despite the fact that
he has sometimes been gently chided for "having generally neglected the
income side" of the problem of "the relations between demand, price,
and income"—with the result that "the relations of demand, price, and
income" were not made quite so "clear" as they might have been (so
Hicks, *Value and Capital,* 27).   Compare, for example, the passages from
Marshall cited below, pp. 168 ff., with those cited below, pp. 210 ff.   Yet
there can be little doubt that even Marshall's use of the concept of a
constant "marginal utility of money" was of such a portmanteau character
as to raise the question whether the problems it summarized are not better
handled by devices better calculated to separate the analytical issues
involved. It must be remembered, for example, that in some contexts the
"constant marginal utility of money" had primary reference, not to changes
in the level of *income,* but to changes in the utility of a "cash balance"
as such. See, for example, Marshall's *Principles,* 335 f., especially the
footnote on p. 335.

[45] See especially, in this connection, what is said below, pp. 298 ff., with
respect to the use of the phrase "the income effect" to describe *both* the
effects of changes in "real income" which follow from a change in the
level and the structure of *money prices,* on the one hand, and, on the
other, those effects which would follow from a change in the amount and
distribution of *money income.*

has been recognized from the very start by sponsors of the use of devices such as "the demand and supply schedules" for particular industries and firms, as well as by those who have expressed a distrust of such devices; and (2) in neither case is the difficulty such as to invalidate the Marshallian concept of "elasticity of demand" as a device helping to explain changes in the structure of money prices, whether we consider a period of "general" price and monetary stability or a period in which both "general" prices and the level of output as a whole are changing.

We may begin with the first of Mr. Keynes's two principal strictures upon the use of demand schedules of the Marshallian type, with their special property of elasticity, in accounting for changes in the structure of money prices. The reasons for rejecting this first stricture as irrelevant to the problem in hand may be stated in the form of a series of counterpropositions:

1. To say that the "demand schedules for particular industries can only be constructed on some fixed assumption as to the nature of the demand and supply schedules of other industries" is to say nothing more than what has been said countless times by those who have insisted that what has come to be called "partial equilibrium" analysis *is continually subject to the limitations imposed upon it by "general equilibrium" analysis of the Walrasian type.*[46]

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[46] This proposition is so elementary and so generally familiar that it is unnecessary here to do more than to point to a simple fact of doctrinal history: namely, that the proposition was emphasized by English commentators on the methodological implications of demand curves of the Cournot-Marshall type at a time when only the beginning of the Marshallian conquest was being witnessed—in the years, that is to say, immediately following the publication of Marshall's *Principles* itself. See, for example, the remarks of Edgeworth in his articles on "Curves" and "Demand Curves" in Palgrave's *Dictionary of Political Economy* (1894), I, 474, 543. What was really added, therefore, by the criticism advanced by members of the "Lausanne school" (see the references given by Schultz, *Statistical Laws of Demand and Supply,* 25, n. 28, and the same author's *The Theory and Measurement of Demand,* 8, n. 7) to what Edgeworth, for example, had emphasized, was an insistence upon the revelance, for the problem in hand, of the formulation of *Walras,* to whom Edgeworth referred only obliquely in his article on "Demand Curves" (p. 543; see Walras's *Eléments,* 484 ff., in addition to the references to Walras given by Schultz in the passages cited above; and cf. also Wicksteed, *The Common Sense of Political Econ-*

2. This limitation was recognized with perfect clarity by Cournot and Marshall, the chief sponsors of the particular analytical device that is represented by "the demand schedules for particular industries," and therefore of the property of those schedules which is represented by the Marshallian "elasticity of demand."

No one, indeed, could have been more explicit than Cournot in emphasizing the fact that, in his discussion of "how, for each commodity by itself, the law of demand, in connection with the conditions of production of that commodity, determines the price of it," he had "considered as given and invariable the prices of other commodities," whereas "in reality the economic system is a whole of which all the parts are connected and react on each other"; so that "it seems, therefore, as if, for a complete and rigorous solution of the problems relative to some parts of the economic system, it were indispensable to take the entire system into consideration." [47] As is well known, Cournot himself regarded the latter procedure as surpassing "the powers of mathematical analysis." [48] He certainly did not conclude from this, however, that either this fact itself, or the economic complexities to which it called attention, made devices such as his "law of demand" (or, as we should say, the demand schedule) utterly useless in analysis of the problems of the real world. On the contrary, he himself proposed to "avoid this difficulty" by maintaining "a certain kind of approximation" in order to effect "a useful analysis of the most general questions which this subject brings up." [49] And one of the questions with which we are here con-

omy, 475 ff., 479, 484). It may not be out of place, therefore, to recall that Walras's own attempt to use geometrical forms in the presentation of his "general" theory of pricing called forth from Irving Fisher the same kind of warning with respect to the limitations upon the use of plane curves for this purpose that has bulked so large in criticism, by members of the "Lausanne school," of analysis of the "partial equilibrium" type. See Fisher's "Introductory Note" to his translation of Walras's "Geometrical Theory of the Determination of Prices," *Annals of the American Academy of Political and Social Science*, III (1892), 46 (cf. the comment by W. Jaffé in *Economica*, New Series, V [1938], 475), and also the criticism of Walras by J. Bertrand cited below, p. 171, n. 56. This episode, while not without its own irony in the light of the use of Walras's name by members of the Lausanne school as a club with which to belabor "partial equilibrium" analysis in all its forms, is itself significant for an understanding of Walras's own attitude toward the usefulness of the devices of "partial equilibrium" analysis. See especially, in this connection, the reference to Walras given at the end of n. 55 to p. 171, below.

[47] See Cournot's *Researches*, 127 f. (cf. his *Principes*, 263 ff.) and the comment on this aspect of Cournot's argument by G. Lutfalla on pp. 222 f. of the latter's edition of Cournot's *Recherches* (1938). Cf. also the reference to Cournot given below, p. 169, n. 52.

[48] *Researches*, 127. Cf. the *Principes*, 264.

[49] See the *Researches*, 127 f. (*Principes*, 264 f.).

cerned is precisely whether the fact that a "certain kind of approxima-
tion" may be involved in the use of demand schedules of the Cournot-
Marshall type does or does not invalidate the use of the Cournot-Marshall
concept of "elasticity of demand" in accounting for changes in the
structure of money prices during periods of "general" price change, as
well as during periods of "general" price stability.

The case of Marshall is still more striking, by virtue of the fact that
he faced the difficulty in question in not merely one way, but in several
ways. His formal recognition of the problem is indicated, of course, by
his introduction of the specific assumption that, for purposes of "partial
equilibrium" analysis, the "marginal utility of money" is assumed to be
constant; for one of the things meant by this proposition was simply
that the "demand schedule" for a given commodity is subject to given
assumptions with respect to the stability of conditions affecting the
demand for other commodities, and that changes in these other condi-
tions may very well require a change in the form of the demand curve
for the particular commodity under examination.[50] There could be no
greater misrepresentation of the substance of Marshall's work, however,
than that involved in the suggestion that he was content, through the
use of this device, simply to *avoid* those problems deriving from the
interdependence of economic variables which it was one of the historic
merits of the Walrasian system to have emphasized so clearly.[51]

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[50] See, for example, Marshall's *Principles,* 132 f., and Note VI of the
Mathematical Appendix (p. 842). Of the references given by Marshall to
commentators on the aspect of his own system which is here under dis-
cussion, particular attention should be called to Barone. See especially
the latter's article "Sulla 'Consumers' Rent,' " *Giornale degli economisti,*
Second Series, IX (1894), 216 ff., and the second of the "Mathematical
Notes" appended thereto (p. 221). It will, of course, be recalled that the
assumption of a constant (or not appreciably varying) marginal utility of
money had already been used by Jevons. See the latter's *Theory of Politi-
cal Economy,* 114, 148, 151 f. On further connotations of the phrase "the
marginal utility of money," even as used by Marshall, see what is said
above, p. 165, n. 44, and the forward references there given.

[51] It should hardly be necessary, in these days, to argue at length on
behalf of the formal compatibility of the Marshallian and the Walrasian
statements of the general problem of pricing. Cf., for example, Schultz,
The Theory and Measurement of Demand, 9. It *is* of some importance,
however, to observe that very much less than justice is done to the positive
potentialities of the Walrasian "system" if one argues *only* that it provided
a kind of check on the results obtained by analysis of the "partial equi-
librium" type: although it is true that this was the principal inference
drawn by most of the members of the "Lausanne school" themselves (see
below, p. 170, n. 55). On the contrary, it is the conception of the Walrasian
"system" as a "circular flow" of *money payments* that has most sig-
nificance for the purposes of further construction of a *positive* kind within
the range of problems with which this work is concerned (cf. the comments
above, pp. 111 ff., on Schumpeter's treatment of the Walrasian "circular
flow"). For it is this aspect of the Walrasian "system" (1) for which there
is no explicit analogy in the Marshallian "system," and (2) which is most

It was Marshall himself, for example, who pointed out that the relation of the concept of elasticity of demand to the existence of *substitutes* for the particular commodity whose "elasticity of demand" is under examination represents a recognition of the fact that "the demand schedules for particular industries" will be affected by changes in "the demand and supply schedules of other industries." [52] Nor is this the only case, within the Marshallian system, in which the "demand schedule for a particular industry" was made dependent upon the "demand and supply schedules of other industries" by the very terms of the construction itself. There are, for example, the cases of joint demand, joint supply, composite demand, and composite supply, the very statement of which involves an explicit recognition of the fact that the demand or supply schedule for any commodity coming under any one of the cases indicated will be what it is as the result of the demand and supply schedules of the complementary or competing commodities, and will therefore be subject to change whenever any of these other demand and supply schedules change.[53] Indeed, one has only to study the details of Marshall's own exposition (such as that presented, for example, in his Mathematical Note XXI, in which the problem was described as involving a demonstration that "our abstract theory has just as many equations as it has unknowns, neither more nor less") to recognize that he himself was perfectly aware that the cases indicated were in reality nothing but examples of that general interdependence of economic

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pregnant with future possibilities with respect to a genuinely fruitful "synthesis" of the "general" Theory of Value, on the one hand, and the Theory of Money and Prices, on the other. See below, pp. 351 ff., 417 ff., 603 f., 622 ff.; and contrast the suggestion of Mr. Lerner that "the chief historical significance of the impressive Walrasian system of equations" has been to provide "an inhibition against applying the supply and demand mechanism to the whole economy," by seeming "to claim the whole field of general analysis as its own even though it could say nothing particularly useful" —nothing, indeed, except that "everything depended upon everything else" ("Some Swedish Stepping Stones in Economic Theory," *loc. cit.,* 581).

[52] See, for example, Marshall's *Principles,* 100: "The demand prices in our list are those at which various quantities of a thing can be sold in a market *during a given time and under given conditions.* If the conditions vary in any respect the prices will probably require to be changed. . . . For instance, the list of demand prices for tea is drawn out on the assumption that the price of coffee is known; but a failure of the coffee harvest would raise the prices for tea." Cf. also *ibid.,* 105 n.: "We must . . . remember that the character of the demand schedule for any commodity depends in a great measure on whether the prices of its rivals are taken to be fixed or to alter with it." The relation of the possibility of substitution to the conformation of the demand curve was, of course, recognized also by Cournot. See, for example, Cournot, *Researches,* 50; and cf. also Jevons, *Theory of Political Economy,* 148 (end of the last complete sentence), 150.

[53] See Marshall's *Principles,* 383 (cf. also n. 2 to the same page); 388, n. 3, 387, 390 f.

variables which Walras chose to handle by his system of simultaneous equations.[54]

The issue raised by Marshall's own practice, therefore, was whether acceptance of the validity and importance of the Walrasian emphasis on the interdependence of all economic magnitudes did or did not reduce the Cournot-Marshall "demand schedules for particular industries," and their property of "elasticity," in the Marshallian sense, to a position of complete uselessness in dealing with the problems of the real world. Surely it is no minimization of the importance of the Walrasian achievement in itself to suggest that the verdict on this point has been provided by the relative usefulness of the results obtained, in the explanation of the events of the real world, by two types of investigators: on the one hand, users of demand schedules of the Cournot-Marshall type; and, on the other, those who, instead of going beyond an acceptance of that warning against a misuse of these schedules which must be regarded as inherent in the Walrasian system, and on to an exploration of the possibilities of a positive extension of the implications of *other* aspects of the Walrasian "system," have contented themselves at best with a stereotyped repetition of the warning itself and at worst with unwarrantedly extreme statements as to what this warning implies with respect to the limitations upon the use of a "partial equilibrium" analysis in attempting to solve the problems of the real world.[55]

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[54] See Marshall's *Principles,* 855 f. That Marshall was perfectly aware that all the cases indicated were examples of that interdependence of economic variables which must be regarded as conditioning any results obtained by the use of "partial equilibrium" analysis, is clear also from the note on p. 100 of the *Principles,* where the cases of composite demand and joint demand are cited as involving the same type of consideration as the case in which the demand schedule for a particular commodity will be affected by the substitutes for that commodity. See also, in this connection, the comment by Barone, "Sul trattamento di quistioni dinamiche," *Giornale degli economisti,* Second Series, IX (1894), 433 n., on the implications of Marshall's treatment of "Joint and Composite Demand" and "Joint and Composite Supply."

[55] The familiar repetition of the charge of sterility against the "Lausanne school" itself summarizes a judgment by a majority of economists on this head (cf., for example, O. Lange, "Die allgemeine Interdependenz der Wirtschaftsgrössen und die Isolierungsmethode," *Zeitschrift fur National-ökonomie,* IV [1932], 57 ff., and see also below, p. 417, n. 23). The same charge could, of course, be leveled against those writers outside the "Lausanne school," such as H. J. Davenport, whose insistence upon stressing the limitations set by the fact of the general interdependence of prices to analysis of a type which "concerns itself with only one commodity at a time" (see, for example, Davenport's *Economics of Enterprise,* 113 ff., 274) ultimately led him to repudiate, in effect, his earlier occasional willingness to make use of the concept of elasticity of demand. (Contrast, for example, Davenport's *Economics of Enterprise,* 50, with his later *The Economics of Alfred Marshall* [1935], 55 ff.; and cf. the comment by R. Opie in the *Journal of Political Economy,* XLIV [1936], 818). It must again be observed, however, that the mere fact that *this particular use of*

3. The difficulty indicated is not such as to invalidate the use of Cournot-Marshall demand curves, and therefore the concept of "elasticity of demand," in the Marshallian sense, as devices helping to explain changes in the structure of money prices, despite statements to the contrary by writers prior to the Keynes of the *General Theory*.[56] On its face,

the Walrasian system, or its equivalent, has been disappointingly sterile, does not mean that *other* aspects of the Walrasian system are not of the first importance for further constructive work on the range of problems with which this work is concerned. Cf. above, p. 168, n. 51, and the forward references there given. Much the same may be said of the uses that have been made thus far of Davenport's "system"; for, on the basis of what is said below, pp. 263 ff., it should be clear that Davenport's otherwise welcome emphasis upon the rôle of money in the pricing process (see, for example, *The Economics of Enterprise,* 114), and particularly his use of the concept of a "money demand for any good" (see below, p. 269, n. 103), could easily have been used as the starting point for the construction of an apparatus for dealing with the forces determining the structure of money prices which would make use of *both* the subject matter of the "general" Theory of Value and the Theory of Money and Prices, whenever either, or both, can be shown to be relevant to the problem in hand. And finally, and most important of all, it is to be recorded that Walras himself, unlike so many of the later representatives of the "Lausanne school," was very far from arguing that his thesis with respect to the general interdependence of economic variables made unusable, or nearly so, particular demand schedules of the Cournot-Marshall type. On the contrary, Walras insisted, these schedules were *"susceptible d'un grand usage"* (*Eléments d'économie politique pure,* 162).

[56] A very long list of examples of such statements could, of course, be culled from the writings of the more extreme representatives of the "Lausanne school." I prefer, however, to call attention to an exquisitely ironical fact of doctrinal history: namely, that a work no less representative of the best of the "Lausanne school" than the *Théorie mathématique de la richesse sociale* of Walras himself was criticized on the ground that the "geometrical character" of those parts of its argument which made use of the equivalent of Cournot-Marshall demand schedules was fatal in view of the fact that "the curves which represent the orders of buyers at various prices must necessarily . . . change for each of them during the course of the market." See pp. 246 ff. of the review of Walras and Cournot by J. Bertrand, as reprinted by G. Lutfalla in the 1938 reprint of Cournot's *Recherches.* (Cf. also the reference given above, p. 167, n. 46, to Irving Fisher's Introduction to the English translation of Walras's "Geometrical Theory of the Determination of Prices," and the quotation from Walras on the "usefulness" of demand schedules of the Cournot-Marshall type, given in the preceding note. For Walras's own reply to Bertrand on the point under discussion, see below, p. 185, and the reference given in n. 89 thereto.) For a later example of rejection of Marshallian demand schedules, with their property of elasticity, on the ground that these demand schedules are subject to change as the result of changes elsewhere in the price structure, see H. J. Davenport, *The Economics of Alfred Marshall,*

what the particular difficulty in question means is that a
given demand schedule for a specific commodity may be
expected to *change* its conformation and therefore its degree
of "elasticity," whenever there are changes in the "demand
and supply schedules" (and therefore in the prices) of other
commodities *which are sufficiently closely related to the
commodity in question to enter appreciably into the calcula-
tions of those individuals the responsiveness of whose de-
mand to changes in the price of the given commodity is
taken for examination.*[57]   In turn, this means simply that

56 f., 65. Davenport, in the passages cited, did not hesitate to declare that
the fact that "there must be a new [demand] schedule for every changing
situation"—"a new curve for the new time"—makes "the term *elasticity*
. . . an offense against clear thinking" and "carries with it the evidence
of previous unclear thinking." However, characteristically enough, Daven-
port's own positive suggestions for dealing with "the response of habits
of consumption to changing opportunities" did not go beyond the state-
ment that these "responses" are "perhaps as accurately described by call-
ing them *modifiable* as elastic." See also, however, what is said of Daven-
port in the preceding note.

   [57] The clause italicized in the text may be taken as providing a con-
tinuing warning of a kind complementary to the warning, provided by the
conception of the general interdependence of prices, against drawing
unwarranted conclusions with respect to the relation between price and
quantity demanded in the case of a single commodity. Specifically, the
clause italicized may be taken as providing a continuing warning against
the drawing of extreme conclusions from the proposition that "in the last
resort all uses of money are rivals to each other in so far as they are not
co-operative, and co-operative in so far as they are not rival" (cf. J.
Robinson, *The Economics of Imperfect Competition,* 20). In this con-
nection, see O. Lange, "Die allgemeine Interdependenz der Wirtschafts-
grössen und die Isolierungsmethode," *loc. cit.,* especially pp. 64 ff. It is
even more important to observe, however, that the argument developed
further in the text is such as to make it clear that *for the purposes of
analysis of the type with which we are here concerned,* the analytical
usefulness of demand curves of the Marshallian type, and therefore of
the concept of elasticity of demand in the Marshallian sense, does not
depend upon the "realism" of the assumptions which would be necessary
if the argument required that the conformation of our demand curves must
remain unchanged over a considerable period of "clock" time (cf. J.
Robinson, *loc. cit.*). All that our argument requires (see below, pp. 177 f.,
under point [5]) is acceptance of a very simple proposition: namely, that
each prospective purchaser makes his decision to purchase or to refrain
from purchasing on the basis of his individual demand curve for a com-
modity as it presents itself in his mind *at the time he makes the decision.*
For this, in turn, is all that is required for the purpose of establishing our
later Proposition III (see below, p. 240): namely, that each "realized"
price is to be conceived of as resulting from the intersection of the market
demand and supply curves prevailing at the time the price is realized,

whenever the changes elsewhere in the price structure are sufficient to effect an appreciable change in the conditions of demand for a given commodity, we must be prepared to *re-draw* the demand schedule for that commodity.

As Marshall put it, these changes would "render it necessary to make out a new demand schedule." [58] This proposition is, of course, capable of being stated in different ways, and with different degrees of elaborateness.[59] None of these elaborations, however, would alter the central contention which is here defended: namely, that there is no basis for the suggestion that the *very concept* of a demand schedule for a particular commodity depends for its usefulness upon the assumption that the schedule remains *unchanged* in conformation and position as between two or more successive realizations of market prices.

It may therefore be observed that it is passages such as that just quoted from Marshall which provide a warning against misinterpretations either of the statement that "Marshall derives his demand curves under the explicit assumption that other things remain equal," or of the suggestion that "Marshallian demand curves" are in all cases to be regarded as *"ceteris paribus* curves" in a sense in which such curves would be *contrasted* with *"mutatis mutandis* curves." [60] A misinter-

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and that the realized price will be what it is as the result of the conformation and position of the respective market curves of demand and supply prevailing at that time. The fact, therefore, that the conformation and position of the curves may have changed *as between two successive realizations of market prices* does not alter the fact that the elasticity of demand for a specific commodity is always a factor affecting its realized price and, therefore, the general structure of realized prices.

[58] Marshall, *Principles,* 462. Cf. also J. N. Keynes's article on "Demand," in Palgrave's *Dictionary of Political Economy,* I, 541: "Unless we confine ourselves to very short periods of time, demand-schedules are themselves *liable to modification"* (italics mine); and Wicksteed, *The Common Sense of Political Economy,* 476: "If we change our supposition as to the price of any one commodity, that very supposition will change the form of the curves of other commodities, throughout their course."

[59] One thinks, for example, of H. L. Moore's "partial elasticity of demand" (cf. Moore's *Synthetic Economics* [1929], 55 ff.); and one thinks also of the contrast between " 'direct' price elasticity" and " 'cross' price elasticities of demand" (see Allen and Bowley, *Family Expenditure* [1935], 142, and cf. the suggestion of Pareto, in his "Considerazioni sui principi fondamentali dell' economia politica pura," *Giornale degli economisti,* Second Series, V [1892], 119, 121).

[60] For an example of the first type of statement quoted, see H. Staehle, "Short-Period Variations in the Distribution of Incomes," *Review of Economic Statistics,* XIX (1937), 133, n. 4 (though see also the quotation from Staehle given below, p. 212, n. 147); and for an example of the second type of statement, see M. Bronfenbrenner, "Applications of the Discontinuous Oligopoly Demand Curve," *Journal of Political Economy,* XLVIII (1940), 420, n. 2.

pretation of Marshall's position would certainly be involved, for example, if the first of the two statements just quoted were to be taken to mean that Marshall argued, either explicitly or implicitly, that the analytical usefulness of the very concept of a demand schedule for a particular commodity depends upon such an assumption, in the sense that the change in the conformation of such a schedule which is necessitated by an abandonment of the assumption is a fatal barrier to the use of these curves for the explanation of certain aspects of the economic process.[61]   A misinterpretation of Marshall's position would likewise be involved if the statement that " 'Marshallian demand curves' are 'ceteris paribus' curves' " should be taken to mean that Marshall himself would simply have refused to use them in cases in which their conformation would be changed (as the result of changes in entrepreneurial or consumers' "expectations" or for any reason whatsoever), instead of continuing to use them after allowance for the effect upon them of these changes (mutatis mutandis).[62]   On the contrary, there is every reason to believe that Marshall, like Wicksteed in the latter's discussion of his "curves of total significance," believed that his concept of a demand curve for a particular commodity, with its property of elasticity, continued to have heuristic significance "in spite of all the modifications which are perpetually taking place" in these curves and "however fluid we may consider . . . [their] form." [63]

It should be observed, moreover, that this argument applies to the "collective demand curve" for a particular commodity quite as much as to the demand curves of individuals for these commodities; and that the analytical relation between the demand curves of individuals for a given commodity and the "collective demand curve" for that commodity remains the same regardless of the fact of change in the conformation of both types of curves.[64]   It is not surprising to discover, therefore, that

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[61] The same warning, of course, must be given against a corresponding interpretation of Wicksteed's proposition that "it seems impossible . . . even ideally to draw up a system of curves which shall be valid simultaneously" (The Common Sense of Political Economy, 476).   In this connection, see the statement of Wicksteed himself quoted below (cf. n. 63).

[62] On Marshall's treatment of the effect, on his demand curves, of "expectations," in particular, see what is said below, p. 192, n. 104.

[63] Cf. Wicksteed's Common Sense of Political Economy, 487.

[64] The contrary, to be sure, seems to have been implied by Edgeworth, when he suggested that unless we proceed upon the postulate that "while the price of the article under consideration is varied, the prices of all other articles remain constant, . . . it is hardly conceivable that, when the prices of several articles are disturbed concurrently, the collective demand curve may be predicted by ascertaining the disposition of the individual" (see Edgeworth's article "Demand Curves," Palgrave's Dictionary, I, 543).   But there is surely no difficulty in "conceiving" of such a "prediction," as long as the claims to "prediction" are kept within the modest limits proper to the use of demand curves as an analytical device. All that we are capable of "predicting" on the basis of an argument such

the representatives of "old" Cambridge, other than Marshall, who have continued to make use of the concept of "elasticity of demand," in the Marshallian sense, in the analysis of changes in the level and structure of prices and output, have certainly not rested their case for the usefulness of the concept on the assumption of *unchanging* elasticity over time.[65] On the contrary, they have proceeded throughout on the basis of the proposition laid down above: namely, that whenever there are changes, including changes elsewhere in the price structure, which are sufficient to effect an appreciable change in the conditions of demand for a given commodity, we must be prepared to *redraw* the demand schedule for that commodity.

4. It may be freely admitted that the difficulty indicated is a troublesome one for certain problems other than that of accounting for changes in the structure of money prices. It is troublesome, for example, in the construction of "statistical" demand curves (since such curves are presented as a measure of the *recorded* response of consumers to given degrees of price change *over a given period*), whenever such

as that of Walras, for example (to which Edgeworth refers), is that the "collective demand curve" will always be what it is as the result of a summation of either (1) the amounts that individual demanders are prepared to demand at given prices, or (2) (if we are concerned with the demand curve as it presents itself to the mind of the *seller*) those amounts which the sellers, estimating "effects . . . in the mass," *think* that the prospective purchasers are likely to demand at given prices (cf. Wicksteed, *Common Sense,* 495) *at the moment when a given amount of commodity is made the subject of bargaining.* A "collective demand schedule" constructed on any other basis would have no meaning. All that Edgeworth's suggestion amounts to, therefore, is that the "collective demand schedule," like individual demand schedules, must be regarded as subject to change as a result of changes, actual or expected, elsewhere in the price structure; and this is a proposition which neither Walras nor any other instructed user of particular demand schedules would ever have been prepared to deny.

[65] See, for example, Robertson, *Banking Policy and the Price Level,* 17 f., on the error of assuming that there will be no significant changes in the elasticity of demand for products of particular industries as between boom and depression; and see also Pigou, *Industrial Fluctuations,* 182 n., and "The Statistical Derivation of Demand Curves," *Economic Journal,* XL (1930; p. 66 of Pigou and Robertson, *Economic Essays and Addresses).* No one, indeed, could have been more explicit than Professor Pigou in his *Theory of Unemployment,* where it was pointed out (p. 39) that "in the language of elasticities, there is not, in respect of any assigned volume of employment, a single elasticity of demand for labor, but a whole family of elasticities with different members referred to different time intervals," and it was stated emphatically (p. 89) that "any study of elasticity which disregards the distinction between booms and depressions *must* be futile" (italics Pigou's).

curves are regarded as implying that there was no appreciable change, throughout the period, in the conformation and position of demand schedules *in the Marshallian sense*. It would, however, be claiming much more for "statistical" demand curves than their principal sponsors have claimed for them to suggest that the limitations attaching to these "statistical" demand curves are identical with those attaching to demand curves of the Marshallian type, when the latter are regarded solely from the standpoint of their analytical usefulness in accounting for changes in realized money prices.[66]

Specifically, of course, the limitations attaching to "statistical" demand curves arise from the fact that they usually represent an attempt to derive, from a series of *realized* prices, information with respect to a schedule of *"ex ante"* relations between prices and quantities demanded at those prices, despite the fact that only one point on these *"ex ante"* schedules will be realized in any single market transaction.[67] In order, therefore, to be able to identify these "realized" prices (which represent prices "realized" in *successive* market transactions) with points on a single *"ex ante"* schedule of the Marshallian type (only *one* point on which will be "realized" in any single market transaction), it is necessary either (1) to provide additional evidence which will create a presumption that there has been no change in *"ex ante"* demand schedules over the period in question; or (2) to develop supplementary statistical techniques designed to reveal what an unchanging *"ex ante"* schedule *would* have looked like if the forces which actually caused it to change its conformation or position had not been operative.[68] For in these ways alone is it possible to argue that a succession of realized prices represents movements *along* a single, and unchanging, demand schedule of the Marshallian type.

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[66] Cf. the comments of Schultz, *The Theory and Measurement of Demand* (especially pp. 61 ff.), and the references to the literature there given; also the comments of E. W. Gilboy, "Methods of Measuring Demand or Consumption," *Review of Economic Statistics,* XXI (1939), 69 f., and especially G. J. Stigler, "The Limitations of Statistical Demand Curves," *Journal of the American Statistical Association,* XXXIV (1939), 470.

[67] In this connection, see the comments of Pigou,. "The Statistical Derivation of Demand Curves," *loc. cit.* (p. 64 of Pigou and Robertson, *Economic Essays and Addresses*). On the use of the term *"ex ante,"* see what is said below, p. 177, n. 70.

[68] The second possibility, which has underlain so much of the work done on "statistical demand curves," was, of course, recognized long before the statistical work itself was undertaken. See, for example, J. N. Keynes, in Palgrave's *Dictionary,* I, 541, on the possibility that the effects of changes which would otherwise vitiate "statistical calculation" of demand curves "could themselves be estimated and allowed for."

5. That this difficulty does *not* invalidate the use of demand schedules of the Marshallian type for the purpose with which this work is concerned becomes clear as soon as attention is called to a proposition that is fundamental for an understanding of the rôle to be assigned to these demand schedules in any attempt to account for realized changes in the structure of money prices: namely, that what is represented by market demand schedules of the Marshallian type is a *set of "plans"* by prospective purchasers of a given commodity *at the time that they reach the decision to purchase or refrain from purchasing that commodity at a given price.* For it should then be clear that the mere fact that these plans may themselves change between successive realized decisions to purchase or not to purchase does not alter the further fact that *the actual purchases themselves may be assumed to be based on calculations whose results are embodied in "plans" the resultant of which is a decision to purchase a given amount if the price is at one level and another amount if the price is at another level.*

The essential element in these propositions is summed up by the statement that the demand schedules of the general Theory of Value are concerned with what has recently been called "the *pre*-formation of market prices." [69]   Or (in another terminology made popular in recent years): the market demand schedules whose continued usefulness is here defended are *"ex ante"* curves representing the *plans* of possible purchasers with respect to the *present* market situation, as that situation is evaluated by the possible purchaser in the light of his own present economic position.[70]   Obviously, such an evaluation, and there-

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[69] See L. Baudin, *La Monnaie et la Formation des Prix* (1936), 25 ff.

[70] The *expressions* "*ex ante*" and "*ex post*" seem to have been introduced for the first time by G. Myrdal in 1933. (See his "Der Gleichgewichtsbegriff als Instrument der geldtheoretischen Analyse," *loc. cit.*, 394. As far as I have been able to discover, these *expressions* did not appear in the earlier Swedish version of Myrdal's essay published in 1931 ["Om penning-teoretisk jämvikt," *Ekonomisk Tidskrift*, XXXIII, 191 ff.]; and it is interesting to observe that it was only in the English version of 1939 that Professor Myrdal added the section of the essay *formally* devoted to the distinction between "'*ex post*' and '*ex ante*'" [*Monetary Equilibrium*, 45 ff.], with its modest assertion that "probably the chief contribution of this essay, if any, is to have originated the concepts *ex post* and *ex ante*" [p. 47].) The terms "*ex post*" and "*ex ante*" were then taken over by a number of Professor Myrdal's Swedish colleagues, as well as by a few writers directly under Swedish influence. (See, for example, D. Hammarskjöld, *Konjunkturspridningen* [1933], 53 f.; G. Mackenroth, *Theo-*

fore the "plans" based upon it, would take into account (1) the ways in which the purchaser's own present economic position and the general market situation have been affected by past events; and (2) the purchaser's expectations with respect to his own future position and the future market situation, as both may be expected to be at the time when the purchaser may expect to be again "in the market" for the commodity or service in question.[71]

The *substance* of these propositions, on the other hand, was recognized with emphasis and clarity by a very large number of the earlier writers

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retische Grundlagen der Preisbildungsforschung und Preispolitik [1933], 141 f. [cf. Mackenroth's reference to an earlier work of Myrdal on p. 136, n. 9, of the work cited, as well as on p. 172*, n. 2, of Mackenroth's article "Ziele und Wege der Geldpolitik," *Weltwirtschaftliches Archiv*, XXXV (1932)]; B. Ohlin, *Penningpolitik, Offentliga Arbeten, Subventioner och Tullar som Medel mot Arbetslöshet* ["Monetary Policy, Public Works, Subsidies, and Tariffs as Methods of Combating Unemployment"] [1934], pp. 7 ff.; and E. Lindahl, *Studies in the Theory of Money and Capital* [1939], 63 f., 175 f.) On the other hand, the characterization of the ordinary demand and supply schedules of the general Theory of Value as "*ex ante* concepts" which "indicate alternative purchase and sales plans" seems to have been first introduced by Professor Ohlin in 1934 (*Penningpolitik, etc.*, 10; cf. Ohlin's later paper of 1937, "Alternative Theories of the Rate of Interest," *Economic Journal*, XLVII, 423, and see also E. Lundberg, *Studies in the Theory of Economic Expansion* [1937], 9). Since "demand schedules" are related, in the text above, to the "plans" of possible *purchasers,* it would be well to add that there is, of course, nothing to prevent a consideration of market demand schedules as entering into the "plans" of possible *sellers* as well. On the contrary, there is every reason for protesting against the assumption that the demand schedules involved in the "plans" of purchasers and sellers, respectively, will be *identical* (cf. N. Kaldor in *Economica* for August, 1934, pp. 340 f.). There is also every reason, however, for protesting against a terminological usage which might obscure the fact that the demand curves involved in the calculations of possible *purchasers* are as much "*ex ante*" concepts as are the demand curves involved in the calculations of possible *sellers.* See below, p. 195, and n. 109 thereto.

[71] The separation of these propositions from the proposition advanced in the preceding sentence of the text should make it clear that the use of *ex ante* concepts in themselves is not necessarily identical with an emphasis on the importance of "expectations" in economic analysis. On the contrary, "*ex ante* analysis" is the broader category, and "expectational analysis" is a subdivision thereof (in this connection, see what is said below, p. 180, n. 73; also what is said below, pp. 225 ff., on the various meanings that may be attached to the concept of "hypothetical" prices). *A fortiori,* obviously, "*ex ante* analysis," as such, is not to be understood as being confined to the *particular instance* of such analysis which is represented by the use of "ex ante" *demand or supply schedules,* and still less as being confined to the problems discussed under the head of "Saving" and "Investment." Contrast A. P. Lerner, "Ex-Ante Analysis and Wage Theory," *Economica*, New Series, VI (1939), 436, and "Some Swedish Stepping Stones in Economic Theory," *loc. cit.*, 580, 583.

on our subject—so large, indeed, that if an important element in the laying of "The Foundations of Dynamic Economics" is to be found in an emphasis on the fact that "the decisions of entrepreneurs to buy and sell (and to some extent also the similar decisions of private persons) nearly always form part of a system of decisions which is not bounded by the present, but has some reference to future events," then these "foundations" were laid long before the element of "expectations" came to be assigned the "revolutionary" significance accorded to it by certain writers in recent years.[72] It is, indeed, the very abundance of the

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[72] The quotations are from Hicks, *Value and Capital*, 113 ff., 123. The claims to novelty advanced in recent years on behalf of an emphasis on the element of "expectations" have varied in extremity. In justice to Mr. Keynes, it must be said that, in the *General Theory* itself, he refrained from extreme claims on behalf of the novelty of this aspect of the argument of the *General Theory*. On specific details, to be sure, he made statements with respect to the treatment of "expectations" by earlier writers which can be shown to be completely without foundation. In general, however, he asserted merely that "the part played by expectation in economic analysis" was one of "the three perplexities which had most impeded" his "progress in writing" the *General Theory*, and for which he had had to find "some solution" before he could "express" himself "conveniently" (*General Theory*, 37). Unfortunately, however, Mr. Keynes has not been so modest in some of his later utterances. See, for example, his article, "The General Theory of Employment," *Quarterly Journal of Economics*, LI (1937), 222, where he lists, as the first of "the main grounds" of his "departure" from "the traditional theory," the alleged fact that "the orthodox theory assumes that we have a knowledge of the future of a kind quite different from that which we actually possess," and that this alleged "hypothesis of a calculable future" has led "to an underestimation of the concealed factors of. utter doubt, precariousness, hope, and fear." Unfortunately, also, certain of Mr. Keynes's followers have been even less modest in their claims on his behalf. We have been told, for example, that in this field Mr. Keynes has "contributed suggestions" which "have provided us with a radically new line of attack"; and it has been further implied that this applies particularly to the emphasis of the *General Theory* on the rôle played by expectation "in the face of an almost complete ignorance of the future," this emphasis, in turn, being characterized as "the single, central and unifying idea which underlies" the book as a whole (cf. G. L. S. Shackle, *Expectations, Investment, and Income* [1938], 2, and *Economic Journal*, XLIX [1939], 501 f.). The high praise thus accorded to the treatment by the *General Theory* of the element of "expectations" has been echoed in some quarters (see, for example, R. Schüller, "Keynes Theorie der Nachfrage nach Arbeit," *Zeitschrift für Nationalökonomie*, VII [1936], 478); while in other quarters the critical reaction has varied from lukewarm to outspokenly hostile (see, for example, the comments of Rosenstein-Rodan, "The Coordination of the General Theories of Money and Price," *loc. cit.*, 277, n. 1; W. Leontief, "The Fundamental Assumption of Mr. Keynes' Monetary Theory of Unemployment," *Quarterly Journal of Economics*, LI [1936], 197, n. 3; J. Schumpeter, *Journal of the American Statistical Association*, XXXI [1936], 792, n. 3; F. H. Knight, "Unemployment: And

evidence that can be adduced in support of this statement, together with the multiplicity of the contexts for which this evidence can be shown to be relevant, that makes it impossible to review this earlier discussion here in all detail. It should, therefore, be sufficient here to point out that a failure to have recognized the *ex ante* character of the ordinary "demand curves" of the general Theory of Value would have been very surprising in the case of those writers in whose "general Theory of Value" great stress was laid upon the *subjective* character of the calculations involved in the pricing process and, therefore, upon the *prospective* character of these calculations.[73]   And in fact there is no evidence what-

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Mr. Keynes's Revolution in Economic Theory," *Canadian Journal of Economics and Political Science,* III [1937], 107; Lundberg, *Studies in the Theory of Economic Expansion,* 188; O. Morgenstern, *The Limits of Economics* [1937], 158 f.).   It is interesting to observe, moreover, that even those who have shown themselves to be aware of the fact that the history of the treatment of "expectations" in economic analysis does not begin with the *General Theory* have suggested that this history is very recent. Professor Hicks, for example, accompanied his remark that the "discovery" by the *General Theory* of the importance of "people's anticipations of the future" is "not altogether a new discovery" by comments suggesting that the list of Mr. Keynes's "forerunners" in this respect would be exhausted by references to "Swedish economics" going back no further than 1929, or to "the writings of the econometrists," of equally recent date ("Mr. Keynes' Theory of Employment," *loc. cit.,* 240). Some of the representatives of this "Swedish economics" themselves, to be sure, have in turn been more modest, in that they have ventured to characterize certain chapters of the *General Theory* dealing with the element of "expectation" as merely "following up . . . numerous suggestions in Marshall's *Principles*"; but these same writers have gone on to suggest that the latter work is the "only" one among "the pre-depression treatises" which "seems to have had . . . in mind" the question: "to what extent are economic actions influenced by anticipations of future events, *i.e.,* by expectations?" (cf. Ohlin, "Some Notes on the Stockholm Theory of Savings and Investment, I," *Economic Journal,* XLVII [1937], 54). And on some of those rare occasions on which it has been suggested that "a more complete survey of the process by which the significance of anticipations was gradually introduced into economic analysis would probably have to begin" with some writer other than Marshall, attention has been directed to writings no earlier than the *Appreciation and Interest* of Irving Fisher, who after all is to be regarded as a virtual contemporary of Marshall (cf. F. A. von Hayek, "Economics and Knowledge," *Economica,* February, 1937, p. 33, n. 3). By way of contrast, the fragmentary set of citations which follows may be taken as providing some indication of the kind of evidence that could be adduced in support of Mr. Hawtrey's categorical statement that, in reality, "the fact that all economic activity is governed by expectations has been universally taken for granted from the beginning of economic science" ("Alternative Theories of the Rate of Interest," *Economic Journal,* XLVII [1937], 439).

   [73] In the light of the comment made above, p. 178, n. 71, on the relation between *"ex ante"* analysis and "expectational" analysis, it is not surprising to discover that individual writers, all of whom may have stressed

ever of such a failure on the part of the writers indicated.[74]

the *subjective* elements in the pricing process, differed in the amount of emphasis they put upon "expectational" elements in that process. Yet if not all *"ex ante"* analysis is necessarily concerned primarily with the element of expectations, "expectational" analysis *is* necessarily *"ex ante"* in character. It follows, therefore, that the use of "expectational" concepts by a given writer on the nature of the pricing process may be taken as proof that he was, in these instances, making use of *ex ante* concepts. I have had this consideration in mind in choosing the citations which follow, and which I present in support of my proposition with respect to the *ex ante* character of the analysis developed, by the writers indicated, in connection with the problem under discussion.

[74] The citations which follow in the text are confined almost entirely to writers, beginning with Jevons, who are generally thought of as protagonists of an emphasis on the "subjective" elements in the pricing process. It would be a very serious mistake to suppose, however, that an emphasis on the element of "expectation" is to be found no earlier than the Jevonian "revolt" against the classicals. No one, for example, could have been more explicit than was that benighted follower of the "classicals," J. R. Mc-Culloch, in insisting that "the prices of commodities will frequently be very much influenced, not merely by the actual occurrence of changes in the accustomed relation of the supply and demand, but *by the anticipation of such changes*" (*On Commerce* [1833], 69 ff. [reproduced in McCulloch's *Dictionary . . . of Commerce and Commercial Navigation*, II, 948 ff. of the 1840 edition] [italics mine]). It is hardly surprising, on the other hand, that the emphasis on the *ex ante* character of the calculations involved in the pricing process which is necessarily implied by an emphasis on the *prospective* and *expectational* character of these calculations, should have been more marked in the contemporary (pre-Jevonian) critics of the stricter "classicals." See, for example, Malthus's *Principles of Political Economy*, 61 n., on the effect upon prices "if a large supply is soon *expected*" or "if a future scanty supply is *looked forward to*," and his general statement on p. 71 of the same work, with respect to the effect upon prices of "actual *or contingent* excess [or failure] of supply" (italics mine). It is in the writings of Tooke, however, that one finds so striking and persistent an emphasis upon the *prospective* elements involved in the pricing process that this aspect of his argument had been singled out for particular comment even before the emergence of the current widespread emphasis upon the importance of the phenomenon of "expectation" in economics generally. See, for example, the comments of Gregory on this aspect of Tooke's argument in the former's Introduction (1928) to Tooke's *History*, 18, 77; and cf. the remarks of Tooke (1) on the effects, upon prices, of "apprehended" (or "contingent," "probable," "anticipated," or "expected") scarcity, as well as of "actual," or "real," scarcity; (2) on the effect, upon prices, of "anticipation of demand" and of "prospective" (or "estimated," or "computed") demand, as well as of "immediate" demand and "consumption"; (3) on the effects, upon current pricing, of an "anticipation of the maintenance of the former range of high prices," or, conversely, of the "prospect" of lower prices, since "the business of production, or supply, proceeds wholly upon anticipation"; and, more generally, his remarks (4) on the effect, upon current prices, of the "opinion" of "quantity" and of *"future supply,"* as well as of the "quantity actually for sale" in the present, in his

"A change of price," wrote Jevons, for example, "is always occasioned by the overbalancing of the *inclinations* of those who *will or will not* sell just about the point at which prices stand." [75]   And an outstanding characteristic of Jevons's argument was the amount of emphasis he put on the rôle played in these "inclinations" by what he called "*anticipated feeling*," particularly as the latter is affected by "the uncertainty of future events." [76]   "This power of anticipation," he pointed out, "must have a large influence on Economics." "A principle of the mind which any true theory must take into account," therefore, "is that of *foresight*." [77]   Hence the necessity for dealing with not only the "*actual* utility" of a commodity but also its "*prospective* utility," or, as he called it elsewhere, "*estimated future utility*, which yet, by allowing for the imperfect force of anticipation, and for the uncertainty of future events, gives a certain present utility." [78]   Hence, also, his refusal to see any "revolutionary" significance in the emphasis by his contemporary, W. T. Thornton, on the necessity for taking "*prospective* demand and supply" into account in any theory of price determination.[79]   Obviously, Jevons observed dryly, "in the actual working of any market, the influence of future events should never be neglected, neither by a merchant nor an economist." [80]

Thoughts and Details, I, 85, 88, 97, 100, 105, 126, 139, 142, 158 f., 164, 191, 196, 201; III, 132; IV, 5 ff., 11 f., 14; *Considerations on the State of the Currency*, 40, 43 f., 63, 103; *Letter to Lord Grenville*, 86 f.; *Inquiry into the Currency Principle*, 129, 133; *History of Prices*, II, 10, 22, 26, 131, 143, 146, 154 f. (footnote), 183, 195, 200, 260, 273, 320; III, 55, 152; V, 88, 165, 168 ff.

[75] *Theory of Political Economy*, 110. I have italicized the words "inclination" and "will or will not," in order to bring out the contrast between Jevons's statement and statements such as that quoted from Joan Robinson below, p. 194, n. 107. Cf. also Jevons's *Principles of Economics*, 56: "Both the holders and desirers, representing supply and demand, hold and desire with different degrees of mental feeling, arising either from different degrees of want of the commodity in question or different *estimates of what will be wanted or supplied by other people in the future. . . .* The constitution of a market thus resolves itself not into any one statement of demand and supply, but a statement of what *would be* demanded and supplied *at every conceivable ratio of exchange*" (italics mine).

[76] Cf. *The Theory of Political Economy*, 33 ff.

[77] *The Theory of Political Economy*, 35, 305. Italics in the original.

[78] *The Theory of Political Economy*, 69 ff., 306; cf. Jevons's *Principles of Economics*, 20. For examples of early adumbrations of this type of emphasis, see the references to Galiani (1750) and Ortes (1774) given by M. Pantaleoni in his *Pure Economics* (first published 1889; pp. 90 n., 92 n. of the English translation) in connection with Pantaleoni's own discussion "Of Actual and Prospective Utility" (pp. 86 ff. of the work cited; see also 26 f.).

[79] See Jevons's *Theory of Political Economy*, 108 ff., and cf. Thornton, "A New Theory of Supply and Demand," *Fortnightly Review*, VI (1866), 430 ff. (the relevant passages were reproduced almost *verbatim* in Thornton's *On Labour* [1869], 60 ff.).

[80] *Theory of Political Economy*, 109. Cf. also Jevons's *Principles of*

Precisely the same kind of awareness of the *ex ante* character of "demand curves" and of the rôle played by the element of prospectiveness in the determination of their conformation was evidenced by Jevons's British contemporary, Fleeming Jenkin, who deserves particular mention in the present context because he sponsored simultaneously (1) a theory of "subjective" value, and (2) the use of the "graphic method" for the representation of what he himself called "demand and supply curves." [81]   The "prices" in his demand curves, Jenkin made

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Economics, 147: "Even when we know the ['actual'] supply, ... the matter is complicated very much by the fact that prices depend upon the *prospective* state as much as upon the immediate state of the market. In practice the supply includes the visible supply for *many months to come,* or even for a whole year, and even if we have accurate present statistics these will not confine or define the *speculations of merchants as to future events*" (italics mine).

[81] Cf. the comment on Jenkin's use of the "graphic method" in Marshall's *Principles,* 476; and see also the comment by Edgeworth, in Palgrave's *Dictionary,* II, 473, on Jenkin's equations for his demand and supply curves (p. 17 n. of Jenkin's *The Graphic Representation of the Laws of Supply and Demand and Other Essays on Political Economy* [reprinted, 1931, in the London Series of Reprints; first published in 1870]). Jevons's own references to Jenkin had to do with the latter as one of the "few English mathematicians . . . who venture to write upon the obnoxious subject of mathematico-economic science" (cf. Jevons's *Theory of Political Economy,* pp. xli, lvii, and 333), rather than with the "subjective" emphasis in Jenkin's value theory. Yet there can be little doubt that Jenkin was quite clear in his own mind as to the importance of the latter aspect of his own work. Cf. Jenkin's *The Graphic Representation, etc.,* 93: "The value of all things depends on simple mental phenomena, and not on laws having mere quantity of materials for their subject"; and p. 141: "There is nothing valuable but thinking makes it so." It may be added that a complete survey of the extent to which an emphasis on the *prospective* character of the calculations involved in the pricing process was to be found in the writings of *pre*-Jevonian protagonists of an emphasis on the "subjective" elements in that process, would have to do justice to the suggestions of H. D. Macleod. On Macleod as having "anticipated much of both the form and substance of recent criticisms [such as those of Jevons, Walras, and Menger] on the classical doctrines of value in relation to cost," see Marshall's *Principles,* 821, and his letter to J. B. Clark of December 11, 1902 (*Memorials of Alfred Marshall,* 414). For a particularly striking example of Macleod's emphasis on *prospective* elements in the pricing process (though the passage in question appeared in a work of Macleod published *after* the appearance of Jevons's *Theory of Political Economy*), see Macleod's *Theory of Credit,* I (1889), 196: "It is sometimes supposed that Value is only affected by the actually existing quantity of produce which is brought into the market. This, however, is not so. The expected quantity which may be brought into the market has a most important influence on the Value of the existing quantity. . . . Hence the word Quantity in the general Equation, must denote the Quantity, *actual* or *expected.* Similarly, the word Demand must denote the Demand, *actual* or *expected*" (italics Macleod's).

clear, are prices "determined *in the minds* of sellers and buyers." "It must be remembered," he insisted, that "the demand curve . . . represents a certain mental state"; the price that will be "selected" by the "cutting" of the supply curve with the demand curve will therefore "depend on the state of mind of the buyers and sellers." [82]   For "the demand curve and supply curve" themselves merely "indicate certain *resolutions* on the part of buyers and sellers": that is, they represent the amounts that individuals "are just willing to [buy and] sell" at given prices.[83]   These "resolutions," moreover, are themselves subject to "uncertain estimate varying day by day according to transactions in the market and the dispositions of holders." [84]   Indeed, "men's minds," and therefore the "resolutions" ("plans") embodied in demand curves, cannot be counted upon to "remain constant for five minutes together." [85]

When, moreover, one turns to the writings of Jevons's great Continental contemporaries who emphasized, with him, the subjective elements in the pricing process, one finds precisely the same clear recognition of the *ex ante* and expectational character of the demand schedules for particular commodities, or their equivalent.   No one could have been more explicit than Menger, for example, in his discussion of the elements which would form the basis of individuals' judgments "with respect to the relative significance to an economy of certain definite goods and complexes of goods."   It was obvious, he pointed out, that these individuals would take account of the "sums of money" for which the goods in question are *expected* to be "sold" or "acquired." [86]   It is

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[82] *The Graphic Representation, etc.*, 85 f., 87 f., 95 (italics mine).

[83] *Ibid.*, 87, 108 f. (italics mine).

[84] *Ibid.*, 89.   Cf. also p. 99, where, in discussing the "motives" determining "the value set on any article by the mind of the seller," Jenkin argued that what would determine "the differences in the supplies at various prices in any given market" (that is, the form of the supply curve) would be "the different *estimates* of the present *and future* demand" (italics mine).

[85] *Ibid.*, 79.

[86] See Menger's *Grundsätze*, 293 f. of the second edition.   It is noteworthy that Menger himself twice italicized the word *"voraussichtlich"* in the passage in question.   Cf. the comment of Hayek, "Carl Menger," *loc. cit.*, 400: "To him [Menger] economic activity is essentially planning for the future, and his discussion of the period, or rather different periods, to which human forethought extends as regards different wants . . . has a definitely modern ring."   A similarly "modern ring," it may be added, attaches to Menger's use of a distinction corresponding to what would now be called the distinction between *"ex ante"* and *"ex post"*: as he put it, the "comparison between *intended (beabsichtigter)* expenditures and the *prospective (voraussichtlichen)* results of an economy, on the one hand, and the . . . evaluation and comparison of the *actually realized (tatsächlich erfolgten)* expenditures and their success, on the other" (*Grundsätze*, 294 [italics mine]).   Cf. Wicksteed (II, 820 f. of the 1933 reprint of *The Common Sense of Political Economy, etc.*) on "the whole direction of resources to ends as a continuous selection between alternatives, guided throughout by a weighing of the significance of the *anticipated* results . . . ; reward and

hardly surprising to discover, therefore, that he devoted particular attention to that "element of Uncertainty" *(Das Moment der Unsicherheit)* which must necessarily play a rôle in these "expectations." [87]

Nor could anyone have been more explicit with respect to the *ex ante* character of demand and supply schedules than was Léon Walras. For the whole of his argument with respect to the "demands" and "supplies" involved in his "curves of effective demand and supply" for particular commodities ran in terms of the *"dispositions"*—that is, the *plans*—of the individual sellers with respect to the amount they would be prepared to buy or sell at given prices.[88] It is not at all surprising, therefore, that when, in 1883, J. Bertrand advanced an argument against the usefulness of the type of analysis represented by these curves which was in all essentials the first of the arguments advanced by Mr. Keynes in his *General Theory,* Walras found the argument quite "easy to refute." [89]

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sacrifice alike being measured and determined by the ultimate significance of the respective products, *as anticipated by the producers;* the points at which things are *bought and sold* simply registering the relative *success or failure of the anticipations under which the alternatives were selected,* and tending to correct them" (italics mine).

[87] See Menger's *Grundsätze,* Chap. II, sec. 5b (pp. 29 ff. of the second edition); and cf. also the comments on Menger's treatment of "the element of anticipation" by G. J. Stigler, "The Economics of Carl Menger," *Journal of Political Economy,* XLV (1937), 234, 245 (cf. the same author's *Production and Distribution Theories* [1941], 140, 152).

[88] See, for example, Walras's *Eléments,* "Lesson" Six (pp. 54 ff.)—a "Lesson" devoted, it should be observed, to the establishment of the meaning of the "curves of effective demand and supply"—and "Lesson" Fifteen (pp. 158 ff.)—a "Lesson" on the meaning of "curves of purchase and sale." That Walras's *"dispositions"* (cf. Edgeworth's use of the expression "the subjective *dispositions* of individual persons," as opposed to "the objective fact of price," in Palgrave's *Dictionary,* I, 543, and the reference to Walras's *Eléments* on the same page; also Edgeworth's *Papers Relating to Political Economy,* II, 308) were thought of as *"plans"* is particularly clear from his distinction between "virtual" *dispositions,* on the one hand, and "effective" *dispositions,* on the other. A "virtual" *disposition* with respect to purchase or sale, according to Walras, was one which would be set up only when a buying or selling price is announced in the market; an "effective" *disposition* was one which would be set up in advance *(Eléments,* 56). Both, however, were clearly conceived of in *ex ante* terms; and it is particularly worthy of note that Walras himself was careful to point out that the "dispositions" which, at the time purchasers or sellers enter a market, are "in the virtual, rather than the effective state . . . *none the less exist"* (italics mine). Cf. also p. 158 of Walras's *Eléments,* where his "curves of purchase and sale," described as representing the "dispositions" of the bargainers with respect to purchase and sale, were also described as resting upon a series of "hypotheses" as to what the price will be. See below, pp. 224 ff.

[89] See Walras's *Etudes d'économie sociale,* 352 n. The statement in the text that Bertrand's argument was "in all essentials the first of the arguments advanced by Mr. Keynes" is, of course, intended to apply to Bertrand's *application* of the proposition stated in the following sentence of the text above. Cf. the following note.

The difficulty raised by Bertrand had to do with the changes in demand schedules induced by realized changes in the structure of prices and quantities available, when these changes result from the action of *others* than the individual whose demand schedule is under discussion.[90] Walras's answer, in effect, was to point out that Bertrand had failed to observe that the market demand and supply schedules involved in the argument of Walras's *Eléments* represented the plans (or, as he would have said, the "dispositions") of the *bargainers at the "moment" they decided to purchase or refrain from purchasing*.[91] The mere fact that a realized market event that has occurred at one "moment" may have forced a change in the plans with which these bargainers *first* came into the market does not mean that these bargainers have no "plans" with respect to purchase and sale, capable of representation by the familiar demand and supply schedules, at a *second* "moment." In his own exposition, Walras pointed out, he had made it clear that "exchange" was thought of as "suspended" until the "plans" ("dispositions") of the buyers and sellers, respectively, would be adjusted in such a way that a realized act of purchase and sale (which always occurs at a specific "moment" of time) would become possible.[92] During this period of "suspension" of realized purchases and sales, to be sure, the bargainers

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[90] See pp. 245 ff. of the reprint of Bertrand's review of Walras, cited above, p. 171, n. 56. Bertrand's argument, to be sure, was directed to the special case in which the realized actions of the "others" with respect to the demand and supply of the *particular commodity taken for examination* would change the *subsequent* conditions of demand and supply *for that commodity*. It is clear, however, that Walras's refutation of Bertrand's argument applies equally well to the case envisaged by Mr. Keynes's first objection to the usefulness of particular demand schedules: namely, the case in which the realized action of the "others" would affect the subsequent conditions of demand and supply for a given commodity by affecting the realized prices of *other* commodities. For Walras's refutation rested upon the contention that the mere fact that the purchase and sales plans ("dispositions") of individuals might change, as the result of changes realized between the moment an original plan is set up and the moment at which an act of purchase and sale is finally executed, does not alter the fact that this final act of purchase and sale will itself be executed on the basis of purchase and sales "plans," capable of representation by curves of demand and supply, as these plans exist *at the moment when the purchase and sale are effected*. And this contention applies with undiminished force to the first argument against the usefulness of these demand curves presented in Keynes's *General Theory,* in which the changes effected between two realized acts of purchase and sale of a given commodity are held to concern *directly* only the prices of commodities other than the one whose "demand schedule" is taken for examination.

[91] See Walras's *Etudes d'économie sociale,* 352 n.: "The theoretical current price is essentially a unique price resulting, *at a given moment,* from a general exchange" (italics mine).

[92] Walras, *Etudes d'économie sociale,* 352 n.: "Exchange remains suspended until the rise or the fall [of bid and asked prices] will have brought about an equality of supply and demand."

would still have their purchase and sales plans ("dispositions").[93] But the very fact that Walras thought of these "dispositions" as subject to change during a period of "suspension" of realized market *actions* shows that he thought of them, from first to last, in what would now be called "*ex ante*" terms.

Given these examples of a clear recognition of the *ex ante* character of the calculations involved in the pricing process, by the three great sponsors of an emphasis on the "subjective" elements in that process, it is hardly surprising that such recognition is to be found also in writers directly under the influence of the three authors just discussed. In England, for example, the most outspoken advocate of the Jevonian emphasis, as well as of the emphasis of the Austrians and the relevant part of the argument of the school of Lausanne, was, of course, P. H. Wicksteed.[94] It is not in the least surprising, therefore, that Wicksteed's

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[93] Since the bargainers would be waiting to see what price would be named, as well as what might be happening to other prices, the "dispositions" in question would be of the "virtual" type: that is, they would be set up by the bargainers on the spot, in the face of a changing situation, instead of being established firmly in advance. It is to be remembered, however, that Walras was quite insistent that these "virtual" *dispositions* "exist" with as much reality as do the "effective" *dispositions*. See above, p. 185, n. 88.

[94] On Wicksteed's relation to Jevons, in particular, see Professor Robbins's Introduction to the 1933 reprint of Wicksteed's *The Common Sense of Political Economy*, pp. vii ff., as well as Wicksteed's own comments in *The Common Sense, etc.*, 1 ff., and in the articles reproduced in Vol. II of the 1933 reprint of that work, 715 f., 807 ff. It should again be emphasized, however, that the mere fact that particular attention is paid here to the recognition, by outspoken adherents of Jevons's theory of "subjective" value, of the *ex ante* and "expectational" character of the calculations involved in the pricing process does not mean that such recognition is not to be found in writers who showed something less than full sympathy with the claims advanced on behalf of the Jevonian "revolution." That the contrary is the case is, indeed, in itself hardly surprising, in view of the fact that the objections of Cairnes, for example, to Jevons's general treatment were not directed primarily against the *inclusion* of an emphasis on "subjective" factors in the pricing process. On the contrary, one of Cairnes's objections to Jevons's claims on behalf of "the employment of Mathematics in the development of economic doctrine" was based on Cairnes's own refusal to concede "either that *mental feelings* admit of being expressed in precise quantitative forms, or, on the other hand, that economic phenomena do not depend upon mental feelings." (Cf. Cairnes, *The Character and Logical Method of Political Economy*, second [1875] edition, pp. vi f. [italics mine], and also Cairnes's comments on R. Jennings, *ibid.*, 110 ff. For a statement of Cairnes's own position with respect to the rôle played by "laws of *mind*" in the explanation of economic phenomena, see Lecture II and Appendix B in the work cited; also see p. 132 n. of the same work, on the necessity for "the tracing of the phenomena of wealth up to definite human motives," instead of only to "ascertained external facts.") It was perfectly natural, therefore, that Cairnes himself should have been prepared to stress the rôle played by the "opinions" of dealers in the processes "under which the

discussion of "markets" and the rôle played in these markets by the "scales of preference" of the bargainers should have run from first to last in terms of the *ex ante* concepts of "estimate," "anticipation," and "expectation." [95]   On the continent, likewise, there is the example pro-

selling price comes to be decided." (See Cairnes's *Leading Principles of Political Economy Newly Expounded,* 106 f.; and cf. also his comment on the influence, on the price of corn, of *"opinion* as to the prospects of the coming crop," in his *Character and Logical Method,* 124, as well as his approving quotation from Tooke, p. 111 n. of the same work, on the "influence on prices" of "the speculative views operating on the minds of both buyers and sellers.") The attitude of Henry Sidgwick to the Jevonian "revolution" was very much more sympathetic than that of Cairnes. (See, for example, the Preface to the first [1883] edition of Sidgwick's *Principles* [p. v of the third (1901) edition].) It was still, however, not the attitude of the worshipful disciple, of the kind that one finds in a writer such as Wicksteed (see, for example, Sidgwick's *Principles,* 7, 9 ff.). It should be of some interest, therefore, to call attention to (1) Sidgwick's discussion of the rôle played in the pricing process by "estimates" (including "estimates" of an "expected rise in prices"), and by "readjustments of these estimates, rendered necessary by the change in price"; (2) his comment on the significance of "differences of opinion on the part of different dealers as to the future prospects of supply (or demand)"; and (3) his emphasis on the fact "that it is not the actual profit, but the *expectation* of profit which—*ceteris paribus*—determines the flow of capital to one industry rather than another" (Sidgwick, *Principles,* 186, 193, 198, n. 2 [italics Sidgwick's]).

[95] See, for example, Wicksteed's *The Common Sense, etc.,* 32 ff., 37 f., 88 ff., 93, 110 ff., 212, 214, 219 f., 228, 231, 234 ff., 244 f., 257, 262, 264, 272 ff., 278 ff., 370 ff., 375 ff., 380, 385, 390 f., 393, 403, 419 ff., 437 f., 439 ff., 452 ff., 487 ff., 494 ff., 514, 638 f.; and the articles reproduced in Vol. II (1933 reprint), 761, 797 ff., 820 f. Particularly significant, for our present purpose, are Wicksteed's comments on the *ex ante* character of the "demand curves" of the "general" Theory of Value, and related concepts. For example: (1) "the collective scale [of 'preferences'] registers the estimates not only of the buyers but also of the sellers at reserve prices; . . . vicarious or speculative estimates are to be reckoned in with the rest"; (2) "where there is no indication to the contrary, a curve must be taken to indicate not a history but an anticipation, and an anticipation that has discounted (not necessarily for what they are worth) all conflicting elements, risks, and reactions as far as they come within the ken of the person who makes the estimate"; (3) "a curve must represent the *estimate formed by the consumer* of the value to him of the successive increments of the commodity, and that estimate will be formed in view of all the immediate effects and remoter reactions and implications which he is capable of appreciating [italics Wicksteed's] . . . ; the anticipations on which they rest will never be perfectly justified; but as anticipations they have already made all the necessary discounts, and they need no kind of supplementing or correction . . . ; his [the consumer's] estimates are based upon anticipations which are constantly being checked and modified by experience"; (4) "the collective curve [of "price-and-quantity demanded"] directly represents the facts of the market in the form in which the sellers actually endeavor to *estimate* them" (italics mine). The quotations are from *The Common Sense,* etc., 212, 419 f., 437 f., 497.

vided by Auspitz and Lieben.[96] For it was a striking feature of the exposition of Auspitz and Lieben that they repeatedly brought into their argument, and provided a special algebraic notation for, those "expected, future prices" and "expected price *changes*" which individuals may be counted upon to take into account in making their decisions with respect to present market action.[97] And there is the further example provided by Mises, whose discussion of the rôle of uncertainty and "estimate" in the calculations underlying the pricing process, like so many other elements in Mises's general theoretical position, may be said to follow directly in the path traced by Menger.[98]

[96] For Auspitz and Lieben's own statement of the relation of their work to the treatment of subjective elements in the theory of value by Jevons, Walras, Menger, Wieser, and Böhm-Bawerk, see their *Untersuchungen über die Theorie des Preises* (1889), pp. xi ff.

[97] See, for example, the *Untersuchungen über die Theorie des Preises,* 274, 276, 278, 280, 282 ff., 287, 297, 299 f., 304, 309 f., 318 ff., 328, 346 f., 349, 352 f., 452 f., 458, 460, 507 ff., 515, 538 f. From these passages it should be clear that even if the "explicit introduction" of "anticipations" into the theory of pricing is dated no further back than their "explicit introduction" into our *equations* by means of a special *algebraic notation,* and even if an emphasis on *realized* "rates of change" in prices is interpreted as involving an emphasis on the *expected* course of prices, this "introduction" of "anticipations" is not due solely to "the writings of the econometrists" of "recent years," such as G. C. Evans and C. F. Roos. (See, for example, Evans's *Mathematical Introduction to Economics* [1930], 36 ff., 143, and Roos's *Dynamic Economics* [1934], 62 ff.; also the references to the earlier papers of Evans and Roos, beginning with a paper by Evans published in 1925, which are given on p. 65 n. of the book by Roos just cited.) Contrast, in this connection, the statement of J. R. Hicks cited in Volume I, p. 48, n. 27, of the present work; and cf. also the comment made above, p. 179, n. 72, on Hicks's statement when judged from the standpoint of an adequate history of the rôle assigned to the element of "expectation" in economic analysis.

[98] See, for example, Mises's *Theory of Money and Credit,* 39 f., 97, 131 ff., 163 f., 190, 203 f.; and cf. the general comment by Professor Robbins on Mises's treatment of "uncertainty" and the consequences of "absence of foresight," in the former's Introduction to the work cited, p. 12. The continuity of the emphasis, within the Viennese tradition, on the *prospective* character of economic calculations *("das In-die-Zukunft-gerichtet-sein der Wirtschaft")* is further evidenced by the insistence, by other contemporary members of the Viennese group, on the proposition that "every act is [that is, rests upon, or involves] a forecast," and their reference of this contention to the fundamental proposition that "what are involved are always *expected* wants" (see O. Morgenstern, *Wirtschaftsprognose* [1928], 36, and the references there given to H. Mayer and P. N. Rosenstein-Rodan). Cf. also Rosenstein-Rodan, "The Role of Time in Economic Theory," *Economica,* New Series, I (1934), 80 ff., and the references to other writers there given; and on the rôle of "expectation" and "anticipation" in economic analysis generally, as viewed by a member of the "younger" Viennese group, see Morgenstern, *Wirtschaftsprognose,* 94 f., 104 f., 106, and the same author's "Das Zeitmoment in der Wertlehre," *Zeitschrift für Nationalökonomie,* V (1934), 447 ff.

When all is said, however, it is Marshall whose treatment of the issues under consideration may be regarded as crucial for the purpose in hand. For it was Marshall's work that finally led to the widespread use of those "demand schedules for particular commodities" whose meaning is here under discussion; and it was his work also that is rightly regarded as having incorporated, in its treatment of certain problems, most of the best that was to be found in the divergent strands of earlier doctrine with respect to these problems. One of these strands was represented by the Jevonian emphasis on the rôle of "futurity" and "prospectiveness" in the pricing process.[99] It is not surprising to discover, therefore, that certain of the relevant parts of Jevons's argument were taken over bodily by Marshall.[100] To be sure, true to his canon of generosity in the treatment of his "classical" predecessors, Marshall was equally insistent upon interpreting a writer such as J. S. Mill, for example, in such a way as to emphasize more sharply the *ex ante* and "expectational" character of Mill's treatment of "supply and demand." [101] In so doing,

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[99] See above, p. 182, and the references given in nn. 76–78 thereto.

[100] See, for example, Marshall's *Principles,* 119 ff.

[101] See, for example, Marshall's paper on "Mr. Mill's Theory of Value" (1876), where (1) he alleged that Mill regarded his "Law of Cost of Production" "as operative only as a result of, or corollary from, the law according to which the action of the producers of a commodity is governed by their *calculations of the circumstances of the future supply and demand in the market*"; (2) he held that Mill's "use of the terms 'supply' and 'demand'" implied that "the circumstances of a market determine the particular exchange value, the *expectation* of which will suffice to induce producers to supply on the average any particular amount of a given commodity during a given period"; and (3) he characterized Mill's "account of market value" as resting on the contention that "the amount which dealers offer for sale at any particular value is governed by *their calculations of the present and future conditions* of the markets with which they are directly and indirectly connected" (pp. 127 f., 130 of the *Memorials of Alfred Marshall* [italics mine]). There can be no doubt that in this case Marshall's generosity in the interpretation of earlier writers was not misplaced. See, for example, the following passages in Mill's *Principles*: (1) Book III, Chap. III, sec. 1, on the *"expectations* of profit," rather than "profits," as the element that is subject to "equalization"; (2) sec. 2 of the same chapter, on there being no need of an "actual alteration of supply" in order to bring about a lowering of price ("the mere possibility often suffices; the dealers are aware of what would happen, and their mutual competition makes them anticipate the result by lowering the price" [cf. also Book III, Chap. IX, sec. 3]); (3) Book III, Chap. XXIV, sec. 1, on "the multitude of circumstances which, by influencing the *expectation* of supply, are the true causes of almost all speculations, and of almost all fluctuations of price"; and (4) sec. 2 of the same chapter, on the rôle played by the "expectations" of dealers with respect to *prices,* in particular, in an "ascending period of speculation" and the ensuing "revulsion" (pp. 451 f., 453 f., 504, 652 ff. of Ashley's edition of the *Principles* [italics mine]). In connection with point (4), in particular, see also Mill's "The Currency Question" (a review of Tooke and Torrens), *Westminster Review,* XLI

he provided an indication of what his personal reaction might have been to the suggestion, in recent years, that the history of the rôle of "expectation" in economic analysis may be said to have begun with his own work on the subject.[102] But if Marshall did not inaugurate the subject, he certainly continued it.[103] And in view of the fact that the

(1844), 585, 589, on the "anticipation" of prices as the basis on which "all dealers . . . necessarily ground all their transactions with one another." The best proof, indeed, that Marshall was not reading too much into Mill on this head is that the latter's emphasis on the "expectational" element, as typified by some of the passages just cited, was seized upon for discussion, either favorable or unfavorable, by writers other than Marshall. See, for example, the citation of Mill's comment on the effects of "potential" as well as "actual" alteration of the supply, in F. A. Walker, *Money* (1878), 246 (the juxtaposition of Mill's "Theory of Value" with that of Jevons [p. 245] is particularly interesting in this connection). For a discussion of Mill's emphasis on the *"expectations* of profit" as opposed to realized "profits," in terms strikingly similar to those used by writers of our own day who have seen, in the introduction of the element of "expectations," destructive consequences for most of the core of received economic theory, cf. T. E. Cliffe-Leslie, *Essays in Political Economy*, 49, 184 of the second edition (1888) (the essays in question were first published in 1874 and 1876, respectively).

[102] See, for example, the quotation from Ohlin given above, p. 180, n. 72. Cf. also J. R. Hicks, "Wages and Interest: The Dynamic Problem," *Economic Journal*, XLV (1935), where the comment (p. 460, n. 2) that Marshall's emphasis on *"probable,"* as opposed to realized, magnitudes occurs in "the last sentence of the book" (Marshall's *Principles*) might suggest that at this point Marshall had opened vistas unknown to his predecessors. To be contrasted with such implications is the treatment of Marshall by Myrdal, himself one of the chief representatives of that "Swedish economics" of "recent years" which has been credited with having provided "forerunners" of the emphasis on "expectations" that is to be found in Keynes's *General Theory* (see above, p. 180, n. 72). Myrdal did, to be sure, give generous recognition to the "expectational" aspect of Marshall's analysis (see, for example, Myrdal's *Prisbildningsproblemet och Föränderligheten* ["The Problem of Price Formation and Change"] [1927], 25 ff.). He explicitly refrained, however, from generalizations of the type quoted above from Ohlin with respect to what "other writers" had had to say on the central problems involved (see particularly, in this connection, Myrdal's *Prisbildningsproblemet, etc.*, p. 11, n. 3).

[103] See the quotations from Marshall's *Principles* given by Myrdal, *Prisbildningsproblemet, etc.*, 25 f. (the principal page references to the eighth edition of Marshall are 372 ff., 377 f., 400 [Marshall's references, at this point, to Adam Smith and von Thünen on the subject of "uncertainty" are typical of his refusal to claim a "revolutionary" character for his analysis], 613 f., 618 f.); and see, in addition, pp. 92 f., 112, 119 ff., 332 ff., 337 f., 362, 456 f., 493 f., 711 of Marshall's *Principles*. Cf. also (1) Marshall's paper on "Remedies for Fluctuations of General Prices" (1887) (pp. 190 f. of the *Memorials of Alfred Marshall*); (2) his *Economics of Industry* (1879), 158 ff.; and (3) his general comment on "prospectiveness" as one of the factors which "mainly affect the demand for and the supply of wealth," in his letter to Professor Cannan of January 7, 1898 (*Memorials*, 405). In

demand schedules of the "general" Theory of Value whose continued validity is here under discussion came into widespread use precisely as the result of Marshall's work, it is of considerable importance to call attention to the fact that Marshall's own discussion of these schedules leaves no doubt whatever as to their *ex ante* and "expectational" character.[104]

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view, moreover, of Mr. Keynes's comments, in one of the chapters of his *General Theory* devoted to the subject of "Expectations" (148 ff.), on the alleged failure of "economists" to do full justice to "the *state of confidence*," it is proper to point here to the rôle assigned to "confidence" by Marshall in the explanation of fluctuations in output and employment as a whole. (See, for example, Marshall's *Principles,* 711, and the reference to his earlier *Economics of Industry* there given.)   And it is equally proper to point not only to (1) the very important rôle assigned to "confidence" and the "expectations" of business men by representatives of "old" Cambridge, such as Pigou and Lavington (see, for example, the index to Pigou's *Industrial Fluctuations,* 376 and 381, under "Confidence, business," and "Expectations of business men," and Lavington's *The Trade Cycle* [1925], Chaps. IV and V, on "Business Confidence"); but also to (2) the fact that both writers avowedly based their own analysis upon Marshall's comment with respect to the rôle of "confidence" (see the references to Marshall in Pigou, *Industrial Fluctuations,* 78, and in Lavington, *The Trade Cycle,* 60).   It should be added, however, that an emphasis on the rôle of "confidence" and the "expectations of business men" in the theory of output and employment as a whole, instead of being original with Marshall, or even with those, like Jevons and Wicksteed, who emphasized the "subjective" elements in the economic process (see, for example, Jevons, *Investigations in Currency and Finance,* 184, 195 f., and his *Primer of Political Economy,* 116 ff., and Wicksteed, *The Common Sense of Political Economy,* 638 ff.), was one of the elements which bulked largest in earlier writings on the subject, including the writings of many of the stricter "classicals."   Precisely the same thing can be said with respect to the degree of understanding evidenced by earlier writers of the relevance, to economic problems, of the element of "confidence" and "expectation" by way of "its important influence on the schedule of the marginal efficiency of capital" (contrast Keynes's *General Theory,* 149).   The evidence for these two propositions is so extensive, however, that it cannot be presented here.

[104] See, for example, Marshall's *Principles,* 332 ff., 337 f., 456. The fact that Marshall, like Wicksteed (see above, p. 188, n. 95), intended that the effect of "expectations" should be *included* in his curves of demand and supply has been recognized by a number of recent writers, even if some of them have insisted that his statements on this head did not make an organic part of his "pure theoretical system" (so, for example, V. Domenidò, "Considerazioni intorno alla teoria della domanda," *Giornale degli economisti,* LXXIII [1933], 44, n. 2). In this connection, cf. G. Tintner, "Monopoly over Time," *Econometrica,* V (1937), 160 f., on "the Marshallian fashion" of dealing with "economic expectations and anticipations"; cf. Hicks, *Value and Capital,* 117, on "the method of Marshall" as amounting to allowing "deliberately for the fact that supplies (and ultimately demands too) are governed by expected prices quite as much as by current prices"; and cf. also p. 120 of the same work. Only formalistic significance, if any,

It is on the basis of these clear facts of doctrinal history that one is justified in characterizing as exceptions to the rule the few instances in which even some of the abler among the earlier writers on our subject, such as Cournot, can be charged with having advanced definitions of "demand curves" which tended to obscure the fact that they are to be regarded as *ex ante* concepts.[105] This was, indeed, not altogether surprising, in view of the fact that Cournot himself (in the words of Jevons) had "commenced with the phenomenal laws of supply and demand," and did not "recede" to an *explanation* of these "phenomenal laws" in terms of the *ex ante* calculations of the economizing individuals whose realized actions provide the material on which these "phenomenal laws" are based.[106] In view, however, of the rôle played in the general Theory of

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therefore, is to be assigned to the statement that while the "reaction of sales to rising and falling prices" and to expectations with respect to further rises and falls "had been noted by the classical economists" (including Marshall), "they had never attempted to take account of this disturbing factor directly in the demand equation" (so R. H. Whitman, "The Statistical Law of Demand for a Producers' Good as Illustrated by the Demand for Steel," *Econometrica,* IV [1936], 139; cf. also, in this connection, what is said with respect to Auspitz and Lieben, above, p. 189, n. 97). The *substance,* at any rate, of the proposition that the ordinary demand and supply schedules may be assumed to register the effect of "expectations" is one that can be found in textbooks which made no pretense to have registered an advance in this respect over Marshall's "classical" utterances on the subject. See, for example, Cannan's *Money,* 21 f.: "The terms on which people exchange things [that is, the realized prices resulting from action based on the 'plans' embodied in supply and demand schedules of the Marshallian type] depend, not on what is, but on what the exchangers believe"; "the price of a thing at any moment," therefore, "is constantly influenced by anticipations of what the demand for and supply of the thing is going to be in the future." (It may be observed, in passing, that the context in which these propositions appear is particularly interesting in the light of the suggestion that the rôle assigned to "expectations" in the general Theory of Value has been in some crucial respects different from the rôle assigned to it in monetary theory [cf. Volume I, 481, n. 55, and the reference to Hicks there given].)

[105] It will be recalled that Cournot's definition of a demand schedule (or, as he called it, the "law of demand") involved a relation between price and "the *sales* [*débit*] or the annual demand"; in other words, it related price to "the quantity *sold*" (*Researches,* 47, 51 f.).

[106] See Jevons's *Theory of Political Economy,* pp. xxix, xxxi; and cf. Edgeworth, in Palgrave's *Dictionary,* I, 446. (The statement by H. Cunynghame, *A Geometrical Political Economy* [1904], 8, that "in the fourth chapter [of Cournot's *Researches*] he clearly expounds the law of diminishing utility, and shows that the price which would be offered for an article depends on the utility of the last increment exchanged as compared with money" is quite without foundation.) It should, of course, be clear that Cournot's loose usage is to be regarded as *explained,* rather than *excused,* by the fact that "he began and ended with the collective demand curve, instead of deriving it from the individual curve." (Edgeworth, in Palgrave's *Dictionary,* I, 542). For as long as the "collective demand curve" is re-

Value since Cournot's day by an emphasis on the *subjective* character of the calculations involved in the pricing process, and, above all, on the *prospective* character of these calculations, it is much less easy to excuse those writers of our own day whose definitions of "demand curves" have failed to bring out their *ex ante* character.

It is certainly misleading to suggest, for example, as Joan Robinson has suggested, that "such conceptions as the amount of raw cotton *bought* in the world per year, or the number of motor cars *bought* in England per month, or the number of silk stockings *bought* in Berwick market per day, may be represented by a demand curve." [107]   Nor is

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garded as it should be regarded—namely, as derived from a summation of the *ex ante* "dispositions" of economizing individuals with respect to purchase and sale at a series of assumed prices—it is as much of an *ex ante* concept as these individual "dispositions" themselves. (In this connection, cf. the statement quoted from Wicksteed above, p. 188, n. 95, on the meaning of the "collective curve" of "price-and-quantity demanded," when the curve is considered from the standpoint of the seller. A purely *"ex ante"* character also attaches to the "collective demand curve," obviously, when the latter is regarded as existing "objectively" outside the mind of the prospective seller, in the sense that it is conceived of as derived from an "adding together" of "a series of individual curves of price-and-quantity-demanded," in which all "reactions" due to such factors as changes in the prices of substitutes have been "discounted." Cf. Wicksteed, *The Common Sense, etc.,* 495; and see also the following note.) Yet the looseness of Cournot, whose relating of his "law of demand" to what Jevons called "the ground and nature of utility and value" did not go beyond the statement that the form of the function $F(p)$ "depends evidently on the kind of utility of the article" (*Researches,* 47), is more easily explainable than the looseness of a writer such as Dupuit, who, despite his emphasis on "utility" as the factor lying behind "the phenomenal laws of demand," nevertheless defined the "quantities" in his demand schedules (or, as he called them, "curves of consumption" or "laws of consumption") as the "quantities *consumed*" (pp. 58, 62, 120, 129 of the 1933 reprint of selected essays of Dupuit, edited [1933] by M. de Berardi under the title *De l'Utilité et de sa Mesure*).

[107] *The Economics of Imperfect Competition,* 17 f. (italics mine). The statement quoted follows a definition of "a demand curve" as representing "a list of prices at which various amounts of a certain commodity *will be* bought in a market during a given period of time" (italics mine). It is clear that the ambiguity attaching to the words *"will be"* in this definition is lessened by the statement quoted in the text; but it is clear also that the lessened ambiguity merely succeeds in bringing the definition of "a demand curve" closer to actual error. Contrast E. Chamberlin, *The Theory of Monopolistic Competition* (1933), 12, where the demand and supply curves of the general Theory of Value are described as representing "the amounts which those in the market *stand ready to buy and sell* at different prices" (italics mine). The usage just quoted is clearly superior to that adopted by Chamberlin in a later note (see the *Quarterly Journal of Economics,* LIII [1939], 643), where the demand curve is characterized as representing the amounts people are *"actually able* to sell," in alleged contrast to the supply curve, which is held to indicate "the amounts people are

the matter greatly helped by the suggestion that "the demand curve is an objective conception, looked at from the point of view of the sellers of the commodity," whereas "a *utility curve* is subjective from the point of view of the buyer." [108] For, in the first place, a "demand curve," in the sense of a series of plans to purchase or to refrain from purchasing at a series of assumed prices, may be looked at from the *point of view of the individual buyer, as well as from that of the seller;* and, when it is so "looked at," it is as "subjective" as a "utility curve." [109] In the second place, the "demand curve," *even* when "looked at from the point of view of the sellers of the commodity," may be quite "subjective," in the sense that it may represent nothing more than entrepreneurial *estimates* of sales possibilities at various assumed prices.[110] In the third place, the only type of *analytical* "demand curve" which is properly to be described as "objective" is the type of "collective demand curve" obtained by "adding together" a "series of individual curves of price-and-quantity demanded," of the kind represented by the "plans" of individual prospective purchasers.[111] And even this type of "demand curve" is "subjective" in the sense that it is derived entirely from "subjective" material, and not from *realized market facts,* of the kind implied by Mrs. Robinson's description of "a demand curve" in terms of "quantities *sold*" at various prices.

Unfortunately, moreover, later discussion of Mrs. Robinson's treatment of "demand curves," whatever may be said of this discussion otherwise, has certainly not helped to make clear that the demand curves of the general Theory of Value are properly to be regarded as *ex ante* concepts, whether one has reference to a curve representing the "plans" of prospective purchasers or the estimate of such "plans" which enters into the calculations of the sellers.[112] Enough has been said, for example, to provide a basis for objecting to the use of the adjective "traditional" in the statement that "the traditional 'market demand curve' for a certain product . . . denotes a functional relationship between the price

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willing to, or *stand ready to* sell" (italics Chamberlin's). For a general discussion of the treatment, in recent literature, of the meaning of "demand" curves, from the standpoint of their "subjective" or "objective" character, see R. Triffin, *Monopolistic Competition and General Equilibrium Theory* (1940), 62 ff., 95.

[108] *The Economics of Imperfect Competition,* 18 (italics in the original).

[109] Cf., in this connection, Edgeworth, *Papers Relating to Political Economy,* II, 309; and cf. also above, p. 193, n. 106.

[110] See again the quotation from Wicksteed given above, p. 188, n. 95, under (4).

[111] See above, p. 194, n. 106. On the sense in which a "collective demand curve," of the type indicated, is to be characterized as "objective," see Wicksteed, *The Common Sense, etc.,* 488.

[112] On the necessity for distinguishing between the two types of "demand curve" indicated, see above, p. 178, n. 70; also what is said below, p. 198, concerning the rôle played by the "long run demand curve" in the "plans" of entrepreneurs.

and the amounts *bought*." [113]   The same must be said of the corollary
designation of "the traditional 'market demand curve'" as "the 'real
demand curve,'" in opposition to "the 'imagined demand curve.'" [114]
For, as we have seen, the abler among the earlier users of analytical "de-
mand curves" made it clear that all their "demand curves" were what
would now be called *"ex ante"* concepts: that is, they were to be thought
of as being "imagined" by prospective purchasers as well as by prospec-
tive sellers. [115]   And the fact that recent writers have failed to follow
the example set by the abler among their predecessors in making clear
the *ex ante* character of analytical "demand curves" has not only meant
a failure to profit from the advances made in the general Theory of
Value over a period of generations; it has also tended to block the way
to the establishment of one of the crucial links in the argument by which
the usefulness of the demand schedules of the general Theory of Value
can be shown to be unaffected by the mere fact that their conformation
and position may change between successive realized decisions to pur-
chase or not to purchase.

It should be observed, finally, that a clear recognition of the *ex ante*
character of the demand schedules of the general Theory of Value should
have the effect of salvaging, and of protecting against abuse, a concept
which would otherwise seem to be particularly exposed to the charge
that when an attempt is made to apply "Marshallian curves" to periods
over which they are virtually certain to change in conformation or posi-

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[113] So N. Kaldor, in *Economica*, August, 1934, pp. 340 f.

[114] So Kaldor, *loc. cit.* and also in the same author's later article, "Market
Imperfection and Excess Capacity," *Economica*, New Series, II (1935), p.
40, n. 1; cf. also P. M. Sweezy, "Demand under Conditions of Oligopoly,"
Journal of Political Economy, XLVII (1939), 568 ff. From the argument in
the text, it should be clear that the objection which is made here is to
the terminology employed, and particularly to the identification of the con-
cept of a "real demand curve"—defined as relating price to "the amounts
bought"—with "the traditional 'market demand curve.'" The objection
is not to the suggestion that we should distinguish between the "demand
curve" as it presents itself to the mind of the prospective seller, on the
one hand, and, on the other, the "demand curve" as it presents itself to
the mind of the individual prospective buyer, or as it may be constructed
on the basis of individual "demand curves" of the latter type.

[115] In this connection, cf. the comment of M. Bronfenbrenner, "Applica-
tions of the Discontinuous Oligopoly Demand Curve," *loc. cit.*, 420, n. 2,
on the "imaginary" character of *"all* demand curves." (The adjective ap-
plied to these curves by Irving Fisher, it may be recalled, was "hypo-
thetical." See Fisher's *Elementary Principles of Economics*, 267.) It
should be added, moreover, that the "plans" represented by *ex ante* de-
mand curves of the prospective buyers may be so completely out of line
with the "plans" represented by the relevant *ex ante* supply curves of the
prospective sellers that no transactions will be actually realized. This fact
makes it impossible to say even that *"one* point" of these "imaginary"
demand curves is necessarily "observable at any period in time" (cf. Bron-
fenbrenner, *loc. cit.*).

tion, or both, these "Marshallian curves fail beyond remedy." [116] The concept in question is that of a *"long-run* demand curve." [117] In any realistic sense of the term, the "long run," in the world we know, is bound to be a period over which the conditions of demand represented by a demand schedule may be expected to change. It is proper to ask, therefore, as some writers have asked, what realistic significance attaches to the concept of a "long-run demand curve" in a changing world. It is equally proper, however, to point out that the objections to the use of the concept of a "long-run demand curve" disappear when this use is confined to the limits proper to an *ex ante* concept.

Specifically, it can be argued that the *present* market decisions of buyers and sellers are *affected by* their judgment with respect to the course of prices and the probable response of demand to prices over a "long" period.[118] In the case of the present market decision of a *buyer,* the "long-period demand curve" may be taken to represent his plans

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[116] So, for example, Schumpeter, *Business Cycles,* 527. For the purpose of determining the precise scope of Professor Schumpeter's comment, it is of some importance to observe that he applies the same proposition not only to "Marshallian curves," but also to the later developments in the theory of pricing which have generally been regarded as improvements over the type of analytical device represented by the "Marshallian curves." "All," he contends, "break down when production and consumption functions . . . change" (*Business Cycles,* 528). Actually, however, Professor Schumpeter's own usage elsewhere would suggest that his argument amounts to no more than a warning against assuming invariance in the form of a given demand function over time. (See, for example, his comments with respect to the necessity for taking account, in any attempt to trace the successive steps "in the cyclical process," of "shifts and distortions of the schedule of demand for balances" as well as "movements along" such a schedule [*Business Cycles,* 604].) For he himself has not hesitated to speak of "the 'movement along a demand curve'" (see, for example, *Business Cycles,* 530, 532, 536); and one could hardly speak of a "movement along a demand curve," or indeed of "demand curves" altogether, if one believed that the concept itself has no realistic meaning.

[117] The distrust of such concepts is no novelty of very recent years. See, for example, J. N. Keynes, in Palgrave's *Dictionary,* I, 541, on the difficulties associated with "demand-schedules" "unless we confine ourselves to very short periods of time." It is significant, however, that in this instance, as in later instances, the difficulties were held to apply to the obtaining of "accurate *empirical* data in regard to the variation of demand with price"—that is, to the derivation of "statistical demand curves" which are *presented as the virtual equivalent of the demand curves of the general Theory of Value* (in this connection, see the comments of Stigler, "The Limitations of Statistical Demand Curves," *loc. cit.,* 477). From the argument that follows, it should be clear that no defense is offered for unwarranted identifications of the type indicated.

[118] On the necessity, generally, for relating "long-period" judgments to *present* market "plans," of the type involved in "market" schedules of demand and supply, in our sense of the term, see what is said below, pp. 238 ff.

for purchase at different possible prices in the future over a period equal in length to that to which these plans apply; and his *present* decision will be related to the long-range "plan" in the way in which military tactics of the moment will be related to the strategy which these immediate tactics are supposed to serve.[119] In the case of the present market decision of the *seller,* the "long-period demand curve" will represent that element of "estimation of the future" with respect to the probable demand for a product which must necessarily underlie all decisions of entrepreneurs with respect, for example, to *production* which will take a "long" period of time.[120]

There is no reason to assume, however, that these "long-period" curves—*ex ante* concepts like the "market" demand curves which are directly relevant to the determination of current realized prices—will themselves remain unchanged *in fact* over a period as "long" as that with respect to which consumption decisions, for example, must be made. On the contrary, it is perfectly possible that there will be re-peated revisions of judgments with respect to the probable future response of demand to price over this "long" period. This is merely another way of saying that there may be repeated changes in the con-formation and position of the "long-run" demand curve.[121] Nor is it

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[119] There is nothing in this statement, it will be observed, which con-tradicts the proposition that this long-range plan may not involve a "nice calculation on the part of the individual as to his future income and re-ceipts," or the proposition that the individual purchaser really "creeps along from purchase to purchase and only at these individual acts does he estimate his needs and abilities" (cf. Fisher, *Mathematical Investigations in the Theory of Value and Prices,* 20). All that is argued is that *to the extent that* the purchaser *does* allow his future purchase plans, however "fluid" (cf. the quotation from Wicksteed given above, p. 174, n. 63), to affect his present action, these future purchase plans may be represented by a "long-run demand curve" which can be shown to affect the *"market* demand curves" representing the purchase "plans" prevailing at the moment of bargaining; just as the military tactics of the moment require for their full explanation knowledge of a broad, "long-run" strategic plan *to the ex-tent that* there is evidence that such a "long-run" plan does exist and does condition present tactics.

[120] It has been suggested by Joan Robinson (*Economics of Imperfect Competition,* 23) that such a "conception" of a "long-period demand curve" is "distressingly vague." From the context, however, Mrs. Robinson's meaning would not appear to be that the conception is *analytically* "vague," or that it corresponds to nothing that really enters into the mind of the entrepreneur (cf., for example, her statement [*loc. cit.*] that "it is obviously some such conception which an intelligent entrepreneur must have in mind when he is deciding what price policy to adopt"). On the contrary, the "vagueness" to which Mrs. Robinson refers would seem to be merely an-other name for that quality of being *subject to change* which Wicksteed called "fluidity" (see again above, p. 174, n. 63). On "long-run demand curves" as an element in the calculations of *entrepreneurs,* see also what is said below, p. 238, n. 35.

[121] The problem is in some respects similar to that involved in the

necessary to make any such assumption of *de facto* constancy in the "long-period demand curve," once the truth of two propositions is recognized. The first proposition is that "long-period demand curves" affect realized market prices on the side of *demand* only insofar as they affect "market" demand curves, in our sense of the term.[122] The second proposition is that the mere fact that the market demand curve may change in position or conformation between successive realized decisions to purchase or to refrain from purchasing does not invalidate the use of the concept of market demand curves for accounting for changes in the structure of realized prices—whether the changes in the market demand curves are due to changes in "long-run" expectations with respect to future price and the future response of demand to price, or to any reason whatsoever.

6. In order, therefore, to demonstrate that the Marshallian concept of elasticity of demand for particular commodities is unable to contribute to an explanation of the different degrees of price change shown by these particular commodities, it would be necessary to demonstrate that no significance whatever, for the problem of relative price change, attaches to two related sets of facts. The first set

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relation between the conception of "monotonically descending cost curves," on the one hand, and "cost curves that shift under the impact of innovation," on the other. See Schumpeter, *Business Cycles*, 91; and cf. Marshall's *Principles*, 463 n., and 809, n. 2. It should hardly be necessary to labor the point that there is no *analytical* objection to the conscious "supposition" of particular conditions with respect to the absence of change or "innovation" over the period to which a "long-run demand curve" (or "supply curve") is held to apply. In this connection, cf. the comments by Fisher on his " 'yearly' utility curve," in his *Mathematical Investigations in the Theory of Value and Prices*, 19 f. The point made here is merely that neither is there any *analytical* objection to the very concept of a "long-run demand curve" on the mere ground that such a curve is likely to *change;* and the further point made here is that this very fact itself makes it possible to use such "long-run" curves in accounting for the present plans, and therefore the present realized actions, of entrepreneurs operating in a changing world.

[122] Such an effect must obviously be present whenever expectations prevailing in any given situation with respect to the *future* market situation are regarded as having economic relevance to the decisions with respect to buying and selling which are required in the present situation. On this matter, see what is said below, pp. 238 ff. It should be clear, however, that the argument, as stated in the text, amounts to an insistence upon *distinguishing* between the "plans" embodied in the "long-period demand curves," on the one hand, and those embodied in the "market" demand curves, on the other. For to say that the former set of "plans" may be expected to *affect* the latter set is not to say that they are *identical* with them. Cf. Haberler, *Prosperity and Depression*, second edition, 191 n. (the last sentence).

is typified by the phenomenon (familiar, as we have seen, to monetary theorists at least since the days of Locke) that the response of individuals' demand for bread, for example, to a given change in the price of bread at a given time may be quite different from the response of individuals' demand for luxury goods to given changes in the prices of these luxury goods at the same time. The second type of fact is one whose "reality" is assumed by the fundamental postulate that must be said to underlie *all* widely accepted variants of the "general Theory of Value": namely, the "fact" of economic calculation by individuals, whose purchase "plans" (or "dispositions," or "resolutions") can be, and have been, represented by Marshallian demand schedules, with their special property of elasticity. The *relation* between these two sets of facts, in turn, is provided by recognition of the proposition that the first set of facts (namely, the observed differences in the response of "demand" to given degrees of price change in the case of different types of commodities) represents precisely the facts to which the Marshallian concept of "elasticity of demand" was designed to call attention.[123]

7. The conclusions, then, are: (1) after all possible allowance is made for changes in the conformation of a given demand schedule for a particular commodity as the result of changes elsewhere in the price structure, the relative differences in the amounts of *different* commodities purchased at any given time in response to a given degree of change in the price of each commodity may be, and in fact are seen to be, different; (2) in order to account for these differences in the actual response of demand to a given change in price,

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[123] It should be observed that, quite apart from the connection established by the facts of doctrinal history, the proposition just stated may be said to follow from the simple fact that the use of an elasticity of demand equal to unity as the dividing line between "elastic" and "inelastic" demands is itself a mere convention, which for many problems is much less significant than another type of dividing line that may be suggested by the terms of the particular problem being analyzed. (An example is provided by the problem of the effect, upon *employment,* of cost- and price-reducing inventions, under varying conditions of "elasticity" in the demand for the product whose price is thus reduced.) This, obviously, is merely another way of saying that what is really important is the different degrees of "elasticity" shown by the respective demands for *different commodities.*

one of the analytical devices of which use must be made is that represented by *ex ante* demand schedules of the Marshallian type; and (3) the differences in the amounts of the purchases of different commodities thus observed and accounted for are themselves of the utmost significance in accounting for *changes in the structure of money prices.*

From these conclusions it follows, in turn, (4) that Mr. Keynes has certainly not demonstrated that the Marshallian concept of elasticity of demand for particular commodities is unable to contribute to an explanation of changes in the structure of money prices and, therefore, to an understanding of the consequences of such changes: any more than he has demonstrated the proposition that, in attempting to judge the comparative effect upon the absolute volume of employment in *different* industries, no light is thrown on the problem by considering differences in the elasticity of the demand for the products of those industries and therefore, in some degree, in the elasticity of demand for the particular types of labor involved in those industries.[124] On the contrary, his own use of the otherwise perfectly familiar proposition that "the demand schedules for particular industries can only be constructed upon some fixed assumptions as to the nature of the demand and supply schedules of other industries" represents an attempt to prove too much. And in attempting to prove too much, he has renounced prematurely the use of a set of analytical devices, developed originally within the "general" Theory of Value, which must

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[124] It may be observed again that Mr. Keynes's own way of dealing with the latter problem amounts to ignoring it altogether. His implied justification for doing so is, again, that he is interested in the nature of the forces determining the level of output and employment *as a whole.* His own argument rests on the implied assumption that, in explaining movements in employment as a whole, the only way in which one could even contemplate using the concept of an elasticity of demand for particular types of labor within particular industries is by way of a mechanical extension, to "industry as a whole," of the concept of elasticity of demand for the products of particular industries and, therefore, for the particular types of labor used within these industries. See, however, what is said above, pp. 160 f., concerning (1) the rôle played in the determination of the level of output and employment in "industry as a whole" by the *structure of money prices;* and (2) the rôle played in the determination of the latter by the "demand schedules for particular industries," as these schedules appear in the "general" Theory of Value.

represent an indispensable part of any apparatus designed to account for the forces actually determining money prices.

IV

Marshallian Elasticity of Demand and "The Amount of Aggregate Effective Demand"

From the argument of the *General Theory*, it is clear that Mr. Keynes regards as much more important the second of the two limitations on the use of "demand schedules for particular industries" in accounting for changes in the structure of money prices: namely, that which follows from the fact that these demand schedules "can only be constructed upon some fixed assumptions . . . as to the amount of the aggregate effective demand." For, according to Mr. Keynes, it is this fact which makes it necessary "to introduce quite new ideas when we are dealing with demand as a whole and no longer with the demand for a single product taken in isolation, with demand as a whole assumed to be unchanged." [125]

It is to be observed that here also, as in the case of the first limitation on the use of "demand schedules for particular industries" stressed by Mr. Keynes, he was not the first "heretic" to raise the objection indicated. This second limitation was also stressed, for example, by J. A. Hobson, whose proposition that "the play of elasticity of demand is . . . inhibited by falling incomes" might easily be taken as providing a confirmation in detail of the general similarity in the points of view of the two writers.[126] In fairness to Hobson, however, it should be pointed out that two circumstances combined to make the consequences of the proposition just quoted much less serious than those following from Mr. Keynes's use of a similar proposition. In the first place, neither Hobson's strictures upon economists for having failed to effect a satisfactory "synthesis" between monetary theory, on the one hand, and the "general" Theory of Value (or "the Law of Supply and Demand"), on the other, nor his claims for his own "synthesis" were so extreme as those advanced by Mr. Keynes.[127] In the second place,

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[125] *General Theory,* 294 f.

[126] For the proposition quoted, see Hobson's *The Industrial System* (1909), 286. On the "general similarity in the points of view of the two writers," see the comments of Mr. Keynes himself in his *General Theory,* 19 n., 364 ff., 371.

[127] See, for example, Chapter X of Hobson's *The Industrial System.*

Hobson, unlike Keynes, actually continued to make use of the Marshallian "elasticity of demand" in his analysis, despite his awareness of the effect, upon elasticity, of changes in *income* as well as of changes in the prices of other commodities.[128]   There is much more reason, therefore, to confine any criticism of the proposition cited above to the particular use made of it by Hobson in the context in which the proposition appears.[129]   A closer parallel to the spirit of the Keynesian attack, on the other hand, is to be found in the contention, by another contemporary "heretic," that what gives to "equilibrium theory its widely recognized irrelevance" is the alleged fact that it "proceeds unrealistically on the premise that our national 'effective demand' for goods remains constant automatically, and then proceeds to examine minutely how consumers, in accordance with a subjective factor, the 'elasticity of demand,' shift their patronage from product to product as the supply and price of products vary." [130]

In this case also, however, the failure of Mr. Keynes to justify the far-reaching conclusions which he draws from his contentions with respect to the relation between the Marshallian "elasticity of demand," on the one hand, and "the amount of aggregate effective demand," on the other, can be established by setting up a series of counter-propositions:

1. The very suggestion that the ideas thus involved with respect to "demand" are "quite new" itself provides a commentary on Mr. Keynes's own criticism of "economists" for having failed to transfer the "homely but intelligible concepts" of "supply and demand," as the latter appear in the "general" Theory of Value, to the Theory of Money and Prices.   For if anything is clear from the history of the concept of a "general" or "aggregate" *money "demand"* in economic literature, it is this: the leading sponsors of this concept were perfectly aware that, instead of representing a direct transfer of the notions of "demand" of the "general" Theory of Value to the Theory of Money and Prices and of

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[128] For examples of Hobson's continued use of the Marshallian "elasticity of demand," see *The Industrial System,* 169 f., 172 ff.; and for evidence of Hobson's awareness of the effect, upon the elasticity of demand, of changes in *income* and in the prices of other commodities, respectively, see pp. 173 f., of the work cited.

[129] See, for example, the comments by D. H. Robertson on this aspect of Hobson's argument, in the former's *Study of Industrial Fluctuation,* 236 f.

[130] So A. Dahlberg, *When Capital Goes on Strike* (1938), 158 n.

output as a whole, the concept of a "general" or "aggregate" money demand involves the creation of a theory of "demand" which is in a number of important respects *different* from the theory of "demand" as it appears in the "general" Theory of Value.[131] It is Mr. Keynes who, by his criticism of economists for having failed to effect the transfer in question, has helped to obscure the fact that the "ideas" involved *are* "quite new" (that is, *different*) in the sense that they require a concept of demand which would *supplement* the range of ideas associated with the demand curves of the "general" Theory of Value.[132]

2. It was precisely a characteristic of the treatment, by earlier writers, of the concept of a "general" or "aggregate" money "demand" that they were perfectly aware that what was involved was the need for *supplementing* the concepts of "demand," and, in particular, of an "elasticity of demand," as these concepts appear in the "general" Theory of

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[131] See, for example, the remarks by Schumpeter on the *naïveté* involved in a simple extension of "propositions that are correct for individual demands and individual prices" to the concepts of "aggregate demand and the price level," in "Das Sozialprodukt und die Rechenpfennige," *loc. cit.,* 678 f.; and cf. the quotation from Schumpeter's review of the *General Theory* given above, p. 110, n. 48, as well as what is said above, pp. 117, 121. It will be observed, of course, that the concept of an "aggregate [money] demand," as thus used, and as I have used it in the text, is to be sharply distinguished from those usages according to which the term "aggregate demand," "total demand," or "general demand" is applied to the collective demand schedule *for a particular commodity.* (See, for example, Jevons's use of the terms "aggregate supply and demand," as applied to "the total demand for a *certain commodity*," in his *Principles,* 57; and cf. also Marshall's use of the expression "the aggregate demand for any commodity" [*Principles,* 387], and the expression "the total demand *curve*" [*ibid.,* 104 n.], as well as his use of the concept of a "general demand of any one person" *for a particular commodity,* represented by "the aggregate [or *compound*] of his demand for it for each use" [*Principles,* 108 n.; cf. also pp. 387 f., of the same work]. See also the use of the term "aggregate demand *schedule*" in Fisher, *Elementary Principles of Economics,* 278, and the use of the term "aggregate demand" in the same sense by A. L. Bowley, *The Mathematical Groundwork of Economics* [1924], 25 f.) And, of course, the concept of a "general" or "aggregate [money] demand," as used in the text, is to be distinguished with equal sharpness from Wicksteed's concept of a "general demand *curve*" for a *particular commodity* (cf. p. 785 of the 1933 reprint of Wicksteed's *The Common Sense, etc.*).

[132] On the absurdity of characterizing the concept of an "aggregate effective [money] demand" as "quite new" in the sense that the concept may be said to have been *introduced into economic theory only in very recent*

Value, and not of *displacing* them.[133]   If there is anything
"new" in Mr. Keynes's argument, on the other hand, it is
precisely the suggestion that the introduction of the con-
cept of a "general" or "aggregate" money demand destroys
the usefulness of analytical devices such as the Marshallian
"demand schedules for particular industries" for the pur-
poses of accounting for the phenomena of the real world.

3. By the terms of Mr. Keynes's own conceptual construc-
tion, the magnitude of "aggregate effective demand" is to be
associated directly with the magnitude of *income*.[134]   Mr.
Keynes's proposition that "the demand schedules for par-
ticular industries can only be constructed on some fixed
assumption . . . as to the amount of aggregate effective
demand," amounts, therefore, simply to the statement that
such schedules can be constructed only on some assumption
as to the amount of income at the disposal of the "demand-
ers." [135]

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times (or, at best, rescued from the "underworlds" of "Karl Marx, Silvio
Gesell, and Major Douglas" after "more than a hundred years" of
"neglect"), see below, p. 686, n. 13. It cannot be repeated too often, indeed,
that what is "new" about Mr. Keynes's use of the concept of an "aggregate
effective demand," and what is emphasized by his use of the term *"elasticity
of effective demand"* (see below, Chapter Thirteen), is his failure to make
clear that the use of such concepts does not, or should not, imply the
transfer to monetary theory of the "homely but intelligible concept" of
"elasticity of demand" as the latter concept appears *in the "general" Theory
of Value*, but that the use of such a concept does, or should, represent a *sup-
plementing* of the latter by a device familiar for generations within monetary
theory.

[133] It should be sufficient, in this connection, to call attention to the
cases of Tooke, Wicksell, and Hawtrey, all of whom made explicit use
of *both* the concept of an "aggregate [money] demand" *and* the Marshal-
lian "elasticity of demand," or its equivalent, in their analysis of the
processes by which money prices are determined. See above, pp. 94, 117,
121, 122, 148 ff.

[134] For our present purpose, it is sufficient to call attention to Mr.
Keynes's two expressions for "income-velocity": namely, $V = Y/M_1$ and
$V = D/M$. (See the *General Theory*, 201, 304. The differences between
M_1 and M—the two denominators, respectively, of the ratios indicated—
are not important here. Cf. what is said on this matter below, p. 678, n.
2, and the forward references there given.) On Keynes's treatment of
the relation between "income" and "outlay from income," which is certainly
relevant to the question of the relation between "income" and "demand"
when these two concepts are used in such a way as to bring out clearly the
successive steps involved in the generation and utilization of money income,
see below, pp. 694 ff.

[135] Cf. Keynes himself, in his *General Theory*, 281.

4. As in the case of Mr. Keynes's first objection to the use of "demand schedules for particular industries" in accounting for changes in the structure of money prices, it can be shown that the principal sponsors of these "demand schedules," such as Cournot and Marshall, were perfectly aware of the fact that the conformation of the schedules, and therefore their degree of "elasticity," depends upon the facts with respect to *income* and therefore upon changes in the amount and the distribution of such income.

Indeed, it must be said at once that if the writers indicated had been unaware of this, they would have been guilty of a degree of retrogression, as compared with what was already available in the economic literature of their own day on the subject of "demand," for which it would be difficult to find parallels. For it is a striking characteristic of the history of discussion of the rôle of "demand" in the "general" Theory of Value that the influence of *income,* and its distribution, on the "demand" for particular commodities took hold much earlier than did that concept of a functional relation between the price of a commodity and the amount of such a commodity demanded which is typified by the Marshallian "demand schedule for a particular industry" and its property of "elasticity." [136] And it is equally striking that when the concept of a functional relation between price and quantity demanded did begin to take hold, as in the case of Say and Tooke, the influence on demand of the amount and the distribution of income was taken explicitly into account.[137] It is not at all surprising, therefore, that Jevons, for ex-

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[136] No one could have been more explicit than Adam Smith, for example, in arguing that among the things on which "the market price of goods depends" are "the riches or poverty of those who demand." See Smith's *Lectures on Justice, Police, Revenue, and Arms,* 177 f.

[137] Say, to be sure, is not usually cited as one in whose writings "the concept of a functional relation between price and quantity demanded began to take hold." See, for example, Schultz, *Theory and Measurement of Demand,* 5, n. 1, and cf. Cournot, *Researches,* 44 f. Cf., however, Say's *Treatise,* p. xxvii n.: "To determine the *quantity* to be *demanded,* the price at which the commodity can be sold must already be known, as the demand for it will increase in proportion to its cheapness" (italics Say's); and see especially Say's *Cours,* Part III, Chap. IV (pp. 166 ff. of the second [1840] edition), with its diagram of the "pyramid" of demand, in which "the number of consumers" of a given product is represented as a function of the price. For our present purpose, on the other hand, it is more important to observe that Say himself immediately added, in his *Treatise,* that "we must also know . . . the *means* of the consumers, as various as their persons," and, further, that "their ability to purchase will vary according to the more or less prosperous condition of industry in general, and of their own in particular"; while in his *Cours* one of the principal purposes of his "pyramid" was to show how the quantity demanded of a given product

ample, was careful to point out that so far as "the quantity of commodity demanded at a price" is concerned, "one main element of the matter must be the comparative numbers of persons of different rates of income living in the community." [138]

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would vary with the number of individuals in given *income* groups. Tooke's use of "the concept of a functional relation between price and quantity demanded" was discussed above (pp. 148 ff.), in connection with the history of the use of the Marshallian elasticity of demand, or its equivalent, to account for changes in the structure of money prices. Attention may therefore be called here to Tooke's emphasis on changes in the demand for different types of commodities as a result of changes in the *income* of the potential consumers of those commodities. See, for example, Tooke's *Thoughts and Details,* II, 24, on the effect of a reduction of income, by direct taxation, on the quantities demanded of luxuries and necessities, respectively; and on the general matter of the influence of changes in income upon the demand for particular commodities, see Tooke's *Thoughts and Details,* III, 91; *Considerations on the State of the Currency,* 117; *History of Prices,* I, 13; IV, 418. It may be recalled also that J. S. Mill, despite the charge that "he had no consistently clear notion of a demand function" (Schultz, *Theory and Measurement of Demand,* 6, n. 3), nevertheless did have a sufficiently "clear notion" to warrant his inclusion in the list of earlier users of rough equivalents of the Marshallian elasticity of demand (see above, p. 149, n. 17; and cf. the comment of Marshall, in his paper on "Mr. Mill's Theory of Value" [*Memorials of Alfred Marshall,* 129, and n. 2 thereto], on the significance of Mill's statement that "the quantity demanded 'varies according to the value'"). It is worth observing, therefore, that Mill specifically included the *"means . . . of purchasers"* among the elements involved in the response of these purchasers to a given change in price (*Principles,* Book III, Chap. II, sec. 4; p. 447 of the Ashley edition). This is, of course, by no means a complete list of instances in which pre-Jevonian and pre-Marshallian writers who made use of what amounted to "the concept of a functional relation between price and quantity demanded" also showed themselves to be clearly aware of the rôle played by *income* in the determination of this "functional relation." See, for example, the comments of F. W. Newman, *Lectures on Political Economy* (1851), 84 ff., on the relation between "the *ability* of the buyer" to purchase, on the one hand, and, on the other, the extent to which the quantity demanded may be expected to "vary with the price." (For examples of Newman's recognition of what amounts to the phenomenon of elasticity of demand, in the Marshallian sense, see also pp. 85, 117, 175 of the work cited; and for examples of his recognition of what has come to be called the "income effect" [cf. below, pp. 218, 298 ff.], see pp. 87 f., 115, 117 of the same work). See also F. Bowen, *The Principles of Political Economy* (1856), 426 f., where the substance of one of Tooke's uses of what amounts to the concept of "elasticity of demand" was juxtaposed with the familiar proposition with respect to "the *ability* to purchase" as well as "the disposition to purchase" as elements necessary "in order to constitute an *effectual* demand" (italics Bowen's).

[138] Jevons, *Principles,* 58; cf. also p. 146 of the same work. From these passages it is clear that, whatever may be said against Jevons's exposition in his earlier *Theory of Political Economy,* he would certainly have been prepared to assent to Marshall's corrective proposition that "the price

What would have been surprising, in the light of these precedents, would have been a failure on the part of the chief sponsors of the concept of "demand schedules for particular industries," such as Cournot and Marshall, to recognize that the conformation of these schedules, and therefore their relative "elasticities," would in all cases be affected by the specific conditions prevailing with respect to the amount and the distribution of the social income. As it happens, however, the facts with respect to the treatment of the problem by both Cournot and Marshall are such that it is impossible to suggest that either of them "really supposed that people's demands for commodities do not depend on their incomes." [139]

In the case of Cournot, for example, it may be observed that in the very same paragraph in which he presented the concept of a "demand D . . . for each article" as "a particular function $F(p)$ of the price

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which the various purchasers in a market will pay for a thing is determined not solely by the final degrees of its utility to them, but by these in conjunction with the *amounts of purchasing power* severally at their disposal" ("Ricardo's Theory of Value," Appendix I to Marshall's *Principles* [p. 818]).

[139] Cf. the comment on Marshall by Hicks, *Value and Capital*, 27; and see below, pp. 210 ff. The cases of Cournot and Marshall are discussed in some detail below because of their generally recognized position as the outstanding sponsors of the concept of "demand schedules for particular industries." They are, of course, by no means the only users of the latter concept who have evidenced an awareness of the bearing of the facts with respect to *income* and its distribution upon the problem in hand. H. Staehle, for example, in his article on "Short-Period Variations in the Distribution of Incomes," *loc. cit.*, 134, cites the name of H. von Mangoldt (1863) along with Cournot and a number of *post*-Marshallian writers (the number of the latter could, of course, be greatly increased: see, for example, Wicksteed, *The Common Sense, etc.*, 483 f., 490 f.). The list of earlier writers presented by Staehle, however, is by no means exhaustive. In addition, for example, to the references to earlier writers given above, p. 206, n. 137, see (1) Fleeming Jenkin, *The Graphic Representation, etc.*, 79, 81, 92, on "the funds available for purchase . . . , which may be called the purchase fund," as a factor which "at each price limits the possible demand," so that an increase in this "purchase fund" may be expected to change the position of the relevant portion of the demand curve; (2) Cairnes, *The Character and Logical Method of Political Economy*, 116 ff., 124, where "the two conditions" affecting what would now be called elasticity of demand were held to be "1st, the disposition of the people . . . to sacrifice other gratifications which it may be in their power to command to the desire of obtaining their usual quantity" of the commodity whose price has risen, "and 2nd, the extent of the *means at their disposal* . . .—that is to say, their *general purchasing power* . . . and *the mode of its distribution amongst different classes*" (italics mine); (3) Sidgwick, *Principles of Political Economy*, 188, on changes in "the amount of wealth in any community," and in "the manner of its distribution," as elements leading to continual variation in the form of the demand curve for any given commodity (or, as Sidgwick put it, in the "scale of variations in demand for

of such articles" (in other words, the concept of a "demand schedule" for a particular article, or, as Cournot called it, the "law of demand" for each article) he pointed out that among the elements on which the "form of this function" would depend is "the average wealth, and . . . the scale on which wealth is distributed." [140] That such a passage, moreover, does not represent an isolated instance of a recognition by Cournot of the relation between his "laws of demand" for "each commodity by itself" and what Mr. Keynes calls "the amount of the aggregate effective demand," as represented by the total of "income" in a community, is particularly clear from an examination of Chapter Eleven of Cournot's *Researches,* which was entitled precisely "Of the Social Income"; for this chapter was intended to show what would happen as soon as one abandons the assumption that not only "the prices of other commodities," but also the "incomes of other producers," could be "considered as given and invariable." [141] In fact, Cournot argued, "an

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any given commodity that would result *ceteris paribus* from any given series of variations in its price"); and (4) Auspitz and Lieben, *Untersuchungen über die Theorie des Preises,* 46 ff., where commodities were divided into three groups according to the degree to which their respective responses of demand to price change may be expected to be affected by the facts with respect to income and its distribution (see also pp. 60 ff. of the same work, on "The Influence of Property Relationships"—that is, of the distribution of income—upon the response of demand to changes in the prices of particular commodities). On the rôle of income in the theory of demand for particular commodities generally, see especially, however, Fisher's *Mathematical Investigations in the Theory of Value and Prices,* 44 ff. It may be observed that the relevant passages in both Auspitz and Lieben, on the one hand, and Fisher, on the other, were cited by Edgeworth in connection with the concept of elasticity of demand. See Edgeworth's article "Elasticity," in Palgrave's *Dictionary,* I, 691. And it may be observed, finally, that Pareto's famous suggestion with respect to the actual distribution of income was first presented in connection with the problem of the derivation and the interpretation of the so-called Law of Gregory King—in other words, the type of phenomenon with which the Marshallian "elasticity of demand" was intended to deal. See Pareto's "La legge della domanda," *Giornale degli economisti,* X (1895), 59, 64, 67; and cf. the summary of the use made by R. Roy of Pareto's "Law" in connection with the "law of demand," given by Schultz, *The Theory and Measurement of Demand,* 120 ff.

[140] Cournot, *Researches,* 47. See, in addition, p. 52 of the same work: "The law of demand may . . . vary . . . if the country experiences a movement of progress or decadence"; and cf. Cournot's *Principes,* 93, on "the pecuniary means" of the prospective purchaser as a factor which will determine his response to a given price, as well as p. 100 of the same work, on "the manner in which wealth is distributed" and the level of general well-being *("l'aisance"),* as factors affecting the form of the "law of demand."

[141] Cournot, *Researches,* 127. On other aspects of Cournot's treatment of the relations between income and prices, in the chapter indicated, see below, pp. 351 f.

increase in the income of the producers of Commodity *A* will affect the demand for commodities *B, C,* etc., and the incomes of their producers, and, by its reaction, will involve a change in the demand for commodity *A*"; just as a *"diminution* of income" in the case of any group of producers would mean that these producers "will have less money available for their own consumptions, which may affect the demand for other commodities." [142]

Nor, Cournot argued, is it merely a change in the *aggregate* "social income" which is important for the purpose in hand. The matter of the *distribution* of the "social income" is likewise of the first importance; for while Cournot admitted, as a "possibility," that a change in the income of a given group of producers might result in such a distribution of income "that the demand for each of these commodities may remain the same as before," he went on to point out that "as a matter of fact, of course, this exact distribution is not admissible, and in general, on the contrary, it must be the case, that a perturbation experienced by one element of the system makes itself felt from that to the next, and by reaction throughout the entire system." [143] Nor, finally, is it to be supposed that Cournot thought of this "reaction through the entire system" as taking place solely through *shifts* in the demand schedules of particular commodities which would leave the *conformation* of these schedules unchanged. On the contrary, he was careful to point out that "an alteration in the method of distribution of the social wealth" is precisely one of the things that may produce "a variation in the form of the function *F(p)*, which expresses the law of demand"—in other words, a change in the degree of "elasticity" characterizing the demand for a particular article.[144] For, as Cournot himself observed, it is precisely differences in *income* which explain why the demands for both "articles of luxury" and "commodities of prime necessity" are characterized by what Marshall would have designated as a relatively low degree of elasticity, whereas the demand for "articles of general consumption, but which nevertheless are not considered of prime necessity" is characterized by a relatively high degree of elasticity.

It is clear, therefore, that Marshall was merely repeating the substance of this part of Cournot's argument when he listed as the very first among the factors determining "the general law of variation of the elasticity of demand, and its consequent responsiveness to changes in price," the difference in the degree of response of demand to price

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[142] *Ibid.,* 127, 129. It should hardly be necessary to emphasize the bearing of such propositions on the type of problem with which concepts such as the "multiplier" are supposed to deal. What it *is* important to emphasize, however, is that Cournot's own treatment of the problem was such as to bring out the importance of studying the interrelations between the problem of the *generation of income,* on the one hand, and the *determination of the prices of commodities,* on the other. On this matter, see below, pp. 351 f.

[143] *Ibid.,* 130.

[144] *Ibid.,* 139 f.

which one would expect to be evidenced by different income "classes."[145]
Nor is there any ground for suggesting that Marshall introduced the
element of "income" only in connection with the initial construction of
a "demand schedule," which was then assumed to remain unchanged.
On the contrary, in his discussion of the factors that cannot be ex-
pected to remain "equal" over a period of time, and would therefore
necessarily affect the conformation of the demand schedule for any given
commodity (which "represents the changes in the price at which a
commodity can be sold consequent on changes in the amount offered
for sale, *other things being equal*"), he was careful to include "changes
in the general prosperity and the total purchasing power at the disposal
of the community at large"—in other words, precisely the changes
referred to by Mr. Keynes as changes in "the amount of the aggregate
effective demand."[146]   And it is worthy of particular note that Marshall
called special attention to the fact that these changes in "general pros-
perity and the total purchasing power at the disposal of the community
at large" would be expected to change the conformation of individuals'
demand schedules for a given commodity not only because they would
represent changes in the *aggregate* of what Cournot had called the "social
income," but also because in most cases they may be expected to affect
the *structure* of incomes—for example, the relation between the amount
of "resources of those with fixed incomes," on the one hand, and the

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[145] See Marshall's *Principles*, 103 ff. Cf. also Marshall's repetition of the
ancient and "classical" proposition that "the effective demand of a purchaser
depends on his means, as well as on his wants" (*Principles*, 242 n.; cf.
Marshall's *The Economics of Industry*, 70; also the *Principles*, 348, and the
quotation from Marshall's Note on "Ricardo's Theory of Value" given
above, p. 208, n. 138). It is, of course, passages such as these which, to-
gether with those cited in nn. 146 and 147, immediately following, must
be taken into account in judging the extent to which Marshall "really
supposed that people's demands for commodities do not depend on their
incomes" (cf. above, p. 208, and the reference to Hicks given in n. 139
thereto). It is certain, at any rate, that no such supposition was attributed
to Marshall by the earlier commentators on his treatment of the "demand
schedules for particular industries." See, for example, the comments by
J. N. Keynes, in his article "Demand" in Palgrave's *Dictionary,* I (1894),
540 f., on the fact that "changes . . . in the wealth . . . of consumers"
may be expected to "cause the demand at a given price itself to vary,"
as well as on the fact that Marshall's discussion of the concept of elasticity
of demand is stated in terms which make it clear that, for purposes of
judging the probable response of quantity demanded to changes in prices,
these prices are to be regarded as "high" or "low" "relatively to the means
of the consumers in question." Cf. also the comment of Edgeworth, in his
article "Demand Curves," *loc. cit.,* 544: "One important cause of alteration
in demand curves is the increase of the consumer's purchasing power."

[146] For the passage quoted from Marshall, see his *Principles*, 109. Cf.
also p. 462 of the same work, where an "increase in the wealth and general
purchasing power of the community" is listed as one of the factors which
"may render it necessary to make out a new demand schedule."

amount of such "resources" accruing, on the other hand, to "those whose incomes depend on the profits of business." [147]

5. As in the case of Mr. Keynes's first objection to the use of demand schedules of the Marshallian type for the purpose with which we are here concerned, the mere fact that a given demand schedule for a specific commodity may be expected to *change* its conformation (and therefore its degree of "elasticity") whenever there are changes in the amount and distribution of the social income constitutes no reason whatever for refusing to make use of these schedules, and therefore of the Marshallian "elasticity of demand," in attempting to account for the differential response of prospective purchasers to given changes in the prices of

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[147] See Marshall's *Principles,* 109. Marshall's own conclusion from this fact was that it is dangerous to deduce any conclusion, with respect to the *aggregate* response of demand to price change, from the behavior of any *one* group of income receivers, on the assumption that the losses of one group of income receivers may be balanced by the gains of other groups. The particular illustration in question must, therefore, be held to bear upon the validity of the charge that Marshall "does not make it clear to the reader that, among the other things which are assumed equal when one reasons in terms of *the* elasticity of demand, the distribution of incomes occupies an important place" (so Staehle, "Short-Period Variations in the Distribution of Incomes," *loc. cit.,* 134). It may be pointed out, also, that the awareness thus evidenced by Marshall of the effect of changes in the level of aggregate income (and its distribution) upon the conformation of demand schedules for particular commodities has been evidenced by the leading contemporary representatives of "old" Cambridge. Recognition of the bearing of changes in *aggregate* income, for example, on the elasticity of demand for specific commodities or specific types of labor is obviously implied in a recognition of the effect, upon such elasticity, of changes in the level of aggregate economic activity "between boom and depression" (cf. the references to Robertson and Pigou given above, p. 175, n. 65). For an example of recognition of the bearing of changes in the *distribution* of income upon the problem in hand, see what is said by Pigou, *The Theory of Unemployment,* 119, concerning the effect, upon the "demand for particular commodities" (including, obviously, the effect upon the *elasticity* of demand for these commodities), of any development whereby "purchasing power is shifted from persons who predominantly desire one sort of commodity to persons who predominantly desire another sort." And for an example of a similar emphasis on the dependence of elasticity of demand (in the Marshallian sense) upon the facts with respect to income and its distribution, with its resultant susceptibility to change as between prosperity and depression, by a writer outside the "old" Cambridge group who nevertheless made use of the concept of elasticity of demand avowedly in the meaning of the concept assigned to it by Marshall, see V. Bloch, *Krise und Einkommen* (1932), 18 n.

specific commodities at a time when the amount or the internal structure of incomes is changing. Again, to be sure, the limitation in question is a troublesome one for certain problems: for example, the problem of the construction of "statistical" demand curves.[148] Again, however, this is a very different thing, indeed, from arguing that the mere fact of change in the conformation of the "demand schedules for particular industries" over a period during which the amount and the distribution of income is changing (in other words, the mere fact that we must be prepared to *redraw* the demand schedules for specific commodities in such a way as to take account of the effect upon these schedules of changes in the amount of income and its distribution) constitutes a reason for abandoning the very concept of such demand schedules in attempting to account for changes in the structure of money prices over that period.[149] For, in

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[148] It will be observed that the fundamental methodological difficulty in the derivation of "statistical" demand curves *which purport to approximate the demand curves of the "general" Theory of Value* is the same in both cases: namely, the difficulty of deriving information with respect to *ex ante* relations between prices and quantity demanded at each price, from data with respect to successive *realized* prices, which may or may not have lain along a single (unchanging) *ex ante* demand schedule. Here, therefore, as before, in all instances in which the outside evidence is not such as to create a presumption of invariance in the *ex ante* demand schedules over the period taken for examination, the problem is to develop supplementary statistical techniques designed to reveal what an unchanging *ex ante* schedule *would* have looked like if the forces which actually caused it to change had not been operative (cf. above, p. 176). That most of these supplementary statistical techniques are still extremely crude must be admitted. There has been too great a readiness, for example, to adopt techniques resting upon the assumption that the changes which it is desired to eliminate have affected only the *position* of the *ex ante* curves, and not their *conformation* (cf. Allen and Bowley, *Family Expenditure*, 125, n. 2). Even in this respect, however, progress has been made in the direction of a clearer recognition of the difficulties involved. See, for example, the references given in the following note.

[149] It is, indeed, not even a reason for arguing that it will never be possible to derive "statistical" demand curves with a fair claim to resembling the *ex ante* curves of the general Theory of Value, *so long as there is clear recognition of the fact that at best the "statistical" curves must virtually always be "short run" curves* (cf. Stigler, "The Limitations of Statistical Demand Curves," *loc. cit.*, 475), in the sense that there would be recognition of the overwhelming presumption that the *ex ante* demand curves involved in the determination of the realized prices of a given commodity at different periods may be expected to differ in conformation, as well as in position. For the obvious procedure, *if the data should permit,* would

view of the *ex ante* character of these demand schedules, all
that this means is that the "plans" which these schedules
represent are subject to change, as the result of changes in
the level or structure of incomes, as between two successive
acts of realized purchase or sale.[150]

6. Given a clear recognition of the fact that realized
money prices are what they are as the result of prospective
buyers' and sellers' "plans," of the type symbolized by our
market demand and supply curves, it follows that one of the
major tasks of the Theory of Prices is to *explain*, as far as
one can on the basis of economic analysis, *why these plans
are what they are,* and therefore *why they change as they do.*
This is obviously impossible if we either (1) abandon the
very concept of alternative purchase and sales "plans," of

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then be to explore the possibility of taking separate periods, each of short
duration, and attempting to measure the different degrees of elasticity
characteristic of each period. Experiments in this direction have, in fact,
been made; although it is hardly unfair to suggest that these experiments
are more important as examples of a recognition of the problem than as
examples of a successful solution of it in the particular cases taken for
examination. See, for example, E. J. Broster, "Elasticities of Demand for
Tea and Price-Fixing Policy," *Review of Economic Studies,* VI (1939),
where an attempt is made to "build up a demand schedule for tea *in
respect of each of several levels of national prosperity*" (171 f. [italics
mine]).

150 Once more it may be observed that this has been recognized from the
very beginning of the popularization of the concept of "demand schedules
for particular industries" of the Marshallian type. See, for example, the
quotation from Edgeworth given above, p. 211, n. 145, with respect to "the
increase of the consumer's purchasing power" as an "important cause of
alteration in demand curves." It has certainly been recognized, moreover,
by the outstanding representatives of "old" Cambridge. See, for example,
Pigou, "The Statistical Derivation of Demand Curves" (p. 69 of Pigou
and Robertson, *Economic Essays and Addresses*), on the effect of changes
in "the *distribution* of purchasing power" in causing "a series of diver-
gencies between the [respective conformations of] demand curves of
successive intervals." It will be observed that there is no formal differ-
ence between statements of this type and statements such as those of Mr.
Keynes to the effect that "the ordinary demand curve for a particular com-
modity is drawn on some assumption as to the incomes of members of the
public, and has to be re-drawn if the incomes change" (*General Theory,*
281). The difference between Mr. Keynes, on the one hand, and Edge-
worth and Mr. Keynes's colleagues of "old" Cambridge, on the other, lies
entirely in the nature of the *consequences* drawn from these statements
by their authors, with respect to the rôle to be assigned to "ordinary demand
curves" in any attempt to account for changes in the structure of money
prices in the world we know.

the type represented by our market schedules of demand and supply; or (2) are willing to use them only on condition that the "plans" themselves are assumed to remain unchanged as between two successive acts of realized purchase or sale.   It becomes possible only in the degree that we are prepared to study the factors which *make for* change in these plans. *Changes in the level and structure of incomes represent precisely one of these factors.*

It may be pointed out once more that some of Mr. Keynes's own followers have implicitly recognized the truth of the proposition here defended: namely, that the mere fact that the elasticity of demand for particular commodities may *change* in time, as a result of changes in the amount of income and its distribution, in no way constitutes a reason for *abandoning* the use of these schedules in accounting for changes in the structure of money prices.   This is true, in particular, of Mr. Harrod; for the "diminishing elasticity of demand" which appears in his "Law of Diminishing Elasticity of Demand" is a phenomenon that is held to follow primarily from changes in the amount and the distribution of *income* over the trade cycle.[151]

It is not necessary to raise here the question as to the factual evidence for Mr. Harrod's "Law." [152]   Nor is it necessary to regard Mr. Har-

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[151] See the references to Harrod given above, p. 162, n. 40. Cf. also, in this connection, Joan Robinson, *The Economics of Imperfect Competition,* 70 f., on the probable effect of an "increase in wealth" upon the elasticity of the demand curves for particular commodities; also p. 319 of the same work, on the effect, upon the elasticity of demand for particular commodities, of any "change in the *composition* of the national dividend"—that is, in the distribution of income between different income groups. The relation of Mrs. Robinson's specific propositions to Mr. Harrod's "Law of Diminishing Elasticity of Demand" should be obvious.

[152] This is not to say, obviously, that the existence of the "Law" may be regarded as demonstrated beyond question. See, on the contrary, Allen and Bowley, *Family Expenditure,* 125, where the position adopted would seem, at least *prima facie,* to be diametrically opposed to that of Mr. Harrod. For, according to Allen and Bowley, "the elasticity of demand for any item with respect to changes in its price is likely to *increase* with income": "demands," they contend, "tend to become *more* elastic as the income level rises" (italics mine). For evaluations of Harrod's "Law" which range from an acceptance of it as "plausible" to outright rejection, see Joan Robinson, *Economic Journal,* XLVI (1936), 691; A. H. Hansen, "Harrod on the Trade Cycle," *Quarterly Journal of Economics,* LI (1937), 530 (p. 58 of the same author's *Full Recovery or Stagnation?* [1938]); H. T. N. Gaitskell, *Economica,* New Series, IV (1937), 473; Hawtrey, *Capital and Employment,* 330; D. H. Robertson, *Canadian Journal of Economics and Political Science,* III (1937), 125 (p. 177 of the same author's *Essays in Monetary Theory* [1940]); A. C. Pigou, "Real and Money Wage Rates in Relation to Unemployment," *Economic Journal,* XLVII (1937),

rod's "Law" as typical of the best that may be expected from a conscious use of the concept of a changing elasticity of demand for particular commodities in order to account for changes in the structure of money prices and thereby the level of output and employment as a whole.[153]   It is of much greater importance to point out that rejection of the concept of elasticity of demand as a factor affecting the structure of money prices on the ground that a particular author may have over-generalized *one particular possibility* in this direction, would represent a distinct retrogression from the standpoint of the construction of an adequate apparatus for dealing with the determination of the structure of money prices—a retrogression, one may add, of precisely the kind that is represented by the rejection of the concept of a changing structure of prices and production under the impact of a given pattern of money flows, simply because of dissatisfaction with the excessive claims to generality made by the sponsors of a particular model making use of such a concept (as in the case, for example, of the "Hayek effect").[154] It should rather be an occasion for rejoicing that, on the particular point under discussion, some of Mr. Keynes's followers have refused to follow him in a direction that would have effectively barred further progress along one of the most fruitful paths toward an adequate synthesis of the Theory of Money and Prices, on the one hand, and the "general" Theory of Value, on the other.

7. The recognition, by sponsors of the concept of "demand schedules for particular industries," such as Cournot and Marshall, of the fact that changes in the level and structure of money incomes are important factors determining the conformation of these schedules, together with their refusal to allow this fact to lead to a rejection of these schedules as devices helpful in accounting for the events of the real

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418 f.; R. F. Bretherton, "A Note on the Law of Diminishing Elasticity of Demand," *ibid.*, 574 ff.; H. W. Singer, "Another Note on the Law of Diminishing Elasticity of Demand," *ibid.*, XLVIII (1938), 138 ff.; H. Makower, "Elasticity of Demand and Stabilization," *Review of Economic Studies*, VI (1938), 25 ff.; and especially J. D. Sumner, "Cyclical Changes in Demand Elasticity," *American Economic Review*, XXX (1940), 300 ff., where the growing literature on the subject of Mr. Harrod's "Law" is discussed in some detail.

[153] It is difficult to believe, for example, that no significance would attach to a more detailed examination than Mr. Harrod has undertaken of the *differences* in the degree of change in elasticity evidenced by *different commodities* over the period of the cycle. What one regrets above all in Mr. Harrod's formulation, however, is his failure to integrate the use of Marshallian demand curves, with their property of elasticity, into a *generalized system of money flows,* of the kind outlined in the chapters that follow.

[154] See below, pp. 304 ff., 314, 372, n. 50

world, must itself be regarded as a recognition of this path to further progress in accounting for the determination of realized prices. From this point of view, the recent efforts, within the "general" Theory of Value, to devise techniques for dealing with the effects of changes in *income* on the structure of relative prices, are likewise to be welcomed, particularly since these efforts have not led, as a widespread adoption of the practice of Keynes's *General Theory* would lead, to a rejection of market demand schedules of the general Marshallian type, with their special property of "elasticity."

By most economists, credit for having inaugurated this recent discussion would undoubtedly be accorded to the two articles by J. R. Hicks and R. G. D. Allen, entitled "A Reconsideration of the Theory of Value." [155] As so often, however, it soon became clear that the unquestionably important work of these two writers amounted, in a number of important respects, primarily to a renewed emphasis upon, as well as a sharpening and further development of, suggestions to be found in the work of earlier writers—in this case, the work of E. Slutsky, in particular.[156]

It is proper to point out here, therefore, that the achievement of all these writers, including Slutsky, is less accurately described as the discovery of a problem, or series of problems, completely neglected by previous writers, than as the articulation and more precise solution of problems with which a large number of earlier writers had dealt in one way or another. It is clear, for example, that the concept of an "income elasticity of demand" was foreshadowed by all those budget studies whose purpose was precisely to discover the relative increase in demand for a given type of commodity which may be expected from a relative increase in income—"Engel's Laws" being a case in point.[157] It is equally clear that the concept was foreshadowed in those discussions of the differential impact of monetary contraction upon the structure of prices and output which made much of the fact that the "distress" occasioned by a loss of income on the part of certain groups of the community would result in different degrees of contraction in the expendi-

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[155] *Economica,* New Series, I (1934), especially 63 ff., 199 ff.

[156] Cf. Schultz, "Interrelations of Demand, Price, and Income," *Journal of Political Economy,* XLIII (1935), 440, 443 ff., 479; and R. G. D. Allen, "Professor Slutsky's Theory of Consumers' Choice," *Review of Economic Studies,* III (1936), 121, 123 f., 126, 128 f., and the references to Slutsky given in both articles.

[157] Cf. Allen and Bowley, *Family Expenditure,* 5 ff.; and H. G. Lewis and P. H. Douglas, "Some Problems in the Measurement of Income Elasticities," *Econometrica,* VII (1939), 210, 214.

ture on different commodities, depending upon the relative "importance" of these commodities.[158]

The same thing must be said of certain aspects of what has come to be called "the income effect." [159]   Discussion of the particular type of "income effect," for example, which is represented by the increase in demand for a given commodity that is "due to the increase in real income which a fall in the price" of that commodity or other commodities entails, goes back not only to Pareto, but at least to the earlier part of the nineteenth century.[160]   Yet it is obvious that these earlier adumbrations can in no sense be regarded as diminishing the merit of those who have inaugurated the recent discussion, within the framework of the "general" Theory of Value, of the relation between changes in income and the structure of relative prices: least of all can they be so regarded when it is observed that the principal figures in this discussion have made it clear that they regard their analysis as *supplementing*, rather than *displacing*, the analysis of Marshall.[161]

8. For the central purpose of the present discussion, however, it is of more importance to call attention to a further

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[158] See, for example, F. A. Walker, *Money in its Relation to Trade and Industry* (1883), 126 ff., 132.   Cf. also Hawtrey, *Currency and Credit*, p. 164 (of the third edition), where, although the subsequent reference is to an "elasticity of demand" which is presumably intended to be the Marshallian concept, the emphasis upon the fact that "the man who finds he has less to spend will economize more drastically on some [commodities] than others" amounts to an emphasis upon what would now be called the "income elasticity" of the demand for these commodities.

[159] "The income effect" is discussed further below, pp. 298 ff.

[160] See, for example, Malthus, *Principles of Political Economy*, p. 353 of the second (1836) edition, on the fact that a fall in the price of one commodity would mean that "a portion of revenue [would be] set free for the purchase of fresh commodities" (cf. also the Editor's Note, 391 n.).   The use of a similar type of argument, in the form in which it had been presented by James Wilson (1840), constituted, indeed, an integral part of Jevons's later expositions of his argument with respect to the effect of crop fluctuations on the level of business activity.   See Jevons's *Investigations in Currency and Finance*, 197; and see also the references given above, p. 207, n. 137, to F. W. Newman, whose *Lectures on Political Economy* were known to Jevons (cf. the latter's *Theory of Political Economy*, 313), although he does not seem to have cited Newman in connection with Wilson's suggestion.   Cf., finally, Cairnes, *Some Leading Principles of Political Economy Newly Expounded*, 207: "One result of the cheapness effected in certain products would be to leave a larger amount of purchasing power available for expenditure in other directions"; and Marshall, *Economics of Industry*, 152.   For the statement of the argument indicated in terms of the concept of an "income effect," see Hicks and Allen, "A Reconsideration of the Theory of Value," *loc. cit.*, 66, and Hicks, *Value and Capital*, 32 ff., 64 ff.; and cf. Pareto's *Manuel d'économie politique*, 583 f.

[161] See, for example, Hicks and Allen, "A Reconsideration of the Theory of Value," *loc. cit.*, 63, where it was explicitly stated that the concept of

striking characteristic of the doctrinal development of recent years: namely, that at the very time that the element of "income" as a factor affecting prices was being subjected to closer analysis within the "general" Theory of Value, it was receiving renewed emphasis *within the Theory of Money and Prices*.[162] For the very fact that the advances thus being made simultaneously in both fields were *converging*, offered a golden opportunity to those interested in effecting a "synthesis" between the two bodies of doctrine to undertake a re-examination of the whole problem of the relation of the *demand curves* of the "general" Theory of Value to the substance of the *Theory of Money and Prices*, when the latter is regarded as a theory of the forces determining the *amount and direction of money expenditure, and the consequences of such expenditure*. Clearly, however, it is precisely this opportunity that must be regarded as having been renounced by those who have rejected outright the concept of "demand schedules for particular industries" as an instrument valid (to say nothing of its being useful) in the explanation of the determination of the structure of money prices.

The chapters that follow represent an attempt to take advantage of the opportunity thus abandoned, and to outline the nature of the argument on which we must rest any "synthesis" of the "general" Theory of Value with the Theory of Money and Prices in which full justice would be done to all those elements in *both* bodies of analysis whose continuing usefulness can be shown to be unquestionable, for the simple reason that the advances made in any *one* of the two fields have not yet succeeded in showing that we can get along without making use of the achievements properly credited to the other. The major purpose of the present chapter, meanwhile, has been to demonstrate that there is no substantial basis for the particular arguments adduced by Mr. Keynes against one of the main propositions upon

"income-elasticity of demand" was one of the senses of "the conception of elasticity of demand" which was to be regarded as *additional* to that given by Marshall"—"income elasticity" being one of the "primary characteristics" to which the *Marshallian* "price-elasticity of demand . . . is reducible" (p. 67). Cf. also pp. 209 f. of the same pair of articles, and Allen and Bowley, *Family Expenditure,* 124 ff., 141 ff.

[162] See Volume I, Chapter Thirteen, of the present work, especially 336 ff.

which the proposed synthesis is constructed. This proposition is that while the Theory of Money and Prices has much to *add* to the particular concepts of the "general" Theory of Value which are represented by the Marshallian "demand schedules for particular industries" and their special property of "elasticity," it cannot *dispense* with those concepts if it wishes to provide an adequate account of the nature of the forces determining money prices in the world we know.

CHAPTER FIVE

Particular Demand Curves and the Determination of Money Prices

THE PRESENT CHAPTER is the first of three devoted to the description of certain aspects of a positive analytical apparatus, designed to account for the determination of money prices, which will make the fullest possible use of *both* the "general" Theory of Value and the Theory of Money and Prices, without omitting any part of either body of analysis which can be shown to be useful for the problem in hand. From what has been said in earlier pages, it should be clear that this is a task which cannot be regarded as new either as a program of work to be undertaken or when viewed in the light of recorded achievement. On the contrary, it is a task to whose formulation and partial execution many hands have contributed in the past, as they will undoubtedly contribute in the future. Some of the propositions which must be invoked in the accomplishment of this task are, indeed, of an extremely elementary nature, the need for whose reiteration is itself a commentary upon some recent discussions of the broader problem. Others among the propositions involved, on the other hand, are anything but simple in their implications; and this means that a fuller development of these implications must be left for more detailed studies to be undertaken in the future. Both of these considerations must be held to justify the schematic form of the argument which follows.

The argument itself is in the form of a series of propositions whose acceptance must be regarded as essential to any satisfactory synthesis of that part of the "general" Theory of Value which is summed up by the concept of demand schedules for particular commodities, on the one

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hand, and, on the other, the relevant parts of the Theory
of Money and Prices:

I. The ultimate goal of any Theory of Prices, like that
of any part of economics which undertakes to explain eco-
nomic reality, is to explain why *realized* prices are what
they are. "Quoted" prices, the prices which are included
in the *"ex ante"* schedules of the general Theory of Value,
"expected" prices, "equilibrium" prices (in most of the
senses of the concept of "equilibrium"), or any kind of
prices other than *realized* prices are to be introduced into
the argument only insofar as they help to explain why prices
actually realized on the market are what they are.

This proposition, one would have thought, is virtually self-evident.
Unfortunately, however, it has not always been so regarded. The fol-
lowing comments may therefore be in order, with respect to the nature
of the problems involved in any attempt to relate the various types of
"prices" indicated to those actually "realized" prices whose determina-
tion it is the task of an adequate Theory of Prices to explain:

1. *"Quoted"* versus *"realized"* prices. The relation between "quoted"
prices, on the one hand, and "realized" (or "actual") prices, on the
other, has concerned writers upon price theory and price statistics at
least since the days of Tooke.[1] On occasion, nothing more has been
meant by "quoted" prices than the prices "quoted" in the course of
bargaining, before a purchase and sale transaction is actually realized.
In this case, obviously, "quoted" prices are identical with those *"ex
ante"* prices which are "quoted," but not necessarily "realized," in the
course of the process of market adjustment. The relation of such prices
to the "realized" prices in which we are ultimately interested is dis-
cussed below.[2] In other cases, it would seem clear that the difficulties
involved are essentially of a statistical nature, in the sense that the
problem consists of obtaining additional information in order to deter-
mine in what respects the prices "quoted" as having prevailed at a
given time in fact differ from the prices actually "realized" at that
time. Specifically:

i. There is the problem of determining whether the prices "realized"
at a given time, in the sense of having been involved in actual money
payments effected at that time, differ in any way from the prices in-
volved in contract agreements made at that time to deliver the com-
modity, and to receive payment for it, at a *later* time.[3] It should be

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[1] See, for example, Tooke's *Thoughts and Details*, I, 68.

[2] See below, pp. 224 ff.

[3] See Tooke, *Thoughts and Details*, I, 68; and cf. Pigou, "The Statistical
Derivation of Demand Curves," *loc. cit.*, 385 (*Economic Essays and Ad-
dresses*, 62).

clear, however, that, for our purposes, the conclusion of a *contract* to buy or sell at a given price is to be regarded as involving "realized" prices *unless the contract is subsequently rescinded*. If it is rescinded, the price mentioned in the abandoned contract is obviously *not* a price which was actually "realized," and so ceases to be part of the record of those "realized" prices whose determination is to be explained.[4] If it is not rescinded, the price "quoted" and the price "realized" *in the sense of taking the form of a money payment* will not differ in *magnitude*. The investigator's sole problem, therefore, is that of seeing to it that in any analysis purporting to record changes in the prices involved in money payments made at *successive dates*, prices known only through "quotation" at the time the *contract* is made should be assigned to the date at which the money *payment* was actually made.[5]

ii. There are the further problems which arise from the fact that the "quoted" price does not give all the information necessary in order to determine what the commodity is whose price is thus "quoted." Tooke, for example, was disturbed by the fact that the quotations for the price of "flesh" did not indicate what *kind* of "flesh" was involved.[6] More recent writers have been troubled by the fact that price "quotations" do not always indicate whether there has been any change in the size of the unit or the quality of the commodity whose price is "quoted" as having changed or remained constant.[7] Still others have been disturbed by the fact that prices "quoted" do not always make clear that the price thus quoted is really lower or higher than the "true" price because of the granting or the refusal of special concessions in the way of credit-terms or additional services.[8] In all these cases, however, it is clear that the problem is a statistical one, to be solved by the correction of "quoted" prices in such a way as to make them equal in amount to the prices actually paid in the particular transactions described.[9]

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[4] In this connection, cf. Volume I, p. 49, of the present work.

[5] Cf. Pigou, "The Statistical Derivation of Demand Curves," *loc. cit.*, 384 (*Economic Essays and Addresses*, 63). The point involved has often been raised in connection with the "truth" of the familiar Quantity Equations. See Volume I, 47 ff., of the present work.

[6] Tooke, *Thoughts and Details*, I, 68.

[7] Discussion of the difficulty in question is of course among the commonplaces of our most elementary manuals on the use of statistics. See also, however, W. L. Thorp, "Price Theories and Market Realities," *American Economic Review*, XXVI (1936), Supplement, 15 f.

[8] See Thorp, "Price Theories and Market Realities," *loc. cit.*, 16 ff.; and cf. Saulnier, *Contemporary Monetary Theory*, 168. The difficulty involved is in many respects (though not in all) similar to that which led Wicksell and others to substitute some such phrase as "the terms of credit" for "the rate of interest." Cf. Volume I, 224, of the present work, and the references given in n. 51, thereto.

[9] It is clear, therefore, that the difficulty involved, while it is a serious one on the statistical side, is hardly to be put on a par with those analytical difficulties with the unsupplemented use of "the concept of a general level

2. *The prices involved in "ex ante" price schedules* versus *"realized"* *prices.* The relation of the *"ex ante"* price schedules of the "general" Theory of Value to the prices actually *realized* in the market was commented upon in the preceding chapter.[10] Here, therefore, it should be necessary to do no more than to point out that the argument, as there summarized, should dispose once and for all of a type of suggestion that in itself would be completely unwarranted. Such a suggestion would be one alleging that a concern with *realized* prices necessarily implies a lack of concern with "prices" which may be regarded as "hypothetical," in the sense that they are the prices which enter the "hypothetical" purchase and sale plans of economizing individuals, when these plans are stated in the form of a series of propositions to the effect that *"if* the price is one dollar" (a "hypothesis"), a given individual will be prepared to buy (or sell) four units of the commodity; whereas *"if* the price is two dollars" (a second "hypothesis"), he will buy (or sell) two units, and so on.

It is clear that to reject such propositions as useless would be to reject as useless the whole of the part of the "general" Theory of Value which is concerned precisely with the rôle played by such "hypotheses" in the determination of prices. For the substance of these "hypotheses" is nothing if it is not the substance of that emphasis upon the "subjective" element of "choice" which constitutes the essence of any "modern" theory of value. The individual's demand schedules for particular commodities of the "general" Theory of Value amount, indeed, to precisely such "hypotheses."[11] It follows, therefore, that the argument presented in the preceding chapter for the retention of these schedules in any adequate theory of the forces determining the structure of realized money prices amounts simultaneously to an argument for the retention, in any such theory, of the concept of *"ex ante"* prices, in the sense indicated. The point made here is merely that these *"ex ante"* or "hypothetical" prices have acquired the importance they have for a realistically

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of prices" which derive, for example, from "the differing behavior of different sections of the price system." Contrast Saulnier, *Contemporary Monetary Theory,* 168.

[10] See above, pp. 176 ff.

[11] Cf. Walras's use of the word "hypothesis" in connection with his "curves of purchase and sale," in the passage cited above, p. 185, n. 88. as well as the reference given above, p. 196, n. 115, to Fisher's characterization of the prices involved in the demand schedules of the general Theory of Value as "hypothetical" prices. Cf. also R. Frisch, "Statikk og Dynamikk i den økonomiske Teori," *Nationaløkonomisk Tidskrift,* LXVII (1929), particularly 335 f., on demand curves as statements of "laws" of "alternative variation," of the general form *"if* the price is so and so much, then the amount sold [read: the amount which (1) buyers are willing to buy; or (2) which the sellers *think* the buyers are willing to buy] *will* be so and so much" (italics in the original); and see the references given above, p. 178, n. 70, to Ohlin and Lundberg on these demand curves, and the corresponding supply curves, as representing "alternative purchase and sales

useful Theory of Prices only because, and insofar as, they can be shown to affect the determination of *realized* prices.

3. *"Expected"* versus *"realized"* prices. As we saw in the preceding chapter, it is one of the oldest propositions of any adequate theory of pricing that the economic calculations lying behind the market actions of "economizing" individuals are overwhelmingly *prospective* in nature.[12] We saw also that this statement, and the references to earlier economic literature which can be provided in support of it, amounts to confirmation of the proposition that "the fact that all economic activity is governed by expectations," instead of being a fact of which the "general" Theory of Value has failed to take account, is a fact which, in the words of Mr. Hawtrey, "has been universally taken for granted from the beginning of economic science."[13] There can be no more question here, therefore, than there was in the preceding chapter, of entering into a detailed discussion of the rôle assigned to the element of "expectation" in economic theory, or of the conclusions (many of them fantastic in the extreme) which have been drawn from the fact that this element must necessarily bulk large in any adequate account of the functioning of the economic process. The most that can be done here is to establish the nature of the relation bteween "expected" prices, on the one hand, and "hypothetical" (or *"ex ante"*) prices, on the other, as a necessary step toward establishing that relation between "expected" and *realized* prices which is necessary if "expectation" is to be held to affect market processes altogether.

i. The type of "hypothesis" which amounts merely to the statement that *"if* the price of commodity A is one dollar, I shall be willing to buy four units of the commodity" is obviously an entirely different thing from the type of "hypothesis" which amounts to a *guess* as to what the price of commodity A, as well as of other commodities, is likely to be in the near or remote future. And since the two types of "hypothesis" are in no sense identical in kind, it follows that "hypothetical" prices in the sense indicated above under I, 2, are not necessarily identical with those prices which are "hypothetical" in the sense that they are the prices

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plans." It will be observed that all of these statements as to the meaning of the demand curves of the "general" Theory of Value stand in obvious contrast to those quoted above, p. 194, n. 107, and p. 196, n. 113, from Mrs. Robinson and Mr. Kaldor, respectively. To the conception of demand curves as representing "a series of alternative plans" it has been objected, to be sure, that such a conception involves the "questionable assumption" that "the various . . . alternatives are thought out in advance in the mind of the individuals" involved (cf. Haberler, *Prosperity and Depression*, second edition, 191, n. 1; also Schultz, *The Theory and Measurement of Demand*, 9 f.). See, however, the references given above, p. 185, n. 88, to Walras's distinction between *"effective* dispositions" (or "plans") and *"virtual* dispositions."

   [12] See above, pp. 178 ff.

   [13] See above, p. 180, and the reference to Hawtrey given at the end of n. 72 thereto.

which economizing individuals "expect" (in the sense of *guess*) will prevail in the near or remote future.[14]

ii. Any attempt to establish the nature of the relation between the two types of "hypothetical" prices must begin with acceptance of the proposition that it is "hypothetical" prices of the *first* type (that is, "*ex ante*" prices in the form of market demand and supply schedules) which alone are *proximately* relevant to the determination of "realized" prices. If this proposition is accepted, it follows that "hypothetical" prices in the sense of a set of prices "expected" to prevail in the future can affect the determination of prices *realized* in any given market situation only by affecting the structure of "hypothetical" prices of the first type: more specifically, by affecting the conformation and position of the market demand and supply schedules prevailing at the time a given price is "realized." [15]

iii. From the very fact that the economizing activity of those who buy and sell commodities and services is necessarily largely *prospective* in nature, an obvious corollary follows. This corollary is that it is inevitable that the "plans" associated with different "hypotheses" with respect to prices (in the sense of the word "hypothesis" first indicated) will be what they are as the result of a series of guesses ("hypotheses," in the second sense of the term indicated above) as to what prices will prevail over a period of such a length as to make the effects of price change over the period relevant to current decisions with respect to purchases and sales. This, of course, is merely another way of saying

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[14] It will again be observed that this proposition provides an argument for avoiding, as far as possible, an outright *identification* of "*ex ante* analysis" with "*expectation* analysis," despite the obvious connections between the two. Cf. what is said on this matter above, p. 178, n. 71, and p. 180, n. 73, and also the following note.

[15] It should be clear that this argument provides one of the principal methods by which it becomes possible to avoid what has been characterized as "a basic difficulty of the whole *ex ante* (expectation) analysis": namely, the difficulty arising when we ask: "How can mere plans about the future influence the present situation?" (Haberler, *Prosperity and Depression*, second edition, 189, n. 3; the identification of "*ex ante*" with "expectation" analysis is Haberler's.) If one accepts the argument advanced in the text, the question is answered, so far as the problem under discussion is concerned, by the propositions (1) that "plans about the future influence the present situation" insofar, and only insofar, as they affect the basis upon which actual decisions are made in "the present situation"; and (2) that the "basis upon which actual decisions are made in the 'present situation'" is summed up by the market demand and supply schedules prevailing at the time a given price is "realized." It is to be hoped, also, that the argument summarized in the following paragraph of the text (under [iii]) will help to resolve the difficulties of those who have seen some conflict between an "emphasis" upon "the *anticipatory* or *forward looking* nature of *ex-ante* items," on the one hand, and an emphasis, on the other hand, upon the necessity for using the familiar market demand and supply schedules (see, for example, Lerner, "Ex-ante Analysis and Wage Theory," *loc. cit.,* 439).

that it is inevitable that expectations with respect to future prices should be an element capable of affecting the conformation of the market demand and supply curves for specific commodities prevailing at any given time, and therefore the prices realized at that time.[16]

iv. The fact that expectations with respect to the future course of prices may change the position and conformation of the market demand and supply schedules prevailing as between any two instances of "realized" prices is no more decisive against the use of these market demand and supply schedules for the explanation of "realized" prices than were any of the other objections, likewise deriving from certain facts of change, which were discussed, and rejected, in Chapter Four of the present volume.[17] On the contrary, it is *only* by tracing the effect of

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[16] See above, p. 178. It is the substance of proposition (iii), in particular, which, according to Professor Hicks, is to be regarded as the "first of Mr. Keynes's discoveries" in his *General Theory:* a discovery which, while it is not "altogether a new discovery," does not go back further than quite "recent years" (cf. the reference to Hicks given above, p. 180, n. 72). The validity of this characterization, when viewed in the light of the plain facts of doctrinal history, may be judged by the reader upon the basis of the by no means complete list of citations given in the preceding chapter (pp. 178 ff.). Here it is necessary to point out only that Mr. Keynes's explicit rejection of the "demand schedules for particular industries" as analytical devices useful in the explanation of the determination of realized money prices would effectively deprive him of any claim to have made the "discovery" imputed to him by Professor Hicks (even if the propositions involved could be regarded as being in any sense a "discovery" of our own day), as long as Mr. Keynes's "discovery" is alleged to throw light upon the way in which "expectations" are to be related to the "demand schedules for particular industries" of the "general" Theory of Value. Cf. also below, p. 228, n. 18, on Professor Hicks's own later comment on *Marshall's* treatment of the relation of "expectations" to his market demand and supply schedules, and the backward reference there given.

[17] See above, pp. 172 ff., 212 ff. It may be observed here that the fact that demand schedules may *change* in conformation and position as between any two instances of realized prices as a result of changes in "people's expectation about what is going to happen to prices and production in the future" has been as clearly recognized as has been the fact that they may change as a result of any of the factors discussed in Chapter Four. See, for example, Pigou, "The Statistical Derivation of Demand Curves," *loc. cit.*, 389 (*Economic Essays and Addresses,* 68). The parallel extends, indeed, even to the point that the question of the possibility of "eliminating" the influence of "expectations" with respect to future prices has been raised in connection with the derivation of *statistical* demand curves, in precisely the same way, for example, that the problem of "eliminating" the influence of changes in *income* has been raised in connection with the derivation of such statistical curves. In addition to the reference just given to Pigou, see, for example, H. Staehle, *Die Analyse von Nachfragekurven in ihrer Bedeutung für die Konjunkturforschung* (1929), 13, 16. It should hardly be necessary to labor the further point

changes in expectations upon those market demand and supply schedules involved in every case in which prices are "realized," that one can be sure that "expectations" with respect to the future course of prices have any effect whatever upon the "realized" prices whose determination it is the province of an adequate Theory of Prices to explain.[18]

v. What Propositions I, 3, iii and I, 3, iv, immediately preceding, amount to is an insistence upon the point that a careful tracing of the effect of "expectations" upon the conformation and position of the market demand and supply schedules which are involved in the determination of "realized" prices is necessary in order to account for whatever effect these expectations may have upon these "realized" prices. It is equally true, however, that a tracing of the relation between (1) "expected" prices, (2) "ex ante" market schedule prices, and (3) "realized" prices is necessary if we are to understand why "expectations" are what they are. For unless we are to make of the so-called "method of expectations" the kind of deus ex machina which (as has been remarked by one writer otherwise quite sympathetic to a proper use of the "method") would lead to "the complete liquidation of economics as a science," we must proceed upon the assumption that expectations are what they are largely as the result of experience of economic processes

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that the possibility of changes in the demand schedules entering into the calculations of potential sellers of a given commodity as the result of "revised demand anticipations" is an element inherent in the whole theory of monopolistic competition. See, for example, F. Machlup, "Monopoly and Competition: A Classification," *American Economic Review,* XXVII (1937), 451; and on the rôle of "expected reactions" of both sellers and buyers in the theory of monopoly and competition generally, see the rest of Professor Machlup's article, *passim,* as well as A. Smithies, "Equilibrium in Monopolistic Competition," *Quarterly Journal of Economics,* LV (1940), 98, 101, 109, 114.

[18] The reader will recall that the method thus indicated for dealing with the effect of "expectations" upon market action was adopted explicitly by a number of earlier writers, including Marshall. See above, pp. 184, n. 84; 188, n. 95; 190, n. 101; 192, n. 104, including the reference there given to Hicks's *Value and Capital.* It may not be out of place here, therefore, to observe that this method is in no sense dependent upon the assumption of a "definiteness" of expectations with respect to future prices, of a degree that has been associated with a concept such as Hicks's "elasticity of expectations" (see the latter's *Value and Capital,* 124 ff.). It should be clear, also, that the argument in the text provides a further reason for insisting that "the plans which are upset must be kept apart from the 'plans' or decisions represented in the instantaneous curves" (that is, the market demand and supply schedules, in our sense of the term, involved in the determination of "realized" prices). Cf. Haberler, *Prosperity and Depression,* second edition, 191 n. The "plans which are upset" are of course those plans that are embodied in demand or supply schedules which apply to a "longer run" than do the market demand and supply schedules involved in the determination of actually "realized" prices. On this matter, see above, p. 199, and below, pp. 238 ff.

as they have been actually realized in the past and as they are being currently realized in the present.[19]

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[19] On the "method of expectations" as bound to lead to nothing less than the "complete liquidation of economics as a science," unless it is recognized that "it is sensible to link actions with expectations only if the latter can be explained on the basis of past and present economic events," see Lundberg, *Studies in the Theory of Economic Expansion,* 175. See also the comments by Professor Schumpeter, in the *Journal of the American Statistical Association,* XXXI (1936), 792, n. 3, on "expectation" as "a mere *deus ex machina* that conceals problems instead of solving them," whenever "expectations are not linked . . . to the cyclical situations that give rise to them," and the similar comments by the same author in his *Business Cycles,* 55; and cf. also the comments of Haberler, *Prosperity and Depression,* 252 f. The proposition, indeed, that the problem is "one of deducing changes in anticipations from the changes in objective data which call them forth" (cf. Hicks, "A Suggestion for Simplifying the Theory of Money," *loc. cit.,* 13) is one that was advanced long before it came to be suggested, in our own day, that such a proposition sums up "the whole problem of applying monetary theory"; and it should be recorded that, when so advanced, it carried no accompanying suggestions to the effect that the problem is peculiar to *"monetary* theory," or that there is something new or "revolutionary" about "expectational" analysis as such (contrast Hicks, *loc. cit.*). On the contrary, the proposition itself was advanced as an early protest against an *abuse* of what has now come to be called "the method of expectations," on precisely the grounds indicated in the passages quoted above from Lundberg and Schumpeter. One may cite, for example, the comment by Roscher, *Geschichte der Nationalökonomik in Deutschland* (1874), 659, on the proposition (advanced by J. G. Hufeland as part of the "theory of subjective value" contained in the latter's *Neue Grundlegung der Staatswirthschaftskunst* [1807], I, 250) that "it is not the real relation of supply and demand that determines price, but the *opinion* [*Meinung*] that men hold with respect to this relation." "A quite correct view," Roscher remarked dryly; "except that one thereby overlooks the fact that in the long run, after all, the opinion of men with respect to reality must itself be determined by just this reality." The same type of comment is to be found in Jevons, who certainly cannot be charged with a complete lack of sympathy with the view that "variations of commercial credit and enterprise are essentially mental in their nature" (see the references to Jevons given above, p. 192, n. 103). "Must there not be external circumstances," he asked *à propos* of the "very excellent papers" of John Mills on Credit Cycles, "to excite hopefulness at one time or disappointment and despondency at another? . . . Surely we must go beyond the mind to its industrial environment." See Jevons's *Investigations,* 184, 195 f.; and cf. the similar remarks on Mills in M. Tougan-Baranowsky, *Les Crises Industrielles en Angleterre* (p. 269 of the French translation of 1913). In view, moreover, of the extreme nature of the statements currently made both on behalf of and in opposition to the present fashionable emphasis upon the element of "expectations," it may not be out of place to call attention to the admirably balanced remarks on the subject made by Mr. D. H. Robertson as long ago as 1915 (see his *Study of Industrial Fluctuation,* 38 f.). For evidence of Mr. Robertson's sympathy otherwise (in strict accordance with the traditions of "old" Cambridge [see above,

vi. Thus, "expectations" help to determine "realized" prices.[20] But the prices thus "realized" *help to determine expectations with respect to the future course of prices.*[21] The simultaneous use of both propositions no more represents "circular reasoning" than does any description of a process in time in which each set of actions is held to be what it is

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p. 192, n. 103]) with an emphasis upon "expectational" elements in the economic process, see p. 61 of the same work, and the reference thére given to the chapter on "The Psychology of Crises" in E. D. Jones, *Economic Crises* (1909)—a chapter, it may be observed, which contains considerable, although by no means exhaustive, bibliographical material bearing upon the treatment accorded by earlier "economists" to the factor of "confidence" (cf. the first of the two references to Keynes's *General Theory* given above, p. 192, n. 103).

[20] The general acceptance of this proposition by earlier writers, as attested by the references given above, pp. 178 ff., can mean only that they would probably have regarded with something akin to stupefaction the extraordinary suggestion made a few years ago by Professor Hicks. This was that a significant difference between monetary theory, on the one hand, and the "general" Theory of Value, on the other, resides in the fact that, in "value theory," "equilibrium" is regarded as being determined by "objective factors like prices," whereas in monetary theory it is determined by "subjective factors like anticipations," so that in monetary theory "anticipations play a part . . . corresponding to the part played by prices in value theory" (cf. the preceding note, and the reference to Hicks there given). This suggestion must be regarded as strange even if it is divested of its demonstrably unfounded distinction between "value theory" and "monetary theory" in the respect indicated. For the very suggestion that, in "value theory," we are concerned *only* with "objective factors like prices," *and not* with the "subjective" calculations that make these "objective" ("realized?") prices what they are, is the kind of false antithesis that would be repudiated at once by any instructed sponsor of a theory of "subjective" value, by whom "the dependence of the objective fact of price on the subjective dispositions of individual persons" (cf. the reference to Edgeworth given above, p. 185, n. 88) would be regarded as the most elementary of methodological postulates. On the basis, indeed, of Professor Hicks's own justly praised work in the field of "general" value theory, in which "subjective" elements certainly play a crucial rôle, one must suppose that he himself really regards the antithesis as a completely false one. Yet I am not aware that he has as yet either expressly repudiated the suggestion quoted above, or given it an interpretation which would make it acceptable even formally.

[21] Cf. Lundberg, *Studies in the Theory of Economic Expansion,* 172; also G. Tintner, "A Note on Economic Aspects of the Theory of Errors in Time Series," *Quarterly Journal of Economics,* LIII (1938), 144 f. It is thus questionable whether any good can come from a debate on the question as to which problem is "primary" from a "theoretical" point of view: the problem of tracing "the effects of changes actually occurring in time," on the one hand, or the effects of *expected* changes, on the other (cf. Myrdal, *Prisbildningsproblemet och föränderligheten,* 21). In one sense, of course, it is true that the solution of the first problem demands the prior solution of the second problem (cf. Myrdal, *loc. cit.*); but in the light of the argument stated in the text with respect to the necessity for

as the result of what has preceded and what is expected to follow.[22] What must be said is rather that the tracing of such a process in time is made possible only through a *combined* emphasis upon (1) the importance of tracing the steps by which "expectations" affect "realized" prices through their effect upon market demand and supply schedules as these schedules appear in each discrete market situation, with (2) an emphasis upon the importance of tracing the effect of each realized market situation upon expectations with respect to future market situations, and therefore upon successive market demand and supply schedules and successive realized prices.[23]

4. *"Equilibrium" prices* versus *"realized" prices.* The reader will have noted that the demand and supply schedules which are held to be directly relevant to the determination of "realized" prices have been consistently characterized as *"market* demand and supply schedules."[24] This usage was in no way accidental. On the contrary, the usage was designed precisely to call attention to the fact that *realized* prices are not necessarily "equilibrium" prices, if the concept of "equilibrium" is to be given most of the connotations which it carries in the "general" Theory of Value. From this follows the necessity of establishing the nature of the *relation between* the "market" demand and supply sched-

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understanding the nature of the factors which, *make expectations what they are,* it would be equally possible to argue that an adequate solution of the second problem demands a solution of the first. The truth of the matter, after all, is that, as in all questions involving the tracing of a filiation of realized events *in time,* questions of "theoretical primacy" have no more meaning here than they had in the ancient problem as to the "theoretical primacy" of the hen or the egg. In this connection, cf. Volume I, 219 f., of the present work, and also the following note. The argument in the text may be regarded as throwing light also upon the falsity of any sharp antithesis between an emphasis upon "subjective" factors and "objective" factors, respectively, in the Theory of Prices. Cf. the preceding note.

[22] The charge of "circular reasoning," as applied to the explanation of the economic process as it unfolds itself in time, has, of course, appeared most commonly in discussions of the general nature of any satisfactory approach to the explanation of the trade cycle. See, for example, A. H. Hansen, *Business Cycle Theory* (1927), 200 ff., and the references there given to A. Löwe, "Wie ist Konjunkturtheorie überhaupt möglich?" *Weltwirtschaftliches Archiv,* XXIV (1926).

[23] It should be clear that the very statement of the problem in these terms implies the use of some form of what has come to be called "period-" or "sequence-analysis." It does not follow, however, that one must accept all that has been said with respect to the nature of such "period-analysis," or with respect to the limitations to which such analysis is alleged to be necessarily subject. On this matter, see what is said below, pp. 367 ff.

[24] It is of some importance that these "market demand schedules" be not confused with the demand schedules involved in the determination of "market price" when the latter is defined in such a way as to make it equivalent to what Marshall called "the true equilibrium price" for the short period. See below, p. 233, n. 27.

ules *directly* involved in the determination of money prices and those demand and supply schedules which are involved in the determination of prices properly to be regarded as "equilibrium" prices. The principal steps involved in the establishment of this relation are the following:

i. The only type of "equilibrium" which is *necessarily* involved in the establishment of any "realized" price is an "equilibrium" between (in the sense of an *equality of*) the quantity demanded and the quantity supplied at a given price. The characterization of such a price as an "equilibrium" price is warranted only in the sense that in the "higgling and bargaining" which may take place *before* a price is "realized," transactions may be impossible at certain prices because the sellers who wish to dispose of a given commodity or service at a certain price are not able to find demanders willing to pay that price.[25] Where this condition exists, no actual transactions will take place at *any* price.[26] The very fact that some transactions *are* actually realized at a given price may therefore be said to indicate that the price thus "realized" must have effected an "equilibrium" (in a very special sense of the term) between quantity demanded and quantity offered at the realized

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[25] The "higgling and bargaining" involved, in other words, is that involved in what has been called the "pre-formation of prices" (cf. above, p. 177, and the reference to L. Baudin given in n. 69 thereto). In other words, it is that which is involved in those steps in the processes of "bargaining" which are not accompanied in each case by successive *realizations* of prices (cf. N. Kaldor, "A Classificatory Note on the Determinateness of Equilibrium," *Review of Economic Studies*, I [1934], 127). As we have seen, it is of course perfectly possible that the demand and supply schedules of the bargainers may themselves change in the process of bargaining. In the case under discussion, this means (1) that the original demand schedules, for example, may change when some demanders discover that the price they would have been *willing* to pay is higher than they *need* pay; or (2) that the original supply schedules may change when some suppliers discover that the price they would have been *willing* to accept is less than that which they *need* accept. What this amounts to saying, however, is that the market demand schedule actually involved in the determination of a given *realized* price may be *different* from that which the demanders had in mind when they *began* the "higgling and bargaining" involved in the "*pre*-formation of prices." It would *not* mean that prices could be actually realized even though they did not represent the point of intersection of a market demand and a market supply schedule, respectively. They could, of course, represent some point other than the point of intersection when use is made of demand and supply schedules *other* than "market" demand and supply schedules, in our sense of the latter terms (see below, under I, 4, ii [p. 233] and I, 4, iv [pp. 236 ff.], respectively); but as long as use is made of market demand and supply schedules in the sense here indicated, it follows, from the very nature of these schedules, that any point other than the point of intersection of these schedules will be a point at which no actual transactions involving *realized* prices can take place.

[26] Cf. above, p. 196, n. 115.

prices, whereas the prices *not* "realized" are not "equilibrium" prices, in this very special sense of the term.

ii. While any "realized" price may thus be said always to represent *an* "equilibrium" price, in a very special (and not very illuminating) sense of the term "equilibrium," it does not follow that it represents a price which Marshall regarded as having "some claim to be called the *true* equilibrium price," even in a period so "short" that, in Marshall's own words, "the supply is limited to the stores which happen to be at hand." [27] For it is possible to establish a number of criteria for the establishment of "equilibrium" in the very short period which go beyond the criterion that a given amount of a commodity was actually sold at a given price. For example, it may be regarded as characteristic of "equilibrium" over such a period that the *whole* of "the stores which happen to be at hand" must be šold; and the realized prices which may be sufficient to move successive *parts* of the stores from the market may not necessarily be the same as the price which would have removed the *whole* if, again in Marshall's own words, a single price had been "fixed on at the beginning and adhered to throughout." [28]

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[27] See Marshall's *Principles*, 330, 333; and contrast D. H. Robertson, "A Survey of Modern Monetary Controversy," *The Manchester School*, IX (1938), 7 (*Essays in Monetary Theory*, 139), where the concept of a "price emerging from the mutual impact, at each moment, of the existing flows of money and of goods" is identified with the "Marshallian concept of market equilibrium." Marshall's usage obviously represents a considerable improvement over that of earlier writers such as Fleeming Jenkin (see the latter's *The Graphic Representation, etc.*, 78 ff.). For it is clear that Jenkin, although he characterized the price "at which the supply and demand curves cut" as "the *market* price," regarded this "market price" as only a "theoretical price," which might or might not coincide with the prices actually realized. By "market price," in other words, he meant substantially what Marshall meant by his "true equilibrium price" in the case of a "temporary equilibrium of demand and supply." Strictly speaking, therefore, Jenkin's "market prices" are *not* identical with our "realized" prices, and his "supply and demand curves" are not strictly equivalent to our market demand and supply schedules, which by definition must be so constructed that a realized price can lie only at their point of intersection.

[28] Cf. Marshall's *Principles*, 333. The reasons why this "true equilibrium price" may *not* be "fixed on at the beginning and adhered to throughout" (cf. Marshall, *loc. cit.*) are summed up by Marshall's proposition that the price offers of buyers and sellers will in reality be governed by their "own need for money in hand" and by their calculations with respect to "the present and future conditions of the market," with all that this implies concerning the possibility that buyers may be "unequally matched" in either respect (*Principles*, 332 f.). The very fact that Marshall himself called attention to these reasons may be taken as evidence that he did not wish the very *concept* of a "demand curve" to rest upon the assumption that "price is fixed at the start and adhered to throughout" (contrast Hicks, *Value and Capital*, 128). For the effect of such a usage would have been to forbid the direct application of "demand curves" of any kind to the problem of the determination of all prices which, as a result of what

iii. Thus, even in the "very short period," it may happen that no single realized price corresponds to what, in the light of some further criterion with respect to the nature of "equilibrium," may be regarded as an "equilibrium" price.[29]   It would be nothing less than absurd, however, to argue that the very possibility of such an occurrence means that we must abandon either (1) the concept of the determination of a realized price as resulting from the intersection of market demand and supply schedules, or (2) the concept of an "equilibrium" price as determined by the intersection of demand and supply schedules which may *differ* from the particular market demand and supply schedules

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Professor Hicks calls "false trading" (*op. cit.,* 129) turn out to be other than "equilibrium" prices. It is impossible to believe that this was Marshall's intention. The very opposite, indeed, is implied by the fact that Marshall *incorporated into* his demand schedules the effects of "expectation" (or, as he sometimes called it, "opinion": cf. the *Principles,* 334 n., and the general comments above, p. 192, n. 104), with all that this implies with respect to the possibility of the *disappointment* of such expectation, and therefore the failure to realize an "equilibrium" price, even in the sense of Marshall's short-period equilibrium. There is, in short, no reason to believe otherwise than that Marshall was here attempting only to give somewhat greater precision to the formulations of those earlier writers who had discussed the extent to which, given specific conditions with respect to the degree of knowledge and foresight on the part of the bargainers, actually realized prices would be likely to approach what was characterized as "a fair or just Equilibrium of . . . Price." See, for example, Tooke, *History of Prices,* V (1857), 89, 165 ff., 171 ff.

[29] The argument, as stated, has obvious applications to other problems involving the extent to which a given "price" realized in the market may be regarded as an equilibrium "price." It clearly applies, for example, to the position of those who have argued, with respect to the rate of interest, that *every* realized rate of interest must necessarily be an "equilibrium" rate of interest, and that there is, therefore, no sense in speaking of a *discrepancy* between the "market" rate of interest, on the one hand, and, on the other, a "natural rate," in the sense of a given point on a schedule representing the rates of profit expected from the use of a bank loan by different potential borrowers. See, for example, the review of Volume I of this work by H. Neisser, in the *Annals of the American Academy of Political and Social Science,* CCI (1939), 260. It should be evident, on the contrary: (1) that there is not the slightest difficulty in conceiving of a discrepancy between the prevailing (currently realized) rate of interest and the "natural rates" of specific potential borrowers, whose decision to borrow or not to borrow may be said to depend upon the relative position of their "natural rate" and the rate currently being realized (on the meaning thus assigned to the "natural rate," see Volume I of the present work, p. 252, n. 50); and it must be equally evident (2) that the mere fact that the realization of a given market rate of interest means that there must have been an "equilibrium," in *one* sense of the term, between the supply of and demand for loanable funds at that rate does not prevent us from characterizing *one* realized market rate as being in some *other* sense more of an "equilibrium" rate than any other. If, for example, our task is that of accounting for changes in the quantity of bank money, it

whose intersection is held to result in a given realized price.[30] On the contrary, *both* must be used if we are to understand the nature of the forces which make realized prices what they are.

For without the use of market demand and supply schedules, in the sense in which the concept has been used throughout the present argument, it is impossible to explain either (1) why, of a given range of *possible* "ex ante" prices, only one is "realized" in a given market situation; or (2) how the *goals* of dealers and consumers, even when these goals are short-period goals, are approached (if they are approached at all) through successive realized market transactions.[31] And with-

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is of the utmost importance to establish two further propositions: namely, (1) that the quantity of bank money may have increased as between the establishment of two successively realized market rates; and (2) that the reason why it increased was that there was a discrepancy between the market rate of interest realized in the first period and the particular rate of profit expected from a bank loan which *would* have had to be regarded as the "marginal" rate in that first period if there was to be no increase in the quantity of bank money (the "natural," or "equilibrium" rate, in another of its senses). In the light of the repeated cautions expressed in Volume I of the present work, it should hardly be necessary to labor the point that I have no intention of defending *all* that has been said in favor of the concept of a "natural," or even an "equilibrium," rate of interest. The point made here is merely that to deny the possibility of a divergence between an actually realized market rate of interest, on the one hand, and, on the other, something called an "equilibrium rate," no matter *how* the latter is defined, is to deny the proposition stated in the text: namely, that it is perfectly possible that no single realized price will correspond to what, in the light of some further criterion with respect to the nature of "equilibrium," may be regarded as an "equilibrium" price.

[30] It is, of course, this recognition of the possibility that "market" demand and supply schedules may differ from those involved in the determination of an "equilibrium" price, which makes it possible to reconcile (1) the assumption that every realized price must lie along a market demand schedule of some kind, with (2) the proposition that all exchanges involving the "realized" prices used in the construction of statistical demand curves, for example, need not necessarily "take place on the ['theoretical'] demand curve" (cf. Stigler, "The Limitation of Statistical Demand Curves," *loc. cit.*, 474 f.). For all that this second proposition can mean is that the demand schedule on which a given realized price is held *not* to lie, is constructed on a basis different from that on which our "market" demand schedules are constructed. On this matter, see also what is said below, pp. 262 f.

[31] The rôle assigned under (2) to "market" demand and supply schedules, in our sense of the term (or, as they are commonly called, "instantaneous" demand and supply schedules), is often obscured in the presentation of certain theorems concerning the path pursued by successive realized prices in relation to what is regarded as an "equilibrium" price, by reason of the fact that the "market" demand and supply schedules, at whose intersection every realized price must be held to lie, are simply left out of the picture altogether. That the results of this procedure have been anything but happy will be clear to anyone who undertakes to translate the as-

out a conception of an "equilibrium" price, even over a period as short
as that indicated by Marshall in the passage cited above, it is in many
cases impossible to understand what these goals are, and therefore
why the successive market demand and supply schedules show the di-
rection and the type of change that they do, and therefore lead to the
successive realized prices actually registered in successive market trans-
actions.[32]

iv. In order to establish the relevance, for the determination of
"realized" prices, of the concept of a "true" equilibrium price, and
therefore of the demand and supply schedules involved in the deter-
mination of such an "equilibrium" price, it is not necessary to establish
either (1) that the individuals who buy and sell will *always* have in
mind the *particular* goals whose realization involves types of calcula-
tion summarized by the particular demand and supply schedules which
are held to be relevant for the establishment of the "goal" represented
by a given equilibrium price; or (2) that these goals will themselves
remain unchanged in the face of a changing market situation.

With respect to (1), for example, it is necessary only to establish the
fact that the goal indicated is one which individuals *may be expected
to regard as reasonable* under institutional and conjunctural conditions
which *may* be found to prevail in the real world. For if it is such a
goal, it is to be expected that it will contribute to an explanation of
some types of market behavior. And if conditions are found in which
the goal indicated is either not reasonable, or is such that individuals
engaged in buying and selling are prevented from pursuing it by institu-
tional or non-economic factors, the further development of our analytical
apparatus must take the form, not of rejecting devices which are in

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sumptions underlying the propositions involved in such theorems with
respect to the position of successive realized prices, into successive pairs of
"market" demand and supply schedules (see, for example, Schultz, *The
Theory and Measurement of Demand,* 77, n. 39). For it will then be
clear that both the particular realized prices involved in the argument and
the assumed response to these realized prices imply a specific type of market
action which by no means need represent the only possible result, or even,
necessarily, the most probable one. On the implications of the proviso
"if they [the "goals" associated with the concept of an equilibrium price]
are approached at all," see the text, above, under (iv).

[32] This argument is of course implied in Marshall's discussion of the
rôle played by the concept of a "true equilibrium price" in the case of a
"temporary equilibrium of demand and supply" (*Principles,* 333 f.). It
will be observed, however, that the case for the use of the concept of an
"equilibrium" price, as stated in the text, does not involve the further
proposition that there is an actual "tendency toward" this equilibrium
price in the sense that the successive realized prices must actually tend
toward the price regarded as the "equilibrium" price upon the basis of the
conditions prevailing or assumed to prevail *at the outset.* See, for ex-
ample, what is said below, in the following paragraphs of the text, with
respect to the usefulness of the concept of an "equilibrium" price when
the "goal" represented by such a price is held to *change* as a result of
changes in the market situation.

fact indispensable for dealing with certain types of situation, but of introducing other devices for dealing with other types of situation.[33] And with respect to (2), it need only be pointed out that the fact that a goal may *change* in the course of time provides no warrant for denying that an individual must be presumed to have *some* goal in mind whenever he enters the market as a buyer or a seller.[34]

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[33] Thus, it is perfectly possible, particularly in cases of what has been called "non-perfect competition," that the "goal" indicated when we make the specific assumption "that entrepreneurs are guided in their decisions solely or predominantly by the desire to maximize profits" may, under certain conditions, be pushed into the background by other "desires" on the part of entrepreneurs. See B. Higgins, "Elements of Indeterminacy in the Theory of Non-Perfect Competition," *American Economic Review,* XXIX (1939), 476 ff. To say that the introduction of such a possibility introduces an element of "indeterminacy," however, instead of meaning that analysis of the conditions for "equilibrium" can provide no guide to the actual conduct of entrepreneurs, means simply that analysis designed to account for such conduct must ultimately include those factors, other than "the desire to maximize profits," which actually guide entrepreneurs in their market decisions. On the general methodological point involved, see Kaldor, "A Classificatory Note on the Determinateness of Equilibrium," *loc. cit.,* 122 f.; and R. Triffin, *Monopolistic Competition and General Equilibrium Theory,* 71. Cf. also the following note.

[34] Cf. Robbins, *An Essay on the Nature and Significance of Economic Science,* 62: "Through history, the given data change, and though *at every moment* there are tendencies towards an equilibrium, yet *from moment to moment* it is not the *same* equilibrium towards which there is movement" (italics in the original). The statement, to be sure, that "at every moment there are tendencies towards an equilibrium" requires careful interpretation if it is not to give rise to misunderstanding. It is, indeed, the very fact that the "goals" referred to in the text may change in the course of time which has been greatly stressed by writers skeptical of the existence of any "tendency towards equilibrium" in the pricing process as the latter operates in the real world. See, for example, M. Abramovitz, "Monopolistic Selling in a Changing Economy," *Quarterly Journal of Economics,* LII (1938), 192, and the same author's *An Approach to a Price Theory for a Changing Economy* (1939), 38 ff., 51 ff., 55 ff., 59 f.; and cf. also Lundberg, *Studies in the Theory of Economic Expansion,* 10 f., 14, 17 f. The extent to which the case for the usefulness of "equilibrium analysis" is dependent upon the reality of such a "tendency" when the latter refers to the tendency of *successive realized market processes to approach a position characterized as one of "equilibrium"* is discussed below, pp. 409 f., 446 ff. Here, however, it should be sufficient to point out (1) that if, by a "tendency toward equilibrium," nothing more is meant than that the actions of entrepreneurs are always affected by what seems to *them* a desirable goal at the moment these actions are undertaken; and (2) that if, by "equilibrium analysis," we mean the type of reasoning which is designed to establish what goals may in fact present themselves to the minds of entrepreneurs; then it is clear that the mere circumstance that progress, in the real world, toward a "goal" originally regarded as reasonable may be abandoned because progress toward *another* goal now seems *more* reasonable provides

For our present purpose, however, what it is really important to establish is the proposition that if any of these goals, changing or unchanging over time, are to be shown to be relevant to the determination of "realized" prices, it is necessary to demonstrate the consistency of these assumed goals with the *market* actions of those individuals whose decisions to act or refrain from acting make realized prices what they are. It is necessary, in other words, to establish the nature of the relation between a given market demand or supply schedule involved in the determination of a given realized price, on the one hand, and, on the other, the demand and supply schedules used to describe the nature of the particular goal which is assumed to dominate the activities of a given individual at the time he enters the market as a buyer or seller. And it is necessary to do so regardless of whether the problem is that of explaining why the two types of schedule may *differ,* or of stating the conditions under which a schedule of the former type may be regarded as a segment of a schedule of the latter type.[35]

v. Many of the difficulties experienced in attempts to establish the relation of "equilibrium" prices to "realized" prices have arisen pre-

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no justification whatever for ceasing to be concerned with the nature of the considerations present in the minds of entrepreneurs when they set up their successive "goals." When, therefore, it is said that "equilibrium" is "indeterminate" whenever "the final position is dependent upon the route followed" (Kaldor, "A Classificatory Note," *loc. cit.,* 125 ff., 132 ff.), all that this can mean is that no account of the actual functioning of the economic process can be regarded as complete until it undertakes, *upon the basis of a study of the successive, actually realized steps in any economic process actually unfolding itself in time,* to establish the nature of the considerations likely to determine the nature of entrepreneurial responses to changes in the market situation, *including the possible changing nature of the goals whose attainment these responses are designed to aid.* See, for example, the cases presented by G. J. Stigler, "Production and Distribution in the Short Run," *Journal of Political Economy,* XLVII (1939), 320, 327; and cf. also Marshall, *Principles,* 463 n., 808 f.

[35] It may be observed that this argument, although it certainly assumes that current market offers will be conditioned in some way by the intentions which are summarized by "long-period demand (or supply)" curves, does not necessarily involve acceptance of the specific proposition that "there will always be one point" on "short period curves which will correspond to the long-period demand (or supply) at this price" (Kaldor, "A Classificatory Note," *loc. cit.,* 135). "Long-period demand (or supply)" curves, like "market" demand and supply curves, are *"ex ante"* concepts (see above, pp. 197 ff.). They represent, that is to say, the "hypothetical" "plans" of entrepreneurs, in particular, with respect to the "long period." In the great majority of cases, however, these long-range "plans" will inevitably be considerably less "definite" than the short-run "plans" (cf. A. G. Hart, "Failure and Fulfillment of Expectations in Business Fluctuations," *Review of Economic Statistics,* XIX [1937], 69). It is therefore doubtful whether, in advancing the type of proposition quoted at the beginning of this note, we gain anything but a spurious precision in the statement of the way in which current market offers will be conditioned by longer range "plans." Cf. Abramovitz, *An Approach,* etc., 36 ff.

cisely from a failure to establish with sufficient precision both (1) the differences and (2) the relations between, market demand and supply schedules, on the one hand, and, on the other, demand and "supply" schedules which are not *market* demand and supply schedules, in our sense of the term. A *cost* curve, for example, whether it is a "short-run" or a "long-run" cost curve, is by no means necessarily identical with a market supply curve.[36] Neither are the "long-run demand schedules" which entrepreneurs are presumed to envisage as a basis for their "long-run" plans with respect to output, by any means necessarily identical with the market demand schedule which an entrepreneur faces, or is prepared to face, whenever he undertakes to market whatever salable output he may have on hand.[37] It is inevitable, therefore, that a fatal asymmetry should characterize any attempt to explain the process of price determination upon the basis of an interaction of demand and supply schedules which do not refer in each case to the same things over the same time period.[38] It should be clear, however, that the strongest protection against the error involved in such a procedure is provided precisely by a method of the type insisted upon here: namely, a method based upon the propositions (1) that the prices which we must ultimately explain are the prices "realized" at specific moments in clock time; (2) that the only demand and supply schedules which are directly relevant to the determination of these "realized" prices are the market demand and supply schedules prevailing at the

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[36] See below, pp. 257 f., 557 ff.

[37] On the distinction between the demand curve which the entrepreneur "faces," on the one hand, and the demand curve which he is *"prepared to face,"* on the other, see what is said above, pp. 178, n. 70, 195, 196, n. 114, 197 f., and also below, pp. 258, n. 81, and 260, n. 84.

[38] Cf. V. Domenidò, "Considerazioni intorno alla teoria della domanda," *loc. cit.,* 42 f. It need be added only that most of the objections raised by Dr. Domenidò to the concept of a "long-run" demand curve, while they are valid against the usage of those writers who have failed to establish with sufficient precision the differences in the nature and purpose of "short-run" and "long-run" demand curves, respectively, are not necessarily valid against uses of the concept of a "long-run" demand curve which make it clear that the concept finds one of its chief uses as an element in the calculations of *entrepreneurs* who must make their longer-range plans for production and sale upon the basis of some hypothesis with respect to the probable response of quantity demanded to price over a period of a length relevant for a judgment as to the relative rationality of the available alternative longer-range plans for production and sale. It is, indeed, the failure to relate the concept of a "long-run demand curve" to the *calculations of entrepreneurs,* and particularly the failure to observe the difference between a forward-looking *calculation* with respect to the probable response of demand to price over a longer period, which may be revised as time goes on, and the *actual* response of demand to price as time goes on, that probably underlies the suggestion that "the notion of a long-run demand curve is in the nature of a fiction" (cf. R. H. Coase, "Some Notes on Monopoly Price," *Review of Economic Studies,* V [1937], 26). On these matters, see also what is said above, pp. 197 ff.

moment the prices are "realized"; and (3) that every proposition with respect to the effect upon "realized" prices of demand and supply schedules (including "cost" schedules) applying to longer periods of time must be translated into a proposition with respect to the effect of such schedules *upon the market demand and supply schedules prevailing at the specific moments in time when specific prices are actually "realized."*

II. In a fully developed money economy, a realized price represents the passage of money for an article sold for money.[39] And the "passage of money for articles sold for money" is precisely what constitutes the subject matter of those aspects of the Theory of Money and Prices which undertake to explain why the dimensions of the stream of money which "passes" for a given commodity or group of commodities is relatively large at one time and relatively small at another.   It constitutes, that is to say, the subject matter which must be held to lie behind the "stream" equations of monetary theory, of which the familiar "Quantity Equations," significantly interpreted, must be regarded as variants.

III. But it also constitutes the subject matter of that part of the "general" Theory of Value which is built upon the proposition that any given realized price is what it is as the result of the conformation and position of the market demand curve and the market supply curve prevailing at the moment the price is realized.   According to this most elementary and least disputable of propositions, the location of the realized price (p) is given by the point of intersection of the market demand and supply curves prevailing at the time the price is realized—this point being defined by simultaneous reference to the price axis, on the one

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[39] The qualification "in a *fully developed* money economy" is inserted in order to take account of such "exchanges" as are still carried on in kind. It is not necessary here to go into the question how far it is true that "barter remains an important factor in modern life" (Anderson, *The Value of Money*, 394; see especially Chap. XI of the same work). The reason why it is unnecessary to go into the question is that whatever may be said with respect to the extent to which the "assumption" that no exchanges take place by means of barter is "essential" for "the quantity *theory*" (cf. Anderson, 201), it is certainly not essential for the validity and usefulness of the quantity *equations,* which are alone relevant here. On the methods employed in order to take account of the element of "barter" in making use of the quantity *equations,* see Volume I of the present work, p. 52 f., n. 39; p. 66 f., n. 74; and p. 540 f.

hand, and, on the other, the axis on which are measured quantity (*q*) demanded and supplied.

I have characterized this as the "most elementary and least disputable of propositions" in full awareness of the fact that it is possible to find in current economic literature what *seem* to amount to attempts to deny it most emphatically. I do so because it is very easy to show that these apparent denials are in fact concerned with considerations entirely different from those which are here at issue. Specifically:

1. *The alleged possibility of an "inequality" between quantity demanded and quantity supplied.* The proposition that, even in the case of *realized* transactions, the "supply" may be in excess of the "demand" (or *vice versa*) is at least as old as W. T. Thornton, who used it as part of his attack upon John Stuart Mill's so-called "equation of supply and demand."[40] No one, in these days, could protest against those parts of Thornton's argument which amounted simply to an emphasis upon the necessity for making explicit use of supply and demand *schedules*.[41] For the use of such schedules transforms the proposition

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[40] See Thornton's "A New Theory of Supply and Demand," *loc. cit.*, 422 ff. (*On Labour*, 46 ff. Since, as was pointed out above [p. 182, n. 79], the relevant passages in Thornton's original article were reproduced virtually verbatim in his later book, the references which follow will be to the book alone.) The parallels between Thornton's attack on Mill's "equation theory" of "supply and demand" and the objections which are still being raised to this "equation theory" are striking, even to the proposition that "if the whole of it were literally true, it would be a truth of small significance" (*On Labour*, 53. Cf. the remark of Chamberlin cited below, p. 261, n. 85, on the "minor importance" of "the revered proposition that price tends to the point where supply and demand are equated"; and compare the quotation from Thornton given below, p. 242, n. 42, with that cited below on page 245 [cf. n. 49] from Lindahl). It may not be out of place, therefore, to express the hope that the parallel will be continued, on the side of those who might wish to defend some forms of Mill's "revered proposition," by a following of the example set by Mill himself in his admirably tempered reply to Thornton. Thornton's "discussion of one of the fundamental questions of abstract political economy (the influence of demand and supply on price)," wrote Mill, "is a real contribution to science"; but he added that it was, in his estimation, "an addition, and not, as the author thinks, a correction to the received doctrine" ("Thornton on Labour and its Claims," *Fortnightly Review*, XI [1869], 506; cf. also pp. 509, 511, 513, of the same article). On certain details of Mill's reply to Thornton, not all of which are to be regarded as entirely happy, see below, p. 243, n. 43; p. 248, n. 54; p. 251, n. 59; p. 252, n. 60; p. 252, n. 62; and p. 254, n. 67.

[41] For Thornton's insistence (with partial acknowledgments to Mill) upon the "emendation of our . . . definition of supply, which is not simply the quantity offered for sale, but the quantity offered for sale at some specified price," and his insistence upon a similar "amendment" of the definition of "demand," see *On Labour*, 45 f. In the light of certain later criticisms of Mill's treatment of "demand" by other writers, and in the

that there is no necessity for "quantity demanded" and "quantity sup-
plied" to be "equalized" in a given market into one alleging that the
demand *price* for a given quantity of a commodity may be above or
below the supply *price* for that quantity of the commodity: or, alterna-
tively, that the quantity demanded at a given price may be greater or
less than the quantity offered for sale at that price.[42]   These are proposi-
tions which no one would deny.   What would be denied by defenders
of our Proposition III is that prices can be *realized* (that is, that sales
can be actually effected) for those quantities of commodity with respect
to which there is a discrepancy between demand price and supply
price.[43]   For if the sale of a given amount of a commodity is effected at

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light also of Marshall's defense of Mill (cf. above, p. 207, n. 137), it is
particularly worth noting that Mill saw no objection whatever to this
part of Thornton's argument.   See Mill's "Thornton on Labour and its
Claims," *loc. cit.*, 508.

[42] The examples given by Thornton (*On Labour*, 49 ff.) were designed
particularly to demonstrate the second type of proposition: or, as Thorn-
ton put it, they were designed to demonstrate that "at the price finally re-
sulting from competition, supply and demand, or the quantity offered for
sale at a certain price, and the quantity demanded at that price, will not
be equal" (p. 47).   It is quite clear that Thornton did not intend to argue
(he certainly did not *say*) that cases could be imagined in which "the
quantity *sold* is not equal to the quantity bought." (Contrast Fleeming
Jenkin, *The Graphic Representation, etc.*, 85.   It should be added, how-
ever, that this statement was clearly an unintentional slip on Jenkin's
part.   For the context in which the statement itself appears shows that
Jenkin must have meant to say that the quantity *offered for sale* at a
given price is not necessarily equal to the amount demanded and actually
bought at that price.)

[43] In this connection, see Walras's *Eléments d'économie politique*, 67 f.
(section 64).   Amusingly enough, Thornton's argument amounted in the
end to an acceptance of precisely the reasoning on which the statement
in the text is based.   For one thing, the examples he gave of an "excess"
of demand over supply in the market (see *On Labour*, 49 ff.) all assumed
that demand price and supply price were equal in *whatever transactions
were realized*.   (Cf. Fleeming Jenkin's diagrammatic representation of one
of the relevant cases presented by Thornton in the former's *The Graphic
Representation, etc.*, 86 [Figure 11].)   What Thornton insisted upon was
that there was no necessity for an "equalization" of the
quantity demanded with the *quantity offered for sale* at this "equal" price.
But the further details of his own argument involved an admission that
the very failure to bring about such an "equalization" between quantity
demanded and quantity offered for sale at a given price would permit the
realization at that price only of an amount of transactions for which de-
mand price and supply price would be equal.   See, for example, *On Labour*,
52, where Thornton admitted that "in the circumstances supposed [in
which the supply price for part of a given stock was assumed to be above
the demand price for that part of the stock] a dealer must either lower
his terms, or *part of his stock will be left on his hands*" (italics mine); and
he admitted also that this is the "truth" (albeit "the one solitary truth")
in the "popular theory" that "the price resulting . . . will be the one at

a given price, this must mean that the seller was *willing* to sell that amount at that price, and that the buyer was *willing* to buy that amount at that price.[44] And since our market demand and supply schedules

which demand and supply—the quantity supplied and the quantity demanded—will be equalized" (p. 46). In other words: the supposed excess at a price over supply at that price in *realized* transactions really had reference, not to the realized transactions themselves, but to those transactions which would *not* be realized precisely because the quantity supplied at a given price would not be equal to the quantity demanded at that price. In effect, therefore, Thornton's insistence that, even in the case of realized transactions, "demand" might "be three times as great as the supply" (or, more generally, that "goods are almost always sold at prices at which supply and demand are unequal" [*on Labour,* 54]) amounted simply to a reaffirmation of what was after all one of the principal implications of the statement (which he ostensibly rejected) that "the price resulting . . . will be the one at which demand and supply—the quantity supplied and the quantity demanded—will be equalized": namely, the implication that in the case of those quantities for which demand price and supply price are not "equalized," no transactions will be actually effected. This, after all, was the substance of Mill's argument on the point in question when, in his reply to Thornton, he contended that Thornton's examples proved merely that "supply" and "demand" would not be "equalized" whenever "there is no price that would fulfill the condition" that the demand price for the quantity in question would equal the supply price for that quantity. "Is it possible," Mill asked, "to have any more complete confirmation of the law, than that in order to find a case in which the price does not conform to the law, it is necessary to find one in which there is no price that can conform to it?" ("Thornton on Labour and its Claims," *loc. cit.,* 510; cf. also p. 513 of the same article. See also Fleeming Jenkin, *The Graphic Representation, etc.,* 85, where Thornton's case was represented by a diagram [Figure 10] in which the demand curve fails altogether to intersect the supply curve, and Jenkin pointed out that if there *is* a point of intersection, "the sale will take place," but *"not otherwise"*). Unfortunately, however, Mill's own exposition suffered from the fact that, like Thornton, he did not distinguish sharply enough between those aspects of the "law" which were concerned with the conditions necessary for the establishment of an "equilibrium" price, on the one hand (in the sense, say, of a price which would "clear the market"), and those aspects of the "law," on the other hand, which were concerned, as our Proposition III is concerned, with the conditions necessary for the *realization* of a market price and of whatever amount of sales is actually effected. Contrast the passage from Walras cited at the beginning of this note; and cf. what is said on this matter below, p. 246, n. 51.

[44] There is obviously no inconsistency between this statement and the cases adduced by Thornton (*On Labour,* 49 ff.) in which the potential buyer was assumed to be willing to buy at the price indicated *not only* the amount he actually buys, but more, or in which the seller was assumed to be willing to sell at the indicated price not only the amount he actually sells, but more. For in all these cases, while the buyer (or seller) may have been willing to buy (or sell) more at the indicated price, the other party to the transaction was not: with the result that these further trans-

are nothing but statements of the amounts which sellers indicate their *willingness* to sell and which buyers indicate their *willingness* to buy at given prices, these realized prices must lie along our market demand and supply schedules as a simple matter of definition. If one wishes, one may say that our Proposition III then becomes "a mere truism." [45] In that case, one is saying that it is necessarily true.[46] That one cannot say, on the other hand, that it is "meaningless" follows from the fact that it is precisely by the use of this "truistic" device that we are able to explain why, at certain bid and asked prices, no transactions can take place at all.[47] And it is precisely by the use of this "truistic"

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actions would not be realized. The "excess" of "demand" over "supply" at given realized prices discussed by Thornton really had reference, therefore, to those formulations of "the law of supply and demand" which purported to set up the conditions necessary for the establishment of an "equilibrium" price, rather than for the establishment of a *realized* price.

[45] Cf., for example, Chamberlin, *The Theory of Monopolistic Competition*, 13 n. It should be observed, however, that our Proposition III does not mean that "demand and supply are interpreted in the sense of the amount actually bought and sold, in which case they are always identical" (Chamberlin, *loc. cit.*). It is one thing to say that *realized* demand (at a price) and *realized* supply (at a price) refer to amounts "actually bought and sold" and are therefore "always identical." It is quite another thing to say that all forms of the "law of supply and demand" require, if they are to hold in all cases, that demand and supply *as such* be "interpreted in the sense of the amount actually bought and sold," and hence are "always identical." Cf. also n. 47, below.

[46] The reader familiar with Volume I of this work will recognize the analogy between the present discussion and the discussion with respect to the "truistic" or (paradoxically enough) the "untrue" character of quantity equations of the general Fisherine form. See especially, in this connection, Volume I, 89 ff.

[47] For an example of the suggestion that certain "interpretations" of "the law of supply and demand" are made "meaningless" by giving to them a "truistic" character, see Chamberlin, *The Theory of Monopolistic Competition*, 14 n. That our *Proposition III* is not made "meaningless" merely as a result of its "truistic" character is evident from a consideration of Chamberlin's assertions *(loc. cit.)* (1) that "the monopolist may choose either (a) his price, or (b) the amount of the commodity actually exchanged"; and (2) that "whatever price he chooses, the amount bought and the amount sold will be equal; and whatever the amount he chooses, it will be bought and sold." The monopolist may, to be sure, "choose" his *asking* price, and he may indicate the amount he would *be willing* to sell at that price. But to say that "whatever price he chooses, the amount bought and the amount sold will be equal," is to obscure the fact that, at certain asking prices, the "amount bought and the amount sold" will be "equal" to *zero*—that is, that at certain asking prices, no sales, and therefore no prices, will be actually *realized;* while to say that "whatever the amount he chooses, it will be bought and sold" is to obscure the fact that, on the realistic assumption that in choosing a given "amount" to be sold, he will also "choose" a price (or series of prices) at which he will attempt to sell this chosen "amount," this amount will *not* be bought and

proposition that, in the case of "collective" demand and supply schedules for particular commodities, we are able to take the first step toward an explanation of why *more* will be sold at some prices than at other prices.[48]

These matters are, indeed, so elementary that when one finds, in a contemporary work of importance, the statement that "in an actual dynamic case, there is no necessity for equality of demand and supply," and that "the prices quoted in the market" may be "regarded as the supply prices of sellers," but only "in certain exceptional cases as the demand prices of buyers," one is forced to conclude that the author must mean something that is in no way in conflict with our Proposition III.[49]   If, for example, the "dynamic case" under consideration is a

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sold if the asking price which he sets upon the chosen "amount" is higher than the price which potential buyers are willing to pay for that amount. A proposition which makes these things clear may be elementary and "truistic"; but it is not "meaningless." On the contrary, it is a proposition which itself makes either "meaningless" or incorrect the statement that "monopoly value has nothing whatever to do with the law of supply and demand" (Chamberlin, *op. cit.,* 14 n.); and it is a proposition, also, which shows the necessity for the establishment of categories intermediate between the interpretation of demand and supply "in the sense of the amount actually bought and sold," on the one hand, and an interpretation, on the other hand, which would relate the "equating" of "demand and supply" only to the establishment of a position properly to be characterized as one of *equilibrium*. On this last point, contrast the last sentences on pp. 14 n., and 15 n , respectively, of Chamberlin, *op. cit.;* and cf. what is said below, p. 247, n. 51.

[48] The reason why Proposition III is a necessary first step toward the explanation of why *more* will be sold at some prices than at other prices is simple enough: it is that what, in the case of the individual, is a matter of whether *any* sale will actually be effected, and *any* price will actually be realized, becomes, in the case of a social group, a matter of *how much* will be sold at various prices as a result of the fact that more individuals will refrain from "realizing" actual purchases at some prices than at others. The reason, on the other hand, why it is *only* a first step toward such an explanation is that we then have to go *behind* the market demand and supply schedules in order to explain, as far as we can by the methods of economic analysis, *why* these market demand and supply schedules have the conformation and position they do have. See Fleeming Jenkin, *The Graphic Representation, etc.,* 85 f., on the meaning of his "first law of supply and demand" (as represented by Jenkin's proposition [p. 78] that "in a given market, at a given time, the market price of the commodity will be that at which the supply and demand curves cut"): "This law only selects one among many possible prices already determined in the minds of sellers and buyers; it does not in any case determine . . . the price at which any one seller chooses to sell or any one buyer chooses to buy. In other words, the law states that the price will be that corresponding to the intersection of the two curves, but in no way *determines what these curves will be.*" (Italics mine).

[49] The statements are quoted from Lindahl, *Studies in the Theory of Money and Capital,* 60, 64 f.

case in which a price is actually realized, the "necessity" for an "equality" of demand *price* and supply *price* for the amount actually sold follows from the fact that if they had not been equal, the sales could not have been effected at all. Similarly, the necessity for a so-called "equality" of the quantities demanded and supplied at the realized price follows from the fact that the excess of quantity demanded at that price over the quantity offered at that price (or *vice versa*) will not get into the total of transactions actually effected at that price.[50] Likewise, if the statement that there is "no necessity for equality of demand and supply" is taken to refer to the fact that the price actually realized is not such as to clear the market of "supply," this can mean only that this actually realized price is not an "equilibrium" price in the sense of Marshall's "true equilibrium price" for the short period; it cannot mean a denial of our Proposition III.[51] And finally, if the prices in-

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[50] On the last point, in particular, see what is said above, p. 242, n. 42 and n. 43, on Thornton's early version of Lindahl's proposition that "in an actual . . . case, there is no necessity for equality of demand and supply."

[51] Cf. what is said above, p. 233, with respect to the relation between Marshall's "true equilibrium price" for the short period, and the realized prices with which we are here concerned. It is worth noting that Thornton's argument with respect to the failure of "price" to "equalize demand and supply" really amounted, in one of its aspects, to the contention that not *every* realized price, nor even *most* realized prices, will necessarily be "equilibrium" prices, in the sense that these realized prices will not necessarily be such as to clear the market of supply (the case of realized price with an excess of the quantity which sellers are willing to offer at that price over the quantity which potential buyers are willing to purchase at that price) or such as to satisfy all demands at the price actually realized (the case of realized price with an excess of the quantity which potential buyers are willing to purchase at that price over the amount which the sellers are willing to offer at that price). It was this belief that led Thornton to argue that even "if the whole of it ["the equation theory" of price determination] were literally true, it would be a truth of small significance." For, he argued, "even if it were true that the price ultimately resulting from competition is always one at which supply and demand are equalised, still only a small proportion of the goods offered for sale would actually be sold at any such price . . . When we speak of prices depending on certain causes, we surely refer to the prices at which all goods, or at least the great bulk of them, not at which merely a small remnant of them, will be sold. How can we say that the equation of supply and demand determines price, if goods are almost always sold at prices at which supply and demand are unequal? . . . Of what consequence would it be . . . that supply and demand determined finally resulting prices, if goods were almost all sold before those prices were reached?" (*On Labour*, 53 ff.) From Lindahl's reference of his proposition with respect to the lack of a "necessity for equality of demand and supply" to the "actual dynamic case," and his further reference of it to the case of "disequilibrium" (*Studies*, 60 ff.), I assume that he also intended to contrast the type of "equality" involved in the case of every realized price with the type of "equality"

volved in the "dynamic case" are prices which are not only "quoted," but are actually realized, they must of necessity have represented "the demand prices of buyers," as well as "the supply prices of sellers," for the quantities actually sold; and this would be true not only "in certain exceptional cases," but in *all* cases. To deny this would be to deny the simple axiom that if the demanders had not been willing to pay, for the amount actually sold, the price insisted upon by the seller for that amount, the sales could not have been consummated and therefore the prices in question would not have been actually realized.[52]

2. *The "indeterminateness" of market price.*

i. The suggestion that an "indeterminateness" may be imparted to the establishment of market price by the fact that the demand and supply curves may coincide at more than a single point, is likewise at

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involved in a price properly to be regarded as an "equilibrium" price. The issues involved in the two types of problem were, unfortunately, separated with insufficient sharpness in most of the statements of "the law of supply and demand" (cf., for example, the comment on Mill above, p. 243, n. 43). One welcomes, therefore, an insistence, such as that of Chamberlin, upon the proposition that the theory of "Equilibrium" should be distinguished from the theory summarized by "the Equation of Supply and Demand" (*The Theory of Monopolistic Competition*, 12 ff.). Such objections as can be raised to Chamberlin's treatment of this "distinction" arise from what would appear to be a failure to do justice to formulations of "*the* law of supply and demand" of such a kind as to make this "law" applicable to problems other than the type of "equating" of "demand" and "supply" which is involved in the determination of an equilibrium price, and yet not of such a kind as to reduce this "law" to the "meaningless truism" discussed above, p. 244, n. 45 and n. 47.

[52] It should be clear, therefore, that the correctness of Chamberlin's statement that "there is no such axiom" as that "a certain price will be established *because* it equates supply and demand" (*The Theory of Monopolistic Competition*, 12) depends entirely upon (1) what is meant by an "axiom"; (2) what is meant by the "establishment" of a price; and (3) what is meant by an "equating" of "supply and demand." I can see no good in a debate on (1), as long as our Proposition III is accepted as unquestionable. As for (2), the only sense in which our Proposition III is concerned with the "establishment" of a price is its *realization*, as contrasted with the mere *asking* or *offering*, or *planning* the asking or offering, of a "price." As for (3), I need only repeat what was said at the end of n. 47 to p. 245, above, with respect to the need for categories of the "intermediate" type there specified. Our Proposition III may be said, indeed, to represent the use of such intermediate categories in order to establish both the validity and the usefulness of the proposition, quoted at the beginning of this note, whose "axiomatic" character has been denied; but it undertakes to do so only *when the terms "supply" and "demand" are employed in such a way* as to make them applicable to the location of a realized price by means of the intersection of market demand and supply schedules *of the type here specified.* I have no desire to defend the supposedly "axiomatic" character of propositions involving *other* meanings of the words "supply," "demand," "equate," or "establish."

least as old as W. T. Thornton.[53] As Fleeming Jenkin suggested in his translation of Thornton's argument into the essential equivalent of modern "supply and demand curves," it amounted to the presentation of a case in which these supply and demand curves would coincide, not at a single point, but over a *segment* of their length parallel to the price axis.[54]

That this case in no way contradicts our Proposition III, however, becomes clear as soon as it is recognized that the curves which thus overlap (rather than "intersect") are not the particular demand and supply schedules whose intersection is held, by our Proposition III, to locate the realized price. These particular demand and supply schedules are those prevailing *at the moment a transaction involving a realized price is consummated*. In contrast, Thornton's *ranges* of demand and supply prices, respectively, and Jenkin's translation of them in terms of demand and supply schedules, refer to the *minimum* (supply) price-offers and the *maximum* (demand) price-offers which the buyers and sellers have in mind *at the beginning of the bargaining process*.[55] Both sets of schedules (or of price-offers), to be sure, are involved in

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[53] See Thornton's *On Labour*, 47 f. Thornton himself did not characterize the cases in question as involving an "indeterminateness" in the process of price determination. They were so characterized, however, by John Stuart Mill ("Thornton on Labour and its Claims," *loc. cit.*, 509, 513 f.), and also by Fleeming Jenkin (*The Graphic Representation, etc.*, 85).

[54] Jenkin, *The Graphic Representation, etc.*, 84 f. (cf. Figures 9 and 10). On Mill's alternative paraphrase of this part of Thornton's argument, see below, p. 251, n. 59. It should be observed, however, that Mill approximated Jenkin's paraphrase of Thornton's argument when he suggested that "if there is a part of the scale through which the price may vary without increasing or diminishing the demand, the whole of that portion of the scale may fulfill the condition of equality between supply and demand" ("Thornton on Labour and its Claims," *loc. cit.*, 509). He should have added, however, that this would be true only if the whole of that "portion of the scale" coincides throughout its length with a corresponding "portion of the scale" of the suppliers.

[55] This is made clearer by Thornton's exposition, with its example of the results to be expected in a Dutch auction, as opposed to an English auction, than it is by Jenkin's verbal paraphrase of Thornton's argument, according to which Thornton's case "is not peculiar to any form of bargain, but represents an unusual state of mind" (*The Graphic Representation, etc.*, 85). For the point of Thornton's references to the two types of auction was to show that in the Dutch auction the bargaining process is such that the prospective purchaser would be more likely to name his *maximum* demand price (say, twenty shillings), and the seller would be more likely to obtain a price in excess of his *minimum* selling price (equal, say, to eighteen shillings), than they would in the English auction. The points, however, that (1) what are involved are *minimum* selling prices and *maximum* buying prices; and (2) that the location of the price actually realized depends "on the relative skill of the two persons in bargaining," are brought out clearly by Jenkin's Figure 10 (*The Graphic Representation, etc.*, 85).

the "*pre*-formation of prices"; and Thornton's maximum demand prices and minimum selling prices (and therefore Jenkin's demand and supply curves containing these "maximum" and "minimum" bid and asked prices) represent a type of "plan" which *conditions* the price offers included in our own curves of market demand and supply.[56] It should be clear, however, that it is the curve of price offers *as it exists at the moment the transaction is consummated* that is directly relevant to the determination of realized prices, no matter how often the curve of price offers may *change* in conformation or position in the course of the bargaining process.[57] It is, indeed, very hard to imagine cases in which the last price offers made upon the basis of the demand and supply schedules prevailing at the moment a sale is effected, and a price is realized, would extend over a price *range* for *both* buyers and

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[56] In this connection, see the discussion of "Sale by auction" in Wicksteed, *The Common Sense of Political Economy*, 252—a discussion made particularly notable by Wicksteed's clear recognition of a proposition which is central for the present discussion: namely, that the mere fact that the markets represented by such "auctions" may result in a "failure . . . to secure" a realized price which may be regarded as that of "final *equilibrium*," does not alter the further fact that "the fundamentally determining conditions are just the same as in the ordinary market," since "the speculative estimate [by buyers and sellers] of each other's scales of preference" may be assumed to operate in both cases. It is to be remembered that Wicksteed himself insisted that the element of "speculative estimate" is precisely one of the factors *affecting* the curves of "price-and-quantity demanded" which are held to be directly relevant to the determination of realized prices, whether or not these realized prices turn out to be "equilibrium" prices. See above, p. 188, n. 95.

[57] The nearest that Thornton himself could come to this resolution of the dilemma raised by his case of "indeterminacy" of price within those "opposite extremes, one above which the price of a commodity cannot rise, the other below which it cannot fall" (*On Labour*, 58), was to say that price would be determined by "competition" (59 f., 64), this "competition" being greatly affected by estimates with respect to "prospective supply and demand." It does not seem to have occurred to him that these "estimates" could be regarded as conditioning the market demand and supply schedules which themselves perform the rôle assigned by him to "competition": namely, that of functioning as "the immediate arbiter of [realized] price." Contrast the references to Wicksteed given above, p. 188, n. 95, as well as what is said above, p. 192, n. 104, with respect to Marshall's inclusion of the expectational element in demand and supply curves. In order, indeed, to establish the relation between such curves, on the one hand, and the determination of *realized*, as well as of "equilibrium" prices, on the other, in a situation in which the bargainers may resort, in the course of the bargaining, to what Fleeming Jenkin called "the artificial production of false supply and demand curves" (*The Graphic Representation, etc.*, 87), it is not necessary to go beyond Marshall's discussion of the rôle played in the bargaining on "Market Day in a Local Corn-Exchange" by the fact that "everyone will try to guess the state of the market and to govern his actions accordingly" (see Marshall's *Principles*, 332 ff.).

sellers.[58]   And as long as this is so, our Proposition III stands firm against the contention that it does not apply to cases in which "indeterminacy" is alleged to be given to the establishment of market price by the fact of an overlapping of demand and supply schedules, in some sense of the latter other than that assigned to them in our own definition of market curves of demand and supply.

ii. The argument is even simpler in the case in which an "indeterminacy" might be alleged to be imparted to price by the fact that the

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[58] This would be true, for example, in neither the second nor the third of the three cases presented by Fleeming Jenkin (*The Graphic Representation, etc.*, 86 f.) as instances in which his "first law of demand and supply" (cf. above, p. 245, n. 48) "cannot affect" price determination.   (The first of these three cases—namely, the case of "simple transactions between man and man for one object"—need not concern us here in detail, except for a mild objection that might be entered against Jenkin's statement that in this case "neither demand nor supply curve can be drawn: the price is that at which the buyer can persuade the seller to part with the article" [p. 86].   For if the buyer is *willing* to try to "persuade the seller to part with the article" at a given price, this "willingness" can be represented by a "demand curve": even though, as Jenkin himself pointed out, this "demand curve" will have the form of a straight line parallel to the price axis, rising from a fixed point on the quantity axis up to a point representing the maximum demand price.   This "demand line," as Jenkin called it, would represent the range of possible demand-price offers for the particular "quantity" of commodity in question.   Similarly, the willingness of the seller "to part with the article" at a given price can be represented by a supply curve—or "supply line," as Jenkin called it—parallel to the price axis, descending toward, but not reaching, the quantity axis: the lowest point on this "supply line" representing the minimum supply price on the range of possible supply-price offers for the particular "quantity" of commodity in question.   See Jenkin's own Figure 10, p. 85.)   In the second of these cases, Jenkin took the instance in which "sealed tenders are sent in for the supply of an article"; and he argued that, although "the supply curve can be drawn," the "demand at a price does not exist, and the demand curve cannot be drawn" (p. 87).   But surely we are dealing here with a case in which "demand at a price" *would* "exist" over a *range*, the upper limit of which would represent the *maximum* demand price (cf. above p. 248, n. 55).   All that is brought about by the fact that "the buyer waits till the tenders are opened, choosing the lowest" (p. 87), is that the "disposition" of the buyer to buy given quantities at given prices may be said to have been only "virtual" up to this time (cf. the reference to Walras given above, p. 185, n. 88), but now becomes "effective," and settles at that point, within the range of *possible* demand prices, which corresponds to the lowest supply price attached to the quantity of the article desired.   The same reasoning applies to Jenkin's third case: namely, that in which "shares in a new company are applied for before any market is found for the shares."   The mere fact that "the demand is unknown until the applications for shares are opened" (p. 87) can mean only that the suppliers, who will have a minimum (reserve) supply price, may defer the announcement of this supply price until the demand prices

curves of demand and supply may intersect at *several* points, instead of intersecting at only one point, or overlapping as in the case just discussed.[59] For again it should be observed that our Proposition III undertakes to relate market demand and supply schedules to *realized* prices, and undertakes to do no more than that. If prices are actually *realized* at each of the several possible points of intersection, our Proposition III holds with undiminished rigor for each of these realized prices.

Not every one of these realized prices, to be sure, need be a price of

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become "known." If these demand prices are all below the minimum supply price, the sale will not be made, and no price will be actually realized. If they are above the minimum supply price, the sale will be made at the demand price most favorable to the suppliers, who will have moved their supply price up to the level of this most favorable demand price. In both of these cases, it will be observed, the "indeterminacy" of price, in the only sense in which such "indeterminacy" is relevant to the present discussion, is removed by the fact that it lies within the power of one of the bargainers to adjust his demand (or supply) curve *at the moment the transaction is consummated,* in such a way that there is only *one* point at which the demand (or supply) price coincides with the supply (or demand) price of the seller (or buyer) whose offer is thus selected by the other party to the bargaining. The only case, indeed, in which "indeterminacy" might be said to remain would be one in which the "tenders" of *both* demanders and suppliers would not only be expressed in terms of a *range* of price-offers, up to a maximum (demand) price-offer and down to a minimum (supply) price-offer, but would also be "sealed" against the possibility of a revision by *either* party of its price-offer in the light of later knowledge of what the other party would have been willing to give or take. In this case (examples of which may be found in certain types of wage-rate negotiation through a third party acting as "arbitrator"), the "indeterminacy" can be removed only by the decision of the third party, who may be said to obtain the result which would otherwise have been obtained by a revision of the market demand or supply curves, or both, if the process of bargaining had been "open" on either the demand or the supply side of the market, or both.

[59] This would be the implication of the principal paraphrase of this part of Thornton's argument by Mill (though see also above, p. 248, n. 54). What Thornton had "really shown," Mill argued, was that the "law of price" (as stated in the principle of the "equalization of supply and demand") "is, in this particular case, consistent with two different prices, and is equally and completely fulfilled by either of them" ("Thornton on Labour and its Claims," *loc. cit.,* 509). As Mill interpreted Thornton, in other words, the latter's statement of the case in question permitted, if it did not demand, the assumption that the supply and demand curves would not coincide at any points other than the "two different prices" named by Thornton in his example. A diagrammatic representation of this case, as interpreted by Mill, would therefore resemble the constructions of Walras and Marshall for those instances in which the curves would have "several points of intersection." (See Walras, *Eléments d'économie politique,* 68 ff. [sections 65 to 68]; and cf. Marshall, *Principles,* 472 n., 806 ff.)

stable *equilibrium*.[60] But the question whether a given realized price is or is not an equilibrium price (and, *a fortiori*, the question whether it is a price of *stable* equilibrium) is a question which leaves our Proposition III entirely untouched.[61] By the same reasoning, the question whether the "equilibrium" price can be said to be "determinate" in a situation in which more than one realized price would have claims to be regarded as an "equilibrium" price, in no way affects the substance of our Proposition III.[62]

If, on the other hand, prices are *not* realized at each of these several possible points of intersection of curves purporting to be curves of "demand" and "supply," this can be only because the curves involved are not market demand and supply schedules in our sense, even though they may be important in those stages in the "pre-formation of prices" which explain why the market demand and supply schedules have the position and conformation they do have.[63] Again, therefore, the substance of our Proposition III is left entirely untouched by the conten-

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[60] On the "stability" of the alternative positions of "equilibrium" resulting from the successive intersections of the curves of demand and supply, see the references to Walras and Marshall given in the preceding note. It is of more importance for our present purpose, however, to observe that the realized prices involved in our discussion need not necessarily be *equilibrium* prices, if by the latter is meant, for example, that the price must be such as to leave no demanders at that price unsatisfied as the result of inadequate supply at that price. In answer to Mill, therefore, Thornton might have protested that at *neither* of the two prices at which, according to Mill, the "law of price" would be "completely fulfilled," would *all* aspects of that "law" be "completely fulfilled": at neither of the prices "finally resulting," for example, would "the quantity offered for sale at a certain price, and the quantity demanded at that price" necessarily be "equal" (see above, p. 242, n. 42). This is one of the instances, in other words, in which Mill's exposition suffered from a failure to distinguish with sufficient sharpness between those aspects of the "law of price" which were concerned with the establishment of an *equilibrium* price, on the one hand, and the conditions necessary for the realization of a market price, on the other. (See above, p. 243, n. 43.) It remains true, nevertheless, that Mill's argument holds with undiminished rigor for the latter aspects of the "law."

[61] This is not to say, obviously, that the problem of the "equilibrium" price, and therefore of the "determinateness" of equilibrium, is of no importance for an understanding of why the market demand and supply schedules involved in our Proposition III have the position and conformation they do have. See again what is said on this matter above, pp. 234 ff., and also below, n. 63.

[62] It is worth noting that while Mill characterized the fact that "the law of price" may be "consistent with two different prices, and is equally and completely fulfilled by either of them" as an indication of "some amount of indeterminateness in its operation," neither Walras nor Marshall did so. Marshall, for example, characterized the case with which he was concerned simply as one of "multiple positions of equilibrium" (*Principles*, 472 n.).

[63] The point, again, is that a demonstration of the reasonableness of these (unrealized) "equilibrium" prices as goals for entrepreneurs to follow

tion that cases can be found in which no "determinate" price can be said to be located by the point of intersection of market curves of demand and supply.

iii. Precisely the same argument, finally, applies to the suggestion that "indeterminateness" is conferred upon the pricing process by the fact that the expectations of buyers and sellers with respect to further rises or falls in prices may give us a demand curve positively inclined, and a supply curve negatively inclined, over part of their respective lengths.[64] It is irrelevant for our purposes, for example, to point out that under such conditions "equilibrium will not necessarily work out at all," on the ground that it is uncertain "whether a decline in price will check itself by leading to more purchases, or will intensify itself by leading to less purchases."[65] The only kind of "equilibrium" which is involved in our Proposition III, as we have seen, is an equality between demand price and supply price for a given quantity of a commodity *in all cases in which prices are actually realized in the market for this quantity of the commodity.*[66] An "equilibrium" *in this sense*

carries with it the presumption that these entrepreneurs' plans for *present* market action, as represented by the market demand and supply curves of our Proposition III, will be *conditioned* by these goals. See above, pp. 234 ff.

[64] See, for example, F. W. Taussig, "Is Market Price Determinate?" *Quarterly Journal of Economics,* XXXV (1921), 396 ff., 402, 410 f. (The whole article, it may be observed here, should be called to the attention of those for whom the type of emphasis upon the element of "expectation" which is to be found in Keynes's *General Theory* provides one of its chief claims to a "revolutionary" character [see above, p. 179, n. 72]. Cf., for example, the last sentence of the paragraph at the top of p. 403 of the article cited, with the brilliantly written passages in Keynes's *General Theory* beginning with the last paragraph on p. 155 of that work and continuing on the following page. It should be added that the number of similar passages that could be cited from earlier writers on the particular point in question is very large.)

[65] Taussig, "Is Market Price Determinate?" *loc. cit.,* 397, 401.

[66] From passages such as those on pp. 397, 400 f., and 410 of the article cited, it is clear that Professor Taussig himself was concerned with the "determinateness" of "equilibrium," in some sense of the latter term other than that indicated in the text, despite the fact that the title of his article was such as to lead one to suppose that its author was concerned with the determinacy of "market" price (cf. also the reference to "any theory of market price" on p. 401, and the statement cited below, p. 254, n. 69), without specification that this "market" price must be that which will establish *"equilibrium"* in the "market." The point obviously bears upon the question of how far certain passages in John Stuart Mill are "inconsistent with the doctrine of the 'equation' " of "supply" and "demand" (cf. Taussig, *op. cit.,* 404 n.). For the "doctrine of the 'equation' " takes on different meanings depending upon whether (1) it is stated in a form which would make it equivalent to our Proposition III, and therefore directly relevant to the question whether purchasers will "buy at all" (cf. Taussig, *op. cit.,* 410) and sellers will "sell at all," so that market prices can be actually realized; or whether (2) it is stated in such a way as to relate it to the establishment of the conditions for "equilibrium."

can be obtained regardless of the direction of slope of demand and supply curves, and regardless of the frequency with which these curves change their conformation or position under the influence of "expectational" factors as between any two instances of realized price.[67] All that is necessary, in order to obtain a realized price, is that the two curves should intersect at some point, whose location is unequivocally given by the position and conformation of the market supply and demand curves prevailing at the moment the price is realized; and this proposition holds regardless of whether the prices thus realized are thought of as "fluctuating" within a "penumbra" established by the probable limits of fluctuation in the position and conformation of market demand and supply schedules over longer periods, or are regarded as not "fluctuating" at all over such periods.[68] There is no reason, therefore, to "admit" that "within the penumbra there is no determined or determinable market price" in any sense of that proposition which would invalidate our Proposition III.[69]

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[67] It is worth noting that Mill himself was perfectly prepared to admit that Thornton was "scientifically right" in suggesting the possibility of "an exception to the rule that demand increases with cheapness." After all, Mill remarked, "this rule, though general, is not absolutely universal" ("Thornton on Labour and its Claims," *loc. cit.*, 509). Unfortunately, however, Mill's further admission that such a case would be one "which the law of equalization [of 'supply' and 'demand'] does not reach," shows that he was in this instance regarding the "law," not as one relevant to the establishment of a *realized* price, but as one relevant to the establishment of an "equilibrium" price, in some sense of the term "equilibrium" other than that indicated in the text.

[68] Cf. Taussig, "Is Market Price Determinate?" *loc. cit.*, 398 ff. It is, indeed, Professor Taussig's discussion of the nature of the forces determining the width of his "penumbra" that shows that the question of the "determinacy of market price" which he discusses is really the question of how far realized market prices may be expected to conform to a price properly to be regarded as an "equilibrium" price. This, it will be observed, is a quite different thing from arguing that "there is much room for fluctuations of opinion *and therefore of price, . . .* and so for *indeterminate phenomena*" (Taussig, *op. cit.*, 405), if the latter proposition is taken to mean that the "fluctuations of price" in question are not capable of explanation by the weapons of economic analysis, including demand and supply schedules of the kind involved in our Proposition III.

[69] Cf. Taussig, "Is Market Price Determinate?", *loc. cit.*, 402. It should be added that the statement that where "the penumbra is wide . . . price is quite unpredictable" (*op. cit.*, 411) cannot be taken to mean that we are incapable of "predicting" price if *we are given adequate information with respect to the conformation and position of the market supply and demand curves involved*. After all, a very large part of the "general" Theory of Value undertakes to "predict" no more than this. Cf., for example, Smithies, "Equilibrium in Monopolistic Competition," *loc. cit.*, 115 n. On the sense in which economics generally can lay claim to the "power of prediction," see again Volume I, p. 45 of the present work, and the references given in n. 19 thereto.

3. *The alleged absence of a market supply curve.* The very concept
of an "intersection of market demand and supply curves" which under-
lies our Proposition III requires acceptance of a further proposition:
namely, that in all cases involving market action there will be a market
supply curve as well as a market demand curve for a particular com-
modity.[70]   The best known example of an apparent denial of this further
proposition is represented by the sweeping dictum of Wicksteed: "But
what about the 'supply curve' that usually figures as a determinant of
price, co-ordinate with the demand curve?   I say it boldly and baldly:
There is no such thing." [71]   The case of Wicksteed, however, is not the
only example of such an apparent denial that can be adduced.   It is
possible to cite other instances in which the existence of a "supply curve"
has been denied: not in general, but only under the special circum-
stances taken for discussion by the writers concerned.[72]   It is very
easy to show, however, that whatever may be said of the validity and
usefulness of these dicta *in the particular contexts in which they have
appeared,* and of the proposed alternatives to the concept of a "supply
curve" which have been presented on the basis of these dicta, neither
the dicta themselves nor the alternative constructions based thereon say
anything whatever against the validity of the elementary type of con-
struction involved in our Proposition III, when that construction is
properly understood.

   i. That this is so in the case of Wicksteed's argument becomes clear
when it is recognized, in the first place, that Wicksteed himself was
perfectly prepared to admit that the representation, by means of a
market supply curve, of what he himself insisted upon characterizing as
a "portion of the ['general'] demand curve" for a given commodity "is
a process which has its meaning and its legitimate function." [73]   This
"legitimate function," according to Wicksteed, is that of displaying "the
process by which the [realized] price is discovered" in the market.[74]
The fact that Wicksteed's celebrated dictum does not contradict our
Proposition III becomes still clearer when attention is called to a further
fact: namely, that Wicksteed's insistence that the method of represent-
ation just indicated "is wholly irrelevant to the *determination* of the
price" could not possibly have meant that the type of "disposition" (to

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   [70] The reader is again reminded that the meaning of, and the rôle as-
signed to, "supply curves" in the determination of realized money prices
is discussed in more detail in Chapter Eleven, below.

   [71] "The Scope and Method of Political Economy in the Light of the
'Marginal' Theory of Value and Distribution," *Economic Journal,* XXIV
(1914; p. 785 of the 1933 edition of Wicksteed's *The Common Sense of
Political Economy and Selected Papers and Reviews on Economic Theory;*
cf. also p. 542 of the latter work).

   [72] See the references given below, p. 257, n. 80, and p. 261, n. 86.

   [73] See *The Common Sense, etc.,* 785, and cf. also p. 516 of the same
work.

   [74] See *The Common Sense, etc.,* 785, and cf. pp. 507, 516 of the same
volume.

use Walras's expression) or "resolution" (to use Fleeming Jenkin's) which the market supply curve is intended to represent is itself "wholly irrelevant to the determination of the price." [75] For these "dispositions" and "resolutions" have to do with the prices at which potential sellers will be willing to sell or to refrain from selling (that is, to "reserve") the commodities they command: they involve, that is to say, the very concept of "reserve prices" which bulked so large in Wicksteed's own system.[76] To have meant to affirm that the facts with respect to the level of "reserve prices" are "wholly irrelevant to the determination" of realized prices would have been to stultify that system in one of its most important aspects.

Similarly, it must be remembered that, according to Wicksteed himself, the ordinary "curve of supply prices . . . is a mere *alias* of a portion of the ['general'] demand curve." [77] It follows that, in order to be able to interpret Wicksteed as having meant to argue that the market supply curve is "wholly irrelevant to the determination of the price" in any literal meaning of "wholly irrelevant," we should be compelled to interpret him as having meant to argue that "the portion of the ['general'] demand curve" which corresponds to the "curve of supply price" is itself "wholly irrelevant to the determination of the price," in the sense that the same market price would be realized regardless of the conformation and position of this particular "portion of the ['general'] demand curve." There is no evidence whatever that Wicksteed would have assented to such an absurdity.[78] His argument, therefore, can have been meant to refer only to the *"ultimate* facts that determine" price, in a sense of the word "ultimate" which is in no way relevant to the validity of our Proposition III.[79]

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[75] For Wicksteed's statement that the mode of representation indicated "is wholly irrelevant to the *determination* of the price," see *The Common Sense, etc.,* 785, and pp. 507, 516 of the same volume. For examples of the use of the expressions indicated in the text by Walras and Jenkin, respectively, see above, p. 185, n. 88, and p. 184, n. 83. It is to be observed that Wicksteed himself used the term "dispositions" in a meaning essentially identical with that in which it was used by Walras. See, for example, *The Common Sense, etc.,* 228.

[76] See, for example, Wicksteed, *The Common Sense, etc.,* 229 ff., 322 ff., 327 f.; and on the relation of the supply curve to the "curve of reserve prices," see especially p. 506.

[77] *The Common Sense, etc.,* 787. Cf. also pp. 498 ff. of the 1933 reprint.

[78] See, on the contrary, *The Common Sense, etc.,* 231 f., 823 f.

[79] The "ultimate facts" in question, according to Wicksteed, "are the amount of [the] stock [of a given commodity], and the state of their own preferences [that is, the preferences of "the persons constituting a market," viewed from their "own" point of view] and other people's preferences" (*The Common Sense, etc.,* 233; cf. also p. 503). The reasons, on the other hand, why Wicksteed denied that the "supply curve" was concerned with "the ultimate facts that determine price" had to do with his objections to the common association of that curve with "cost of production," which he insisted upon regarding as "merely the form in which the desiredness

ii. In recent years, on the other hand, the question whether "there is any such thing" as a "supply curve" has been raised, not in the sweeping form in which it was raised by Wicksteed, but in connection with special problems, such as that of monopoly.[80] It is again easy to show, however, that the "supply curves" whose "existence" or usefulness has been brought into question in this recent discussion are not the "*market*" supply curves involved in our Proposition III.

In some cases, for example, the difficulty has arisen from a simple failure to identify the particular "curve," in the diagrams used, which corresponds to the "market supply curve" of our Proposition III: that is, the curve which does in fact represent the quantity that a monopolist is "willing to sell," and the price at which he is willing to sell it, in the bargaining preceding each realized transaction.[81] In particular, there has been altogether too great a readiness to characterize *cost* curves as "supply curves," without adding a sufficiently emphatic reminder that while these cost curves certainly *condition* the price and quantity offers made by a monopolist in a given market situation, they by no means necessarily represent the price and quantity offers made by a monopolist

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a thing possesses for someone else presents itself to me" (*The Common Sense, etc.,* 787 f.; cf. pp. 380 ff. of the same work). It is clear that the question whether there is or is not "some principle other than that of desiredness, co-ordinate with it as a second determinant of market price" (*op. cit.,* 788) has nothing directly to do with the validity of our Proposition III, regardless of what may be thought of Wicksteed's method of stating the issues involved in the discovery of the "*ultimate*" "determinants" of price.

[80] The point may be said to have been raised *implicitly* by E. S. Chamberlin in connection with his discussion of the rôle played by "demand and supply curves" under monopoly (*The Theory of Monopolistic Competition,* 13, especially n. 2). Cf. E. R. Hawkins, "A Note on Chamberlin's Monopoly Supply Curve," *Quarterly Journal of Economics,* LIII (1939), 641 f., where it was contended that Chamberlin's argument was "tantamount to saying that there is no supply curve" under monopoly. In his "Reply," in the same issue (p. 643), Chamberlin insisted, quite correctly, that his argument, *as such,* was not "tantamount to saying that there is no supply curve" under monopoly, since he had explicitly characterized his curve *SS'* as the "supply curve." From the discussion which follows, however, it should be clear that Chamberlin's *SS'* is not a market supply curve in the sense in which that term is employed in our Proposition III; and it should also be clear, from that discussion, that whether either of these curves is to be regarded as "interesting" (cf. Chamberlin's "Reply," p. 643) depends entirely upon the problem in which one happens to be "interested."

[81] From Chamberlin's argument on p. 13, n. 2, of his *Theory of Monopolistic Competition,* it is clear that this type of supply curve is not represented by *AM* in his Figure I (p. 13). (This is not to deny, of course, that *AM would* represent "the monopolist's supply curve in those cases in which—contrary to the conditions assumed by Chamberlin—"the quantity *OA*" *is* "thrown on the market regardless of price.") Neither, however, is the monopolist's market supply curve *SS'*; since the latter does not represent the prices at which the monopolist is willing to sell various quantities.

in a given projected transaction.[82]   Even, moreover, where the "monopoly supply curve" has been distinguished from a curve of *costs*, it has

(For example, the price at which, in Chamberlin's Figure I, the monopolist is willing to sell the quantity *OA* is not *AE,* but is *AQ.* Cf. also the following note.)   In Chamberlin's Figure I, the market supply curve of our Proposition III, for all quantities up to the quantity *OA,* would be *KQ.* For it is this curve that represents the amounts which, under the special cost- and revenue-estimates assumed in Chamberlin's Figure I, the monopolist is willing to sell at the price *AQ. If,* moreover, *we can assume that DD′ represents the alternative purchase plans of the potential buyers at various prices, instead of merely the estimate of those plans made by the monopolist,* then it will be the intersection of the market supply curve of which *KQ* is a segment with the demand curve *DD′* which will make *AQ* the *realized* price and *OA* the realized volume of sales (quantity sold). (On the significance of the clause italicized, see what is said below, p. 260, n. 84.)   It need be added only that the mere fact that a curve such as that of which *KQ* is a segment is parallel to the quantity axis over a considerable range, is no more of an argument against calling it a supply "curve" than a similar contention would be in the case of Chamberlin's horizontal "demand curve for the product of any individual seller" (*The Theory of Monopolistic Competition,* 17); though the purist may prefer to call this part of the market supply curve a "supply line" rather than a "supply *curve*" (cf. the references to Fleeming Jenkin's use of the terms "demand *line*" and "supply *line*" given above, p. 250, n. 58).

[82] The unhesitating identification of supply curves with *cost* curves is so common in recent, as well as in earlier, discussion that no lengthy list of citations need be given here.   (On the general point involved, which has great significance for the case of competition as well as for monopoly, see what is said below, pp. 557 ff.)   Of the writers who have taken pains to distinguish between cost curves and supply curves, Chamberlin should be mentioned particularly (see, for example, his *Theory of Monopolistic Competition,* 20).   Yet even Chamberlin's *SS′* is a curve "either of average or of marginal *costs*" (*op. cit.,* 15 n.), and not a market supply curve in our sense of the term (cf. the preceding note).   It is hardly surprising to discover, therefore, that the curve *SS′* proves to be "uninteresting" (cf. Chamberlin's Reply to Hawkins, *loc. cit.,* 643), when it is assigned the rôle properly to be accorded to a market supply curve of the form, say, of *KQ.*   For the curve *SS′* becomes "interesting" only when it is assigned the function properly assigned to *cost* curves: namely, the function of helping, *along with other factors, such as the expected conditions of demand,* to decide *what the supply price will be* in any given market situation.   This is a quite different thing, it will be observed, from saying that cost curves are in themselves sufficient, in all cases, to decide what the market supply price will be; and this in itself is enough to justify rejection of an identification of cost curves with market supply curves.   It should be observed, in particular, that Chamberlin's *SS′* does not in fact satisfy the criterion that it should define "maximum amounts which a monopolist would be willing to put on the market at various prices" (cf. Chamberlin, "Reply," *loc. cit.,* 642 f.).   Again it may be pointed out that under the conditions assumed in Chamberlin's Figure I with respect to estimates of cost and revenue, the "maximum amount which a monopolist would be willing to put on the market" at price *AE,* for example, is not

sometimes been defined in such a way as to obscure the relation of
market supply curves to the determination of *realized* prices.[83]   And,

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OA, but zero; since, according to these estimates, the optimum results for
the monopolist would be obtained by attempting to sell the quantity *OA*,
not at the price *AE*, but at the price *AQ*.

   [83] It is possible, for example, to conceive of a type of "monopoly supply
curve" which would represent the amount of commodity, not that a
monopolist would be willing to sell at a given price *in a given situation*
(as in the case of *KQ*), but the amounts he would be willing to sell at
various prices under *different conditions with respect to cost- and revenue-
estimates*.   (It would likewise be possible, of course, to construct a "monop-
oly supply curve," with the same general implications, which would be
of the Auspitz-Lieben form [see below, p. 263, n. 93]: that is, a curve
showing the amounts that a monopolist would be willing to sell at various
possible "money *demands*" for the quantities in question.   The argument
which follows, however, would hold in all essential respects for this type
of "monopoly supply curve" as well.)   No formal objection could be
raised to the presentation of a "monopoly supply curve" of this type,
providing it be understood (1) that the supply prices included in such a
curve are what they are as the result of the monopolist's expectations con-
cerning changing conditions of cost and revenue for the amounts of com-
modity involved; and (2) that in the explanation of the determination of
realized prices and *realized* amounts sold, a curve of this type can be used
only in conjunction with a *market* supply curve of the type of our *KQ*.
From (1), it follows that Chamberlin is right in insisting that a monopoly
supply curve of the general form indicated would have to consider "all
possible cost curves . . . as well as all possible demand curves" ("Reply,"
loc. cit., 644); though it follows also that, after such "consideration," the
monopolist may decide that one set of expectations with respect to the
probable cost and demand conditions for each level of output is more
reasonable than other sets of expectations, and may therefore base his
curve of supply prices for various levels of output upon these "most rea-
sonable" estimates of cost and demand for each level.   The usefulness of
such a device will depend upon the extent to which it is regarded as a
realistic picture of the way in which a monopolist draws up his "plans"
for market action.   It is much more important to observe, however, that
it is impossible to represent the "market action" taken by the monopolist
upon the basis of such a "supply curve" unless, in accordance with (2),
supplementary use is made of a *market* supply curve of the type *KQ*, one
such market supply curve being drawn parallel to the quantity axis at
each point on the "monopoly supply curve" which is held to condition the
monopolist's market action in each projected transaction.   It is easy to
see why market supply curves of the type *KQ* would be not only unneces-
sary, but actually wrong, in the case of *competition*.   Whenever, under
competition, the supply price for a given quantity of commodity is greater
than the demand price for that quantity, it will be impossible to realize
that supply price not only for *this particular quantity of commodity*, but
also for a smaller quantity of commodity: because some competitor is
bound to offer to supply a smaller amount at a lower price.   Under monop-
oly, however, such supply offers at lower prices are not inevitable.   The
monopolist can maintain his supply price at any level he chooses; and if
the purchaser's demand curve does not lie *entirely* below this supply price,

finally, there has often been an inadequate appreciation of the implica-
tions, for the interpretation of both demand and supply curves, which
follow from a clear recognition of their *ex ante* and "expectational"
character.[84] In short: the statement that only "minor importance"
attaches to "the revered proposition that price tends to the point where
supply and demand are equated," whatever its significance in the par-
ticular context provided by the recent discussion of the theory of

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this supply price will be actually realized; though of course the amount of
sales realized will be considerably less than the monopolist would be
willing to make at the price indicated. Thus, it is market supply curves
of the form *KQ* whose intersection with the market demand curve will
determine what the realized price will be, and what amount will be sold
at that realized price, and not the intersection of the market demand
curve with a "monopoly supply curve" of the type described earlier in
this note.

[84] It has been suggested, for example, that there is no need to conceive
of "price" under monopoly as resulting from the intersection of a market
demand schedule with a market supply schedule, in the manner indicated
by our Proposition III. Indeed, it has been argued that it is meaningless
to conceive of it in this way, on the ground that, according to Chamberlin's
Figure I, for example, the "price" *AQ* is really "established" by the inter-
section of *AM* with *DD'*, *AM* in turn being located by the intersection of
dd' with *SS'*; so that our "market supply curve" *KQ* is not only superfluous
for the "determination" of price or the quantity sold, but actually is
capable of being drawn only after price and the quantity "sold" has already
been "determined." The error in this argument, however, lies in confus-
ing *supply* prices and *expected* quantities "sold" with *realized* prices and
realized quantities sold. What is "established" or "determined" by the
reasoning underlying Chamberlin's Figure I is the monopolist's *supply
price:* if this is what is meant by the statement that *KQ* can be drawn only
after "price" has been "determined," there can be no objection. What is
objected to is the further suggestion that there is enough, in Chamberlin's
Figure I, without the use of a market supply schedule of the form *KQ,*
to tell us what price will be actually *realized,* and what amounts will be
actually *sold.* That, *for this purpose,* the use of a market supply curve of
the type *KQ* is neither "meaningless" nor "uninteresting" follows from a
consideration of what will happen if we cannot in all cases (and we
cannot) make the assumption italicized above on p. 258, n. 81: namely,
that *DD'* represents the alternative purchase plans of the potential buyers
at various prices, instead of merely the estimate of those plans made by
the monopolist. It is conceivable, for example, though probably not com-
mon, that the estimates of the monopolist with respect to the position and
conformation of *DD'* may go completely astray, and that the whole of
DD' will lie below *KQ.* In such a case, as we have seen (above, p. 244,
n. 47), no sales will be effected, and no price will be realized, even though
KQ would still be the "supply curve," since it does represent the amounts
that the monopolist is *willing* to sell at the price *AQ.* It is, moreover, not
only conceivable, but probable, that in many cases the monopolist will
so misjudge the conformation of the demand curve *DD'* that while the
realized *price* may still be equal to *AQ,* the amount of realized *sales* (the
"amount actually bought and sold") will be, not *OA,* but something con-

monopoly, is not a statement which in any way invalidates, or renders less "meaningful," the substance of our Proposition III.[85]

iii. Precisely the same thing, moreover, must be said of the proposition that "the supply curve of *labor*" may be said to "exist" "when there is atomistic competition among workers for jobs"; but that "union wage-fixing activity causes it to disappear."[86] For it requires only slight consideration to observe that the "supply curve" which is thus held to "disappear" is a supply curve (namely, "the *competitive* supply curve") other than that which actually confronts the prospective purchaser of labor in a given market situation dominated by trade-union regulations.[87] In the latter situation, there is still a market "supply curve" whose position, in combination with the position and conformation of the demand curve of purchasers of labor, will determine how much labor will be actually hired and at what wage rate, and therefore the height of "realized" wage rates and the amount of "realized" purchases of labor ("employment"). This "market supply curve" is, of course, a line parallel to the "quantity" axis at the level fixed by trade-union

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siderably less than OA, and therefore very much less than OC—the amount of realized sales being determined, not by the fact that the monopolist "chooses" to sell the amounts OA or OC at the indicated price (in the sense that he would be *willing* to sell those amounts at the indicated price), but by the point of intersection of the demand curve DD' with the market supply curve KQ. These are results which can be obtained only by the use of market supply curves and market demand curves of the type indicated in our Proposition III; and this is merely another way of saying that a market supply curve of the form KQ is neither meaningless nor superfluous when our problem is that of relating analysis of the type summarized by Chamberlin's Figure I to the determination of *realized* prices and *realized* quantities sold.

[85] For the statement quoted with respect to the "minor importance" of "the revered proposition" in question, see Chamberlin in his "Reply" to Hawkins, *loc. cit.*, p. 644, and cf. above, p. 241, n. 40, and p. 246, n. 51.

[86] Cf. M. Bronfenbrenner, "The Economics of Collective Bargaining, *Quarterly Journal of Economics,* LIII (1939), 542. Interestingly enough, the contention that the market supply curve for labor would "disappear" was not, to my knowledge, advanced in those parts of the older discussion of the peculiarities of the "supply curve of labor" which were concerned with the proposition (at best ambiguous and at the worst in contradiction with the facts) that, in the *absence* of trade-union activity, labor has no "reserved price." For examples of discussion of the latter proposition itself, see Thornton, *On Labour,* 55, 69 ff.; Fleeming Jenkin, *The Graphic Representation, etc.,* 95 ff., 104 f.; Marshall, *The Economics of Industry,* 200; Wicksteed, *The Common Sense, etc.,* 322 ff. And for examples of the type of supply curve which was held to represent the case of supply supposedly offered without reservation with respect to price, see Walras, *Eléments d'économie politique,* 161 (section 153), and Fleeming Jenkin, *op. cit.,* 95 (including the reference to Jenkin's Figure 7 [p. 83]).

[87] Cf. Bronfenbrenner, "The Economics of Collective Bargaining," *loc. cit.,* 542 f., 545 f.

stipulations.[88]   Whether the wage rate thus involved is properly to be
regarded as "the equilibrium wage," and whether the employment
realized is properly to be regarded as "the equilibrium volume of em-
ployment," are questions which may properly be raised when our prob-
lem is other than that of identifying the "market supply curves" which
are involved in given realized transactions with respect to the purchase
of labor.[89]   But the "supply curve of labor" which is relevant to the
determination of these "equilibrium" magnitudes should never have been
regarded as *identical*, under all circumstances, with the market supply
curve. The alleged "disappearance," therefore, of the particular type
of "supply curve of labor" which is relevant to these other questions
again leaves the substance of our Proposition III completely untouched.

4. *Realized prices and market demand curves.* It may be observed,
finally, that only a confusion of the "market demand schedules" of our
Proposition III with *another type* of demand schedule could lead to a
rejection of Proposition III on the ground that "it is a rash assumption
in general" that realized "exchanges take place on the demand and
supply curves," and that "every price-quantity observation . . . repre-
sents the intersection of two curves." [90] For the price which, it is
argued by these commentators, need not necessarily lie on "the demand
curve" is an "equilibrium" price, and not a "market price," in the literal
sense of a price actually realized on the market.[91]   And the "demand
curve" on which the *"equilibrium* price" would lie, and which would
bring it about that all realized exchanges would "by definition" result
in "equilibrium" prices, is not our market demand curve; it is rather
the type of demand curve used by Marshall in his discussion of the de-
termination of the "true *equilibrium*" of market price.[92]   What this
means, of course (apart from the warning that it carries against a
misinterpretation of the results obtained in attempts to derive "statistical
demand curves") is that we must be prepared, in the manner insisted
upon in our Proposition I, to *relate* these "equilibrium" prices and the
demand curves held to be relevant to the determination of these

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[88] In this respect, obviously, there is no difference between the "market
supply curves" of labor monopolies and those of other types of monopoly.
See above, p. 258, n. 81.

[89] They are questions, also, which may properly be raised whenever we
go on to ask what factors may be expected, over longer periods, to *condi-
tion* in some degree the position of "market supply curves" of labor, of
the type indicated in the text. Cf. D. H. Robertson, "Wage-Grumbles,"
in *Economic Fragments,* 43 f., and the reference to Pigou given on p. 44.

[90] Cf. Stigler, "The Limitations of Statistical Demand Curves," *loc. cit.,*
474.

[91] Cf. Stigler, *loc. cit.*

[92] See above, p. 235, n. 30; and cf. Stigler, *loc. cit.,* on the need for the
presence of "recontract" (which need of course not be present in reality)
if we are to be able to assume that "all changes will by definition take
place on the demand curve" which is required if "equilibrium" is to be
obtained.

"equilibrium" prices, to the actually *realized* prices with which we are here concerned, and to whose determination the market demand and supply schedules of our Proposition III are alone *directly* relevant. What it does not mean is that Proposition III itself is either invalidated, or is made useless, by the very fact that its truth is established virtually "by definition." For the latter is true also of any proposition alleging that all realized exchanges need *not* necessarily take place on a "demand curve" which, "by definition," is held to be the curve required in order to establish an "equilibrium" price.

IV. In formal terms, the consistency of Propositions II and IV is established by the fact that *we can regard as applying to either proposition* the expression $D = pq$, in which D represents the *amount of money spent* by the "demander" of a given quantity of commodity (q) in the purchase of q, and p represents the money price of a unit of that commodity.

In terms of our Proposition III, the magnitude of D, as just defined, will of course be given by the area of the rectangle obtained by dropping perpendiculars from p to the x (quantity) and y (price) axes, respectively, since the perpendicular to the quantity axis is the realized price p, and the perpendicular to the price axis is the quantity purchased, q. These "areas"—and therefore the possible values of D— are of course represented, in the case of "Auspitz and Lieben" demand curves, by the *ordinates* of such curves.[93]

[93] On the propriety of characterizing the type of demand curve indicated as an "Auspitz and Lieben" demand curve, see what is said below, p. 268, n. 102; and on the formal translation of the more familiar type of "demand curve" into an Auspitz and Lieben curve, see especially H. Cunynghame, *A Geometrical Political Economy,* 108 ff., 111. (Cf. also the treatment of analogous concepts on the side of supply by A. L. Bowley, *Mathematical Groundwork of Economics,* 33 f.) When, as in the case of Auspitz and Lieben, the ordinates are taken to measure the amount of *money* that will be offered for particular quantities of commodity (measured by the distance along the abscissa from the origin to the point on the abscissa to which the ordinates are drawn), it is clear that the quantities involved are precisely the same as in the more familiar type of demand curve. For, since $D = pq$, it follows that $p = D/q$. And since the ordinates of an Auspitz and Lieben curve correspond to our D's, and the abscissas correspond to our q's, it follows that *price* is represented on an Auspitz and Lieben curve by the tangent of the angle made by a line drawn from the origin to the point on the Auspitz and Lieben curve from which the ordinate (D) is drawn to the abscissa (q). See Auspitz and Lieben, *Untersuchungen über die Theorie des Preises,* 18 f.; Cunynghame, *A Geometrical Political Economy,* 111; and Edgeworth, *Papers Relating to Political Economy,* II, 336. There are no reasons, therefore, for objecting to the "transformation" of one type of curve into the other, *within the range of problems with which this work is concerned.* On the con-

The concept of "demand" summarized by the D of the expression $D = pq$ is, of course, of very ancient lineage. Indeed, even the algebraic expression $D = pq$, when regarded as a statement of the proposition that a realized price (p) will be what it is as a result of the relations between the dimensions of a realized money "demand" (in the sense of quantity of money $[D]$ expended upon the commodities in question) and the quantity (q) of the commodity bought by this realized money demand, is one of the oldest propositions of the "general" Theory of Value: it dates, as we have seen, from the latter part of the eighteenth century.[94]

What was missing in these earlier writers, of course, was a clear recognition of the relation of the magnitude of the realized money demand (D) for a particular commodity to the position and conformation of Marshallian demand and supply schedules for particular commodities—a recognition which was made impossible by the comparative lateness of any general acceptance of the concept underlying such schedules.[95]   Yet it would be quite wrong to suppose that the older

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trary, each type of demand curve has its advantages for different purposes. For an example, on the other hand, of the objections that might be raised to certain uses of such "transformations" within the *theory of international trade,* see Viner, *Studies in the Theory of International Trade,* 584 f.

[94] See above, p. 20, n. 49, and the reference to Fasiani there given. It may be added that while of course the algebraic expression used, by the early Italian writers cited by Fasiani, to summarize the proposition that price is to be regarded as determined by the relation between "demand" and "supply," could be applied to the case of "direct exchange" of two commodities without the intermediacy of money (see below, p. 268, n. 101). it is striking that the exposition of these early writers should so often have been such as to indicate that they usually had in mind cases in which the "supplies" were offered against a demand in the form of *money.* See, for example, the passage from Verri, on which the formula of Frisi (cf. above, p. 20, n. 49) was avowedly based, and which is quoted by Fasiani, "Note sui Saggi Economici di Francesco Fuoco," *loc. cit.,* 98. In this passage, Verri went out of his way to indicate that he thought of increased "supplies" as being offered against "the universal commodity"— that is, *money.* The conception of "demand" as a *money* demand is, indeed, particularly clear from the details of Verri's exposition. See especially pp. 127 ff., of the edition of Verri's *Meditazioni sulla economia politica* contained in Custodi's *Scrittori classici di economia politica,* Parte Moderna, XV.

[95] It will be observed that the statement in the text specifically refers to the *general acceptance* of the concept represented by a Marshallian "demand schedule." For an example of what may be regarded as an early adumbration of the latter concept, see the quotation from Galiani given above, p. 20, n. 49. The early uses of the so-called "Law of Gregory King" are of course also relevant in this connection (see above, pp. 147 ff.). It remains true, nevertheless, that the more common concept of "demand" in the earlier literature was that represented by the D of the expression $D = pq$. In the argument which follows, therefore, I have not hesitated to refer to this concept of "demand" as the "older" one.

(and, as we shall see, the more inclusive) concept of "demand" which is represented by our D disappeared from the literature on the "general" Theory of Value as soon as the concept of a demand schedule, of the Marshallian type, made its appearance. Cairnes, for example, may be taken as typical of writers of the generation immediately preceding that in which, as a result of Marshall's work, the concept of a demand schedule became part of the everyday equipment of economists; and Cairnes's treatment of the concept of "demand" itself represented a kind of struggle to relate the older concept of "demand" to what amounts to the modern concept of a demand schedule.[96] And the same thing may be said of the attempt by Simon Newcomb, of the same generation as Cairnes, to provide what Newcomb himself characterized as "a single theory of demand as a mathematical quantity," in the sense of an account which undertook to bring together into a unified theory the "separate results" previously obtained by Newcomb with respect to the concept of "demand" in his description of the functioning of the economic process.[97]

It would also be a mistake, moreover, to suppose that the emergence into general use of the concept of a demand schedule in the modern sense led in all cases to a refusal to make use any longer of the older concept of "demand," in the sense indicated by the D of the expression $D = pq$. Cournot, for example, certainly made use of the *concept*, even though he did not use the notation just indicated; and indeed a careful reading of his exposition shows that he was perfectly aware of the relation between the older concept of demand and the "demand"

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[96] See Cairnes, *Some Leading Principles of Political Economy Newly Expounded*, 25, 27 ff., 36 ff.; and cf. Marshall, "Mr. Mill's Theory of Value," *Memorials of Alfred Marshall*, 129, n. 2, and *Principles*, 97, n. 1.

[97] See Newcomb's *Principles of Political Economy,* Book IV, Chap. IV (pp. 348 ff.). Among the "separate results" previously obtained by Newcomb, it should be observed, was the conception of "demand" (in the sense of "quantity demanded") as a function of price (see Newcomb's "First Law of Demand," on p. 217 of his *Principles*); and this concept of demand was discussed both (1) in relation to the facts with respect to *income* (p. 218) and (2) in such a way as to make his distinction between "sensitive" and "insensitive" commodities roughly equivalent to the Marshallian "elasticity of demand" (pp. 218 f.). In the later chapter cited at the beginning of this note, on the other hand, Newcomb's concern was primarily with the concept of demand which he himself called "the market demand" (M). Newcomb's discussion of his concept of "the market demand" was made unnecessarily complicated, to be sure, by the fact that he insisted on defining this "market demand" in such a way as to make it dependent upon the assumption of a "fixed price" for the commodity against which the "market demand" was directed. It is clear, nevertheless, that Newcomb's M is the virtual equivalent of our D; whereas his own D is equivalent to our q, in the expression $D = pq$. Newcomb's expression $D = M/P$ (p. 350), therefore, when transliterated into the form $q = D/p$, is seen to be the equivalent of our $D = pq$. On Newcomb's further treatment of the relation between the two concepts of "demand," see below, pp. 272 f.

involved in his own "law of demand"—that is, in "demand schedules"
of the type made familiar by the work of Marshall.[98]   The matter is
clearer, to be sure, in the case of Walras. For Walras can hardly be
accused of unfamiliarity with the concept of a "demand schedule," in
the modern sense of the term. It is of considerable interest to ob-
serve, therefore, that, in illustrating his proposition that "the effective
demand . . . for one commodity in terms of another is equal to the
effective supply . . . of this other commodity multiplied by its price
in terms of the first commodity," Walras actually wrote a series of
algebraic expressions which are directly translatable into our $D = pq$,
and therefore into the ancient formulations of the proposition that
"price" is determined by "supply" and "demand," as that proposition
was stated before the concept of demand and supply *schedules* became
widely accepted in economic literature.[99]   The point is still clearer,

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[98] Cournot's own D, with its various subscripts (D_0, D_1, and so on),
was defined as the *amount of a given commodity sold*. (Cf. Cournot's
Researches, 47, 51, 53 f.; and see above, p. 193, n. 105.) It is therefore the
equivalent of the q in our expression $D = pq$. The magnitude, on the other
hand, which, in Cournot's exposition, cőrresponds to *our D* was character-
ized by Cournot as "the total *value* of the quantity sold," and was repre-
sented by the expression $pF(p)$. See Cournot's *Researches*, 52 ff. Since
Cournot himself wrote, with respect to his own D, the expression $D = F(p)$,
and since, as we have seen, his own D is equal to our q, it follows that his
expression $pF(p)$ is equal to our pq, and therefore, on the basis of our
equation $D = pq$, is equal to our D.

[99] Cf. Walras's *Eléments*, 57 ff. The expressions in question are of the
general form $O_b = D_a p_a$, in which O_b represents the quantity of commodity
(*B*) exchanged for commodity (*A*), and p_a is the price of commodity (*A*)
in terms of (*B*). If, in the manner later adopted by Auspitz and Lieben,
we let O_b represent the quantity of *money* exchanged for commodity (*B*);
and if we remember, further, the proposition of J. S. Mill, adopted in
substance by Walras, that "the money and the goods . . . seeking each
other for the purpose of being exchanged . . . are reciprocally supply and
demand to one another" (see Mill's *Principles*, Book III, Chap. VIII,
sec. 2; p. 491 of the Ashley edition), then it follows that O_b, as so inter-
preted, is equivalent to our D, and that Walras's D_a, like Cournot's D,
is the equivalent of our q. (The justification of the statement that Mill's
proposition was adopted in substance by Walras becomes particularly
clear if the passage which appears on pp. 157 ff. of the "definitive" edition
of Walras's *Eléments*—and which likewise makes use of expressions of
the form indicated—is read in the version in which it appeared in the
first edition of the *Eléments* [151 ff.]. For in the earlier version, one of
the two "commodities" thus regarded as "reciprocally supply and demand
to each other" was *money*, instead of merely the *"numéraire"* commodity.
On the reasons for, and the meaning to be attached to, this kind of sub-
stitution of *"numéraire"* for the *"monnaie"* or *"numéraire et monnaie"* of
the earlier edition of the *Eléments*, see my article, "The Monetary Aspects
of the Walrasian System," *loc. cit.* 179 ff.)   Hence the expression $O_b = D_a p_a$
reduces to our expression $D = pq$. It may be added that Walras followed
Cournot in using expressions not only of the form $D_a = F_a(p_a)$, but also

moreover, from the work of Marshall, on the one hand, and that of Auspitz and Lieben, on the other. As we have seen, Marshall's discussion of Cairnes's criticism of John Stuart Mill's treatment of the concept of "demand" shows that Marshall was perfectly aware of the essentially *complementary* nature of the relation between the older concept of "demand," on the one hand, and the concept of "demand" summarized by a "demand schedule" of the type that he himself preferred to use *for most purposes*.[100] That this was not an idle gesture in Marshall's own case is shown by the fact that, in certain problems (specifically, problems of international trade theory), he made use of a type of "demand curve" which is itself based upon the concept of "demand" for whose superiority Cairnes had argued with such vigor.[101]

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of the form $D_a p_a = F_a(p_a)p_a$. See Walras's *Eléments,* 60, and cf. above, p. 266, n. 98. It was only to be expected, therefore, that he should also have written $O_b = D_a p_a = F_a(p_a)p_a$. In the light of what has been said with respect to the relation between Walras's expression $O_b = D_a p_a$ and our own expression $D = pq$; and in the light of the further fact that Walras continued to make use of expressions of the form $O_b = D_a p_a$ in the very "Lesson" of his *Eléments* (pp. 157 ff.) in which he presented his "purchase curve" *(courbe d'achat),* and explicitly identified this "purchase curve" with the Cournot demand curve (cf. Walras's *Eléments,* 162), it should hardly be necessary to labor the point that the full expression $O_b = D_a p_a = F_a(p_a)p_a$ provides all that is necessary to justify the substance of our Proposition IV: namely, that the expression $D = pq$ may be regarded as applying to our Proposition III, as well as to our Proposition II.

[100] See the references to Marshall given above, p. 265, n. 96. The failure of later writers to heed Marshall's example in recognizing the *complementary* nature of the two concepts of "demand" has led, as one might have expected, to a series of futile quarrels within monetary theory itself. See, for example, the discussion of the concept of a "demand for money" in Cannan, *Money,* 72 f. (though contrast p. 76 of the same work), and T. Greidanus, *The Value of Money* (1932), 45 f.

[101] According to Edgeworth (Palgrave's *Dictionary,* I, 542), the type of "demand curve" in question was "first introduced by Professor Marshall in a paper read before the Cambridge Philosophical Society, 1873." In the printed *Proceedings* of the Society, however (II, 318 f.; published in 1876), the only type of demand curve described was one of the Cournot type; though the discrepancy may be explained by the fact that, according to Marshall himself (*Principles,* first edition [1890], p. xi, n. 1), his paper had been only "briefly reported" in the printed *Proceedings*. It would seem clear, at any rate, that the first printed document by Marshall containing demand curves of the type used more extensively by Auspitz and Lieben was the privately printed set of chapters on *The Pure Theory of Foreign Trade* (1879; see pp. 7 ff., and the charts inserted at the end). That the type of demand curve in question is in fact based upon the concept of "demand" sponsored by Cairnes is clear from the fact that in both cases "Demand" is defined as "quantity *offered*" in the purchase of a commodity, or, as Marshall put it, the amount which the prospective purchaser "would be willing to *give*" for the commodity (see Cairnes, *Some Leading Principles of Political Economy Newly Expounded,* 25 f., and Marshall, *The Pure Theory of Foreign Trade,* 7) (italics mine in both

It is the work of Auspitz and Lieben, however, which may be said really to have demonstrated the possibility of constructing a consistent and "powerful" set of weapons for price analysis (the second adjective was applied to the work of Auspitz and Lieben by Marshall) which was based throughout upon the concept of "demand" preferred by Cairnes.[102]   And even if comparatively few, among later writers on the "general" Theory of Value, adopted the practice so emphatically recommended by Cairnes and so consistently adhered to by Auspitz and

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cases). It is true, to be sure, that Marshall's examples were always in terms of an exchange of one commodity, such as cloth, for another commodity, such as linen; but, as Edgeworth pointed out, there is no reason why the place of one of these "commodities" in the diagram could not be taken by "the amount of money," or, as Cairnes put it, "the quantity of purchasing power" offered in the purchase of a given quantity of a "commodity." In addition to the reference to Edgeworth's article in Palgrave's *Dictionary* given at the beginning of this note, see Edgeworth's *Papers Relating to Political Economy*, II, 294, 335 f., 359; and cf. also the following note. It is clear, therefore, that those, among Marshall's disciples, who have recognized the validity of our Proposition IV, have merely been making explicit what may be regarded as having been implicit in Marshall's analysis. See, for example, the reference given above, p. 21, n. 51, to Lavington's use of the expression $P = D/S$ as a "quite general expression [for price as determined by "demand" and "supply"]—one which is applicable not only to money but to any kind of commodity," and his subsequent translation of the expression $P = D/S$ into a Quantity Equation of the Fisherine form; and see also Robertson, "A Survey of Modern Monetary Controversy," *loc. cit.*, 7 (*Essays in Monetary Theory*, 139), on "the Marshallian concept of market equilibrium [cf. above, p. 233, n. 27], with price emerging from the mutual impact, at each moment, of the existing flows of money and of goods" as amounting essentially to "the concept of the quantity theory" (read: "quantity equations").

[102] For the characterization of the work of Auspitz and Lieben to which reference is made in the text, see Marshall's *Money, Credit, and Commerce,* 331 n. Particularly noteworthy, in the treatment by Auspitz and Lieben, is not only the fact that they applied the general type of "demand curve" in question to the theory of "*domestic* values" as well as to the theory of foreign trade (contrast the comments of Marshall, *The Pure Theory of [Domestic] Values,* 1 f.), but also the fact that "demand" was always understood by Auspitz and Lieben as a sum of *money,* rather than a quantity of "linen" or "cloth" offered in exchange for the quantity "demanded." On the second point, in particular, see, for example, Auspitz and Lieben's *Untersuchungen über die Theorie des Preises,* pp. xiv, 15 ff., 28, 42, 44, 61, 181, 250; and cf. the comment by Lieben himself in his article, "Die mehrfachen Schnittpunkte zwischen der Angebots- und der Nachfragekurve," *Zeitschrift für Volkswirtschaft, Sozialpolitik, und Verwaltung,* XVII (1908), 616. The generous acknowledgment by Lieben, in the article just cited, of Marshall's priority in the use of the general type of curve in question, despite the fact that Auspitz and Lieben had made their own findings quite independently of Marshall (cf. Lieben, *loc. cit.,* and the acknowledgment of this fact by Marshall in the passage cited at the beginning of this note) should prevent any serious quarrels as to whose name should be attached to this type of curve for purposes of

Lieben, there is a sufficient number of cases in which explicit use was made of the older concept of "demand" to support the suggestion that this older concept has at no time really disappeared from the literature on the "general" Theory of Value.[103]

When, on the other hand, one turns to monetary theory, the antiquity of the lineage of the expression $D = pq$ is even clearer. Recognition of the fact that it is *money* which, in a money economy, must be regarded as in some sense the *"exchangeable representative of Demand"* (ὑπάλλαγμα τῆς χρείας), is at least as old as Aristotle.[104] For

easy reference. Yet the two distinguishing characteristics of the treatment by Auspitz and Lieben to which attention has been called are sufficient to justify a preference for referring to these curves as "Auspitz and Lieben curves," rather than as "Marshall's curves," particularly when the latter are discussed in a context concerned with their applicability to problems other than those (of foreign trade theory) to which Marshall himself applied them. Contrast H. Cunynghame, *A Geometrical Political Economy*, 2, 107 ff. On the other hand, the second peculiarity of Auspitz and Lieben's treatment of these curves—namely, their consistent use of *money* as the "commodity" representing "demand"—would recommend a more specific designation of their curves than merely as "Supply-and-Demand Curves" (cf. Edgeworth, *Papers Relating to Political Economy*, II, 353 n., 355 ff.), or even (as in the case of supply, or production) as "integral supply curves" (cf. Bowley, *Mathematical Groundwork of Economics*, 31 ff.).

103 The older concept has, in fact, appeared in modern economic literature much more frequently than one would suppose if one had regard only to the actual use, by later writers, of demand curves of the general Auspitz and Lieben type—either in the theory of international trade or in occasional applications outside of the latter field, such as those of Edgeworth (cf. the latter's *Papers Relating to Political Economy*, II, 302 f., 308 f., 335 ff., 359). See, for example, the reference to Lavington's *The English Capital Market* given above, p. 268, n. 101. It should be observed that, according to Lavington, "if this equation is applied to express the value of cloth or copper," the D ("demand"), unlike the D of Cournot and Walras (which, as we have seen, is the equivalent of our q), is "the amount of *money* exchanged against cloth or copper." For a further example of the use of "demand" in this sense, in discussions of the "general" Theory of Value, see Davenport, *Economics of Enterprise*, 39 f., 52, 274, 312, on the concept of "the money demand for any one good"; and cf. Robertson, *Banking Policy and the Price Level*, 25, 28, on the "money-demand" for a particular commodity such as "iron."

104 See the *Nicomachean Ethics*, V, v. The characterization of money as the "representative" of that "general purchasing power" which is identified with "Demand" is to be found also in writers such as Cairnes. The latter's exposition, however, is such as to make it much clearer than it is from the passage in Aristotle just cited, that the phrase "representative of demand" is intended to call attention not merely to the function of money which has sometimes been called the "common denominator of value" function, but also to the fact that money acts as the *"instrument* of demand" in market transactions. See Cairnes's *Some Leading Principles of Political Economy Newly Expounded*, 24 f., 28; and cf. Cairnes's use of the expression "instrument of demand" on p. 208 of the same work,

our purposes, however, it is more important to observe that there was very early recognition of the fact that money acts not only as the "representative," but also as the *"instrument"* of Demand.[105] This concept was of course implicit in the reasoning of all those writers, from the mercantilists to Hume, in whose argument with respect to the effect of monetary expansion and contraction upon output the concept which Wicksell called the "moneyed demand" occupied a central place; and, as we have seen, the actual characterization of money as the "instrument of demand" is to be found in writers as "orthodox," either by their own estimate of themselves or by the estimate of others, as Cairnes and Newcomb.[106]

Of still more *direct* importance for our present purpose, however, are the attempts of earlier writers to relate this concept of a "moneyed demand" for specific commodities to both (1) the general formulation of the determination of price by "demand" and "supply," on the one hand, and, on the other hand, (2) those magnitudes, such as the quantity of money and its velocity of circulation, which had appeared in *monetary* theory from quite early times. It can hardly be denied that some of the attempts in this direction in the later eighteenth and early nineteenth centuries were of the crudest possible kind.[107] Yet it must be

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[105] Cf. the preceding note.

[106] See the references to Cairnes and Newcomb given above, p. 106, n. 37. On the general "orthodoxy" of Cairnes it is hardly necessary to comment (cf., for example, the reference to Keynes given below, p. 313, n. 194). For an indication of the extent to which Newcomb believed himself to be departing from what he himself regarded as the "established body of principles" which he believed economics to represent (Newcomb, *Principles,* p. iii), see pp. vi, 434 ff. of his *Principles.* See, moreover, what is said above, p. 105, n. 36, on Tooke's attribution to James Mill, who was taken as a representative of "orthodox" monetary theory, of the concept of money as "the instrument of demand." All of these writers were of course merely accepting implicitly the proposition, as Davenport put it, that "demand, in the money economy, is money demand" (*Economics of Enterprise,* 40).

[107] The "solution" of the problem presented in 1772 by Frisi, for example, and copied literally by Fuoco in 1827, consisted merely of a mechanical juxtaposition of an equation of the form $P = D/S$ with the primitive "quantity equation" of Henry Lloyd. (See the references to Fasiani given above, p. 154, n. 24, and also the reference there given to an analogous formulation by Lubbock.) Actually, the formulation of Frisi, in particular, was by no means an entirely irrational one. For it started from the proposition that earlier writers, such as Verri, who had regarded price as determined by "demand" and "supply," had abstracted from the *particular* changes in "demand" and "supply" which might be expected to be associated with the process of monetary expansion and contraction (cf. Frisi's "Estratto del saggio sulla teoria della moneta del General Lloyd," in Custodi's *Scrittori classici,* Parte Moderna, XVII, 375). What Frisi's combination of the two formulas amounted to, therefore, was the suggestion that a crude adjustment for the changes due to the functioning of the monetary mechanism might be made by multiplying "demand"

said of the writers concerned that they at least recognized the existence
of the problem; and if they failed to provide a satisfactory solution
of it, their failure was no more egregious than, if it was as egregious as,
that of writers such as William Newmarch, whose treatment of the
problem invited the sharp criticism of Cairnes.[108]

Actually, of course, an adequate solution of the problem awaited the
fuller development, within *both* the "general" Theory of Value and
monetary theory, of an analytical apparatus for explaining *why* mone-
tary expenditure, both on individual commodities and in the aggregate,
is as large as it is. In the case of monetary theory, this meant the
fuller development of the type of analysis which came to be summa-
rized in the familiar "Quantity Equations," when the latter are inter-
preted as "stream" equations.[109] Only one comment need be made
here, therefore, in addition to what was said above in Chapter Three
with respect to the historical connection between the development of

by a factor which would register changes in the quantity of money, and
"supply" by a factor which would register changes in the quantity of
goods sold as a result of monetary expansion and contraction. As it hap-
pens, it is clear, from Verri's exposition, that he did *not* abstract from the
effects, upon "demand" and "supply," of monetary expansion and con-
traction. On the contrary, he regarded such expansion and contraction
as working *through* "demand" and "supply" (see the page references to
Verri's *Meditazioni sulla economia politica* given above, p. 264, n. 94).
Fundamentally, therefore, Verri and Frisi were in agreement not only as
to the nature of the problem to be solved, but also, in part, as to the
nature of the required solution; though it need hardly be emphasized
that the *details* of their solution were of an extreme crudity.

[108] See the references to Cairnes given above, p. 154, n. 24. In the
passages indicated, Cairnes himself argued that "the process by which an
increased production of gold operates in depreciating the value of the
metal" is that the new money "acts, first, *directly* through the medium
of an enlarged money demand [for other commodities], and, secondly,
indirectly through a contraction of supply" of these commodities. He
therefore dismissed the argument of Newmarch that "the depreciation of
money may occur by a process which is neither of these, when money
operates upon prices neither through demand nor yet through supply, but
'by reason of augmented quantity,'" with the curt comment that he him-
self was "wholly unable to conceive the process here indicated."

[109] It will be remembered that two of the writers (namely, Frisi and
Lubbock) discussed above in connection with the attempt to "synthesize"
the doctrine of "supply" and "demand," as it appears in the "general"
Theory of Value, on the one hand, with the teachings of monetary theory,
on the other, actually made use of "quantity equations" as part of their
attempt. See above, p. 270, n. 107, and the references there given. The
chief difficulty, in both cases, was of course that neither writer can be
said to have had a clear grasp of the concept which came later to be
called a "demand schedule" for a specific commodity—though it must be
added that Lubbock came closer to an appreciation of the concept than
did Frisi. See above, p. 154, n. 24. For a fuller statement of the rôle
played in any adequate synthesis of the two bodies of doctrine by "stream"
equations of the general Fisherine form, see below, pp. 280 ff.

the concept of a "moneyed demand" and the Quantity Equations.[110]
It is this: that it would be completely untrue to say that those responsi-
ble for the development of these equations were entirely unaware of
the nature of the relation of the concept of "demand" underlying the
equations and the "demand" which appears in the "general" Theory of
Value. Newcomb, as we have seen, attempted to effect a synthesis of
the two.[111] If his efforts were not entirely successful, it was not be-
cause he evidenced a lack of interest either in the nature of the forces
determining the prices of individual commodities, or in the relation of
these forces to those summarized by the Quantity Equations. What
Newcomb lacked was a clear grasp of analytical devices, now familiar
within the "general" Theory of Value, of the type represented by
Marshallian demand schedules for individual commodities.[112] What he

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[110] See above, pp. 104 f., and the references given in nn. 36 and 37 thereto.

[111] See above, p. 265, and n. 97 thereto.

[112] One may say this and still admit that Newcomb was aware of that
conception of "demand" *as a function of price* which represents the funda-
mental idea underlying the concept of a Marshallian demand schedule.
See above, p. 265, n. 97. The truth of the matter is that Newcomb's un-
willingness to use anything but the simplest algebra prevented his writing
an actual *equation* in which the quantity demanded was represented as a
function of price. Instead, he contented himself with saying that "the
fact that two quantities vary inversely is expressed algebraically by saying
that one is equal to some constant quantity divided by the other" (*Prin-
ciples,* 350). In other words, his implied equation for a demand schedule,
in this particular instance, was that of a rectangular hyperbola. This fact
is the more disappointing in the light of two other characteristics of
Newcomb's general argument. The first of these was that his own dis-
cussion of the difference between "sensitive and insensitive commodities,"
to which he referred in connection with the proposition just quoted, was
concerned precisely with implied demand schedules many of which would
not be represented by a rectangular hyperbola. The second of these other
aspects of his discussion was that, instead of considering subsequently only
the case in which the "third" quantity (representing the product of the
price and the quantity, respectively, of a given commodity) is a *constant*
—that is, the case described by a rectangular hyperbola—he himself dis-
cussed cases in which this product would vary. See also, in this connection,
Newcomb's more general proposition, on p. 207 n., of his *Principles,* to the
effect that "if one quantity varies inversely as another, it is equal to the
quotient of some third quantity [not necessarily a *constant*] divided by
the other." When all is said, therefore, it remains true that Newcomb's
treatment of demand schedules, or their equivalent, showed something
less than a complete mastery of the concepts involved; and the point made
here is that it was his failure, in particular, to write an equation for a
demand schedule of the form of Cournot's $D = F(p)$ that prevented him
from taking the further step by Cournot and Walras. That step,
as we have seen, was the use of expressions of the form $pF(p)$, whereby it
becomes possible to retain the concept of "demand" as a function of price
(Cournot's D), and still use "demand" in the sense of *our D* (Newcomb's
M, when reinterpreted in the light of the fact that the price need not be
kept "fixed"—cf. above, p. 265, n. 97).

recognized, on the other hand, was that the formal consistency of the theory of "demand" in the two bodies of doctrine is established by the possibility of using an equation of the general form $D = pq$ to summarize the results obtained in either body (or both bodies) of theory.[113]

It was only to be expected, therefore, that the next great step forward would be taken by an economist who, while accepting as fundamental for the solution of the problem the use of "stream" equations of the type of Newcomb's "equation of societary circulation," had a clearer grasp of the later-popularized demand schedules for particular commodities, of the "general" Theory of Value, than Newcomb had. Such an economist was Irving Fisher. And that Fisher did actually indicate the nature of this required next step will be denied only by those who have not taken the trouble to study the details of his argument.[114]

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[113] See especially, in this connection, Newcomb's *Principles*, 350 and 354. In the first passage, an expression which is the essential equivalent of our $D = pq$ was related to Newcomb's previously established proposition that the "demand [for a particular commodity] varies inversely as price." In the second passage, Newcomb's problem was to relate this expression to the proposition that the "market demand varies directly as the *flow of the currency*," which had been previously associated with Newcomb's own "quantity equation" (*Principles*, 317 ff., 323; cf. also p. 351). His method of solving the problem was (1) to express the "market demand" as what amounts to a function of the "flow of the currency" (see equation *a*, p. 354); (2) to insert this expression for the "market demand" in his earlier equation of the general form $D = pq$ (see equation *b*, p. 354); and (3) to make this expression comparable to his first equation of the form $D = pq$ by adjusting (see equation *c*, p. 354) for the fact that his "market demand," as first defined, had assumed that the price would remain "fixed." It will be observed that Newcomb's attempt at "synthesis," for all its crudity, is superior to that of earlier writers, such as Frisi and Lubbock, who had likewise attempted to combine a formulation of price as determined by "supply" and "demand" with a "quantity equation." For Newcomb's exposition was much more nearly successful in making clear that the "flow of the currency" affects prices *through* its effect upon the "market demand," instead of representing an element standing *side by side* with "demand." See, in this connection, what is said above, p. 153, n. 24; p. 270, n. 107; and p. 271, n. 108.

[114] See what is said on this matter above, pp. 106 ff. It should be added that Fisher himself is by no means the only sponsor, in our own day, of "stream" equations of the general form associated with his name, who has clearly recognized the need for relating this type of "stream" analysis to the type of consideration with which the demand and supply curves of the "general" Theory of Value are intended to deal. See, for example, E. Petersen, *Macro-Dynamic Aspects of the Equation of Exchange* (Oslo, 1938), 102 ff.: "The theoretical limit for pushing back the analysis [of spending and of refraining from spending money upon commodities in time] would be reached when [all the forces determining] the decision of the buyer and the seller in every transaction were [read: would be] represented in our system. That would bring us back to the utility function for each commodity for every single consumer and the corresponding production functions."

Here, therefore, it is necessary to point out only that among these details is the fact that Fisher's own exposition of the argument underlying his "equation of exchange" was built up by successive steps which included both (1) the use of a term E ($=$ "expenditure") corresponding to the D of our formulation; and (2) the description of the prices against which the "expenditure" was being directed as a series of "individual" prices (p) for "particular" commodities (q), each "individual" price itself being regarded as "subject to special variation under the influence of its particular supply and demand."[115]   Fisher's argument, that is to say, implicitly rested upon a use of the proposition underlying the argument which follows: namely, that any adequate "synthesis" of that part of the "general" Theory of Value which is summed up by the concept of "demand schedules" for particular commodities, on the one hand, and the type of analysis summed up by the "stream" analysis of monetary theory, on the other, must begin with acceptance of the proposition that the formal consistency of the two bodies of analysis is established by a proper understanding of the relation of both bodies of analysis to the implications of the common formulation $D = pq$.

V. Whether we regard the expression $D = pq$ as referring to Proposition II or to Proposition III, the D in this expression is to be taken only as a chapter heading for analysis of an extremely elaborate character. The subject matter involved in the two cases, however, is so greatly different in nature as to warrant the conclusion that the use of *both* bodies of analysis is necessary if we are to explain why D is as large as it is at one time and as small as it is at another.

VI. Specifically, the concept of "demand" which bulks largest in most versions of the "modern" Theory of Value is not the D of the formulation $D = pq$, but rather the D_p of the expression $D_p = F(p)$, which is the general expression for the demand schedule for a particular commodity.[116]   Yet

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[115] See, for example, Fisher's *Purchasing Power of Money*, 24 f., 192, 197, 355 ff., 358 ff. It is interesting to observe that, through a curious linguistic accident, Fisher's equation $e = pq$ actually appears as $d = pq$ in French translations and paraphrases of his work, by virtue of the translation of Fisher's "Expenditure" into the French *"Dépense."* See, for example, Lambert, *La théorie quantitative de la Monnaie*, 107 ff.

[116] See the references to Cournot and Walras, respectively, given above, p. 266, notes 98 and 99. The fact that it is possible to cite Walras, as well as Cournot, in this connection should be a sufficient reminder that the characterization of the expression $D_p = F(p)$ as "the general expression for the demand schedule for a particular commodity" is itself merely

from Proposition III it follows that the conditions of "demand" which are summarized by the expression $D_p = F(p)$ constitute one of the elements which will determine the magnitude of the D of the expression $D = pq$, whenever the latter is regarded as applying to the determination of the price of a particular commodity.[117]   And since all recorded money prices are the prices of "particular" commodities, this, in turn, amounts to saying that *the whole of that part of the "general" Theory of Value which undertakes to establish the nature of the forces determining the form of the function $D_p = F(p)$ must constitute an essential part of the theory of the determination of money prices.*

From the argument which has preceded, and particularly from the historical material presented in section i of Chapter Four, it should be clear that the reason for italicizing this proposition is the inherent importance of the methodological position underlying it, and not the *novelty* of that methodological position itself.   On the contrary, it would be no great exaggeration to suggest that acceptance of the position indicated has been implicit in the argument of the abler writers on the Theory of Money and Prices from the very earliest times.   For the essence of this methodological position is that every realized money price is what it is not only because of the operation of monetary factors, but also of factors with which it is the special province of the "general" Theory of Value to deal; and this is a proposition which long antedates the emergence into general use of the particular part of the "general" Theory of Value which is summarized by the expression $D_p = F(p)$.

Acceptance of this proposition is to be found, for example, in Bodin, whose argument rested implicitly upon the proposition that any adequate list of the factors leading to a rise in the price of a given com-

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a shorthand expression; for the more "general" expression of "demand" would represent it as a function not only of the price of the particular commodity taken for examination, but also of all other prices.   In what follows, that is to say, the reader is expected to bear in mind the relevant part of the argument of Chapter Four (above, pp. 166 ff.), with respect to the continued applicability of this aspect of the Walrasian system.   See also below, pp. 412 ff.   At no time, however, should it be assumed that the particular aspect of the Walrasian system thus indicated is the *only* aspect which is relevant to the problems with which these chapters are concerned.   See above, pp. 112 ff., and p. 168, n. 51, and especially the forward references given in the latter note.

[117] Cf., in this connection, what is said above, pp. 265 f., notes 98 and 99, with respect to the implications of the use, by Cournot and Walras, respectively, of expressions amounting, in our notation, to $D = pD_p$, or $D = pF(p)$.

modity would have to include not only factors associated with the
working of the monetary mechanism, but also factors, peculiar to the
particular commodity taken for examination, which need not be asso-
ciated in any way with the functioning of the monetary mechanism.[118]

Nor did Bodin represent an isolated instance among earlier writers
on money. From what was said in Chapter Four concerning the use,
by John Locke, of what amounts to a crude adumbration of the con-
cept of "elasticity of demand" to account for differential price change
during periods of monetary expansion and contraction, one would cer-
tainly be justified in characterizing Locke, also, as a sponsor of the
methodological position indicated above; and indeed Locke could not
have been more explicit in making clear his position on this point.
For what Locke argued was that the money price of any "single com-
modity," such as wheat, would be what it is not only as a result of
changes in the quantity of *money*, but also as a result of changes in the
"quantity" of the particular commodity involved *"in proportion to its
vent"*; and it is clear, from the details of his argument, that he intended

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[118] In this connection, see the comment on Bodin by Roll, *History of
Economic Thought*, 62; and cf. also the reference to Menger's comment on
this aspect of Bodin's argument given above, p. 69, n. 40. The point indi-
cated in the text was raised at the very outset of Bodin's *Response à M.
de Malestroit*, when he questioned Malestroit's right to use changes in the
price of a commodity such as *velvet* to measure the degree of monetary
depreciation, in view of the fact that non-monetary factors affecting the
demand for and supply of velvet had changed so greatly as to forbid the
use of changes in its price as "typical of . . . [the changes in the prices]
of all things" (p. 6 of Hauser's edition of the *Response*; p. 124 of Monroe's
Early Economic Thought). It will be observed that Bodin thus raised the
fundamental problem involved in the construction and interpretation of
index numbers of commodity prices long before the device itself was
dreamed of. Relevant also to the question of the rôle assigned by Bodin
to non-monetary factors affecting the demand for and supply of particular
commodities, and therefore their money prices, are Bodin's comments on
the influence of "monopolies," "scarcity, caused partly by export and partly
by waste," changes in fashion set by "the pleasure of kings and great
lords," and other "particular developments which make things rise above
their ordinary price, such as provisions in time of famine, arms in time of
war, wood in winter, water in the deserts of Lybia . . . or handicraft
products and hardware in places where there is none made" (*Response*, 9 f.,
16 ff., 31; Monroe, *Early Economic Thought*, 127, 132 ff., 134, 137). It will
be observed that all of these cases are capable of being subsumed under
what has been characterized as Bodin's "general Law of Value": namely,
that *"c'est . . . l'abondance qui cause le méspris"* (see above, p. 13, and
note 23 thereto). It will be observed also that this "Law," for all its
crudity of statement, rests upon observation of precisely the same type
of market fact as that which underlies Cournot's "Law of Demand"—
namely, $D = F(p)$; so that it is in fact anything but absurd to suggest
that Bodin's discussion of the nature of the forces determining money prices
really involved an implicit acceptance of the methodological proposition
indicated in the text.

to include, in these changes of "quantity . . . in proportion to the vent" of a "single commodity," changes which may have nothing directly to do with the functioning of the monetary mechanism.[119]

What strikes one, indeed, about the history of this proposition is the amazing pertinacity with which it has been repeated by successive generations of economic theorists. It was clearly inherent, for example, in Ricardo's acceptance of Lauderdale's proposition with respect to the "four sources of variations" in the "value" of particular commodities.[120]

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[119] In addition to the references given above, pp. 146 f., notes 12 and 13, see those given by I. Emrich, *Die geldtheoretischen und geldpolitischen Anschauungen John Lockes* (1927), 54, in support of his interpretation of Locke as having argued that the "purchasing power of money," which can be tested only by reference to the prices of "individual commodities," may be regarded as a "function of three variables": namely, (1) the "quantity of money"; (2) the "quantity" of the individual commodities; and (3) the "vent" of such commodities. The similarity of Locke's position to that of Bodin extends to two other features of their respective arguments. In the first place, Locke virtually raised the fundamental problem involved in the construction and interpretation of index numbers of commodity prices when he asked how far a "single commodity," such as wheat, could be taken as a "standing measure" of the degree of price change due to the operation of monetary factors, by reason of the possibility that wheat might be presumed to retain "the same quantity of it, in proportion to its vent" (*Considerations*, 250)—just as Bodin had raised the question with respect to velvet and "produce" (cf. the preceding note). In the second place, the fact, emphasized above, that Locke must be interpreted as having adumbrated the concept which later came to be called "elasticity of demand," means that he must be regarded as having made use of a type of device for the explanation of change in the prices of particular commodities which, as in the case of Bodin, rests upon observation of precisely the same type of market fact as that which underlies the expression $D = F(p)$. The list of early writers whose exposition involved an implicit acceptance of the methodological proposition stated in the text could, however, be extended almost indefinitely. An implicit acceptance of the proposition in question must be attributed, for example, to those writers, such as Frisi and Lubbock, who attempted to provide an algebraic formulation which would summarize the proposition that any given money price would be what it is as a result not only of *monetary* changes, but also of non-monetary factors affecting the supply of, or demand for, particular commodities (see above, p. 270, n. 107, and the references there given). Cf. also the references to, and the summary of the argument of, J. G. Büsch (1780), given by Hoffman, *Kritische Dogmengeschichte der Geldwerttheorien*, 77.

[120] See the reference to Ricardo given above, p. 148, n. 16. It will be observed that the continuity of doctrine extends to such details as (1) the association of the argument with the problem of the construction and interpretation of index numbers—or, as Lauderdale put it, the problem as to the extent to which the variation in the "value" of any one commodity may be taken as a "measure" of the changes in "the value of other commodities" (cf. Ricardo's *Principles*, 375); and (2) the recognition of the relevance, for the explanation of differential price change during periods

It was inherent, also, in the arguments of those historians of prices, such as Tooke and Jevons, who wrestled with the problem of deciding how far changes in the price of a specific commodity could be regarded as due to monetary changes, on the one hand, and how far, on the other hand, they could be regarded as due to price-making factors of the kind ordinarily discussed within the "general" Theory of Value; or, as Tooke himself put it, how far they could be regarded as due to the fact that specific commodities are "liable in each particular instance to be influenced by circumstances [of a non-monetary character] affecting the supply and demand," as well as by "variations in the quantity of money or currency." [121] It can hardly be denied that the arguments of Tooke and Jevons can be criticized from the standpoint both of their respective statements of the problem and the details of their respective "solutions." [122] It can also hardly be denied, however, that the arguments of both writers, and of Jevons in particular, rested upon an implicit acceptance of the methodological proposition advanced above: namely, that individual prices are what they are not only because of the operation of "monetary" factors but also because of the operation of factors with whose nature it is the specific function of the

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of monetary expansion and contraction, of the concept which later came to be called "elasticity of demand" (see above, p. 148, n. 16). It is striking, in fact, that the first breach in the continuity of doctrine on this head should have come from those writers who criticized Ricardo for having seen no "inconsistency" between the position summarized in the passage he quoted from Lauderdale, on the one hand, and, on the other, a "theory of prices" which, instead of "having no reference whatever to the quantity of the circulation," insisted precisely upon introducing the "quantity of the circulation" as a factor *also* affecting money prices. See, for example, Laughlin, *Principles of Money*, 245; and cf. what is said above, p. 150, n. 19, and p. 152, n. 21, on Tooke.

[121] See, for example, Tooke's *Inquiry into the Currency Principle*, 68, 125, 131. For Jevons's discussion of the considerations involved in any attempt to determine how far a given rise in the prices of individual commodities could be attributed to changes on the side of money, and how far to non-monetary factors affecting the demand for and supply of the specific commodities involved, see his *Investigations*, 16 ff., 21 f., 23 f., 43 ff., 50 ff., 54, 76, 88 f., 120, 122 f., 129 f., 147 f., 179.

[122] On the objections to Tooke's mode of stating the problem, as well as to certain details of his attempt at its solution, see above, p. 150, n. 19, and p. 152, n. 21. The weaknesses that may fairly be charged against Jevons's treatment, on the other hand, are illustrated by his categorical refusal, in his own empirical studies, to examine in detail what he called "the individual circumstances of commodities" (*Investigations*, 50), on the ground that "for the particular purposes of our inquiry it is better not to know the details concerning the articles" (*Investigations*, 54). This position of Jevons, in turn, can be traced, as Mr. Keynes rightly argued in his *Treatise* (I, 80 ff.), to a faulty conception of the issues of economic theory underlying the construction and interpretation of index numbers. It does not follow, however, that *all* that Jevons had to say with respect to the usefulness of the concept of an "average of prices" was of no positive significance. On this matter, see below, pp. 330 ff., 333 ff.

"general" Theory of Value to deal.[123]   And precisely the same thing must be said of Menger's classification of the forces determining money prices into two groups: namely, (1) those factors affecting only what he called the "internal" exchange value of money, and (2) the additional factors that would have to be taken into account in any attempt to provide a complete explanation of changes in what he called the "external" exchange value of money.[124]

There can therefore be not the slightest doubt as to either the antiquity or the continuity of a general acceptance, in "traditional" economic theory, of the general methodological position underlying our Proposition VI. It will be agreed, however, that while it is important that there should have been a general acceptance of the methodological position indicated, it is of much more importance that the fruitfulness

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[123] It is worth observing that Jevons himself, unlike Tooke in his later writings, did not carry his emphasis upon the importance of including the *second* group of factors to the point of arguing for the virtual exclusion of emphasis upon the first (see again the references to Tooke given above, p. 150, n. 19, and p. 152, n. 21). Jevons, that is to say, was not guilty of the type of analysis against which our Propositions VII and VIII (see below, pp. 280 ff., 285 ff.) are particularly directed, in any degree approximating that in which Tooke can be regarded as having been guilty.

[124] In this connection, see what is said above, pp. 68 ff. It should be added that Menger's use of the concept of changes in the "internal exchange value of money," in the sense of changes due to the working of the monetary mechanism, while it can hardly be regarded as an entirely happy one in other respects, cannot fairly be charged with the objections rightly urged by Mr. Keynes, in his *Treatise*, against the concept of "changes on the side of money" when such a concept is used as a basis for the construction and interpretation of index numbers purporting to describe changes in "the value of *money as such*" (see Keynes's *Treatise*, I, 80 ff.; italics in the original). For Menger himself explicitly repudiated the use of the concept for such a purpose ("Geld," *Collected Works*, IV, 89 ff.). His own position, on the contrary, was that satisfactory results in the way of discovering "the true factors determining the formation and the movement of prices, on the side of money as well as on the side of the commodities purchased," would be obtained only by an investigation designed to "trace the individual influences upon price movements as to direction and degree," in each individual case (*Collected Works*, IV, 91). He insisted, in other words, upon precisely the procedure rejected by Jevons on the ground that "the whole inquiry would be thrown into confusion by any such attempt" (Jevons, *Investigations*, 54; cf. also p. 147 of the same work). There can be little doubt that the instinct of Menger was much sounder than that of Jevons; though it must be added that Menger himself can hardly be said to have been fully alive to all of the issues raised by attempts, such as that of Jevons, to attach a meaning to the concept of "average" movements of prices which would have significance for certain purposes of monetary theory. On the latter point, see what is said below, pp. 330 ff., 333 ff., concerning the concept of movements in the "general level of prices."

of the methodological position itself should have been demonstrated by specific analysis in which the position was actually applied. That it was so applied is sufficiently established by our account, in Chapter Four of the present volume, of the extent to which use has actually been made by monetary theorists, in their discussion of the reasons for differential changes in the money prices of particular commodities, of that concept which came into economic theory almost simultaneously with the concept of a "demand schedule" $(D_p = F(p))$ itself—namely, the concept of "elasticity of demand," in the Marshallian sense of the term. It is only in our own day that the validity of the methodological position itself has been implicitly challenged by a writer of standing, as a result of his explicit repudiation of the concept of a "demand schedule for a particular industry" as a weapon useful in the explanation of those market events which it is the task of economic theory to explain, and his consequent refusal to consider the further steps required if the relation of this concept to the main body of the Theory of Money and Prices is to be established with all possible clarity. That writer, as we have seen, is the Keynes of the *General Theory*.[125]

VII. Proposition VI, however, is a very different thing indeed from a proposition alleging that the type of analysis which is presented within the "general" Theory of Value by way of accounting for the form of the function $D_p = F(p)$ is *all* that is needed in order to account for the determination of money prices, so far as the "demand" side of the problem is concerned. On the contrary, in order to describe the particular demand curve which is involved in the determination of a given realized price, it is not sufficient merely to establish the *general form* of the function $D_p = F(p)$. It is necessary also to establish, among other things, the *position* of the particular schedule, of the general form $D_p = F(p)$, in the system of co-ordinates of which the price axis represents *absolute* money prices.[126] There is nothing in the "general" Theory of Value, as ordinarily expounded,

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[125] In justice to Mr. Keynes, it should be pointed out that he has shown a somewhat greater readiness to use the analytical devices of the "general" Theory of Value in dealing with the conditions of *supply* for particular commodities. Even here, however, there are aspects of his discussion to which serious objection must be raised. See below, pp. 539 ff., 553 ff.

[126] It is of the first importance to point out here that this statement must not be taken to imply that monetary theory is concerned *only* with the determination of the *position* of particular demand schedules with respect to an axis measuring absolute money prices, and has nothing to say with respect to their *form*. See what is said on this matter below, pp. 304 ff.

which provides an answer to this question. In order to provide such an answer, we need a special *"money equation,"* such as is represented by the Fisherine equation $MV = PT$.

The history of the discovery (and successive rediscoveries) of this proposition provides a series of illuminating illustrations of the frequency with which an unwillingness to see what older writers were driving at has led to the adoption of a position supposedly superior to the "cruder" monetary theory of one's predecessors, but in fact definitely inferior to it.

It is clear, for example, that recognition of the need for a special "money equation" was implicit in the argument of all those earlier writers, from Bodin and Locke onward, who insisted that a method must be found for explaining not only such changes in money prices as might be due to special factors affecting "individual" prices, but also those changes in prices which are of a "general" character.[127] It was implicit also in the argument of the more self-conscious "value theorists," from Ricardo and J. S. Mill to Marshall, who expounded their "general" theories of Value on the basis of the explicit assumption that no force was operating to change the general (absolute) scale of money prices.[128] For these writers were certainly aware that analysis of this type could in no sense be regarded as providing a complete account of the nature of the forces determining money prices in the world we know. The proof of this awareness is provided not only by the care they took to emphasize that their "general" Theory of Value was explicitly based upon the assumption indicated, but also by the fact that in every case the writers involved went on to develop analysis with respect to the nature of the forces determining the *absolute* height of the prices bound together in the price system. And the point which interests us here is that the substance of this analysis was in all cases such as to be capable of summary by a special "money equation." [129]

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[127] See, for example, Bodin's *Response,* 31 (Monroe, *Early Economic Thought,* 137 f.), on the nature of the problem involved when one has regard to "l'enchérissement *en général,*" and not merely "changements particuliers"; and see also Locke, *Considerations,* 250, on the differences between a "particular case" of the "value of money, in exchanging for *any one commodity,*" and those cases which may be regarded as illustrations of a "general rule," in which the "general vent of *all* the commodities" is involved (italics mine).

[128] See the references to Ricardo given above, p. 36, n. 97. Cf. Mill's *Principles,* Book III, Chap. i, section 3 (p. 439 of the Ashley edition); and the references to Marshall's *Principles,* given above, p. 74, n. 58.

[129] Detailed comment upon Ricardo's interest in the nature of the forces determining the "absolute" height of money prices is made unnecessary by the simple facts with respect to Ricardo's place in the history of theories of the "value of money." In this connection, cf. the comments of Mitchell, "Postulates and Preconceptions of Ricardian Economics," *loc. cit.,* 216 f. Ricardo, to be sure, did not formally present his implied

If, therefore, writers of our own day have felt called upon to insist on the necessity for the explicit introduction of a special "money equation" into the Theory of Prices if we are to account for all aspects of the process of price determination, it was not because the "classical" writers had been unaware of this necessity.[130]   On the contrary, the

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"money equation" *as* an "equation." That such an "equation" was presented, however, albeit in non-algebraic form, by J. S. Mill, who in this respect was merely restating the position of Ricardo, has been pointed out by Irving Fisher, who can hardly be accused of being incapable of judging what such an "equation" implies (see Fisher's *Purchasing Power of Money*, 25, n. 2). Marshall, likewise, did not formally present his implied "money equation" as an "equation." Again, however, it should be remembered that the "equation" implicit in Marshall's theory of the Value of Money was later made explicit by the work of members of the Cambridge school—specifically, by the "cash-balance" equations of Professor Pigou and the Keynes of the *Monetary Reform*.

[130] The earlier writers have, to be sure, been charged with such an unawareness by a number of critics in our own day. In every case, however, the criticisms advanced against the particular writers named can be shown to be without foundation. It is to be feared, for example, that when Professor Mises (*Theory of Money and Credit*, 116) charges Walras and Kemmerer with having developed "what is merely a theory of *variations* in the value of money," instead of a theory of "what determines the exchange ratio [between money and commodities] itself" (cf. Mr. Hawtrey, in the *Economic Journal*, XLV [1935], 512), he raises a distinction which is not only one of doubtful validity in itself, but is one whose applicability to the particular writers mentioned is peculiarly out of place. In the case of Walras, for example, the introduction of a specific "money equation" (first in the form of a "Fisherine" equation, and later in the form of a "cash-balance" equation) was designed precisely to account for that determination of the absolute level of money prices which would otherwise be unaccounted for by his equational system; and the fact that Kemmerer's "money equation" was in all essentials that of Fisher means that Fisher's explicit insistence upon the necessity for a "money equation" in his system of price equations, on grounds implicitly identical with those of Walras, shows that Professor Mises's criticism has as little validity against Kemmerer as it has against Walras. (See also what is said below, p. 283, n. 132, concerning Walras and Fisher.) Precisely the same thing must be said against the suggestion, by Professor Hicks (*Value and Capital*, 252 f.) that Wicksell "dropped the money equation" from his system, so that his "price-system consists of a perfectly determinate core—the relative prices of commodities and the rate of interest—floating in a perfectly indeterminate aether of money values," with the result that "the money price-level is . . . utterly arbitrary." No evidence is provided for this remarkable proposition beyond the statement that "no genuine money circulates in his [Wicksell's] system." In the first place, however, it is perfectly clear, from the passages cited by Professor Hicks (*Value and Capital*, 252 n.) in support of the latter proposition, that when Wicksell assumed that "no *money* circulates" (for example, *Interest and Prices*, 75), he meant that he assumed that no *metallic* money circulates—an assumption that has nothing whatever to say with respect to the use of a "money

advisability of insisting upon what should have been regarded as a very obvious fact has been repeatedly indicated by the necessity for refuting the arguments of writers, from Tooke onward, who have set themselves in opposition to the "classical" position. For it was writers such as Tooke who insisted explicitly upon the *incompatibility* of the equivalent of a special "money equation" with the proposition that prices are determined by the "demand" for and the "supply" of particular commodities, and who argued, either explictly or implicitly, that it is possible to construct an adequate "theory of prices" having no reference whatever to the "quantity of the circulation" or to any of the other magnitudes included in the special "money equations" of earlier writers.[131] What is really worth noting, in this connection, is that it was precisely a writer such as Fisher, whose special "money equation" is the most familiar of all "quantity equations," who was also most explicit concerning the necessity for supplementing the apparatus of the "general" Theory of Value with a set of analytical devices constructed for the special purpose of explaining why money prices are as high *absolutely* as they are.[132] And it is equally worth

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equation" in the sense in which the term is here used. (See, for example, what is said in Chapters Seven to Ten of Volume I of the present work on [1] the place of the quantity of *"bank* money" (which certainly is supposed to "circulate" in Wicksell's system) in the Fisherine "money equation"; and on [2] the contributions of Wicksell to our understanding of the forces determining the quantity of "bank money"; and see especially [3] what is said on p. 183, n. 73 and p. 221, n. 43 of Volume I, with respect to Wicksell's alleged emancipation from the supposed "tyranny which the concept 'quantity of money' has until recently exercised on monetary theory.") In the second place, it must remain a mystery that any reader of Wicksell's work could have come to the conclusion that in Wicksell's system "the general level of money prices (the value of money) is left indeterminate" (Hicks, *Value and Capital,* 253). It should be sufficient to refer readers of the present work to Chaps. Three to Eight of Wicksell's *Interest and Prices.*

[131] See the quotation from Laughlin given above, p. 278, n. 120, and also the references there given to the discussion of this aspect of Tooke's argument in Chapter Four of the present volume. The relation of Laughlin's argument to that of Tooke was clearly recognized by the former. See Laughlin's *Principles of Money,* 265, and also the quotation from Tooke given on p. 259 n., of the same work.

[132] See especially Fisher's *Purchasing Power of Money,* 174 ff. Fisher referred (p. 176) to David A. Wells for an example of the type of reasoning that he was opposing. There can be little doubt, however, that he would have regarded the position of Laughlin, and therefore of Tooke (cf. the preceding note), as, if anything, a clearer statement of the opposing position. It may be observed also that Fisher himself regarded his argument with respect to the need for a special "money equation" (if there was not to be "always just one too few equations to determine the unknown quantities involved") as inherent in that statement of the conditions for the determination of a system of money prices in terms of n equations and n unknowns which he had presented in his earlier

noting that the refusal to accept this conclusion was most pronounced in the case of precisely those writers who were at once most insistent in their claims that their analysis represented a more adequate "synthesis" of the "general" Theory of Value with the Theory of Money and Prices and most contemptuous of the type of analysis summarized by equations of the general "Fisherine" form.[133]

Mathematical Investigations in the Theory of Value and Prices (see the reference to the latter work given in *The Purchasing Power of Money*, 174, n. 2). This was, of course, precisely the position of Walras, whose formal argument with respect to the need for a special "money equation" was, like his first "money equation" itself, in all essentials identical with that of Fisher. See my "Léon Walras and the Cash Balance Approach," *loc. cit.*, 573 ff., and the references to the first edition of Walras's *Eléments* there given. There can be no question, therefore, as to the correctness of the suggestion that any formal solution of the problem under discussion ultimately "derives from Walras" (cf. Schumpeter, *Business Cycles*, 452, n. 1). Of the later "solutions" of the problem along the lines indicated, that of F. Divisia has been singled out for particular comment (see, for example, Marschak, "Money and the Theory of Assets," *loc. cit.*, 311; also Schumpeter, *Business Cycles*, 452, n. 1). It need be pointed out here only that (1) while the "solution" of Divisia may be regarded as superior to that of Pareto, it can hardly be regarded as superior to that of Walras, with which it is in all essentials identical; and that (2) Professor Divisia himself has since acknowledged the priority of Fisher, and therefore, by implication, of Walras. On this matter, see my article on "The Monetary Aspects of the Walrasian System," *loc. cit.*, 152, n. 17. It may be added, finally, that the particular "money equations" presented by all of these writers had none of the crudity of a "money equation" which would either assert that "the absolute height of prices is determined" *solely* by the "amount of money" or would imply that we should be justified in assuming that "output . . . as well as the velocity of circulation" are in all cases "fixed by other forces" (cf. A. P. Lerner, "Keynes' 'General Theory': A Rejoinder to Professor Cassel," *International Labour Review*, XXXVI [1937], 590). This fact provides its own commentary upon the further suggestion that the case for the use of a "money equation" as a means of determining "the *absolute* height of prices" is destroyed by assumptions of this type, alleged to result from "relying blindly on an equational system of determination" (so Lerner, *loc. cit.*).

[133] In this connection, see, for example, the references given above, p. 20, n. 48, to Laughlin's claims with respect to the consistency of his own argument with the general "principle of demand and supply," in alleged contrast to his "quantity theory" opponents; and cf. also the reference given above, p. 104, n. 36, to Laughlin's rejection of the conception of the absolute level of money prices as affected by the dimensions of the "moneyed demand"—a conception which has underlain the usage of the most eminent sponsors of "stream" equations of the general Fisherine form. Of the treatment of the problem in Keynes's *General Theory*, it may be remarked here that, despite Mr. Keynes's continued formal rejection of equations of the latter type, he was forced to re-introduce one of these equations in disguised form when he approached the problem of the determination of the absolute level of money prices. See below,

VIII. The relation of this "money equation" to the determination of *individual* money prices has often been obscured as a result of misleading statements concerning the nature of the issues involved.[134]   The true nature of this relation is established, however, by the proposition that the first, and in many respects the fundamental, purpose of a "money equation" of the general form $MV = PT$ is to establish the nature of the forces determining the magnitude of the *sum of realized money demands,* which may be written ΣD, and which represents "aggregate money demand," in one sense of the latter term.[135]   For *only if we know the absolute level of the total amount of money-spending power which is available for realizing money demands, as well as the degree to which this money-spending power is actually utilized in the realization of such demands* (that is, the magnitude of $MV = \Sigma D = \Sigma pq$), can we determine what will be *the absolute level of any one of these individual realized demands* (the individual D's), and therefore *the absolute level of the various individual p's involved in individual expressions of the form $D = pq$.*[136]

Chapter Fourteen.   This is not to say, however, that all of Mr. Keynes's followers have evidenced an adequate appreciation either of the significance of this step in Mr. Keynes's argument, or of the bearing of other implications of "stream" equations of the Fisherine type upon other, and much more dubious, aspects of the Theory of Prices presented in Keynes's *General Theory.*   In addition to the reference to Lerner given in the preceding note, see below, pp. 570 ff.

134 See, for example, what is said below, pp. 319 ff., concerning certain common misconceptions of the rôle in *monetary* theory of expressions of the form $MV = PT$.

135 In many cases, of course, the concept of an aggregate "money demand" has been associated, not without reason, with the aggregate demand exerted by disbursements out of *income.*   This matter is discussed in the fine print section below.   For our present purpose, however, it is sufficient to call attention to (1) the historical connection between the concept of a "general demand," on the one hand, and *stream equations* of the general form $MV = PT$, on the other, regardless of whether "income" variants of these stream equations were used or not (see above, pp. 104 ff., and especially nn. 36 and 37 thereto); and (2) the concession, by a writer such as Mr. Hawtrey, who has been so insistent upon the importance of distinguishing between "the purchase of a thing out of income" and "the purchase of a thing with a view to resale," that there is nevertheless a sense in which the MV of the Fisher equation may be "regarded as constituting the total of demand" (cf. above, p. 121, n. 75).

136 On the relation of the magnitude of "demand" (D), in the sense

1. The statements italicized should require no detailed demonstration in the eyes of anyone familiar with those aspects of the "general" Theory of Value which deal with (1) the influence of variations in *income* upon the demand for any one commodity; and (2) the accepted devices for dealing with the phenomenon of "joint demand," particularly when the latter is regarded as merely a special case of the general phenomenon of "complementarity."

i. The solution, for example, of the problem of deriving a specific demand schedule for a commodity which is demanded "jointly" with other commodities in order to obtain a desired combination of commodities is impossible unless the demand price for the whole combination is given.[137] What is true in this respect of the demand *schedules* involved must necessarily be true of the *realized* demands involved, by virtue of the expression $D = pF(p)$.[138] And the fact, established by our Proposition IV, that the expression $D = pq$ may be applied to expressions of the general form $MV = PT$, as well as to expressions of the general form $D = pF(p)$, must mean that the same argument necessarily holds with respect to the expression $\Sigma D = MV = \Sigma pq$.

ii. That the same type of reasoning is involved in that part of the "general" Theory of Value which is concerned with the effect of changes in *income* upon the demand for particular commodities will be clear from Proposition IX, below.[139] Nor, upon the basis of the second part of Proposition IX, can there be any doubt as to what this means for *monetary* theory: it means, clearly, that the same type of reasoning is necessarily involved in those parts of *monetary* theory which are concerned precisely with the rôle of changes in the level and distribution of money income in the determination of money prices.[140]

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indicated, and the height of the "prices" associated with each level of "demand," see below, pp. 341 ff. It will be observed that the argument stated in the text makes it clear that while there is unquestionably a sense in which we are completely justified in adding the realized demands for individual commodities in order to obtain a figure for "aggregate demand," it would be extremely misleading to interpret this procedure as meaning that the aggregate realized demand is what it is as a *result* of the magnitude of individual realized demands. See, in this connection, Schumpeter, "Das Sozialprodukt und die Rechenpfennige," *loc. cit.,* 679. On the possibility, on the other hand, that the magnitude of a given aggregate demand may be affected *in a subsequent time-period* by the changes in the price structure brought about by changes in the magnitude of the realized money demands for individual commodities, see below, p. 290, n. 149, and p. 295, n. 158.

[137] See Marshall, *Principles,* 383, 855.

[138] Again it may be observed that this proposition, correctly understood, is not the analytical equivalent of a proposition alleging that the aggregate demand is what it is as a *result* of the magnitude of individual realized demands. See above, n. 136, and the reference to Schumpeter there given.

[139] See below, p. 296.

[140] See especially, in this connection, Schumpeter, "Das Sozialprodukt, etc.," *loc. cit.,* 678.

2. All that is needed here, therefore, is a demonstration that the same type of argument must lead to an acceptance of the necessity for using a *"total transactions* equation" of the general form $MV = PT$, if we are to understand why the absolute level of any one "realized" demand (D) is as large as it is.[141]

This demonstration, however, has already been provided, in Volume I of the present work, by the establishment of two propositions which are of the greatest importance in themselves. The first of these propositions established the necessity for dealing with other "price levels" than an "income" price level, and therefore for using "stream" equations *in addition to* "stream" equations of the "income" type, if we are to have a *complete picture of the economic process*.[142] The second of these propositions established the fact that a concern with the components of a "total transactions equation" other than the "income" components is necessary *even if we are interested only in ascertaining the nature of the forces which make the magnitudes included in an "income equation" as large as they are:* a conclusion which followed from the argument associated with the concept of a "composite demand for cash balances," and all that this concept implies with respect to a "competition for reserves of purchasing power," and the dimensions of the associated *streams* of "purchasing power." [143]

For what the first proposition means is that the D's involved, for example, in the expression $D = pq$ when the expression refers to the realized "demands" of *entrepreneurs,* are in most cases as much a matter of concern to the monetary theorist as are the D's involved in the expression $D = pq$ when that expression refers to the realized "demands" of consumers; and since a "total transactions equation" of the general form $MV = PT$ necessarily *includes* a series of "partial" equations purporting to describe the particular transactions in which *entrepreneurial* spending is involved, an insistence upon the use of a "total

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[141] The reader may be reminded that the argument of Newcomb with respect to the rôle of the concept of a "money demand" and its relation to the "flow of the currency" (see above, p. 273, n. 113), was directly applied to the concept referred to here as a "total transactions" equation, and was characterized by Newcomb himself as "a method of representing the exchanges within a social organism *considered in their totality"* (*Principles of Political Economy,* 315; italics mine). See also the reference to Hawtrey's comment upon the Fisher equation given above, p. 285, n. 135.

[142] See Volume I, 490, and n. 16 thereto, and especially 494 ff. The validity of the proposition summarized in the text becomes still more obvious when account is taken of those aspects of the economic process which have to do with the various interrelations *in time* of the different sectors of this process, as in all problems associated with the *generation of money income.* For any account of the latter must necessarily be concerned with the rôle played in the process of income generation by the magnitude and the direction of *entrepreneurial* expenditure. It involves, that is to say, the type of apparatus outlined briefly on pp. 369 f., and 382 f., of Volume I and applied further below, pp. 430 ff., 433 ff., 438 ff., and 494 ff.

[143] See Volume I, 518 ff., and especially 521 ff.

transactions equation" *in addition to* a stream equation of the "income" type amounts to an insistence upon dealing with the phenomenon of a "moneyed demand" in all sectors of the economic process.[144] And what the second proposition means is that the use of a "total transactions equation" is required as a supplement to an "income equation" even if we are concerned *only* with the phenomenon of a "moneyed demand" as it appears in the market for consumers' goods or in any market in which all purchases are made out of "consumers' income," in Hawtrey's sense of the latter term.

3. It is by the use of our Proposition VIII that we are able to handle, with the greatest ease, problems that have often been associated with difficulties purely factitious in nature. This may be illustrated by a consideration of the relevance of Proposition VIII for what has been called by J. G. Koopmans "the alleged 'Law of Compensatory Price Changes.' " [145]

The "alleged 'Law of Compensatory Price Changes,' " according to Koopmans, states that "a change in general prices brought about by non-monetary factors is an impossibility, because—insofar as 'nothing changes on the side of money'—a rise or fall in individual prices must necessarily and in all cases be compensated by an opposite ('contrary') movement in other prices." [146] According to Koopmans, on the other

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[144] See Volume I, 512, and especially 516 ff. It should be clear that the argument in the text provides an additional commentary upon the position of certain critics of the argument presented in Volume I in order to demonstrate the necessity for supplementing analysis running in terms of a "plurality" of stream equations by the use of a "total transactions equation," and *vice versa.* Cf. what is said above, p. 103, n. 32, on the suggestion that the argument of Volume I with respect to the concept of a "plurality" of equations of the general Fisherine form amounts to "defending the individual parts and not the original whole."

[145] See Koopmans, "Zum Problem des 'Neutralen' Geldes," *loc. cit.,* 221, and especially 288 ff. Koopmans's argument has been the subject of a considerable amount of later discussion, most of it distinctly favorable to Koopmans's position. See, for example, E. Roll, *About Money* (1934), 115 ff. (also the comments by the same writer in his article "Menger on Money," *loc. cit.,* 458) ; L. Baudin, *La Monnaie et la Formation des Prix,* I, 323 ;. W. Zawadski, "Changes in the Price Level under the Influence of Maladjustment of Supply and Demand," *Economica,* New Series, IV (1937), 119 ff. In some cases, even though Koopmans's monograph was not cited directly, his argument was discussed in the form given to it by writers who did cite him. See, for example, Lambert, *La Théorie quantitative de la Monnaie,* 215 f., and the reference to Baudin given on p. 215, n. 1.

[146] Koopmans, "Zum Problem, etc.," 289. I have ventured to substitute the phrase "general prices" for Koopmans's "the average [*durchschnittliche*] price level" in order to put the best possible face on Koopmans's supposed "refutation" of the "alleged 'Law.' " It is clear that, if one were prepared to make sufficiently ingenious (and unscrupulous) use of the concept of an "average price level," the "alleged 'Law' " could always be made to

hand, this "alleged 'Law' " is invalidated as a general proposition be-
cause it rests implicitly upon a series of assumptions with respect to
the *elasticity of demand* for particular commodities which may or may
not correspond to the facts in all instances.[147]

It should require only the slightest reflection, however, to observe
that this "alleged 'Law' " could never have been advanced by anyone
having a firm grasp of the argument summed up by our Proposition
VIII. For all that this proposition asserts is that, if "nothing changes
on the side of money"—*in the sense that* $MV = \Sigma D$ *is assumed to re-
main unchanged*—a rise or fall in the *money demand (D)* for any in-
dividual commodity must necessarily be compensated for by an opposite
("contrary") movement in the *money demands* for other individual
commodities.[148] *Given* the assumption that MV $(= \Sigma D)$ remains un-

hold, by the simple device of using a sufficiently arbitrary system of
weights in the construction of the "average price level." For, as is pointed
out in the text, the whole argument turns upon what happens to the dif-
ferent q's in the "money equation"; and as long as one is prepared to
obscure the *actual* changes occurring in the q's by the use of an arbitrary
system of weights, it will always be possible to present an empirical
"proof" that in fact the "average price level" will remain unchanged
under the conditions indicated. It is for this reason that one can only
agree with Professor Schumpeter when he argues that "under what con-
ditions the price level can rise and fall depends entirely upon the way
in which it [the price level] is computed" ("Das Sozialprodukt, etc.," *loc.
cit.,* 680 f.; cf. also the comments of Koopmans himself, "Zum Problem,
etc.," 292, n. 1, 297 f., 314 ff.). The use of the expression "general prices,"
on the other hand, makes the "refutation" of the "alleged 'Law' " more
impressive *prima facie,* precisely because the expression can be used in a
sense which, instead of requiring the use of an arbitrary "average," would
refer only to a "general" movement in the *individual* prices in the ex-
pression $p_1q_1 + p_2q_2 \ldots + p_nq_n$. On the concept of a "general" price
level, and its relation to an "average" of prices, see what is said below,
pp. 330 ff., 333 ff.

[147] Koopmans, "Zum Problem, etc.," 290, 294 ff., 313 ff. It will be ob-
served that I have avoided the statement of the "alleged 'Law' " in terms
which would suggest that it represents a view "opposed" to what Koop-
mans (*op. cit.,* 289) calls "the doctrine of 'cumulative' price changes." On
the reasons for regarding such a statement of the issues as involving a
false antithesis, see below, p. 290, n. 149, and also below, p. 295, n. 158.

[148] This proposition is called by Koopmans ("Zum Problem, etc.,"
300 n.) the "Law of Compensatory Change in Price-*Sums*" (that is, in the
pq's representing different commodities). And it is worth observing that
whereas Koopmans gives no explicit references to abler writers who are
to be regarded as protagonists of "the alleged 'Law of Compensatory
Changes in *Prices*' " (see below, p. 291, n. 151), he *is* able to cite writers
of standing, such as Hawtrey and Machlup, as having made use of "the
Law of Compensatory Change in Price-*Sums*." On Koopmans's alleged
"refutation" of the latter "law," see the following note; and on the true
nature of the issues involved in an attempt to appraise critically a posi-
tion such as that of Hawtrey, see below, p. 295, n. 158.

changed, this follows as a simple matter of arithmetic.[149] That the individual *prices* corresponding to these individual D's may be affected in different degree, as a result of the different degrees of elasticity shown by the demand schedules for the individual commodities is, indeed, indicated by the fact, emphasized above, that the expression $D = pq$ may be written in the form $D = pD_p$ or $D = pF(p)$. The very fact, however, that the expression $\Sigma D = \Sigma pq$ may also be written in the form $MV = \Sigma pq = \mathrm{PT}$ shows that there is no reason whatever for arguing that "a change in the general price level brought about by non-monetary factors is an impossibility." For what this would amount

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[149] When, therefore, Koopmans undertook to refute what he calls, "the Law of Compensatory Change in Price-*Sums*" ("Zum Problem, etc.," 300 ff.), he had to be prepared either to refute the processes of arithmetic, or to *change the conditions of the problem* by pointing to factors which may lead us to expect that in fact the changes in the price structure brought about by an initial "non-monetary factor" *may be expected to change the magnitude of MV*. The latter, indeed, was the method he followed, despite details of his argument which might seem to imply the contrary. Since, however, the possibility of a *subsequent* change in MV has been clearly recognized by writers who have undoubtedly supported "the Law of Compensatory Change in Price-*Sums*" (see especially, in this connection, Schumpeter, "Das Sozialprodukt, etc.," *loc. cit.,* 677), most of Koopmans's argument turns upon entirely factitious issues, such as (1) whether the statement that "nothing changes on the side of money" is or is not to be interpreted as permitting the possibility that V, for example, may change as a result of factors that are to be regarded as "non-monetary" in character (see, for example, "Zum Problem, etc.," 305, n. 1); or (2) just what is to be understood by the condition that money must be kept "neutral" (see, for example, "Zum Problem, etc.," 313 f., 324). It should hardly be necessary to labor the point that the question whether the magnitude of MV ($= \Sigma D$) is in reality *likely* to remain unchanged is not in itself a factitious problem. See what is said on this matter below, p. 294, n. 158. All that is argued here is that the answer to this question cannot be given in terms of a contrast between "the alleged 'Law of Compensatory Price Changes'" (or even the "Law of Compensatory Changes in Price-*Sums*"), on the one hand, and "the doctrine of 'cumulative' price changes," on the other. For, in all cases, what is required is the use of the whole of the detailed analytical apparatus lying behind the M and V of our "money equation," in order to ascertain the possible effects upon these variables of whatever change in the price structure is brought about by the changes in individual prices. Even, for example, if one were to accept the contention of the "alleged 'Law of Compensatory Price Changes'" with respect to the constancy of the "general price level" under the conditions indicated, there would be no *a priori* reason for supposing that the changes in the price structure occurring *under cover of a "stable" general price level* cannot be such as to lead, in *subsequent periods,* to a "cumulative" movement in the M and V of the "money equation," and therefore in the "general price level." It is for this reason, among others, that the alleged *antithesis* between "the alleged Law of Compensatory Price Changes" and "the doctrine of 'cumulative' price changes" is a false antithesis.

to is the patently absurd contention that the magnitude of T, in the Fisher equation, could not change as the result of variations in the amount of individual commodities *sold* in the face of the particular conditions of demand for particular commodities that happen to be prevailing at any given time.[150]

The really interesting question, therefore, as so often, is how far it is true that "the alleged 'Law of Compensatory Price Change'" has in fact occupied an important place in the literature.[151] And indeed it is extremely difficult to find any clear evidence that it was ever held by any writer of standing.[152] What is more worthy of note, however, is

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[150] It should hardly be necessary to emphasize here that the "amount sold," and therefore the magnitude of T, will be affected also by the conditions of *supply*, and particularly by the conditions prevailing with respect to the level of the "reservation prices" of the sellers. On the relation of this element to the supply schedules for particular commodities of the "general" Theory of Value, on the one hand, and the "rate of sale" of the Theory of Money and Prices, on the other, see below, pp. 554, 555.

[151] It is worthy of note that Koopmans himself, instead of giving specific references to the literature, contents himself with saying that the argument involved has been "repeatedly represented in the literature of economic theory" (see "Zum Problem, etc.," *loc. cit.*, 288 f.). The single reference to earlier users of the *expression* "the Law of Compensatory Price Changes" given by Koopmans, moreover (p. 289, n. 1), is to E. von Mickwitz, "Kassenhaltung und Preisniveau," *Archiv für Sozialwissenschaft und Sozialpolitik,* LXII (1929), 582; and it should be observed (1) that even Mickwitz was an *opponent,* not a supporter, of "the alleged 'Law'"; and (2) that, as Koopmans himself observes, Mickwitz gave no references to writers charged with supporting "the alleged 'Law.'" On p. 221 of his "Zum Problem, etc.," Koopmans cites G. M. Verrijn Stuart as a "typical representative" of certain views alleged to have led to the conclusion that "changes in the general price level can be brought about *only* by causes operating 'on the side of money.'" He does not specifically attribute to Verrijn Stuart, however, the "alleged 'Law of Compensatory Price Changes'"; and in a later passage (p. 297, n. 3), Koopmans summarizes the position represented in one of Verrijn Stuart's later publications as amounting to a *denial* of the necessity for a "compensatory price change" in the sense in which it is supposed to be regarded as necessary by the "alleged 'Law.'"

[152] The case of Tooke provides an example of the danger of misinterpreting isolated phrases of individual writers in order to be able to fasten "the alleged 'Law'" upon them. For Tooke may be regarded as the prototype of writers anxious to establish the possibility of changes in "general prices" as a result of "non-monetary factors," including those which would now be discussed under the head of differing elasticities of demand for particular commodities (on the latter aspect of Tooke's argument, see above, pp. 148 ff.). It is hardly surprising to find, therefore, that Tooke did not argue that the rise in the price of agricultural produce, for example, which would follow from a "scarcity" of such produce, would be "compensated" for, "under a fixed amount of currency," by a fall in the prices of other commodities. He argued, on the contrary, that

that the clearest cases of avoidance of the error involved in any literal adherence to "the alleged 'Law of Compensatory Price Change' " are to be found in those writers who made explicit use of a "money equation" of the general form $MV = PT$. This is true, for example, of Lubbock, who, as we have seen, would have to be taken into account in any history of attempts to incorporate at least a rough equivalent of the Marshallian "elasticity of demand" into a theory of the determination of money prices which also makes use of a "money equation" of

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such a rise might advance "money prices" generally, even "under a fixed amount of currency." See Tooke's *Thoughts and Details*, I, 89. What is really worth noting, however, is that Tooke himself occasionally used a type of exposition which would probably have been seized upon as proof of Tooke's support of "the alleged 'Law of Compensatory Price Change' " if Tooke had elsewhere shown himself to be more sympathetic than he was to an emphasis upon *monetary* factors capable of effecting changes in "general" prices. On at least one occasion, for example, Tooke may have seemed to argue that, in cases in which there is no "increase in the sum total of demand," a redistribution of the segments of this "sum total of demand" between, say, governmental and private expenditure "would result in a rise in the prices of some commodities and an *"exactly equivalent fall"* in the prices of other commodities (*Thoughts and Details*, II, 10). But the context, in this case, shows clearly that the italicized phrase was not to be understood as excluding such changes in the *relevant q*'s as might result in a "rise in general prices" *when the latter expression is used in some sense other than that in which Tooke here used the term.* For the compensating "rises" and "falls" in prices to which Tooke here had reference were rises and falls in what Tooke called "the *aggregate* of prices" (*Thoughts and Details*, II, 11), which was in all essentials the equivalent of Koopmans's "price-*sums*." Tooke did point to cases, to be sure, in which "contrary" movements in different groups of prices might be occurring simultaneously (see, for example, his *History*, I, 210 [paragraph 4]; II, 139, 190, 212); and he certainly argued, at least in his earlier writings, that a *change* in MV, such as might result from a "forcible contraction of the circulation," might emphasize such divergent tendencies in the movements in the prices of different commodities as might have existed in the absence of a change in MV. The nearest, however, that he seems to have come to suggesting anything resembling the "alleged 'Law of Compensatory Price Change' " was in his proposition that a "tendency in opposite directions of classes of articles, constituting nearly equal values, forms a *prima facie* presumption that the currency, as regarded its operations on prices, was in a quiescent state" (*History*, II, 318). If anything is clear, on the other hand, it is that Tooke did not conclude, from this proposition, that changes in individual prices (due, for example, to differences in their respective elasticities of demand in the face of changed conditions of supply) would *necessarily* either be "compensated for" by changes in other prices, or could not lead to the kind of cumulative process of monetary expansion envisaged by the alleged "opposite" of the "alleged 'Law of Compensatory Price Change' "—namely, "the doctrine of 'cumulative' price changes." See, for example, Tooke's *Thoughts and Details*, I, 89, and his *Considerations on the State of the Currency*, 44.

the type indicated.[153] And the results are even more striking when one passes to the case of Irving Fisher, who has done more than any other single writer to popularize the concept of a "money equation" and to show its relation to the demand and supply schedules of the "general" Theory of Value.[154] For Fisher, instead of making use of anything resembling "the alleged 'Law of Compensatory Price Change,'" actually went out of his way to demonstrate precisely that changes in the "general price level" *are* possible as the result of rises or falls in the prices of individual commodities, even when these rises and falls are not directly due to the functioning of the monetary mechanism.[155] And it is worthy of more than passing comment that Fisher's argument

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[153] See above, p. 152 f. It should be observed (1) that Lubbock, instead of taking the case in which the magnitude of MV would remain the same, took the case of "a diminution of the circulating medium" (*On Currency*, 37); (2) that he was prepared to accept the conclusion that "the price of each article may be diminished in exactly the same proportion as the circulation" only upon the assumption that *"the transactions which are measured by"* the equivalent of our q's would "remain the same"; (3) that he regarded "the effect upon [the prices of] different articles," by way of changes in these q's, as likely to be "various," precisely as the result, among other things, of "altered production and supply" of these different articles; and (4) that the nearest he came to supporting anything resembling "the alleged 'Law of Compensatory Price Change'" was his sponsorship of propositions analogous to the so-called "Law of Compensatory Price-*Sums*"—as when he argued, for example, that if the magnitude of his E (compare Volume I of the present work, pp. 12, 57 f., 61, 574 f., 589) is constant, or nearly so, "the price of each article in *mercantile* operations will be diminished in greater proportion" than the quantity of the circulating medium.

[154] On the more general aspects of Fisher's treatment of the relation of the "money equation" to the demand and supply curves of the "general" Theory of Value, see again what is said above, pp. 106 ff., and the references given in nn. 38, 41, 44, and 45 thereto.

[155] See *The Purchasing Power of Money*, 179, 181, 312 ff. It will be observed, for example, that Fisher was very careful to point out that the proposition that "if prices for one commodity are changed . . . the effect on the price level will be neutralized by compensatory changes in other prices" will hold *only* if we assume that there will be no "change in the *number of sales*" (*The Purchasing Power of Money*, 178 f. [italics mine]; see also pp. 194 f. of the same work). He then went on to show that in fact "a decrease in the price of any particular commodity will usually be accompanied by an increase in the amount of it exchanged"— the extent of the increase depending upon the elasticity of the demand for the particular commodity involved—with results, for the "general price level," in all essentials identical with those obtained by Koopmans. See *The Purchasing Power of Money*, 178 ff., 382 ff. It should also be clear, from an examination of these passages, that the validity of Fisher's argument on the point in question can be tested without introducing the whole series of factitious issues which have been associated with loosely stated propositions involving the concept of a "passivity of prices." Contrast Lambert, *La Théorie quantitative de la Monnaie*, 215 f.

as to *why* this must be so is precisely that outlined above, in terms of the relation of differing elasticities of demand to the "amount bought" of particular commodities, and *therefore to the T of the "money equation."* [156]

The "discovery," therefore, that, if the total of money expenditure is held constant, "changes in the prices of individual goods" need not necessarily "lead to completely compensatory movements in the prices of other goods" is hardly a discovery of those writers who have shown themselves particularly distrustful of the concept of a "general level of prices," or of the use of index numbers to measure something called the "purchasing power of money." [157]   Nor can it be said to have been ignored by writers, such as Mr. Hawtrey, whose major emphasis has concerned those aspects of the functioning of the economic system which are associated particularly with the functioning of the *monetary* mechanism. [158]   Indeed, the more one examines earlier writings on the

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[156] See, for example, *The Purchasing Power of Money,* 384. It is worth noting that Fisher himself referred also, in this connection, to his *Mathematical Investigations in the Theory of Value and Prices,* a considerable part of which (see especially pp. 42 ff.) may be regarded as a *locus classicus* for the treatment of the issues involved, by virtue of its emphasis on the effects, upon money prices, of changes in the amount and distribution of "money income" and of particular conditions of demand and supply for particular commodities. The passages just referred to, indeed, are to be recommended particularly to those who have argued that an insistence upon the use of a "money equation" of the general form $MV = PT$ involves a blindness to the problems discussed and the techniques used within the "general" Theory of Value.

[157] Contrast Roll, "Menger on Money," *loc. cit.,* 458. It may be added that a careful reading of the passage in Menger's "Geld," to which Roll refers (*The Collected Works of Carl Menger,* IV, 89 ff.), makes it bear very favorable comparison with the statements of later writers with respect to "the alleged 'Law of Compensatory Price Changes,' " from the standpoint of its freedom both (1) from inclusive statements as to what has been "customarily" held with respect to "the alleged 'Law' "; and (2) from the factitious issues introduced as a result of a concern with the concept of "neutral money." On Menger's real position with respect to the latter concept and its supposed equivalents, see what is said above, pp. 68 ff., and nn. 39 and 40 thereto.

[158] This much is admitted by Koopmans, who points out ("Zum Problem, etc.," 300 n.) that Hawtrey recognized the relevance, for the problem of compensatory *price* change, of the relative degrees of elasticity of demand for the particular products involved. Koopmans therefore attributes to Hawtrey only the "Law of Compensatory Change in Price-*Sums*" (see above, p. 289, n. 148). For examples of Hawtrey's use of the latter "Law," and of his recognition of the relevance, for the problem of relative *price* change, of the particular elasticities of demand for the individual commodities involved, see, in addition to the passages quoted by Koopmans (cf. Hawtrey's *The Art of Central Banking,* 309, 322), Hawtrey's *Good and Bad Trade,* 85 f., 134 f., 140 f. Actually, of course, Hawtrey's general argument may be regarded as open to criticism not because of his use of

subject, the more one becomes convinced that the attribution to the
abler among the authors of these earlier writings of a sponsorship of
the "alleged 'Law of Compensatory Price Change' " is possible only
upon the basis of an extremely careless reading of what they had to
say on the subject.[159] And one might add that the more one reads
of more recent discussions of the issues raised by this "alleged 'Law,' "

the "Law of Compensatory Change in Price-Sums," which involves the
explicit hypothesis that *aggregate* demand remains unchanged (see, for
example, *The Art of Central Banking*, 324), but because of his use of two
further propositions which are themselves very much less self-evident.
These further propositions are: (1) that there is no reason for supposing
that there will be any difficulty in maintaining aggregate demand at the
same level as before, despite the changes in the price structure brought
about by "non-monetary" changes in the demand for and supply of com-
modities, with all that such changes in the price structure may mean for
the schedule of expected profit rates and, therefore, the demand for bank
loans and the quantity of bank money, or the rate of spending of entre-
preneurial cash balances; and (2) that "except in the special instance of
an actual famine, the fluctuations of demand or supply produce no fluctua-
tions in trade as a whole," since "the depression or prosperity of one trade"
may be expected in virtually all cases to be "compensated by the prosper-
ity or depression of others" (so, for example, *Good and Bad Trade*, 87;
cf. also *Trade Depression and the Way Out*, 72 f.). On the latter issues,
which involve the whole question of the rôle to be assigned to "monetary"
and "non-monetary" factors in the trade cycle generally, see Saulnier,
Contemporary Monetary Theory, 56 f., 66, 106. It should be clear, how-
ever, that these issues have nothing directly to do with the validity and
usefulness, for the explanation of the determination of money prices, of
our Proposition VIII, which is implicit in Hawtrey's analysis, or with
the fact that Hawtrey's argument is clearly free from any suggestion of
acceptance of an "alleged 'Law of Compensatory Price Change,' " in the
form in which the "alleged 'Law' " is stated by Koopmans and his fol-
lowers.

[159] Cf. above, p. 291, n. 152. It would be a sheer misrepresentation of
Fisher's position, for example, to cite, as proof of his alleged sponsorship
of the "alleged 'Law of Compensatory Price Changes,' " statements such
as that on p. 197 of his *Purchasing Power of Money:* "There exists . . .
a compensation in price movements in the sense that the failure of one
set of prices to respond to any influence on the price level will necessitate
a correspondingly greater change in other prices." For it should be clear,
from the quotations and references given above, p. 293, n. 155, as well as
from the next sentence in Fisher's text ("the quantities sold likewise vary,
and their variations are bound up with those of prices"), that the "com-
pensation" to which Fisher refers is a compensation in what Koopmans
calls "price-*sums*." The same thing may be said of Schumpeter, whose
argument with respect to the "equalizing" of changes in one set of prices
and quantities by "corresponding" (and opposite) changes in other prices
and quantities ("Das Sozialprodukt, etc.," *loc. cit.*, 677) is expressed through-
out in terms of what Schumpeter himself calls "the sum of *products*"
(Produktensumme)—that is, the sum of the "products" of individual *p*'s
by individual *q*'s.

the more one becomes convinced of the superiority, as a weapon for dealing with these issues, of the apparatus typified by "money equations" of the type indicated above, when these "money equations" are interpreted in the light of the purposes assigned to them by the ablest of their sponsors, and particularly in the light of the purpose assigned to them by our Proposition VIII.

IX. Within the "general" Theory of Value, the central idea underlying our Proposition VIII (namely, that we need information with respect to the magnitude of the aggregate amount of spending power available for satisfying all the "demands" for individual commodities if we are to understand why any one of these "demands" is as large as it is) is recognized by taking account of the element of aggregate individual *income* as a factor affecting the position, as well as the conformation, of any given demand schedule.[160]　But since the "general" Theory of Value, in its ordinary formulations, makes no attempt to account for the *absolute level* of the money incomes involved, this must be left for the Theory of Money and Prices, which has attempted to fill the breach by developing a theory of the generation and utilization of money income, as well as of the *distribution* of such money income, whenever that distribution can be shown to be affected by, or reflected in, the working of the monetary mechanism.[161]

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[160] In this connection, see the references given above, pp. 206 ff. From the abundance of those references, it should be obvious that recognition of the relation between the income and the demand of an individual is hardly a discovery which has followed historically from (1) the extension of the theory of exchange to three or more commodities, (2) identifying the sum of the prices of "other commodities" *possessed* by a given individual with his "wealth," and then (3) relating this "wealth" to transactions averaged over several "situations" or "periods" (see Schultz, "Interrelations between Demand, Price, and Income," *loc. cit.,* 434 ff.; and cf. the same author's *Theory and Measurement of Demand,* 28 ff., 36 f., 39 ff.). It should be equally obvious from these references that while no one would wish for a moment to deny credit to Slutsky, on the one hand, and to Hicks and Allen, on the other, for having sharpened the issues involved, the first "explicit introduction of income into the demand function" is fairly attributable to these writers (cf. Schultz, *Theory and Measurement of Demand,* 50, 644) only in a very narrow sense of the word "explicit."

[161] The extent to which the abler among the sponsors of an "income approach" to the Theory of Money and Prices were aware of the relation thus indicated between the level of money income, on the one hand, and the "demands" for individual commodities, on the other, may be judged from passages such as that in Schumpeter, "Das Sozialprodukt, etc.," *loc.*

X. Most of the recent developments, within the "general" Theory of Value, with respect to the rôle of *income* in the theory of price determination, have been stated in terms of the effect of changes in "real" income.[162] In a completely developed money economy, however, a figure for "real" income can be obtained only from information with respect to (1) the absolute height of money incomes, and (2) the absolute height of the money prices of the particular commodities which the money income is used to purchase.[163]

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cit., 678. It is anything but clear, on the other hand, that writers on the *"general" Theory of Value* have been aware of how much *monetary* theory has to offer by way of filling the gaps that would otherwise exist in our understanding of the rôle of "income" in the determination of money prices. This is particularly true with respect to those aspects of monetary theory which are concerned with changes in the *distribution* of income over the period of the trade cycle; and this fact helps to explain why writers who have looked only within the "general" Theory of Value for help in understanding the "dynamic" aspects of "demand," and particularly those "dynamic" aspects which are associated with changes in *incomes,* have found little that has as yet "proved of much heuristic value." (So, for example, Schultz, *The Theory and Measurement of Demand,* 56. It is worth contrasting Schultz's comments on the "dynamic" aspects of demand [pp. 56 ff., 143 ff., 630 ff., of the work cited] with what is said below, pp. 304 ff.)

[162] See, for example, Hicks and Allen, "A Reconsideration of the Theory of Value," *loc. cit.,* 66, on "the increase in demand for a commodity X, which results from a fall in its price . . . as consisting of two parts, one of which is due to the increase in *real income* which a fall in the price of X entails" (italics mine). Cf. also pp. 69 and 210 of the same pair of articles; Allen and Bowley, *Family Expenditure,* 124, 144; Schultz, "Interrelations of Demand, Price, and Income," *loc. cit.,* 444 ff., 450, 459, 465, 468, 476 (cf. the same author's *Theory and Measurement of Demand,* 41 ff., 45, 622, 634, 636, 642); Hicks, *Value and Capital,* 31 f. On the aspects of these writers' treatment of the problem which indicate an awareness of the difficulties which are concealed by the use of a "real income" concept, see the following note. It may be observed here that the treatment of the relation of income-changes to changes in prices which is to be found in Fisher's *Mathematical Investigations in the Theory of Value and Prices,* 44 ff., runs entirely in terms of *money* incomes (in the literal sense of incomes received in the form of money) in relation to the structure of *money* prices, and not in terms of the effect upon "real" income of a given change in money prices.

[163] See, in this connection, the reference given below, p. 301, n. 170, to A. A. Young's comment on "real" *versus* "nominal" prices. It is of considerable importance to add that, strictly speaking, it is necessary to have information also with respect to the proportion of money received which is actually *disbursed* subsequently. One of the ways in which this problem could be temporarily side-stepped would be to define "real income" as the value, in terms of commodities and services, which a given money

We have already seen, however, that information with re-
spect to the nature of the forces determining the absolute
level of money prices and money incomes can be provided
only by the Theory of Money and Prices.[164]   The latter
therefore again becomes a necessary part of any analytical
equipment designed to account for the causes and conse-
quences of what have come to be called "income effects" in
the process of price determination.

It is fair to ask, indeed, whether one can be sure that the final verdict
of economists may not be that the older methods for dealing with the
effects of changes in "income" upon prices, and of changes in prices

would have *if* it were to be disbursed in its entirety. Since, however,
the actual disbursement of money income received may itself make the
structure of prices, as well as their general level, different from what it
would be if the money income were *not* disbursed, it is obvious that this
procedure itself involves a series of assumptions that may or may not be
realized in the concrete case. The method more commonly followed by
the writers cited in the preceding note has therefore been (1) to *assume*
that all money income received is in fact disbursed, or (2) to confine the
money "income" to "*expended*" income," so that "money income" and
"money expenditure" are taken to be identical in amount, and may there-
fore be used interchangeably. See, for example, Allen and Bowley, *Family
Expenditure,* 3, 10, 12, 124 f., 141, 144; and Schultz, *Theory and Measure-
ment of Demand,* 30. The relation between the magnitude of money in-
come, in the literal sense of income received in the *form* of money, and
the magnitude of money expenditure out of that income, is, of course, not
only a problem of the greatest importance in itself; it is emphatically a
problem which can be dealt with adequately only if we make full use of
the analytical instruments devised by monetary theorists for dealing with
precisely this problem. On the problem itself, see Volume I, 354 ff., 379 ff.,
of the present work, and also what is said below, pp. 614 ff., 694 ff.

[164] See Proposition IX (above, p. 296). From Proposition XI (below,
p. 304) it will be clear also that it is not only to the explanation of the
absolute level of money prices and incomes that monetary theory has
something to contribute: there is also the question of the effect of the
functioning of the monetary system upon the *structure* of prices. That the
latter point is of the utmost importance should be clear as soon as it is
remembered that any attempt to compute "real" incomes necessarily raises
the index number problem in all its ramifications. (Cf. J. Marschak,
Elastizität der Nachfrage [1931], 127, n. 1; and see below, p. 301 f.) This
fact alone (to say nothing of the problems raised when one takes account
of changes in the *distribution* of the "national income") should urge the
greatest possible caution in the use of results obtained with respect to the
effect of "income" upon the demand for particular commodities when the
figure for "income" used is "national income" "deflated by a cost of living
index" (cf. Schultz, *The Theory and Measurement of Demand,* 636 f., and
see the comments of Schultz himself, *ibid.,* 630 ff.).

upon "income," are in fact the more "workable," when full account is taken of the range of problems whose solution is skirted by the use of the concept of an "income effect" in describing the process of price determination.[165]

The chief specific claim made on behalf of the conceptual pair, "income effect" and "substitution effect," is, of course, that it enables us to separate analytically two types of effect upon the demand for a given commodity of a fall, say, in its price: the first effect resulting from the "increase in real income" that such a fall in price is held to entail, and the second effect resulting from the tendency to substitute the commodity that has fallen in price for other commodities that remain relatively high in price.[166] It should require only slight reflection, however, to demonstrate that the "income effect," as so understood, either (1) comes far short of covering the range of phenomena covered by the recognition, by writers such as Cournot and Marshall, of the influence of changes in "income" upon the demand for particular commodities, or (2) involves a series of assumptions that need by no means be realized in all cases.[167]

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[165] In some ways, this question has already been raised by Mr. Harrod, who has asked whether "the Marshallian approach" may not be "sufficiently workable" in connection with certain of the problems for whose solution the concept of an "income effect" has been introduced; and Mr. Harrod has also suggested that "only a rather lengthy experience" will be able to decide which method is the more satisfactory (see the *Economic Journal*, XLIX [1939], 295 f.). Mr. Harrod, however, is content to rest his *explicit* defense of "the Marshallian approach" on the realism in certain cases of "the assumption that any particular commodity plays so small a part in the life of an individual, that the income effect of a change in its price is negligible." He has not attempted explicitly to evaluate the usefulness of "the Marshallian approach" when the latter is interpreted in the manner indicated in Chapter Four of the present volume—that is, when demand schedules are allowed to change in conformation as between any two successive realizations of prices, as a result either of changes elsewhere in the price structure, or of changes in the level of money income which are not accompanied by cancelling effects in money prices. See, however, what is said above, p. 162, n. 40, and pp. 215 ff., on Mr. Harrod's own use of the method thus indicated, in order to take account of changes in *income*. In any case, it should be clear that the defense of "the Marshallian approach" presented in this work goes very much further than does that of Mr. Harrod quoted earlier in this note; and it should be clear, in particular, that the defense goes far beyond the suggestion that "the old approach will serve well enough for those who are confining their exposition or thinking to essentially static problems" (Harrod, *op. cit.*, 296).

[166] See the references given above, p. 297, n. 162.

[167] It is of some importance to stress the fact that a very great deal depends upon what is claimed for the concept of an "income effect" in terms of "the range of phenomena covered." If, for example, nothing more is claimed for the concept than that implied in its contrast with the "substitution effect" in the case of a change in the price of one particular commodity, reservations to the use of the concept are not nearly as serious

For the level of an individual's "real income" may change not only as the result of a fall in the price of a given commodity, but also as a result of a rise in his *money income,* with the money prices of commodities remaining the same; and there is no *a priori* reason for supposing that the two types of "income effect" will be the same, for any given individual, in the two cases. Specifically:

1. Even if the "purchasing power" of the individual concerned is exactly the same in the two cases, particular individuals may be sufficiently misled by the "money illusion" to embark upon a consumption program in the case of an increase in *money income* that they might not have been prepared to embark upon if their money income had remained fixed in amount.[168] Much is to be said, therefore, for the continued use of an apparatus which, while it *continues to use the concept of elasticity of demand in the Marshallian sense of the term,* separates the effects upon the conformation of demand schedules which are due (1) to changes in the prices of commodities other than the one taken for examination (the "income effect," in one of its aspects), from those which are due (2) to changes in the level of money income, with money prices remaining the same.[169]   For such a procedure makes no

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as those which may be introduced when "the income effect" is held to be a covering term for *all* the "effects" of changing "incomes" upon prices. Unfortunately, however, the exposition of some of the most eminent sponsors of the concept of an "income effect" has been such as to suggest that virtually all of the latter are meant to be covered by the concept of an "income effect." See, for example, Hicks, *Value and Capital,* 27 f., on Marshall as having "generally neglected the income side" of the problem of the determination of the prices of particular commodities, and as having been "vague about the effects of changes in income upon [the] demand" for such commodities; and see also Mr. Hicks's discussion, *under the head of "income effects,"* of the effect of price changes upon the *seller* of the commodity whose price has changed (*Value and Capital,* 36, 64 f.; cf. below, p. 302 f., and nn. 171 and 172 thereto), as well as his discussion, under the head of "income effects," of the effect of changes in the *rate of interest* upon the administration of income over time (*Value and Capital,* 232 ff.).   For examples of what would be included within the range of problems suggested by the treatment of the "effects of changes in income on demand" by Cournot and Marshall (to say nothing of other writers), quite apart from the special application of the distinction between the "income effect" and the "substitution effect" of a change in the *price of a given commodity* to the explanation of the magnitude of the *demand for that commodity,* see above, pp. 208 ff.

[168] The importance of taking account of the "money illusion" in such cases is of course a commonplace in discussions of monetary policy in relation to "frictions" growing out of wage controversies.   It should hardly be necessary, therefore, to labor the familiar point that the mere fact that individuals may be suffering from an "illusion" does not detract from the reality of the *consequences* of the actions taken under the influence of that "illusion."

[169] It is of some importance to observe that the particular type of "income effect" indicated under (1) (on which see, for example, Hicks, *Value*

assumption whatever with respect to the identity of response of a given consumer's demand for a particular commodity, in the two cases of (1) an increase in "real income" as a result of a fall in the money price of a given commodity, and (2) a change in the level of money income, with money prices remaining the same.

2. In all cases in which the movement of all money prices and all money incomes is not uniform, the interpretation of a fall in a given money price as an increase in the "real income" of income recipients necessarily introduces the index-number problem, with all the pitfalls which that problem involves.[170] Much is to be said for the use of a technique which avoids these pitfalls as far as possible. And it can be said that the "older" method of dealing with the effect of a fall in a given money price in relation to "income" does help to avoid these pitfalls. For, according to this method, the fall in a given money price is regarded as affecting the quantity of particular commodities demanded either by causing a movement along a given demand schedule or by changing the conformation or position of a given demand schedule. In all cases, the demand schedules involved refer to the hypothetical "plans" of *particular individuals* in the face of changes of *particular prices* and changes in the particular incomes of *these individuals;*

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and *Capital,* 45) is *only* one of the phenomena that have been discussed under the head of "income effects," even when this discussion has been confined to the consequences of the fall in the price of a given commodity. There is also, of course, the phenomenon represented by the effect of a fall in the price of a given commodity on the demand for *that commodity* as a result of the increase in the "real income" which such a fall is regarded as representing (cf. Hicks, *Value and Capital,* 31). The point is worth making because of the fact that it is the *latter* type of "income effect" which comes nearest to representing a genuine addition to our analytical apparatus for accounting for the phenomena of "demand." The type of "income effect" indicated in the text, on the contrary, was a commonplace of economic theory in general, and of business-cycle theory, in particular, for generations before the term "income effect" was introduced into economics. See, for example, the references to Malthus, Jevons, and others, given above, p. 218, n. 160; and cf. also the familiar discussions, in business-cycle theory and elsewhere, of the effect of a fall in price of a given commodity subjected to cheapened manufacturing costs upon the demand for *other* commodities, and therefore upon aggregate employment. This fact is certainly relevant for the question of the range of problems for whose adequate treatment the concept of an "income effect" really represents a genuine addition to our analytical equipment, in the sense that it enables us to deal with problems for whose adequate solution no alternative apparatus had existed before the term "income effect" was introduced.

[170] Cf. above, p. 298, n. 164. It should be clear also that the argument, as stated in the text, constitutes a further reason in support of those who have argued against the use of the concept of "real," as opposed to "nominal" prices, wherever possible. See, for example, A. A. Young, "Some Limitations of the Value Concept" (p. 204 n., of Young's *Economic Problems, New and Old*).

with the result that the statement that the fall in a given price brings about a change in the "real income" of prospective purchasers of that commodity is subject to continued check on the basis of the facts of each particular situation.

3. A further set of difficulties is introduced by the fact that a fall in a given money price may actually reduce the *money receipts*, and therefore, possibly, the *money income* of the *sellers* of the particular commodity that has fallen in price.[171]   To talk, in this case, of a necessary increase in the "real income" of *these* individuals is obviously an absurdity, even in cases in which these individuals are also consumers of the commodity in question.   It is hardly surprising, therefore, that sponsors of broader interpretations of the concept of an "income effect" should have attempted to take this difficulty into account.[172]   Yet it is anything but clear that these attempts have been such as to discourage the growth of a conviction that the concept of an "income effect," introduced originally to break down a total effect of price changes into components showing more clearly the "effects" of different operative factors, has come to cover phenomena which not only are of great complexity in themselves, but have also been dealt with in the past by analytical devices permitting a more careful treatment of the separate factors involved.   Much is to be said, therefore, for the continued use, in particular, of an apparatus which separates sharply the effects of changes in *money income* from those effects of changes in money *prices* which may, under certain circumstances, be translated into changes in "real incomes."[173]

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[171] This proposition is of course one that goes back very far in the history of economic theory. See particularly, however, what is said below, pp. 350 f., in connection with Cournot. The most familiar application of it in business-cycle theory is perhaps in discussions of the effect, upon general business activity, of changes in agricultural crops, where the proposition stated in the text has been used to *supplement* what might be called the "Wilson-Jevons effect" (cf. above, p. 218, n. 160) by an examination of the effect of a fall in the price of agricultural products upon the level of farm *incomes*, in order to determine what the *net* effect of such a fall in price is likely to have upon the total business situation. It is not without interest to observe that a parallel to the use of a portmanteau device, such as the concept of an "income effect" becomes when it is made to include the effect upon the incomes of the *sellers* of a commodity, is provided by the use, in such problems, of concepts such as the "effort elasticity of demand" for a given commodity. On this matter, see above, p. 143, n. 6.

[172] See, for example, Hicks, *Value and Capital*, 36 f., 64 f.

[173] In terms of the notation indicated in Volume I of the present work, this means an analysis which would translate realized *price* changes into realized *income* changes, for each of the sellers involved, by means of expressions of the general form $PT_{t \atop n} = (PT)_{I \cdot t \atop n} + (PT)_{NI \cdot t \atop n}$: these expressions being, in the case of particular commodities, of the special form $pq_{t \atop n} = (pq)_{I \cdot t \atop n} + (pq)_{NI \cdot t \atop n}$. The analysis would then proceed to a study

4. As we have seen, the meaning of the assumption that a fall in the money price of a given commodity necessarily affects the "real income" of a prospective purchaser of that commodity depends largely upon what is meant by "real income."[174] Specifically, it is necessary to make clear whether the increase in "real income" comes about only when the money income is expended upon commodities, or whether it is held to come about as the result of the fact that the fall in price makes it *possible* to secure more commodities if and when the money income is expended. We have seen that the sponsors of the concept of an "income effect" have not been unaware of this difficulty, and have attempted to take account of it in various ways.[175] Yet it is not unfair to point out that the difficulty does not even arise when use is made of an apparatus which is concerned throughout with changes in money prices and the relation of these prices to both the *generation* and the *utilization* of *money* incomes, with all that this implies with respect to (1) the *effect* of price changes *upon* money incomes; (2) the degree to which these money incomes are utilized (disbursed); and (3) the *direction* in which they are disbursed, with all that the latter in turn implies with respect to changes in the price structure and the further generation and utilization of money income, including both the degree and direction of utilization.[176]

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of the operation of subsequent "income effects" upon the disbursement of the sum represented by $(PT)_{I \cdot t \atop n}$, with the help of expressions of the general form $(PT)_{I \cdot t \atop n} \gtreqless M_i V_{i \cdot t \atop n+1}$ and $M_i V_{i \cdot t \atop n+1} = (PT)_{i \cdot t \atop n+1}$, or $\Sigma D_{t \atop n+1} = \Sigma pq_{t \atop n+1}$, the latter expression being then broken down into a series of equations for the prices of individual commodities, of the general form $D_{t \atop n+1} = pq_{t \atop n+1}$. On the meaning of the subscripts used, see Volume I, 369, 382 ff., of the present work, and on the implications of the expression $(PT)_{I \cdot t \atop n} \gtreqless M_i V_{i \cdot t \atop n+1}$, in particular, see below, n. 176.

[174] See above, p. 297, n. 163.

[175] See above, p. 297, n. 163.

[176] In terms of the notation reproduced above in note 173, the framework for the study of the effects indicated under (1) is provided by expressions of the general form $PT_{t \atop n} = (PT)_{I \cdot t} + (PT)_{NI \cdot t}$, or, in the case of particular commodities, $pq_{t \atop n} = pq_{I \cdot t \atop n} + pq_{NI \cdot t \atop n}$. The framework for the study of the problem indicated under (2) is provided by expressions of the general form $(PT)_{I \cdot t \atop n} \gtreqless M_i V_{i \cdot t \atop n+1}$, and particularly of the form $V_{i \cdot t \atop n+1} = \emptyset \; [(PT)_{I \cdot t \atop n}, M_i V_{i \cdot t \atop n+1}]$. (See Volume I, 383, n. 88.) The framework for the study of the problem indicated under (3), finally, is provided by expressions of the general form $M_i V_{i \cdot t \atop n+1} = (PT)_{i \cdot t \atop n+1}$, or $\Sigma D = \Sigma pq_{t \atop n+1}$, the latter expression being then broken down into a

XI. To say that the money prices which must be used in any attempt, of the kind necessitated by the existence of a fully developed money economy, to translate a given money income into a given "real" income, are *the particular money prices of the commodities which the money income is used to purchase,* is to introduce at once the concept of a *structure* of money prices.[177] This means, however, that we introduce simultaneously the problem of the nature of the *forces determining* this structure. The bearing, upon the latter problem, of that part of the "general" Theory of Value which is summarized by the expression $D_p = F(p)$ was established in the preceding chapter, of which, in this respect, our Proposition VI may in turn be taken as a summary.[178] What must be emphasized here, however, is that at least an equally important element in the solution of the problem is *provided by monetary theory, and by monetary theory alone.*

There is very little within the "general" Theory of Value, for example, which accounts directly for the particular sequence in which, in the course of monetary expansion and contraction, the money incomes of particular sectors of the community are successively raised and lowered.[179] Yet it

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series of equations for the prices of particular commodities (or groups of such commodities) of the general form $D_{t_{n+1}} = pq_{t_{n+1}}$. (It should be observed that we are considering here only the "demands" exercised out of *income*. A comparable set of expressions could of course be provided for the "demands" exerted by "traders" out of their non-income "receipts." On the general point involved, see above, p. 287.) I must leave for another occasion a more detailed application, to specific problems, of this type of apparatus for dealing with problems involving the generation and utilization of money income.

[177] This, after all, is what is implied by the statement that the proposition indicated involves the "index-number problem." See above, p. 298, n. 164. On the relation between the solution of the problem of "price dispersion"—for that is a third way of describing the problem involved—by the use of *averages* (index numbers), on the one hand, and by the explicit use of the concept of a *structure* of prices or of "price groups," on the other, see, in addition to what is said above, p. 69, the discussion presented below, pp. 333 ff.

[178] See above, pp. 275 ff.

[179] The considerations on the basis of which it can be argued that the "general" Theory of Value has *something* to contribute to a solution of this problem are chiefly two. In the first place, a recognition of the relation between the determination of money incomes, on the one hand,

is clear that this is of the most vital importance for any judgment as to the probable sequence and the direction in which the demand schedules for particular commodities change, in both position and conformation, during the course of a monetary expansion or contraction.[180] It is equally clear (1) that this sequence in the change in position and conformation of these demand schedules is of the utmost importance in accounting for any change in the structure of money prices during the course of such expansion or contraction; and (2) that these changes in the structure of money prices are themselves of the utmost importance in accounting for *subsequent* changes in the level and structure of money incomes.   Here again, therefore, monetary theory provides an indispensable complement to the "general" Theory of Value when our task is that of accounting for changes in the structure of money prices and money incomes.

That it is necessary to trace the *shifts* in demand schedules over any given period, if we are really to understand the nature of the forces determining the prices of particular commodities realized during that

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and the determination of the money prices of commodities and services, on the other, is a necessary part of any technique designed to reveal the particular sequence in which the money incomes of particular sectors of the community are successively raised and lowered; and such a recognition has appeared explicitly, or may be regarded as implicit, in certain formulations of the "general" Theory of Value (see below, pp. 350 ff.). In the second place, by Proposition VI, virtually the whole of the "general" Theory of Value must remain a *part* of any adequate analytical equipment for accounting for the determination of money prices. These propositions hold, of course, in situations involving general monetary expansion and contraction as well as in those which do not. Yet from the discussion presented in the fine-print section of the text, below, it should be clear that it is still true that *most* of the analysis required if we are to account for "the particular sequence in which, in the course of monetary expansion and contraction," money incomes and demand schedules, and therefore realized money prices and subsequent money incomes, may be expected to change *during periods of monetary expansion and contraction* is to be found in monetary theory, and in monetary theory alone.

[180] This follows, of course, from (1) the argument summarized in the preceding chapter (pp. 205 ff.) with respect to the effect of changes in "income" upon the conformation, as well as the position, of the demand schedules for particular commodities; and from (2) the argument summarized by our Proposition VII, with respect to the need for establishing the position of a particular demand schedule in the system of co-ordinates of which the price axis represents *absolute* money prices.

period, has of course been accepted as axiomatic by writers upon the determination of "statistical" demand curves.[181] Nor can these writers be criticized for having failed to recognize either (1) that these *shifts* in demand schedules may be much more important in the explanation of price change than the facts with respect to elasticity shown by given demand schedules over the period; or (2) that changes in the level and structure of *incomes* are of the greatest importance among the factors leading to these shifts in particular demand schedules.[182] If these writers are to be criticized at all, it is chiefly on the ground that when they have discussed the nature of the forces leading to shifts in particular demand schedules, they have generally failed to convey an adequate appreciation of what was already available in *monetary* theory for just this purpose.[183] For those periods in which

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[181] See, for example, Staehle, *Die Analyse von Nachfragekurven in ihrer Bedeutung für die Konjunkturforschung*, 26 f., 45 f.; and Schultz, *The Theory and Measurement of Demand*, 73 ff., 129, 143 ff., *et passim* (see the Index [p. 816], under "Shifting of curves" and "Shifts of demand curves").

[182] Cf. the Foreword to Staehle, *Die Analyse von Nachfragekurven*, also pp. 32 f. of the same work; Gilboy, "Methods of Measuring Demand or Consumption," *loc. cit.*, 72; Schultz, *The Theory and Measurement of Demand*, 82, 120 ff., 143, 225, 563, 630 ff.

[183] Occasionally, to be sure, one finds a recognition of the fact that the nature of the forces leading to shifts in particular demand schedules is a matter closely related to the explanation of *cyclical* processes, in which monetary changes of course play an important rôle. See, for example, Staehle, *Die Analyse von Nachfragekurven*, Foreword, also pp. 26 f., 46. It can hardly be said, however, that the workers on the actual statistical derivation of particular demand schedules have come very close to an adequate appreciation of the proposition that a very considerable part of the explanation of the shifts in these particular demand schedules is to be found in an analytical investigation of the steps in the cyclical process. In most cases, indeed, they have come no closer than to introduce, as part of an attempt to measure the "routine of change" of these demand schedules, either (1) an index of "general business conditions," or (2) "time," considered as "a catch-all for the resultant of those factors which cannot conveniently be measured separately." See, for example, Schultz, *The Theory and Measurement of Demand*, 230, 264, 269, 279, 299, 302, 312, 321 f., 409; 55, 70, 241, 341, 344, 359, 433, 454, 487, 512, 561. With respect to the first device, it should be sufficient to point out that the whole argument of those who have regarded shifts in the demand schedules for particular commodities or groups of commodities as of importance for the cyclical process rests upon the presumption that the demand schedules for particular commodities cannot be expected to change uniformly over a period when "general" business conditions are changing. If this is so, it follows that the use of an index of "general" business conditions as a method for measuring the degree of shift in particular demand schedules will necessarily be devoid of meaning in a very large number of cases. And with respect to the second device, it may be pointed out that, apart from the analytical nihilism to which its use must lead if it is pushed far enough, Professor Schultz himself proposed its use only as a measure of the influence of those factors "which change more or less smoothly," and

important monetary and other cyclical changes are known to have taken place, it is hardly adequate, for example, to present a picture of the "routine of change" followed by particular demand schedules, believed to have shifted during the period taken for examination, chiefly in terms of changes due to changes in *population*.[184] And to attempt to "eliminate" the effect of these *monetary* changes by the simple device of dividing a given series of absolute money prices by an index of the "general" level of prices is to miss the whole point of precisely those sectors of monetary theory which have been concerned to demonstrate the extreme improbability of the type of uniform change in money incomes and money prices which such a procedure necessarily implies.[185]

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insisted that he was "far from suggesting that the abrupt changes in demand which are due to innovations and inventions could be profitably studied in this manner" (*The Theory and Measurement of Demand*, 55, and n. 73 thereto). Since a large number of the changes which can be held to be of influence in accounting for the cyclical process are of this "abrupt" character, it is clear that this amounts to a virtual surrender of any attempt to deal, by the use of the device indicated, with the "routine of change" *actually* shown by demand schedules over the cycle. On the possible suggestion that this conclusion itself means a surrender of any hope of obtaining significant results from the use of the *concept* of a series of "abrupt" shifts in particular demand schedules over the period of the cycle, see n. 185, below.

[184] In the work of Schultz, for example, the only important factors regarded as likely to lead to shifts in demand schedules—apart from (1) changes in the "purchasing power of money" (on which see the next sentence in the text, above), (2) "general business conditions," (3) the "catch-all" category of "time," and (4) such changes in "tastes" as could be related to changes in the relative prices of substitute commodities or could be deduced from other facts that could be held to account for changes in per capita consumption of the commodity taken for examination—were (1) population changes (or analogous changes in the "number of animal units"), and (2) changes in the *national* income. See, for example, *The Theory and Measurement of Demand*, 82, 93, 143 ff., 200, 215, 240 f., 269 ff., 324, 338 ff., 351 ff., 434 ff., 470, 487, 489. From the passage from the latter work cited above, p. 298, n. 164, it is clear that Professor Schultz himself was not entirely unaware of the crudities involved in his use of the *national* income to measure the effect of "income" changes upon particular demand schedules. Yet it is striking that on at least one occasion he was willing to include "changes in income" among the forces which could be taken into account by being considered as "directly proportional either to the law of population growth or to some function of it" (*Theory and Measurement of Demand*, 143 f.).

[185] For examples of the procedure indicated, see Schultz, *The Theory and Measurement of Demand*, 69, 71, 93, 240, 409, 487. On the objections to the procedure, cf. the comments of Stigler, "The Limitations of Statistical Demand Curves," *loc. cit.*, 472 f. It is of course clear that the reason why devices of this degree of crudity have been used in connection with the derivation of "statistical" demand curves is that it enormously simplifies the "solution" of what is otherwise a practically insoluble problem. It does not follow, however, that the *problem with which we are here con-*

When, on the other hand, one turns to what *monetary theory* has to say with respect to the "routine of change" that the demand schedules for particular commodities may be expected to show during periods of monetary expansion and contraction, the contrast with these over-simplified assumptions is extremely striking. The *locus classicus* in eighteenth-century literature for analysis of the type indicated here is of course the discussion of Cantillon. For it was Cantillon who

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cerned is an "insoluble" one. That problem is not the presentation of "statistical demand curves," but the determination of the nature of the forces, associated with the working of the monetary system, which make money prices what they are. And the specific aspect of that problem with which we are here concerned involves a demonstration of the proposition that if we are to have an adequate understanding of the reasons why the *ex ante* demand schedules for particular commodities may be presumed to change in position and conformation over the period taken for examination, we must be prepared to draw upon the whole of *monetary* theory for suggestions as to why money incomes and expenditure out of these incomes can be supposed to change in the degree and direction that they do. Nor is there anything in this argument which suggests that we have no interest in what *statistical data* may be held to show with respect to the probability of occurrence of any one of the "model sequences" with respect to the movement of particular demand schedules which have been provided by the relevant sectors of monetary theory. What the statistical data give us are the facts with respect to the level and distribution of realized money incomes, on the one hand, and the level and distribution of the realized expenditure out of these incomes upon specific commodities, on the other. We know, from the argument developed above, that the magnitude of this realized expenditure will depend in part upon the conformation and position of the *ex ante* demand schedules for the particular commodities involved. In attempting, moreover, to determine how far these changes in expenditure upon particular commodities are due to movements along a given *ex ante* demand schedule, and how far they are due to shifts in such schedules, we are not completely helpless on the "statistical" side. For, given an adequate analytical apparatus for *explaining* why movements along these *ex ante* schedules and shifts in these schedules come about, we may examine all statistical data which bear upon such explanations, in order to determine the degree of plausibility attaching to any given proposition with respect to the probable movements in the particular *ex ante* demand schedules which are held to be involved in the determination of given money prices in given historical situations. From the point of view of such a program, the mere fact that the demand schedules involved do not "stay put" long enough, or follow a "routine of change" controlled by sufficiently few factors, to enable us to provide a measure of these demand schedules as they would appear if they *did* "stay put," provides no reason whatever for either (1) abandoning the concept of particular demand schedules as factors affecting the determination of money prices; or (2) pretending to have provided measures of such demand schedules, and of their "routine of change," at the cost of adopting grossly oversimplified hypotheses with respect to what these "routines of change" may be expected to be in the world we know.

insisted emphatically upon the necessity for tracing the differential impact of an increased stream of money-spending power upon the prices of individual commodities as a result of the differential utilization of this increased money-spending power in "demand" for different commodities, according to (1) the distribution of this additional spending power among different classes of the population (that is, its effect upon the structure of money incomes and other money receipts), and (2) the "idea of those who acquire the money" (that is, the decisions of "those who acquire the money" with respect to the direction of their expenditure).[186] And that Cantillon did not represent an isolated instance in the eighteenth century is clear from the analysis of a writer such as J. G. Büsch (1780), whose description of the steps involved in the process of monetary expansion, for example, ran entirely in terms of (1) the effect of such expansion upon the incomes of the particular groups in the community who receive the new money-spending power; and (2) the *choices* of the individual recipients of this additional spending power with respect to its utilization, including (i) the extent to which the "demand" thus made *possible* will be actually exerted, and (ii) the way in which this "demand," if exerted, would be likely to be distributed over different types of commodities.[187]

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[186] See Cantillon's *Essai*, 215 ff., 236 ff. (pp. 163 ff., 179 f. of the Higgs translation). In view of the fact that the tracing of the "routine of change" evidenced by shifting demand schedules for particular commodities has usually been regarded as belonging to the "dynamics" of the subject, it is of some interest to recall that Cantillon's analysis has been characterized as being precisely a contribution to "monetary dynamics." See Volume I, 84, n. 30; 172, n. 38; 307, n. 13, of the present work, and the references there given. From these references, as well as from those to J. G. Büsch given in the following note, it will be clear that Cantillon was far from being the only eighteenth-century writer who was aware of the fact that a differential impact upon money incomes and therefore upon prices was virtually inevitable during the progress of a monetary expansion. High praise has been accorded to the comments by Hume in this connection (see, for example, the famous passage in Hume's *Essay Of Money* [Volume I, 303 f. of the 1777 edition of his *Essays and Treatises on Several Subjects*]); and of "certain of the descriptions," presented by other eighteenth-century writers, such as Forbonnais, of the "mechanism of introduction" of money into the economic system, with their emphasis upon the differential impact of this new money upon incomes and prices, it has been said that they are "worthy to remain classic and have not been surpassed" (so, for example, Harsin, *Les Doctrines Monétaires et Financières en France du XVIᵉ au XVIIIᵉ Siècle*, 256). Yet there can be little doubt that it is Cantillon who stands out among all the writers named for his contributions on the point in question.

[187] On point (1), see, for example, (a) Büsch's description of the sequence of the creation of money income and expenditure out of income that may be expected to accompany that geographical movement of workers which may itself be induced by the spending of the "new" money by its first recipients (*Abhandlung von dem Geldsumlauf*, I, 179); (b) his description of the ultimate rise in wage rates and wage incomes which such spending

In the early nineteenth century, on the other hand, the most famous discussion is to be found in the writings of Tooke. For it was Tooke who emphasized the differential effect upon prices of war-time expenditure, by way of the different direction imparted to "demand" by such expenditure.[188] Tooke's argument, moreover, must be regarded as having adumbrated the later discussion by Newmarch of the differential effect, upon incomes, of the monetary expansion following the gold discoveries, and, through the "demands" exerted by outlay from these incomes, upon the prices of different types of commodities—these individual prices being actually thrown by Newmarch into a series of

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may be expected to engender (I, 197 f., 205); and (c) his description of the differential effects upon incomes that may be expected to result from (1) differences in the degree of awareness as to what is happening that is likely to be shown by individuals engaged in different types of occupations, and (2) the different degress of economic power which individuals are capable of exerting (I, 207 ff.). On the rôle of individual *choices* in the process, one may cite Büsch's general insistence upon the importance of describing the process of price change in such wise as to do justice to the methodological proposition that "the prices of things are the result of the free deliberation of men, who seek to use money in ways that seem good to them" (I, 203; cf. also I, 238), instead of resulting mechanically from either the mere fact of an increase in the quantity of money, or from a supposed "agreement, avowed or tacit, among men," or being deducible from a simple examination of the "proportion of money brought into circulation to the mass of all saleable things and the total of all remunerable services" (I, 237). More specifically, attention may be called to Büsch's insistence upon emphasizing the consequences of differences in what he actually called "the propensity to spend" (*der Hang zum Aufwande;* I, 118) of different individuals (I, 196), or of the same individuals under differing conjunctural conditions (as when the increase in spending and the resulting increase in money incomes is expected to be only temporary [I, 202 ff.]); and attention may be called also to his insistence upon the probability of an attempt, on the part of individuals receiving the new money-spending power, to extend both the amount and the range of their consumption, the ultimate results upon the prices of both necessities and luxuries thus depending, as Cantillon had put it, upon the "idea of those who acquire the money" (Büsch, *Abhandlung,* I, 121, 178 f., 196). That Büsch was, in fact, familiar with Cantillon's work is clear from his references to the latter. See the preface to Büsch's *Abhandlung,* pp. 3b and 4, and also II, 546 of the same work; though Büsch's own comment, in the preface to his *Abhandlung,* that Cantillon's discussion "teaches us too little about the main thing" *(belehrt zu wenig über die Hauptsache)* can hardly be regarded as a generous estimate of the importance of Cantillon's contribution.

[188] See Tooke's *Thoughts and Details,* II, 9 ff., 22 ff.; IV, 13; and cf. his *History of Prices,* I, 93 ff., 100 ff.; II, 349. It is hardly surprising that the problem raised by Tooke should be attracting renewed interest in our own day. See, for example, E. B. Schumpeter, "English Prices and Public Finance, 1660–1822," *Review of Economic Statistics,* XX (1938), 28; and E. S. Mason, "The Impact of the War on American Commodity Prices: A Preliminary Review," *ibid.,* XXI (1939), 147.

price "groups" in accordance with either the probabilities or the recorded evidence with respect to the direction in which these particular "demands" could be shown to have been exerted.[189]   And if, as we have seen, there were differences between Tooke, on the one hand, and a writer such as Sir John Lubbock, on the other, with respect to the rôle played in the process of price change by changes in the quantity of money, there was no significant difference between them with respect to the point under discussion.   For both writers called explicit attention to the differential impact upon money incomes (and therefore upon the prices of the particular commodities which the recipients of these additional incomes choose to "demand") which may be expected to be associated with the process of monetary expansion in virtually all cases.[190]

Nor is there any clear ground for arguing that *all* the "classical" economists, unlike the writers cited, were blind to the central point at issue.   Adam Smith, to be sure, gave no evidence of an awareness of the significance of anaysis of the kind for which Cantillon had provided the imperishable prototype.[191]   It is true, also, that certain aspects of Ricardo's exposition left much to be desired on the point at issue; just as it is undoubtedly true that John Stuart Mill, despite the defenses that have been offered on his behalf by writers of eminence in our own day, was on occasion guilty of a type of exposition that can only be regarded as unfortunate from the standpoint of the issues under discussion here.[192]   Yet it must not be forgotten that in the one branch

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[189] See Volume I, pp. 314, n. 33, and 503, n. 50, of the present work, and the references there given to those parts of the *History of Prices* for which Newmarch was chiefly responsible.

[190] See the references to Lubbock given in Volume I, 502, n. 49. On the "formal differences" between Tooke and Lubbock to which reference is made in the text, see above, p. 154, n. 24.

[191] Cf. what is said on this matter above, pp. 28 f., with respect to the bearing of this fact upon an evaluation of the contributions of Smith and Cantillon, respectively, to an adequate "synthesis" of the Theory of Money and Prices, on the one hand, and the "general" Theory of Value, on the other.

[192] The objectionable aspects of Ricardo's exposition are typified by passages such as that in which he argued that "the alteration in the value of money arising from [the] scarcity or abundance [of money] will operate in an equal proportion on the prices of all commodities" (cf. Volume I, 502, of the present work, and the references to Ricardo's writings given in n. 48 thereto). In the case of Mill, they are typified by statements such as that "the relations of commodities to one another remain unaltered by money" (*Principles,* Book III, Chap. VII, sec. 3 [p. 488 of the Ashley edition]). In justice to Mill, it should be pointed out that, in the very next chapter of his *Principles* (p. 491 of the Ashley edition), he showed himself ready to take account of the fact that the recipient of any "increase in the quantity of money . . . doubtless adds, in the first instance, to the demand only for certain kinds of goods, namely, those which he selects for purchase" (cf. the similar comment in Mill's

of "classical" analysis which has shown the greatest vitality down to the present day—namely, the "classical" theory of international and interregional trade—the very essence of the argument involved acceptance of the proposition that the money flows associated with international and interregional transfers must be expected to evidence a differential expenditure-impact upon different price "groups": in particular, the groups of "domestic" and "international" prices, respectively.[193]

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review-article, "The Currency Question," *loc. cit.*, 587). It should be pointed out also that Mill admitted (1) that "it is of course possible that the influx of money might take place through the medium of some new class of consumers, or in such a manner as to alter the proportions of different classes of consumers to one another, so that a greater share of the national income than before would thenceforth be expended in some articles, and a smaller in others, exactly as if a change had taken place in the tastes and wants of the community," with the result that "until production has accommodated itself to this change in the comparative demand for different things, there would be a real alteration in values, and some things would rise in price more than others, while some perhaps would not rise at all"; and he admitted (2) that unless the "increase in the quantity of money" resulted in a strictly *proportional* change in money incomes, there would be "an alteration of the proportions in the demand for different commodities" (*Principles,* Book III, Chap. VIII, sec. 2; pp. 491 f. of the Ashley edition). The same type of emphasis, finally, must be regarded as implicit in the proposition, advanced in the later editions of Mill's *Principles,* with respect to the possible effect of an increase in the "circulating medium" in bringing about a "real increase of capital" by way of a changed distribution of receipts and expenditure as between "producers or dealers," on the one hand, and consumers, on the other (Book III, Chap. XI, sec. 2 [p. 512 of the Ashley edition]; cf. M. Fanno, "Cicli di produzione, cicli di credito, e fluttuazioni industriali," *Giornale degli economisti,* LXXI [1931] 359 n., and Hayek, *Preise und Produktion,* 20 f.). It is such passages as these which provide some basis for Mr. Hawtrey's defense of Mill on the ground that certain of the latter's propositions to which exception has been taken by writers such as Mises were "merely devices to convey to the reader the significance of a static principle" (*Economic Journal,* XLV [1935], 513). Yet even if one were prepared to pass over Mill's statement that changes in the distribution of incomes and the demand for particular commodities were in the nature only of "accessory" circumstances attending the "increase of money" (*Principles,* 492), his statement that "the relations of commodities to one another remain unaltered by money" remains an extremely unfortunate one.

[193] See Volume I, 503, of the present work, and especially n. 53 thereto. It may be observed that the central point with which we are here concerned remains entirely untouched by the differences, much discussed in recent years, between the so-called "classical" theory of the mechanism of international transfer and the theory represented by those, of "the modern school of thought," who "postulate shifts in international demand schedules to right or left without any necessary intermediation of gold flows" (cf. M. A. Heilperin, *International Monetary Economics* [1939], 153 ff., and the references there given). For *both* types of analysis are concerned with the nature and the consequences of "shifts in demand

There is always, in any case, the analysis of J. E. Cairnes, whose general "orthodoxy" not even Mr. Keynes has called into question.[194] For the case of Cairnes provides a proof, if proof is necessary, that there was nothing in the "classical" position generally which made it inevitable that a writer sympathetic to the broad· "classical" position could be expected to show no interest in the differential price change which would be bound to result from (1) the differential effect upon money incomes associated with the process of monetary expansion, and (2) the particular "habits and tastes" of the particular members of the community who benefit from such changes in money incomes.[195] And it is worthy of particular notice that whatever may be said of the extent to which Cairnes's "orthodoxy" prevented the establishment of complete sympathy between him and opponents of other aspects of the "orthodox" tradition, there was no difference between them on the point at issue. Jevons, for example, accepted Cairnes's argument on this point virtually without reservation, just as he accepted with the greatest enthusiasm the comparable argument of Cantillon.[196] And the same thing may be said of Adolf Wagner, whose intellectual sympathy with much of the "Banking School" position of Tooke might have predisposed him to be as critical as Tooke often showed himself to be

schedules" which are held to result from changes in the level or distribution of money incomes as a result of the international transfer of money-spending power. Such differences as exist between the two "schools of thought" on the subject have to do with the different possible *ways* in which the "demand schedules" may "shift," and the different consequences that may be held to follow from the different assumptions made in each case.

[194] Cf. the characterization of Cairnes as an "orthodox economist" in Keynes's *Laissez-faire and Communism* (1926), 35. The date of publication of Mr. Keynes's pamphlet is worth noting. See, for example, Mr. Keynes's comment on p. 23 of the work cited, with respect to "what the economists are *supposed* to have said" on the basis of "what the popularisers and the vulgarisers said," and compare this comment with (1) Cairnes's own comment on the relation of "the miscellaneous literature of economic discussion" to "the doctrines of the science as expounded in the works of acknowledged masters" (see Volume I, 191, 295 f., of the present work); and (2) Mr. Keynes's later manner in dealing with the alleged substance of "orthodox" or "classical" tradition.

[195] In addition to the references to Cairnes given in Volume I, 172, n. 38, and 503, n. 50, see the discussion of the relation between monetary expansion, differential changes in money incomes, and "demand and supply," in Cairnes's *Essays,* 4 ff., 40 f., 56 ff., 114.

[196] For Jevons's comments on Cantillon, see above, p. 25, nn. 64 and 65; and cf. the references to Cairnes in Jevons's *Investigations,* 52 n., 125, as well as Jevons's own "Classification of Incomes according as they suffer from Depreciation," and his discussion "Of Expenditure as affected by Depreciation," *Investigations,* 76 ff., 81 ff. On the less fortunate aspects of Jevons's treatment of the analytical consequences of the fact of differential price change, see what is said above, p. 69, n. 40, and the forward references there given.

of anything deserving to be called the "orthodox" theory with regard to the effect of money upon prices. For Wagner not only refrained from attacking the position of the "orthodox" Cairnes on the question at issue, but himself went on to outline an apparatus, of a type which neither Tooke nor Cairnes themselves seems to have envisaged, which might have served for the development of later doctrines with respect to the causes and consequences of differential movements in price "groups." [197]

It is hardly surprising, therefore, that a writer as anxious as Wicksell to avoid unnecessary conflict with what he regarded as "authoritative" views upon the subject of the effect of money on prices, should have seen no conflict between these views and his own positive argument with respect to the effect of changes in the rate of interest upon the structure of *relative* prices, by way of its effect as a "capitalization factor" and the subsequent operation of the "bidding-up process" upon the distribution of the aggregate stream of monetary expenditure over the different sectors of the economic process.[198] Much the same may be said of the argument of Ludwig von Mises, whose analysis of the effects upon the price structure of an increased stream of money-spending power, by way of its effect upon the distribution of money incomes and other money receipts, on the one hand, and, on the other, the distribution of the outlay from these money incomes and money receipts between goods of "higher" and "lower" orders, respectively, is certainly of enough originality to warrant a characterization of his argument more apt than that of "neo-Wicksellian." [199] Professor Schumpeter's acknowledgment, in turn, of the relation, to the analysis

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[197] See Volume I, 319, of the present work, and especially the references to Wagner's *Sozialökonomische Theorie des Geldes* given in n. 50 thereto. It was only very late in his career that Wagner advanced the particular suggestion there indicated: namely, that special significance might be held to attach to the distinction between the money demand exerted by producers "for the real means of production," on the one hand, and consumers' expenditure "upon objects representing the needs of private consumption," on the other. It is hardly surprising, therefore, that he did not develop the suggestion at length. Yet in justice to him it should be mentioned that, to my knowledge, the distinction in question is not even suggested by either Tooke or Cairnes, with whose writings Wagner was certainly familiar.

[198] See above, p. 94, and the references given in n. 10 thereto. Also relevant in this connection, of course, is Wicksell's treatment of the phenomenon of "forced saving" (see above, p. 95, n. 11).

[199] See the comments on this aspect of Mises's work in Hayek, *Prices and Production,* 22 (cf. also the quotation from Mises's *Theory of Money and Credit* [pp. 362 f.] given on p. 65 of the same work), and *Monetary Theory and the Trade Cycle,* 133. It should be added that Professor Mises by no means confined his argument with respect to the effect, upon the structure of money prices, of monetary expansion and contraction, to a description of the particular effect indicated in the text. See, for example, his *Theory of Money and Credit,* 139 ff., 208 ff.

of Mises, of that part of his own argument which was summed up by the concept of "forced saving," regardless of other differences between the two writers, shows how widespread the agreement actually has been as to the necessity for taking into account the type of effect upon the price structure with which we are here concerned.[200]   And when it is added that the concept of "forced saving," or its analogues, provides a link backward to Léon Walras and forward to the "real levies" and "imposed lacking" of such representatives of "old" Cambridge as Robertson and Pigou, the *coup de grace* is given to any suggestion that virtually all "current economic theory" has attempted to eliminate the problem of the influence of money upon the structure of relative prices by arguing that, given an "initial impulse . . . 'on the side of money,' such as an inflation of the currency, . . . all individual prices tend to be affected *equally*." [201]

More important for our present purpose, however, is the establishment of a further conclusion: namely, that regardless of the extent to which the writers just cited may have differed in the details of their analysis, they were agreed (1) that it is the task of monetary theory to establish a series of "model sequences" tracing the successive steps involved in the process of monetary expansion and contraction; and (2) that a central element in these sequences must be the establishment of the time-order in which the demand schedules for particular commodities may be expected to shift upward or downward in the course of these processes, as the result of changes in the level and distribution of money incomes and other money receipts, on the one hand, and of outlay from these incomes and money receipts, on the other.[202]   That the earlier writers, in particular, did not state their

[200] For Schumpeter's reference to Mises in this connection, see the former's *Theory of Economic Development*, 109, n. 1. (On the use of the *term* "forced saving," however, see Mises's *Geldwertstablisierung und Konjunkturpolitik* [1928], 45, n. 1). On the more general aspects of Schumpeter's treatment of the phenomenon of differential price change during periods of monetary expansion and contraction, see above, pp. 118 f., and the references given in nn. 67–69 thereto. Cf. also Schumpeter's "Das Sozialprodukt, etc.," *loc. cit.,* 687 ff.

[201] Cf. Volume I, 500 ff., of the present work, and the references to Keynes's *Treatise* there given. On the rôle of Walras in the history of the development of the idea of "forced saving" and related concepts, see my "The Monetary Aspects of the Walrasian System," *loc. cit.,* 150, n. 12, and the references there given.

[202] Certain aspects of Wicksell's analysis have been restated in the form of a "model sequence" by Lundberg, *Studies in the Theory of Economic Expansion,* 52 ff. It would be easy to show, however, that the particular aspects of Wicksell's argument selected by Lundberg are by no means the only ones relevant for the construction of a "model sequence" of the type indicated. Indeed, *all* the examples given above, beginning with Cantillon, are to be regarded as implying the use of "model sequences." It should hardly be necessary to emphasize the bearing of this fact upon the suggestion that the idea of using "sequence analysis" is a novelty in economic literature. See below, pp. 368 ff.

respective arguments in terms of shifts of demand schedules is hardly surprising, in view of the fact that the very concept of a demand schedule can hardly be said to have become generally familiar prior to the work of Alfred Marshall. What is really striking, however, is that the statement of an argument of the type indicated above, in terms of successive shifts of the demand schedules for particular commodities, occurred at least as early as the year 1889, which witnessed the publication of the *Untersuchungen über die Theorie des Preises* of Auspitz and Lieben.[203]

The direct citation of one of the relevant passages in Auspitz and Lieben by Ludwig von Mises, in connection with the latter's description of the processes leading to differential price change during periods of monetary expansion and contraction, is proof enough that the substance of their argument could be accepted without reservation even by writers who preferred to use a terminology which did not involve explicit reference to "demand schedules," of either the Marshallian or the Auspitz and Lieben type.[204] It happens, indeed, that the more explicit uses of the Marshallian terminology, in particular, in connection with the type of phenomenon under discussion, are to be found outside, rather than within the narrow circle of the members of the "old" Cambridge group, in whose writings the description of the relevant processes in terms of shifts of demand schedules for the particular reasons here indicated has often been impeded as the result of a predilection for discussing these processes in terms of the effects of shifts in "real" demand functions.[205] But if members of the "old" Cambridge

[203] See especially pp. 64 ff., and 548 ff., of the work cited. On the relation of Auspitz and Lieben demand schedules to demand schedules of the Marshallian type, see above, p. 263, n. 93.

[204] See Mises, *Theory of Money and Credit*, 210.

[205] See, for example, Pigou, *The Theory of Unemployment*, 210, 235, 238, 241 f., 272 ff., 279 ff., 284 ff., 295 f., 313. In the light of the comments made above, p. 143, n. 6, with respect to this particular predilection of "old" Cambridge, it should hardly be necessary to labor the point that what is involved in this particular instance is chiefly a question of exposition, though much more than a mere question of exposition might be involved in other cases. There is no difficulty, in any event, in citing writers outside "old" Cambridge, apart from Auspitz and Lieben, who have stated their argument in terms of successive shifts of demand schedules, without suggesting that these demand schedules must necessarily be translated into "real" terms. See, for example, the passages in Schumpeter's *Business Cycles*, 154, 196. (These passages should be read in the light not only of (1) the further passages cited above, p. 315, n. 200, in connection with Schumpeter's treatment of the phenomenon of differential price change during periods of monetary expansion and contraction, but also (2) his insistence elsewhere that "we shall not be able to rest content with an analysis for which the shifting of a demand or supply curve is a thing which just happens": that, on the contrary, the very fact that "certain kinds of shifts are amenable to rule or law" points to one of the ways in which we may "build the economic cycle into our general theory" [*Journal of Political Economy*, XLII (1934), 256].)

group are to be criticized for having failed to couch their analysis more explicitly in terms of those shifts in the demand schedules for particular commodities which may be expected to be associated with the operation of the monetary mechanism, this criticism can come gracefully only from those writers who have themselves provided a statement running in just such terms. It certainly cannot come gracefully from those who, in discussing the relations between monetary theory and the "general" Theory of Value, have discussed the concept of demand schedules for particular commodities in terms that are more than a little misleading—as when, for example, the writers concerned have failed to make clear that the mere fact that these demand schedules have often been constructed upon the assumption of a "fixed value of the money unit" says nothing whatever against the use of just such schedules in describing the processes involved when the "value of the money unit" may be regarded as changing.[206] And still less could it come gracefully from those, like Mr. Keynes, who have ruled out the possibility of an explicit statement of the kind indicated above, by their formal rejection of the concept of demand schedules for particular commodities as a weapon useful in accounting for processes realized in the world we know.[207]

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[206] See, for example, Anderson, *The Value of Money*, 52 f., 54 n., 95.

[207] It should be sufficient, in this connection, to call attention to the results of Mr. Keynes's adoption of this position, in one of the few cases, in the *General Theory*, in which any particular importance was attached to the "distribution of effective demand between the products of each individual industry" (p. 286). Mr. Keynes admits that "the way in which we suppose the increase in aggregate demand to be distributed between different commodities may considerably influence the volume of *employment*," variations in which it is the avowed major purpose of the *General Theory* to explain. One might have hoped, therefore, that he would have devoted some attention to the nature of the *forces determining* the "way in which . . . the increase in aggregate demand" is "distributed between different commodities." In particular, one might have hoped for a demonstration that the particular analytical devices of the "general" Theory of Value which were designed to deal with precisely this problem need not in fact be introduced into the problem at all. Instead, we are told merely that the reason why, "as aggregate expenditure changes, the corresponding expenditure on the products of an individual industry will not, in general, change in the same proportion" is "partly because [1] individuals will not, as their incomes rise, increase the amount of the products of each separate industry, which they purchase, in the same proportion, and partly because [2] the prices of different commodities will respond in different degrees to increases in expenditure upon them." The first of these reasons obviously introduces that whole range of issues with respect to the effect of changes in *income* upon the distribution of expenditure over individual commodities with which the "general" Theory of Value has been particularly concerned in recent years, and which, when summarized in the concept of "income elasticity of demand," can be said to have *supplemented* the Marshallian "elasticity of demand," but can hardly be said to have replaced it (see above, p. 218, and especially

n. 161 thereto). If, on the other hand, the second of Mr. Keynes's reasons has anything whatever to say as to why, "as aggregate expenditure changes, the corresponding expenditure on the products of an individual industry will not, in general, change in the same proportion," it is only because of its implicit use of the *Marshallian* "elasticity of demand": for it is only by the use of such a concept that any connection can be established between changes in "the *prices* of different commodities" and resulting changes in the amount of expenditure devoted to each of these commodities. The particular passage just cited may therefore be taken as testing (1) how far Mr. Keynes's "synthesis" of the "general" Theory of Value with the Theory of Money and Prices has effected a synthesis at one of the points at which a synthesis was most needed, and (2) how far he has demonstrated the possibility of getting along *without* such a synthesis.

CHAPTER SIX

Stream Equations and the Price System

THE ARGUMENT summarized by Propositions I to XI
in the preceding chapter must be complemented by a
further series of propositions, the purpose of which is to
establish the relation of these earlier propositions to the con-
struction of what may be characterized as a *moving system
of economic quantities.* The key to the solution of this
problem is provided by an adequate understanding of the
rôle played in such a construction by *equations representing
the impact of a stream of money expenditure against a rele-
vant stream of objects sold against such expenditure,* all of
which may be written in the general form $MV = PT$.

In this chapter, we shall be concerned with the relation
of "stream" equations, of the type indicated, to the concept
of a *system* of money prices. In Chapter Seven, we shall
be concerned with the relation of these "stream" equations
to the functioning of an economic *process* in time. In
Chapters Eight and Nine, we shall be concerned with the
conclusions that may be drawn, from the analytical struc-
ture outlined in Chapters Five to Seven, from the standpoint
of the light thrown by this analytical structure both upon
earlier work on the monetary aspects of a general system
of "economic dynamics," and upon the possibilities for fur-
ther work in the field.

XII. It is a mistake to reject expressions of the general
form $MV = PT$ as an element in an adequate synthesis of
the "general" Theory of Value with the Theory of Money
and Prices on the supposed ground that these expressions
can be concerned only with the determination of the "gen-
eral" price level, or the "value of money," whereas the "gen-
eral" Theory of Value is concerned only with particular

prices and their interrelation.[1]   For this contention ignores completely the possibilities inherent in the use of *"partial"* equations of the *general form* $MV = PT$ as a method of dealing with a "plurality of price levels." [2]

XIII. What this means, for our present purpose, is that the expression $MV = \Sigma D = \Sigma pq$ may *be broken down into as many ΣD's and Σpq's, and therefore as many MV's, as are necessary for the solution of a given problem.*[3]   The expression $MV = \Sigma D = \Sigma pq$ may be reduced, for example, to a series of expressions for the prices (p) of *each* of the commodities (q) included in the aggregate Σpq.[4]   Or it may be reduced to a series of expressions representing the aggregate money expenditure of each *individual,* of the form $\Sigma D_1 = \Sigma (pq)_1$, in which the subscripts would refer to expenditure by the *individuals* 1, 2, 3, ... n.[5]   Partial "money

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[1] For an example of a rejection of expressions of the general form $MV = PT$, on the ground indicated, see Hayek, *Prices and Production,* 4 ff.; and cf. also the reference to A. F. W. Plumptre given above, p. 103, n. 32.

[2] For the general argument involved, as well as for examples, from the earlier literature, of the use of equations of the general form $MV = PT$ in connection with the concept of a "plurality of price levels," see Volume I, 512 ff.

[3] It will be evident that the only respect in which the present argument differs from that presented in pp. 512 ff. of Volume I is that, by including the expression ΣD in the formulation $MV = \Sigma D = \Sigma pq$, it demonstrates the relevance of the earlier argument to the problem of the determination of the magnitude of the realized "demands" for particular *commodities,* in a sense applicable to the findings of the "general" Theory of Value as well as to those of the Theory of Money and Prices.

[4] It may again be observed that this was made quite clear by even so supposedly extreme an addict of the use of "averages" and "aggregates" as Irving Fisher. In reality, Fisher proposed to build up his "total transactions" equation by the use of a series of "arrays" in which the individual items were made to represent "the quantity of a *particular kind of goods purchased,"* and in which all of the first series of "Q and p arrays" were made to relate *"only to one commodity."* See *The Purchasing Power of Money,* 355, 358 (italics mine).

[5] Once more Fisher's usage may be said to have provided a model for the procedure here indicated. For he not only went so far as to propose the use of a series of subscripts, of the kind indicated in the text, to indicate the particular "individuals" involved, but also went on to write a series of expressions involving the use of the term ΣE_1 in what amounts essentially to the sense assigned to our ΣD_1. See *The Purchasing Power of Money,* 355 ff., 359. (On the relation of our D to Fisher's E, see also what is said above, p. 274, and in n. 115 thereto.) It may be observed also that the "development of the theory of the equation of exchange" presented by G. C. Evans in his *Mathematical Introduction to Economics,* 94 ff.,

equations," in other words, may be written in such a way as to correspond precisely to equations for realized individual prices or realized individual expenditure out of income or other money receipts, such as those which provide the main subject of study within the "general" Theory of Value, but which, as was shown by our Propositions VII to XI, require for their full explanation the supplementary type of analysis summarized by "money equations" of the type indicated.

I have already had occasion to deal in passing with certain objections, raised by commentators upon Volume I of the present work, to the argument for continued use of "stream" equations of the general *form* MV = PT in connection with the use of the concept of a "plurality" of price levels; I need not, therefore, repeat the refutation of these objections here.[6] In view, however, of the fact that our Proposition XIV, which follows immediately, is concerned with the relation of both "partial" and "total transactions" equations of the general Fisherine form to a "system" of equations of the Walrasian type, I venture to comment upon an objection which has been raised to the argument of Volume I on grounds that will certainly be familiar to all who have worked with "systems" of equations: namely, that, in this particular case, the proposed usage brings it about that "the number of unknowns is considerably enhanced without a corresponding increase in the number of equations."[7]

It is, indeed, difficult to discover the basis for such a charge, even when the discussion is kept entirely on the formal level to which so many writers have confined their arguments with respect to the relation of the number of equations to the number of unknowns. For one

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follows Fisher's example in building up the final equation from the consideration of "an economic system composed of *n* individuals," separate notation being given not only to the total expenditure of a given individual, but also to the "various commodities" against which this expenditure is directed. And it should be observed, finally, that in following this procedure within the Theory of Money and Prices, these writers were simply following the model set by the corresponding sectors of the "general" Theory of Value. See, for example, the "commodity equations," having to do with the amount of "each *commodity* bought and sold," and the "personal equations," having to do with the sum spent and received by "each *person*," in Bowley, *The Mathematical Groundwork of Economics,* 21.

[6] For an example of the type of objection in question, and the answer to it, see above, pp. 103 f., and nn. 32 and 33 thereto; and pp. 287 f., and n. 144 thereto, including the references to Volume I given in the footnotes indicated.

[7] So H. Neisser, in *Annals of the American Academy of Political and Social Science,* CCI (1939), 261.

must begin with the fact that an equation summarizing the forces determining the "value of money," of the general form $MV = PT$, was specifically included by Walras and Fisher in their respective "systems" of equations; and no one has thus far ventured to suggest that writers as expert as Walras and Fisher in the counting of equations and unknowns were guilty of an undercounting of the unknowns, or an overcounting of the equations, involved in the formal solution of their respective equational "systems." [8]   Nor has anyone ventured heretofore to suggest that Fisher, in particular, was guilty of formal error in this respect when he broke down his "total transactions" money equation into a series of separate money equations for the expenditure by individuals on individual commodities, in precisely the way proposed in the present work.[9]

In any case, moreover, if any degree of seriousness is to attach to the charge that the proposed procedure brings it about that "the number of unknowns is considerably enhanced without a corresponding increase in the number of equations," this charge must be translated from the purely formal statement which it represents as it stands, into an argument demonstrating that the proposed analytical system leaves a considerable sector of the relevant processes unexplained.[10]   Yet it is precisely this kind of argument that has not been provided.   The suggestion, for example, that the separate magnitudes (such as the relevant M's) included in the "partial" equations of the proposed system "would not be given under any circumstances" is not only completely unsupported, but can easily be shown to be either false or meaningless.[11] For if the M of a total transactions equation is "given," the magnitude of the separate M's of the "partial" equations is likewise "given," by the simple device of treating the magnitude of the particular price-quantity group (PT) involved in a particular "partial" equation as a component of a "composite demand for cash balances," in the sense in which the latter concept was used in Volume I of the present work.[12] The argument with respect to the "composite demand for cash balances" is one that involves matters of genuine substance.   If it is to be overthrown, it must be overthrown by an argument involving con-

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[8] In this connection, see above, p. 283, n. 132.

[9] See again the references to Fisher given above, p. 320, nn. 4 and 5, and also the reference given in n. 5 to G. C. Evans, who likewise can hardly be accused of unfamiliarity with the problems raised by a comparison of the number of unknowns with the number of equations involved.

[10] The argument under consideration here should be contrasted, in this respect, with the argument of Fisher concerning the necessity for including a "money equation" in a given system of equations designed to summarize the elements involved in the pricing process. See above, p. 283, n. 132.

[11] The statement quoted is from the review by Neisser cited above, p. 321, n. 7.

[12] See Volume I, 518, 521 ff., 584, 598.

siderations of equal substance, and not by an argument against the use of a "plurality" of "partial" money equations which is based solely on an alleged discrepancy between the number of equations and the number of unknowns, and which is neither of clear substantive meaning in itself, nor of clear validity from even a purely formal point of view.

XIV. The use of "partial" money equations (that is, equations representing realized money demands for particular commodities, or for particular groups of commodities) must, however, necessarily be supplemented by a *"total transactions equation"* (that is, an equation representing the sum of realized demands and of all other money payments made in a given period of time). The reasons for this can be stated, and indeed have been stated in the present work, in terms of a "competition" of individual sectors of the price- and income-structure for monetary "purchasing power." [13] *They can also be stated, however, in terms of the requirements of any theoretical apparatus which would do justice to the phenomenon of the general interdependence of economic variables.*[14] In either case, an understanding of the nature of the forces determining the *sum* of the terms in each member of a "total transactions equation" (an understanding which can itself be acquired only through a prior investigation of the nature of the forces determining

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[13] Cf. Volume I, 523. For the purposes of the present argument, which is concerned primarily with the relation of the dimensions of a given *stream* of money *expenditure* to the dimensions of the stream of total money *expenditure,* the statement of the problem in the terms suggested by Roos (cf. Volume I, 523, n. 106) is obviously more directly relevant than its statement in terms of a composite demand for cash balances; and indeed it was usually stated in terms similar to those of Roos by earlier writers (cf., for example, the references to Newcomb and Lubbock given in Volume I, 52, nn. 36–38). See, however, what is said on the relation between the two concepts in Volume I, 523, n. 106.

[14] In this connection, see the comments in Volume I, 532 ff., on the relation between "partial equilibrium" and "general equilibrium" analysis, on the one hand, and, on the other, the alleged "independence" of the individual "price levels" involved in a system of "plural" price levels. From the discussion presented in the fine print section below, however, it should be clear that the implications of this general argument become much more far-reaching when account is taken of the argument summarized by our Propositions I to VI (above, pp. 222–280). On the rôle of "stream" equations in establishing the precise nature of the interdependence of economic variables *in time,* see below, Propositions XIX–XXII (pp. 351–403), and also what is said below, pp. 416 ff.

the magnitude of the individual terms in each member of such an equation) requires the full use of the substance of that part of the Theory of Money and Prices whose task it is to explain why the *aggregate of money expenditure, on the one hand, and the aggregate of objects sold against this expenditure, on the other, are as large as they are.* From these propositions, it follows that a "total transactions" equation of the general form $MV = PT$, instead of being a formulation which is inconsistent with, or retrograde in comparison with, price analysis of the "general interdependence" or "system" type, *represents at once a necessary part of such analysis and a summary of the whole system of money- and commodity-flows which, in a fully developed money economy, must constitute the very subject-matter of a "general" theory of pricing.*

It is easy to demonstrate that an adequate appreciation of the implications of Proposition XIV, when the latter is interpreted in the light of the propositions presented earlier, as well as of those presented below, would have made impossible a series of misunderstandings which in the past have militated against the construction of a genuinely satisfactory "synthesis" of the "general" Theory of Value, on the one hand, and the Theory of Money and Prices, on the other. Specifically:

1. An adequate appreciation of these implications would have removed objections such as (i) that "the equilibrium picture in which *all* goods are involved supplies no data from which to construct any of the magnitudes above or below the margin of the demand and supply curves of any given good"; or (ii) that any analytical scheme (such as that of Schumpeter) which is based directly upon the Walrasian conceptions of (*a*) the "circular flow" of money payments, and (*b*) the concept of a general interdependence of economic magnitudes, is "quite distinct" from "supply and demand analysis" (of the particular demand-schedule and particular supply-schedule type), for at least one supposedly decisive reason: namely, that in the first type of analytical scheme "the individual is the center of interest, and his reactions *toward all kinds of goods* is emphasized; whereas in supply and demand analysis, the *good*—one good—is the center of interest, and the price-offers streaming toward it from all kinds of individuals is emphasized." [15]

For, in the first place, the answer to the point made under (i), with respect to the "magnitudes above or below the margin of the demand and supply curves of any one good," is given by the argument of our Proposition VI, with respect to the relation of the concept of a "realized" demand for a particular commodity to the *ex ante* demand schedule

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[15] The quotations are from Anderson, *The Value of Money,* 95 (italics in the original).

for that commodity.[16] Similarly, the answer to the question raised, under both (i) and (ii), with respect to the relation of the demand schedule for one good, on the one hand, and the reactions of individuals "toward all kinds of goods," on the other, is given by the argument presented in Chapter Four with respect to the relation of "general" equilibrium analysis to the conformation of these "particular" demand schedules.[17] What matters most for our present purpose, however, is that it is precisely a system of "stream" equations representing the "realization" of specific prices which makes it possible to deal *either* with (1) an individual's realized demand for one commodity or for "all kinds of commodities," on the one hand; or with (2) the realized demands of several individuals for the "one good" which is taken for particular examination, on the other.[18] The types of analysis involved, from both monetary theory and "general" value theory, are, to be sure, "quite distinct" in the sense that they deal with "distinct" phases of the general theory of the determination of money prices.[19] They are not "distinct," however, in the sense that any one of them can be omitted from a theory of the determination of money prices which pretends to completeness. And the claim made on behalf of the apparatus here outlined is that all of these aspects are not only represented in it, but are explicitly related to each other through the use of the central co-ordinating device represented by "stream" equations of the general form $MV = \Sigma D = (PT)$.

2. The present work is not the first in which it has been argued that an aggregative "stream" equation representing the direction of money expenditure against a mass of objects sold against such expenditure may be regarded as "only a condensed expression of the system of equations for the formation of prices." [20] Nor is it the first that has argued that every system of "partial" analysis (whether it is of the kind found within the "general" Theory of Value or is of the kind represented by the "partial" stream equations of the Theory of Money and Prices) must be supplemented by an apparatus in which specific place has been given to what have been called *"total* categories," and by a realization of the importance of *monetary* theory, in particular, for an understanding of the causes and consequences of changes in these *"total* categories." [21] Unfortunately, however, these instances of a recogni-

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[16] See above, pp. 274 ff.

[17] See above, pp. 166 ff.

[18] See above, p. 320, and the references given in nn. 4 and 5 thereto.

[19] Cf. the comments of Anderson, *The Value of Money,* 95.

[20] Cf., for example, Myrdal, *Monetary Equilibrium,* 22, where the statement quoted in the text is applied to Lindahl's equation $E\ (1 - s) = PQ$. On Lindahl's equation itself, see Volume I, 328, n. 78, of the present work.

[21] See, for example, Lundberg, *Studies in the Theory of Economic Expansion,* 23 f. Cf. also Lundberg's remarks, on pp. 4 f. of the work cited, concerning the relation between the type of "equilibrium" analysis, on the one hand, which "explains price and production of a single commodity under different market conditions, technical circumstances, etc.," and what

tion of the rôle played by "stream" equations, in particular, in providing the required "ligamen" between the individual prices in any *system* of money prices have often been accompanied by arguments the effect of which is to make the final formulation not only unnecessarily exclusive, but also actually incomplete.[22]

This is true, for example, of characterizations of an aggregative "stream equation" as representing "only a condensed expression of the system of equations for the formation of prices," whenever such characterizations are applied to "stream" equations of the "income" or "consumers' goods" type.[23]   For, as has been pointed out by critics of the exclusive use of an aggregative "stream" equation of the "final goods" type, such a procedure inevitably "tends to obscure the different behavior of different sections of the price system," since many of these "sections" are necessarily excluded from an equation representing

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he calls the *"total* adaptation process," on the other (italics mine). It should be observed, however, that Lundberg himself makes only one reference (p. 7) which can be regarded as referring even obliquely to the kind of "partial analysis" which makes use of "partial" stream equations of the general form $(MV) = D = pq$. Cf. also below, p. 327, n. 25; and on the rôle of "stream" equations in *"process* analysis," including the analysis of "adaptation" processes, see Chapter Seven, immediately following.

[22] The "equation of exchange" is described as "the monetary ligamen" by Schumpeter in his *Business Cycles*, 44. It must be added, to be sure, that any characterization of the "equation of exchange" as "a *condition of equilibrium*" (cf. Schumpeter, *loc. cit.*) must be interpreted in the light of the argument presented in Volume I of the present work (pp. 73 ff.) with respect to the validity and usefulness of the familiar "quantity equations" in the analysis of other than equilibrium "conditions." There can be no objection if, on the other hand, the characterization of the Fisher equation, for example, as a "condition" of equilibrium is meant to imply no more than that this equation, or its equivalent, plays a crucial rôle in the description of the functioning of that *"system"* of prices which constitutes the subject matter of theories of "general" economic "equilibrium." On the arguments for preferring, in such cases, to work with the concept of "general economic *interdependence"* rather than "general economic *equilibrium,"* see below, pp. 407 f., 412 ff.

[23] This is true, for example, of the statement of Myrdal cited above, p. 325, n. 20.   In justice to Professor Myrdal, it should be pointed out that his own detailed analysis makes implicit use of a *"plurality* of price levels."   See, for example, Volume I, 497, and the reference to Myrdal given in n. 29 thereto (the passage in question appears on p. 27 of Myrdal's *Monetary Equilibrium*).   It is clear, therefore, that we have here another instance in which what seems at first glance to be an argument for an apparatus designed to deal solely with the determination of the prices of *consumers'* goods is in reality an argument for an apparatus capable of dealing with a *"plurality* of price levels" (cf. Volume I, 495 ff., of the present work).   Yet it must also be clear that this very fact makes the description of a *consumers' goods* equation as "a condensed expression of the system of equations for the formation of prices" necessarily misleading as to the coverage assigned to such a "system."

only "final" transactions in "final" goods.[24]   And the same kind of un-
fortunate exclusivism can be charged against sponsors of analysis in
terms of "total categories" and in terms of the relation of the magni-
tude of these "total categories" to the functioning of the monetary
system, whenever these sponsors have shown themselves unsympathetic
to the use of "stream" equations of the general form $MV = PT$.[25]   For
if the argument of the present work is sound, it is *only* by the use of
such equations, and of the detailed analysis summarized by the specific
variables included therein, that we are able (1) to determine why the
aggregate of money expenditure is as large as it is, and therefore why
any component of this aggregate money expenditure is as large as it is;
and (2) to trace the processes by which the events recorded by changes
in the magnitude of these "total categories" are realized in the world
we know.

3. Even from the argument as presented thus far, it should be clear
that there is no basis whatever for the suggestion that only an inade-
quate "appreciation of Walras's *general* theory of money" could permit
a desire to emphasize the importance of those contributions by Walras
to monetary theory which are summed up by his use of "money equa-
tions" of the "Fisherine" and the "cash balance" types, respectively.[26]

For if, by "Walras's general theory of money," is meant the monetary
aspects of his general analytical system, and particularly his treatment
of the phenomenon of the general interdependence of economic variables

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[24] Cf. Saulnier, *Contemporary Monetary Theory*, 168.   From the context,
the criticism in question would seem to be directed quite as much against
a failure to break down the "price level of final goods" into significant
price groups as it is against an exclusive concern with "the price level
of final goods."   From our Proposition XIII (p. 320), however, it should
be clear that the use of "partial" stream equations is consistent with
as detailed a breakdown as is desired. The purpose of Proposition XIV
(p. 323), on the other hand, is to emphasize the point that an adequate
"reformulation of theoretical techniques in the interests of a more faithful
representation, and a more adequate handling, of realities" (Saulnier, p.
168) demands not only a *breakdown* of our stream equations into equa-
tions with a narrower coverage, but also the use of an inclusive *system*
of such equations of sufficient breadth of coverage to deal adequately
with all sectors of the pricing process.   On the general point involved,
see again what is said above, pp. 287 ff.

[25] Such a lack of sympathy must be imputed, for example, to Lundberg,
whose own "summation" of the equations relating to the "partial fields"
(*Studies*, 25) makes no reference to the type of "summation" represented
by a "total transactions" equation of the money-"stream" ($MV = PT$)
type, just as his discussion of the "partial fields" themselves makes no refer-
ence to "partial" equations of the money-"stream" type (cf. above, p. 326,
n. 21).   See also what is said below, p. 341, and the reference to Lundberg
given in n. 59 thereto.

[26] Cf. the comment by G. Del Vecchio, in the *Giornale degli economisti*,
LXII (1932), 99, on my "Léon Walras and the 'Cash Balance Approach'
to the Problem of the Value of Money."

in terms of a system of money- and commodity-flows, the only answer
can be that it is precisely an explicit extension of the relevant aspects
of the general Walrasian technique that is provided by the analytical
"system" here presented, with its emphasis upon the rôle played by
the "money equation" (and the possible breakdown of that equation
into a series of "partial" equations) in providing the necessary monetary
"ligamina" of a system such as that of Walras.[27]   And it should be
equally clear that the effect of emphasizing the possibilities, inherent
in "Fisher's equation," for a further development of our understanding
of the nature of the forces determining money prices, instead of repre-
senting a step backward "after the great Walrasian attempt," is simply
to stress the possibilities for further analytical construction inherent
in this "attempt." [28]   For it must never be forgotten that in his treat-
ment of the rôle of the "money equation," Fisher was merely repro-
ducing an argument which had been presented *in nuce* in the "great
Walrasian attempt" of some forty years before.[29]

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[27] It is, indeed, something of a commentary on the extent to which the
Walrasian system has been misrepresented that we should have had to
wait until comparatively recent years for a demonstration that the Wal-
rasian system is in fact a "system of money- and commodity-flows," and
was *not* a "system" based upon "barter assumptions." (In this connection,
see the references to my article, "The Monetary Aspects of the Walrasian
System," given above p. 70, n. 44.) The same thing must be said of
the demonstration that the Walrasian analytical structure is useful for
purposes other than the "simultaneous" determination of the whole system
of prices at a given point of time. Of the writers of our own day, it is
Schumpeter who deserves most credit for having shown a proper ap-
preciation of these two aspects of the Walrasian "system," and particularly
the second of the two. On this matter, cf. what is said above, pp. 111 ff.,
concerning Schumpeter's treatment of the Walrasian "circular flow." On
the nature of the "extensions" to the Walrasian system which are pro-
posed in the present work, and which have to do particularly with its
further adaptation for tracing processes realized in *time,* see below,
Proposition XXI (pp. 365 ff.), as well as what is said above, p. 71, n. 48.

[28] The phrases quoted are from the review by G. Del Vecchio of Vol-
ume I of the present work in the *Giornale degli economisti* for October,
1938, 812. See also the following note.

[29] In view of the fundamental identity of purpose and results obtained
by the two writers concerning the "rôle of the 'money equation' " in the
analysis of a "system" of money prices, it should be clear that the dif-
ferences between them with respect to the *form* of the "money equation"
(differences which, it may be observed, are no greater than the differences
between the Walras of the first edition of the *Eléments* and the Walras
of the later editions) are of little significance for our present purpose,
particularly in the light of a correct understanding of the *relation* be-
tween the Fisherine V and the Marshallian K (cf. Volume I of the
present work, 416 ff.). The question whether, in the light of this funda-
mental identity of purposes and results, Fisher's work is in fact to be
regarded as a "step forward" or a step backward as compared with the
work of Walras (cf. Del Vecchio, *loc. cit.*) can be answered best by ask-

Precisely the same thing must be said of a further suggestion: namely, that a concern with the meaning of the familiar "money equations," and the mass of detailed analysis which these equations must be held to summarize, involves "ignoring" the problem of integrating these equations into "the theory of general economic equilibrium." [30]   To contend, for example, that such a concern implies no interest whatever in "equations of exchange [in the sense in which that expression is used within the "general" Theory of Value], equations of production, and equations of capitalization," and therefore in the "laws of price-formation for products, services, and the elements of income," is to show a complete failure to accept the implications of the whole series of Propositions advanced thus far.[31] If these implications are to be rejected, they are to be rejected upon the basis of a detailed argument designed to show just where and why the argument breaks down, and not upon the basis of a stereotyped repetition of propositions which can be shown to be belied either by the history of doctrine or by a simple comparison of the heuristic properties of the rejected propositions, on the one hand, and the supposedly superior alternatives to these propositions, on the other.[32]

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ing a counter-question: namely, whether, if it had not been for Fisher's emphatic insistence upon the importance of the Quantity Equations as a weapon useful in monetary analysis, anyone would have even noticed that Walras himself had presented an equation of this type in the first edition of his *Eléments*. It can hardly be denied, that is to say, that Fisher's work did represent a "step forward" from Walras's position *as that position was understood* (or misunderstood) until very recent years.

[30] Cf. G. Demaria, "La teoria dei prezzi," *loc. cit.*, 285.

[31] The quotations are from Demaria, "La teoria dei prezzi," *loc. cit.*, 286. It may be added that while some excuse might be found for a failure to see the relation between a concern with the familiar "money equations," on the one hand, and some of the problems indicated in the passage quoted, on the other, it is extremely difficult either to explain or excuse such a failure when the special problem of "capitalization" is involved. On this matter, see especially Volume I, 232 ff.

[32] For an example of the type of "stereotyped repetition" to which reference is made in the text, I may cite the suggestion that "all quantity equations . . . are mere identities without theoretical significance" (Demaria, "La teoria dei prezzi," *loc. cit.*, 285). Professor Demaria regards this proposition as an "obvious truth," which, he insists, is not only unshaken by the argument of Volume I of the present work but was not even "taken into account" therein. The reader of Volume I must be allowed to pass his own judgment upon the merits of this contention; although I may add that I myself am content to refer such a reader to the comments by L. Einaudi upon this aspect, as well as upon other aspects, of the argument of Volume I discussed by Professor Demaria, in Professor Einaudi's article, "Della moneta 'serbatoio di valori' e di altri problemi monetari," *Rivista di storia economica*, IV (1939), especially pp. 137 ff. Much the same thing, I fear, must be said of the indications provided by Professor Demaria with respect to the devices which he regards as of greater heuristic significance for the Theory of Prices than those presented

XV. From Propositions XII and XIII it follows that a "total transactions equation," while the necessity for its use is demonstrated by our Proposition XIV, need by no means take the form of an equation leading to the determination of a "general" price level. As far as our argument has gone, a "total transactions equation" may take the form of an expression which includes, in its second member, only a series of individual money prices or a series of groups of such prices.[33] Yet it would be unjust to those who have defended both the concept of a "general price level" and a "total transactions" equation of the general form $MV = PT$ if no attempt were made to demonstrate that their argument can be stated in such a way as to make clear its relevance for the problem of constructing an adequate synthesis of the "general" Theory of Value with the Theory of Money and Prices. Specifically:

i. When, for the misleading expression "general level of prices," we substitute the older expression "the *scale* of prices," it is seen that in many cases no more was meant by the users of the former expression than is meant by our Proposition VII: namely, that a special "money equation" is needed to explain why the *absolute* "scale of prices" is as high as it is.[34]   This problem is a real, not a factitious problem; *the need for its solution, therefore, is in no wise less-*

in Volume I of this work. "For example," he writes (p. 286), "there is not a word [in my Volume I] about the marginal utility of money." For evidence as to the accuracy of this statement, see the references to Volume I given above, p. 60, n. 22; and cf. also Einaudi, "Della moneta 'serbatoio di valori,' etc.," *loc. cit.,* 146 ff. The reader himself, however, must be allowed to judge the relative heuristic value, when judged as devices for integrating the substance of monetary theory into "the theory of general economic equilibrium," of the concept of a "marginal utility of money," on the one hand, and the series of Propositions presented here, on the other.

[33] It is worth observing that the second member of the "money equations" of Walras and of Schumpeter, respectively, took the form precisely of a series of individual money prices. In this connection, see my "Léon Walras and the 'Cash Balance Approach,'" *loc. cit.,* 576 f., and the references to Walras's *Éléments* there given, as well as the reference to Schumpeter's "Das Sozialprodukt und die Rechenpfennige" given above, pp. 117 f., n. 67. Cf. also Fisher's *Purchasing Power of Money,* 25 f., and the references to the same work given above, p. 320, n. 4.

[34] See above, pp. 280 ff.; and cf. the argument of Wicksell, in Chap. Three of his *Interest and Prices,* with respect to the relation between the theory of "the determination of the *average* price level" (p. 18), or, as he

*ened by the fact that movements in the "*SCALE*" of prices may be accompanied by movements in the* STRUCTURE *of prices.*

That (1) a concern with the explanation of a change in the "scale" of prices (or a "general" change in prices) is in no sense inconsistent with a concern with the explanation of changes in the "structure" of prices; and that (2) the solution of one of these problems does *not* automatically provide a solution of the other, has been recognized by economists for generations.[35] No one, for example, could have been more explicit than J. E. Cairnes in insisting upon tracing "the disturbance effected in the *relation* of prices" during the course of a monetary expansion; yet he was certainly not prepared to deny that we must also be ready to describe both the causes and the consequences of the fact that "prices *generally*" may rise, even if they rise "with unequal steps." [36]   There is just as little reason, therefore, for denying a similar proposition advanced by Irving Fisher: namely, that while "it is evident . . . that prices must constantly change *relatively to each other*, whatever happens to their general level"; and while "it would be as idle to expect a uniform movement in prices as to expect a uniform movement for all bees in a swarm"; nevertheless "it would be as idle to deny the existence of a *general* movement of prices because they do not all move alike, as to deny a general movement of a swarm of bees because the individual bees have different movements." [37]   It is *this* contention that must be denied by all those who would object to the very concept of a *"general* movement of prices"; yet there is no

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sometimes called it, "the absolute level of money prices" (p. 23), on the one hand, and the theory of the determination of "relative prices," on the other; also the argument of Schumpeter in his *Business Cycles,* 452 f. For examples of a use of the expression "the *scale* of prices" in earlier economic literature, see above, p. 107, n. 42; and see also the reference to M. A. Heilperin given below, p. 333, n. 42.

[35] This follows, indeed, from our demonstration that discussion of the forces causing changes in the *structure* of money prices has from the very beginning been introduced into discussions of processes (such as those associated with monetary expansion and contraction) which would be expected also to affect the absolute *scale* of money prices. In addition to the references given in the notes immediately following, see the references given above, pp. 275 ff., to Bodin, Locke, and later writers; and cf. the argument of Schumpeter, *Business Cycles,* 454, in support of his proposition that "the price of any individual commodity, as we observe it at any point of time, must be interpreted as the result of two distinct components: the price level and the price system": so that we must say that "the two components of change, however inextricably mixed, are logically distinct" (see also the continued references to *both* the "price *system*" (or price "structure") and the "price *level*" on pp. 3, 27, 137, 451, 732, of the same work).

[36] See Cairnes's *Essays in Political Economy,* 56 f.; and cf. what is said above, p. 108, n. 42, on this aspect of Cairnes's argument.

[37] See Fisher, *The Purchasing Power of Money,* 193 f. The italics are Fisher's.

evidence that a denial of this contention has been contemplated by even the most extreme critics of the concept of a "general" price level.

When, for example, Mr. Keynes, in his *Treatise*, compared the "influence of monetary changes in price-levels" to "the effect of moving a kaleidoscope on the coloured pieces of glass within," he cannot have meant to affirm that a concern with changes in the position of the pieces of glass *within* the kaleidoscope was inconsistent with a concern with the movements of the *kaleidoscope itself*.[38] And indeed the best proof that it is impossible to evade the concept of a *"general* movement of prices" is that the most pertinacious critics of the concept of a "general" price level have been impelled to re-introduce, in one way or another, some analytical equivalent of the concept of a movement in the "general" level of prices.[39] In the case of Keynes's *Treatise*, for example, despite its emphasis upon the necessity for working with a "plurality of price levels" and for distinguishing between movements in the "price level" of consumers' goods, on the one hand, and the "price level" of "investment goods," on the other, it was found desirable to introduce the concept of a "price level of output as a whole." [40] Similarly, despite Professor Hayek's sharp attacks upon the usefulness of concepts, such as that of a "general" price level, which are alleged

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[38] For the passage quoted, see the *Treatise*, I, 92.

[39] The examples which follow are taken from writers who have regarded themselves, or have been regarded by others, as outspoken opponents of the very use of the concept of a "general" price level. It would be very easy, of course, to provide examples of writers who have *not* suggested that their emphasis upon the importance of tracing changes in the *structure* of money prices warrants the abandonment of the very concept of a "general" price level. In addition, for example, to the references to Schumpeter given above, p. 331, n. 35, see F. A. Fetter, "Interest Theory and Price Movements," *American Economic Review*, XVII (1927), Supplement, 86 ff., where, despite the author's insistence (p. 87) that it is "evident that during . . . a process of price change the whole scheme of relative prices would be disarranged," the discussion continued to be concerned with the "problem of general price changes," or, as they were sometimes called, "changes in the general price level," or "changing general prices."

[40] It need hardly be emphasized that this comment does not imply full approval of Mr. Keynes's treatment of the *relation* between the "price level" of consumers' goods, on the one hand, and either the "price level of investment goods" or the "price level of output as a whole," on the other. In this connection, see Volume I, 264, 530 ff., of the present work. The point is merely that Mr. Keynes did find it necessary to introduce a concept, such as that of the "price level of output *as a whole,"* which was used in connection with a number of problems of precisely the type discussed by others in terms of a change in the "general price level." Such a problem, for example, is that of the difference between a change in relative prices, on the one hand, and a change in general prices, on the other. In this connection, see Volume I, 263 f., and the references to Keynes's *Treatise* there given. On Keynes's treatment of the concept of a "general price level" in his *General Theory*, see above, pp. 155 ff.

to imply an attempt "to establish causal relations between aggregates or general averages," he has found it necessary to speak, for example, of both the fact and the consequences of a "general fall of prices." [41] It is clear, therefore, that the concept of "general" movements in prices is by no means the pure figment of our imaginations that it has some- times been represented as being. And from this fact it follows that while much is to be said for the use of expressions such as "movements in the scale of prices," or in "the scale of magnitude of money values," in place of "movements in the general price level," it cannot be argued that those who have in the past made use of the concept of "general" movements in prices have in all cases been guilty of a practice which is either meaningless or necessarily vicious.[42]

So clear is this conclusion, indeed, that the only matters calling for comment are certain arguments or modes of expression associated in the past with the concept of "general" movements in prices, which can hardly be said to have helped to clarify the nature of the issues involved. Specifically:

1. The measurement of "general" changes in prices. There is no logical reason why a picture of changes in the height of a given "swarm" of prices could not be obtained by simply plotting the indi- vidual prices in such a "swarm," and then generalizing concerning the movements of the "swarm" on the basis of the picture of the movement of individual prices thus obtained. Some such procedure, in fact, may be said to have been either implied or explicitly followed both by "classical" writers such as Ricardo, on the one hand, and by opponents of certain "classical" monetary doctrines, such as Tooke.[43] From the

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[41] In this connection, see the comment on Hayek's argument by Hawtrey, in the latter's Capital and Employment, 256. For Hayek's attack upon the usefulness of the concept of a "general level of prices," see pp. 5 ff., 88, of Prices and Production, and especially p. 25, where the author ventures the opinion "that, in the near future, monetary theory will . . . even throw overboard the concept of a general price level." It would be very easy to cite further instances in which writers who have begun with brave denunciations of the very concept of "general" price movements have ended by introducing the equivalent of that concept at a later stage in their argument. Contrast, for example, the sweeping statement of J. Pedersen and O. S. Petersen, An Analysis of Price Behaviour During the Period 1855–1913 (Copenhagen and London, 1938), p. 8, on the entire lack of "significance" of "a general index of prices—however weighted," with (1) their concession, on p. 123 of the same work, that there may "pos- sibly" be "an element which has affected all prices, and which may be designated the general price movement"; and with (2) their discussion, on pp. 232 f., of what they call "the general price level."

[42] For an example of a use of the expression, "the scale of magnitude of money values," in a context which would suggest that the concept is different in substance from that of "the 'price level,'" see M. A. Heilperin, International Monetary Economics, 43, and n. 2 thereto.

[43] On the usage of "the classical writers" with respect to the concept of "the level of prices," see Viner, Studies in International Trade, 314,

days of Jevons, however, workers in the field of price statistics have felt it necessary to provide a *single figure* which would represent the movements in the "swarm." [44] It should hardly be necessary to labor the point that such a procedure can be justified only insofar as (i) the standard rules of statistical significance are applied in the interpretation of the meaning of the "average" which such a single figure necessarily represents; and (ii) we carefully refrain from reading more into this single figure than is justified by an "operational" view of the processes employed. [45]

With respect to (i), for example, it should be clear that the justification for the use of an "average" of price movements to summarize a "general" movement of prices becomes weaker in the degree to which it can be shown that very few, if any, prices actually moved in the way indicated by the "average." The methodological difficulty involved is, of course, analogous to that raised whenever an arithmetic average of items forming a U-shaped distribution is presented as "typical" of the items in that distribution. In those cases in which inspection of the individual arrays shows, for example, that all individual "prices," instead of remaining stable, fluctuated in opposite and compensating directions, the statement that prices "generally" remained stable over the period is at best extremely misleading and at worst actually false. In all such cases, on the contrary, it would be much better to give up, as even sponsors of the concept of a "general price level" have suggested, any attempt to represent the "general" movement in prices by a single figure. [46] In those cases, on the other hand, in which inspec-

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379 f.; and on Tooke's attitude toward the use of "average prices," in general, and of index numbers, in particular, see Gregory's Introduction to Tooke and Newmarch, *History of Prices,* 14 f., 42.

[44] On Jevons's predecessors in the use of index numbers, see the summary given by C. M. Walsh, "Index Numbers," in *Encyclopaedia of the Social Sciences,* VII, 652. Yet it is impossible to deny either that it was Jevons who really "opened the *theory* of the subject" (Walsh, *op. cit.,* 653) or that Jevons was "the first economist effectively to introduce index-numbers of prices into *Monetary Science*" (Keynes, *Treatise,* I, 80; italics mine). As far as the present argument is concerned, moreover, it should be pointed out, in justice to Jevons, that while he certainly wished to obtain a "single figure" for "the general variation of price of *all* commodities," he was perfectly prepared to *accompany* this "single figure" with figures designed to indicate "the variations peculiar to each commodity or partial group" (*Investigations,* 120, 136 ff., and Plate VII [facing p. 142]).

[45] The term "operational" is here used in the specific sense assigned to it by P. W. Bridgman. See, for example, his *The Logic of Modern Physics,* 5 f.

[46] See, for example, the comments of Hawtrey, in his *The Art of Central Banking,* 279; and contrast the argument of Fisher which is summarized by the title of Chap. Nine of his *The Purchasing Power of Money:* "The Dispersion of Prices Makes Necessary an Index of Purchasing Power." In partial defense of Fisher, however, and in addition to what is said

tion shows that in fact a large number of prices did move in a way which is not grossly misrepresented by the single figure for an "average" movement in prices, there can be no *a priori* objection to the use of this "average" figure, particularly if care is taken subsequently to compare the movements of the individual prices in which we are interested with the movement in the "average." [47]   It must be clear, in any case, that the objections that have been raised to certain uses (or abuses) of index numbers as devices for *measuring* a "general" movement in prices certainly do not apply to the concept of a "general" movement in prices itself.   For, as we have seen, it is possible to speak of a "general" movement of prices simply upon the basis of inspection of arrays of individual prices that remain uncombined into a single index figure.

With respect to (ii), on the other hand, it should always have been clear that the figure obtained by averaging the movements of different prices can never be regarded as meaning more than that it is a figure which was obtained by the "operations" indicated.   We are not warranted in assigning any "reality" to the movements in this figure over and above the "reality" which is represented by the individual price movements thus combined in the average.[48]   One can only agree, there-

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above, p. 109, with respect to his emphatic recognition of the *fact* of a "dispersion of prices," it may be pointed out here that he was perfectly prepared, elsewhere in his *Purchasing Power of Money*, to consider the use of a *plurality* of index numbers, the use of each being held to depend "on the *purpose* of the index number" (see pp. 205 ff.).   Yet there can be little doubt that the argument of Chap. Nine of that work suffers from a failure to deal adequately with the objection stated in the text.

[47] It will be recalled that Jevons did undertake just such a subsequent comparison, which he summed up under the head of "comparative variations" in prices.   See the references to Jevons given above, p. 334, n. 44. Similar procedures are of course by now a commonplace in statistical practice.   Cf. also, in this connection, the comment by Schumpeter, *Business Cycles*, 204 f.

[48] In this connection, cf. the comments on the "existence," in the "real economic world, of such a thing as a 'general price level,'" in Heilperin, *International Monetary Economics*, 266, 268.   One must beware here, however, of multiplying logomachies.   For example, the statement in the text may seem, at first glance, to be in direct contrast to the statement of Professor Schumpeter that "the price level . . . is not a mere statistical aggregate or a mean like the average height of recruits of a given age in a given population, but a real thing existing independently of the statistician" (*Business Cycles*, 453; cf. also pp. 166 n., 475, 484, of the same work). But while I cannot help believing that a statement such as that just quoted is very likely to lead to misunderstanding, I may point out that a genuine difference of opinion would exist only if Professor Schumpeter had said that the price level is "a real thing existing independently of *individual price movements*" (cf. the text, above).   For it seems clear, from passages such as those cited from Professor Schumpeter above, p. 331, n. 35, that his argument amounts only to the contention that movements in the *absolute* "scale" of prices are "real" phenomena, calling as much for

fore, with those critics, like the Keynes of the *Treatise,* who have objected to the concept of an "objective mean variation of general prices" insofar as such a concept implies the existence of something "objective" apart from (1) the individual prices themselves and (2) the fact that these individual prices are capable of being combined by certain processes of averaging.[49] It should be equally obvious that one can only agree even more emphatically with the objections that have been raised to a related type of argument: namely, the argument of those who may have implied that a process of averaging, of the type indicated, in itself gives us a "real" measure of the extent to which "monetary" influences, on the one hand, and "non-monetary" influences, on the other, have affected the movements of particular prices.[50] Yet again there is nothing in all this which argues against either the concept of a "general" movement in prices or the use of index numbers to measure this general movement, when all that is meant thereby is that, quite apart from any movements that may have taken place *within* the "swarm" of prices, the whole swarm may be said to have moved upward or downward.

2. *Individual prices "versus" the price level.* Perhaps no single proposition has raised more obstacles to acceptance of the concept of movements in the "general price level" than that typified by the statements made by Fisher in support of his contention that "it will not help, but only hinder the reader to mix with the discussion of price *levels* the principles determining individual prices relatively to each other." [51] Of these statements, the most important, for our present purpose, are statements such as (1) that "price *levels* must be studied independently of individual *prices*"; (2) that since "the general level of prices" is "one of the bases" of the price of sugar, say, and since "the price level is not determined by individual prices, but . . . , on the contrary, any individual price presupposes a price level," it follows that "we have more need to study the price level preparatory to a study of the price of sugar than to study the price of sugar preparatory to a study of the price level"; and therefore (3) that "much con-

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explanation on their own account as do changes in the internal structure of what he characterizes as the "price *system."*

[49] See Keynes's *Treatise,* I, 79 ff., and especially pp. 85 ff.

[50] The contention in question has, of course, often been associated with the contention discussed in the preceding sentence of the text. See, for example, the references to other writers given by Keynes in the passages of his *Treatise* cited in the preceding note. It should be added, however, that the acceptance of a distinction between the "monetary" causes of changes in particular prices and the "non-monetary" causes of such changes has by no means always involved the particular thesis with respect to the interpretation of index numbers attacked by Keynes in his *Treatise.* Cf., for example, the well-known comments by Menger on this point in his "Geld" (*The Collected Works of Carl Menger,* IV), 89 ff., and by Mises in his *Theory of Money and Credit,* 188 ff.

[51] *The Purchasing Power of Money,* 175.

fusion will be escaped if we give up any attempt to reason directly from individual prices," and hold fast to "the fundamental distinction between those influences affecting the general price level and those affecting the rise and fall of a particular price with respect to that level." [52]

It can hardly be gainsaid that propositions of this type are particularly likely to lead to a misunderstanding of both the meaning of the concept of a "general price level" and its rôle in the general theory of the determination of money prices. As we have seen, the slightest respect for the "operational" aspects of the problems indicated reveals that any measure of the movement in the general "price level" is *derived* from data with respect to movements in "individual prices." From this point of view, the "general price level" may in fact be said to be "determined" by individual prices. From this point of view, also, it can *not* be said that there is a "fundamental distinction between those influences affecting the price level and those affecting the rise and those affecting the rise and fall of a particular price." And from this point of view, finally, it can *not* be argued that "price *levels* must be studied independently of individual prices." For in all of these cases we run against the hard "operational" fact that we cannot derive our knowledge of movements in something called the "general" price level from anything but our knowledge with respect to particular prices. No conclusion is possible, therefore, other than that the "general price level" *is* influenced by factors affecting particular prices.

Yet nothing is capable of easier demonstration than a further proposition: namely, that if a writer such as Fisher is to be criticized for having advanced propositions of the type indicated, he is to be criticized only on grounds of exposition, and not on grounds of substance. For if anything is clear from Fisher's argument, it is that his main contention amounted to no more than that advanced above: namely, that a special body of analysis (summarized, in this case, by the variables of the equation $MV = PT$) is necessary if we are to understand why the *absolute* "scale" of prices is as high as it is. This much was argued by none other than Wicksell, who has so often been given credit for having departed from the supposedly traditional practice of regarding it "as self-evident that . . . a change in the general price level must be due to entirely different circumstances from a change in individual prices." [53] Thus, there is, to be sure, not the slightest reason for denying that changes in the general price level occur "because" of changes in individual prices. But one has only to study the details of the argument of either Wicksell or Fisher with respect to the nature of the forces *making the general "scale" of these individual prices what it is,* to realize that their argument on behalf of the retention of the

[52] *The Purchasing Power of Money,* 175, 177, 179 ff.

[53] See the comment by Ohlin in his Introduction to Wicksell's *Interest and Prices,* p. xiii; and cf. the passage italicized by Wicksell himself on p. 23 of *Interest and Prices.*

concept of movements in a "general" price level amounts to nothing
more than this: that any adequate theory of the determination of
money prices must be prepared to deal directly not only with the prob-
lem of the *relation* between individual money prices, but also with the
nature of the forces which make the absolute *scale* of these individual
prices what it is at any given time.

3. *The "multiplicative factor" and the structure of money prices.*
The last of the obstacles to a general acceptance of the argument sum-
marized by our Proposition XV, 1, is directly traceable to the unfor-
tunate exposition of one writer—namely, Gustav Cassel—whose argu-
ment otherwise could have been regarded as merely a restatement of
the contention that no theory of the determination of money prices can
be regarded as complete if it fails to deal with the nature of the forces
determining the *absolute* "scale" of prices.[54]   As Cassel stated the argu-
ment, any "equation-system" constructed upon the basis of a pre-
liminary abstraction from the effects of changes in the quantity of
money must be regarded as "indeterminate in the sense that it deter-
mines . . . prices only up to [the introduction of] a multiplicative fac-
tor," the determination of whose magnitude it is for *monetary* theory
to explain.[55]   This, he suggested, amounted only to a restatement of
the proposition that an "equation-system" which leaves no room for
such a "multiplicative factor" "determines only the relative, not the

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[54] The exposition in question is to be found on pp. 151 f. of Cassel's
Theory of Social Economy. For later discussions of the concept of a "mul-
tiplicative factor," in the sense indicated, see Myrdal, *Monetary Equilib-
rium,* 11 ff.; Lundberg, *Studies in the Theory of Economic Expansion,* 7;
Lindahl, *Studies in the Theory of Money and Capital,* 327 f.; and J. Peder-
sen and O. S. Petersen, *An Analysis of Price Behaviour,* 141. The fact that
all the writers just cited are Scandinavian would seem to confirm the
suggestion that such difficulties as have arisen in connection with the con-
cept of a "multiplying factor" have derived specifically from the exposi-
tion of Cassel. The point is worth stressing—if for no other reason, be-
cause of the light it throws on the validity of the charge that the argu-
ment in question is typical of all "equilibrium theorists," including Walras,
who are thus charged as a group with having drawn "the line of division
clearly and sharply between the theory of price formation proper on the
one side, and monetary theory on the other" (so Myrdal, *Monetary Equi-
librium,* 11; Lundberg, on the other hand, cites *only* Cassel in this con-
nection). The truth of the matter is rather that the problem under
discussion provides a further instance of the danger of accepting Cassel as
a faithful interpreter of the Walrasian position, particularly with respect
to the rôle of money in general economic theory. On certain general
aspects of the relation of Cassel's "system" to that of Walras, see the
comments of Wicksell, in his essay on "Cassel's System of Economics"
(*Lectures on Political Economy,* I, 225 f.); and on the rôle of money, in
particular, in the two writers' respective "systems," see my article, "The
Monetary Aspects of the Walrasian System," *loc. cit.,* 164, n. 35; 173, n. 54;
and 185.
[55] Cassel, *Theory of Social Economy,* 151 f.

absolute, prices." [56]   And indeed if Cassel had stopped at this point, his argument would have amounted to no more than this.

Unfortunately, however, he did not stop at this point. Instead, he went on to advance a contention which immediately provided an opening for an attack upon the very concept of a "multiplicative factor" as a contribution to the solution of the relation between the "general" Theory of Value, on the one hand, and the Theory of Money and Prices, on the other. This contention was nothing more nor less than that the introduction of a "multiplicative factor" of the type indicated here *"has no influence upon"* either the form of the demand and supply functions for particular commodities included in an "equation-system" constructed initially in abstraction from the effects of monetary expansion or contraction, or on the position of the curves represented by these functions, *relatively to each other*.[57]   It should hardly be necessary to labor the point that this assumption is not only in direct conflict with what we know of the world about us, but is completely unnecessary from the standpoint of an adequate "integration" of the "general" Theory of Value, on the one hand, with the Theory of Money and Prices, on the other.

For it should be clear, upon reflection, that there is no reason why,

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[56] *Theory of Social Economy,* 151. In the passage cited, Cassel speaks only of *"our* equation-system," in a way which would probably suggest to most readers that he is referring only to the particular "equation-system" that he himself had presented up to that point in his argument. Unfortunately, however, Cassel has not hesitated, on later occasions, to assert that *"the* equilibrium system of equations can only determine *relative* prices" (see, for example, the *International Labour Review,* XXXVI [1937], 439). The effect of this pernicious type of statement can be traced elsewhere in current economic literature. See, for example, W. W. Leontief, "Interrelation of Prices, Output, Savings, and Investment," *Review of Economic Statistics,* XIX (1937), 116, where it is stated that "the proposition that the material set-up of our economic system determines only the *relative* and not the absolute prices of all the commodities is so familiar that it hardly deserves further discussion." For our present purpose, however, it is preferable to give Cassel the benefit of the particular interpretation of the statement in his *Theory of Social Economy* which is indicated in the text.

[57] See Cassel, *Theory of Social Economy,* 151, especially the first sentence of the last paragraph. A proposition similar to that of Cassel as stated above is characterized as a "fundamental assumption" of orthodox analysis by Leontief, "The Fundamental Assumption of Mr. Keynes's Monetary Theory of Unemployment," *loc. cit.,* 192 ff. But, apart from a single mention (without documentation) of Ricardo, who can hardly be taken as representative of the best that "orthodox" theory has had to offer on the particular matter under discussion, no references to the literature are given by Leontief in support of this characterization. This fact obviously bears directly on Leontief's characterization (p. 197 of the article cited) of Keynes's "attempt to modify" what is regarded as "one of the basic static assumptions of the 'orthodox' economists" as "the essentially novel contribution of the *General Theory."*

in order to establish the nature of the effect of monetary expansion and contraction upon the general "scale" of prices, it is necessary to assume either (1) that changes in the "scale" of prices are unaccompanied by changes in the *structure* of money prices; or (2) that monetary expansion and contraction are incapable of affecting the structure of money prices unless they also affect the *scale* of money prices. In formal terms, one may say that there is no reason why the *same* "multiplicative factor," interpreted as summarizing the effect upon the price system of monetary expansion or contraction, need be the *same* in the case of all demand and supply functions, or why it should be assumed that the demand and supply functions to which this "multiplicative factor" is applied need be the same during or after the monetary expansion or contraction as they were before.[58] Given, therefore, the connotations that have come to be attached to the concept of a "multiplicative factor" as a result of Cassel's usage, much is to be said for abandoning the *expression* altogether. In the same way, much is to be said for avoiding the statement that the purpose of the "money equation" is to provide us with an otherwise "missing equation" for the determination of the "Value of Money" or the "general price level." It should be clear, on the contrary, that it would be much better to say that the

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[58] It should be clear that the formal use of a series of *differing* "multiplicative factors" would be at best a very inferior substitute for a careful description of the processes of the differential generation and utilization of money incomes and other receipts, on the one hand, and of the resulting differential impacts upon the price structure, on the other, which would in fact bring about the indicated changes in (1) the individual supply and demand functions themselves, and (2) their relation to one another. It is, in fact, just such a description which is made possible by the use of a series of "partial" "stream" equations of the kind contemplated by the general analytical apparatus here outlined. It should be observed, moreover, that an adequate understanding of the system of "stream" equations thus indicated should make unnecessary any misunderstanding of the apparent paradox that the so-called "money equation" is at one and the same time (1) an equation for the determination of what has sometimes been called the "Value of Money"; and (2) an equation representing a summation of *all* the "partial" "stream" equations in the system. For once it is observed that each of these "partial" equations, in the system outlined, already *contains* the particular "multiplicative factor" which applies to the particular sector of the economic process involved, it becomes obvious that these equations provide a measure of the "value of money" in each of these particular sectors. This fact, indeed, may be said to provide a partial justification of the position of those who have argued, or implied, that a special "money equation" is not really necessary to the complete system of equations, since the "monetary factor" is already included in the equations when the latter are properly stated. It is necessary to add, however, that *only* a "partial" justification can be accorded to those writers (such as Pareto) who consistently avoided any frontal attack on the *problems with which the "money equation" was intended to deal.* On the characterization of the "money equation" as a "Value of Money" equation, see also what is said in the following note.

fundamental purpose of this "money equation" is to provide us with a framework for analyzing the nature of the forces which make the *aggregate of money expenditure* what it is, and therefore make the absolute "scale" of prices, among other things, what it is.[59] But in neither case is one justified in using the unfortunate modes of exposition or argument sometimes associated with the concepts of a "general price level" or the "Value of Money" in the past as an excuse for refusing to accept the unchallengeable contention of users of such concepts with respect to the need for a "money equation" if we are to have a complete account of the nature of the forces determining money prices in the world we know.

ii. The second major purpose assigned to the concept of a "general price level" by the abler users of that concept will best be understood if one asks why all the good purposes served by the concept of a *"scale* of prices" could not be served equally by the concept of a *sum* of realized money prices.[60]   It is, after all, the *sum* of realized money prices

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[59] One of the "other things" which may be expected to change as a result of changes in the "aggregate of money expenditure" is indicated by Proposition XV, ii, immediately following. For an example, on the other hand, of the type of irrelevancy which has tended to intrude itself when the purpose of the "money equation" is characterized as that of providing an otherwise missing equation for the determination of the "Value of Money," see Myrdal's discussion of the concept of a "multiplicative factor," as yielded by a special "money equation," in his *Monetary Equilibrium,* 12 ff.   In the passage cited, Myrdal goes so far as to assert a "logical impossibility" of effecting "a closer integration of . . . monetary theory with general price theory" with the help of a "money equation," on the ground that "money cannot be treated as one of the goods in the system of price formation," because "these notions [of 'demand' and 'supply'] lose their theoretical accuracy if applied to money." Contrast, in this connection, the comment by Lundberg, *Studies in the Theory of Economic Expansion,* 24, n. 1, on the relative merits of the type of attempt to bridge the alleged "separation of the theory of money from the general theory of prices" which is based, on the one hand, on a discussion of the peculiarities of money as, say, a "store of value," and the type of analysis, on the other hand, which is based on an emphasis upon the causes and consequences of changes in those "total categories" with whose determination (though Lundberg himself does not make this clear) certain of the "stream" equations of monetary theory are in fact concerned.

[60] For examples of a use of the concept of a *"sum* of prices," see the references given above to Tooke (p. 151, n. 20) and Schumpeter (p. 118, n. 67). It may be observed here that the issue stated in the text has sometimes been obscured by the use of the expression "the *aggregate* price level" as a synonym for the *"general* price level" (or the "scale" of prices). See, for example, A. A. Young, "The Measurement of Changes in the General Price Level," *Quarterly Journal of Economics,* XXXV (1921; p. 267 of the same author's *Economic Problems New and Old).*   It should

which is necessarily equal to that "sum of realized money demands" (ΣD) the determination of whose magnitude, according to our Proposition VIII, it is one of the most important purposes of the "money equation" to explain.[61] The answer to this question is provided, however, as soon as it is remembered that the "sum of prices" which is equal to the "sum of realized money demands" is represented by the expression Σpq, or, as Schumpeter has called it, the "sum of the products" *(Produktensumme)* of prices *times quantities sold.*[62]

For what this means is that whenever we deal with *"total categories"* such as the concepts of a "general" money demand, on the one hand, or the *"sum* of prices," on the other, we are confronted with a problem that hardly exists when we are concerned with "micro-economic" quantities such as *individual* prices and the realized money "demands" for these individual commodities.[63] Specifically: in the case of an increase in the amount received from the sale of an individual commodity, there is virtually no difficulty in determining whether the increase in the amount thus received is due to an increase in the price paid for *each unit* sold, or is due to an increase in the *number of the units sold* at a given price.[64] And because this is so, there is virtually no difficulty in determining whether an increase in the realized money demand for an individual commodity (that is, the amount of money spent upon an individual commodity) represents a purchase of the same quantity of the commodity at a higher price per unit, or the purchase of a larger

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be clear that the concept of an "aggregate price level," as so defined, differs from the *"sum* of prices" in the vital respect that the former is obtained by *dividing* the "sum of prices" by "the quantity of goods exchanged." See Young's *Economic Problems,* 261.

[61] See above, pp. 285 ff.

[62] Cf. the references to Schumpeter given above, p. 118, n. 67.

[63] The terms "micro-economic" and "macro-economic" are borrowed from Lindahl. See the latter's *Studies in the Theory of Money and Capital,* 51 ff., 74, 78 ff., 111 ff. On further aspects of the rôles assigned to "micro-economic" and "macro-economic" analysis, respectively, in the analytical apparatus outlined in the present work, see below, pp. 498 ff.

[64] The reason for this, of course, is primarily that the very statement that the "price" of a given commodity has risen, say, is almost invariably taken to mean that its price *per unit* has risen. The same generalization cannot, of course, apply to the statement that the *"sum* of prices" has risen.

quantity of the commodity at the former price per unit.[65]

There is nothing, however, in the case of an increase in either the *"sum* of prices" or of "general" money demand which tells us whether more commodities are being sold at the old prices, or the same amount of commodities are being sold at higher prices. Yet it is absolutely vital to the whole theory of the effect upon *output and employment* of *changes in the magnitude of "general" (money) demand* that we should be able to determine which of the two possibilities is being realized.[66] It is only fair to point out, therefore, that, historically, *one of the purposes for which the concept of a "general price level" was designed* was precisely that of enabling us to distinguish those changes in the "sum of prices" (or in "general" money demand) which represent changes in the p's in the expression Σpq (or its equivalent), from those which represent changes in the q's.[67]

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[65] This result follows, of course, from the fact commented upon in the preceding note. For when we are given the information that (1) there has been an increase in the realized money demand for a commodity *and* that (2) this realized demand has resulted or has not resulted in a change in "price" *per unit,* we are simultaneously given information with respect to change in the *quantity sold,* by virtue of the equation $D = pq$. This, however, is precisely what cannot be said of the statement that an increase in *"general* money demand" has resulted in an increase in the "sum of prices."

[66] Cf., for example, Hawtrey, *Capital and Employment,* 68: "If general demand is insufficient to pay for the total output of the community *at the prevailing price level,* a part of the output will remain unsold" (italics mine). Since the proposition stated in the text may be said to sum up the whole of the theory of the effect of changes in "general (money) demand" upon output, its full discussion, as well as a discussion of its earlier history, must be left for another occasion. I venture to suggest here, however, that an enormous amount of confusion would have been avoided within this sector of monetary theory in the past if more writers on the effect, upon output and employment, of changes in "money demand" or the amount of monetary "purchasing power," had made use of the late Professor Cannan's expression "money-spending power" in place of "purchasing power." See Cannan's *An Economist's Protest,* 378, n. 1, and 397. It should be added that an integral part of the analytical apparatus required for an adequate solution of the problem indicated in the text is that presented below in Chapter Eleven.

[67] The clearest illustration of this fact is provided by the case of Simon Newcomb. In addition to what is said in n. 37 to p. 106, above, with respect to the apparent conflict between Newcomb's statement of the problem and that of Fisher, see Newcomb's *Principles,* 207, 213, 352 ff., 386 f. Yet, given the historical association between "stream" equations of the Newcomb-Fisher type and the concept of a "general (money) demand"

iii. That the substance of this argument is relevant also to the problem of the determination of the inner constitution of the *system* of money prices follows from a proposition which should be regarded as beyond question: namely, that any description of the "system" of money prices which would run in terms solely of the location of individual *prices* without specification of the *amounts sold* at those prices is only a half-description of a "system" of prices, in any proper sense of the latter term.[68]

For the description of the determination of a "system" of realized prices which is presented in this work runs in terms of a series of "stream" equations, each of which represents the "mutual impact" of a stream of money spending power and a *stream of objects sold* against that stream of money spending power. It is possible, as we have seen, to narrow the dimensions of these individual streams of money spending power and of the objects purchased thereby, down to a series of streams of money spending power directed in each case against a *single commodity*.[69] In application, however, it has been found desirable to supplement "micro-economic" analysis of this type by a more "macro-economic" type of

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(cf. above, pp. 104 f., and nn. 36 and 37 thereto), it must be obvious that the very process of historical evolution whereby *a special term for the "amount of commodities sold"* was inserted in these equations (or their nonalgebraic equivalents) is itself a proof of recognition of the importance of the issues involved. On this matter, see Volume I, 93 ff., of the present work. It should hardly be necessary, therefore, to discuss at length the bearing of these established facts of doctrinal history upon propositions such as (1) that equations of the Newcomb-Fisher type amount only to "mere identities" stating that the "sum of money prices is equal to the sum of money prices" (cf. Volume I, 91 f.); or (2) that "for people who are used to thinking in terms of these quantity equations it is extraordinarily difficult to bear in mind . . . that the effect of a change in the flow of money payments is predominantly on the volume of goods sold, and not on prices" (so N. Kaldor, in the *Economic Journal*, XLIX [1939], 497).

[68] In terms of the model sketched below, pp. 479 ff., such a description would amount at best to a two-dimensional picture, the axes of which would represent *time* and *price*, respectively, in contrast with a three-dimensional model, of the kind described below, in which the third axis represents *quantity sold* at each of the prices included in the two-dimensional *chart*.

[69] See above, p. 320.

analysis, the essence of which is summarized by the concept of a "plurality of price levels," on the one hand, and, on the other, a series of "stream" equations, each of which includes one of these "price levels." It should be clear that, apart from very unusual cases, none of these "price levels" will be given by knowledge solely with respect to the dimensions of the particular stream of *money expenditure* directed against goods whose prices are included in any one of these "price levels." [70]

If, therefore, we are to obtain an adequate picture of the "system" of money prices emerging from the impact of these streams of money spending power upon the objects sold against such streams, it will still be necessary to separate the components which make up even the *partial* "sums of prices" into (1) changes in *prices,* and (2) changes in the *quantities sold* at these prices. As long as this is so, it must be admitted that the concept of a "general price level" has performed an historic function of the utmost significance in bringing out the general point involved. And this general point should not be lost even upon those whose interest in the internal constitution of the *system* of money prices would prevent their own use of the concept of a "general price level" *itself* for any purpose other than that summarized by our Proposition XV, i—a purpose, it may be repeated, which is none other than that involved in the argument for providing an apparatus designed to explain why the *absolute* "scale" of money prices is what it is.

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[70] The "unusual cases" referred to are, of course, those in which the "quantities of all commodities" would remain "constant," so that, "with unchanging kinds and quantities of commodities, constant expenditure defines identical price levels." See Schumpeter, *Business Cycles,* 454 f., 491; and contrast the comment of Kaldor cited above, p. 344, n. 67, with respect to the results that may be expected from "people who are used to thinking in terms of these quantity equations."

CHAPTER SEVEN

Stream Equations and Process Analysis

THUS FAR our argument has been concerned primarily with the relation between that part of the "general" Theory of Value which is summarized by "demand schedules for particular commodities" of the Marshallian type, on the one hand, and the "stream" equations of monetary theory, on the other, when our problem is that of establishing the relation between these demand schedules and the "system" of money prices. Nothing, on the other hand, has bulked larger in recent discussions of the nature and content of economic "dynamics" than the insistence that such "dynamics" must be concerned with the unfolding of a *process in time*.[1] The purpose of the propositions which follow, therefore, is to establish the fact, among others, that it is precisely by the use of the "stream" equations of the Theory of Money and Prices that an apparatus for dealing with such a "process" is able to *retain* and to *supplement* the substance of that part of the "general" Theory of Value

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[1] From what is said below in Chapter Eight (pp. 451 ff.), it will be clear that nothing but misrepresentation of the substance of received economic doctrine would be involved in any statement to the effect that the *substance* of economic "dynamics," of the type indicated in the text, is itself of "recent" origin. It is undoubtedly true, on the other hand, that intensive and self-conscious discussion of the implications of "process" analysis and its relation to the analytical devices of "general" economic theory is a phenomenon of only recent years. Nor can there be any serious doubt that this intensive discussion is to be traced principally to the concern of the younger Swedish economists with the implications of that part of the argument of Wicksell which they themselves have discussed under the head of "the cumulative *process*." See, for example, Lindahl, *Studies in the Theory of Money and Capital,* 158 ff.; Myrdal, *Monetary Equilibrium,* 24 ff., 40 ff., *et passim;* Hammarskjöld, *Konjunkturspridningen,* 19 ff.; Ohlin, "Some Notes on the Stockholm Theory of Savings and Investment, I," *loc. cit.,* 54 ff.; Lundberg, *Studies in the Theory of Economic Expansion, passim,* but especially pp. 45 ff.

which is typified by "demand schedules for particular commodities" of the Marshallian type. Specifically:

XVI. Taken by themselves, market demand schedules of the Marshallian type, and the body of analysis which they are designed to summarize, are intended to deal only with *discrete situations,* in each of which a price is determined by the intersection of the market demand and supply schedules prevailing at the moment the relevant price is realized. Nothing in these market demand and supply schedules tells us how we *pass from one discrete situation to another.*[2]

It should hardly be necessary to emphasize that this proposition is not intended to convey the impression that the principal users of Marshallian demand schedules—and particularly the representatives of "old" Cambridge—have themselves made no effort to *supplement* this type of analysis by another type designed precisely to trace the "repercussions" of "changes in the supply and demand for a particular commodity" upon "the whole price system" and "such phenomena as general contraction and expansion processes."[3] It is true, of course, that some of the representatives of "old" Cambridge have been critical of the looseness of certain types of analysis avowedly based upon the

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[2] On the suggestion that this limitation applies not only to analysis confined to the use of market demand and supply schedules of the Marshallian type, but also to analysis of the Walrasian type, see below, pp. 351 ff.

[3] The phrases quoted are from Ohlin, "Some Notes on the Stockholm Theory of Savings and Investment, II," *loc. cit.,* 230. It should be added, however, that virtually all the force is taken out of what Professor Ohlin seems clearly to have intended to be an adverse comment, by his reservation that his strictures do not apply to "money and cycles theory"—including, if one is to judge from the reference to D. H. Robertson on p. 234 of the article cited, the "monetary and cycles theory" of *"old"* Cambridge. For if what is desiderated is "a co-ordination of the theories of price, money and cycles" (Ohlin, *op. cit.,* 235), it should be clear that more credit is to be given to those who, in their discussion of "general processes of expansion and contraction," *continued* to use the weapons developed originally in connection with "particular equilibrium" analysis (cf. Ohlin, *op. cit.,* 230, n. 2) than can be given to those, like Mr. Keynes, who, in presenting a "theory of processes of general contraction and expansion" (Ohlin, *op. cit.,* 233), have explicitly renounced the use of "particular equilibrium" devices such as market demand schedules of the Marshallian type. It may be observed, finally, that even Professor Ohlin's contrast (*op. cit.,* 233) between "textbooks in price and distribution theory," on the one hand, and "the theory of money and business cycles," on the other, loses a large part of its force in the light of the passages from Marshall's *Principles* cited below, p. 349, n. 6, particularly when this contrast is itself judged in the light of the context provided by Professor Ohlin's comment (*op. cit.,*

general idea of "repercussions" of the type indicated.[4] It is certainly not true, however, that they either denied the possibility of, or showed a lack of interest in, analysis designed to trace these repercussions in detail.[5]

In order to be convinced of this fact, one has, indeed, only to consult those passages of Marshall's *Principles* in which the topic of discussion was precisely the necessity for taking account of propositions such as that factors which "hinder production in any branch of industry necessarily increase unemployment in other branches"; that when there is "little occupation" in certain trades, "those whose skill and capital is specialized in these trades are earning little, and therefore buying little of the produce of other trades"; that these "other trades . . . earn less, and therefore they buy less: the diminution of the demand for their wares makes them demand less of other trades"; so that "this commer-

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233) upon what might be expected from "Cambridge economists" generally, on the one hand, and the "somewhat 'revolutionary' flavour, from the point of view of economic theory," of Keynes's *General Theory*, on the other hand.

[4] See, for example, Robertson, *A Study in Industrial Fluctuation*, 122, 125, 205, and Pigou, *Industrial Fluctuations*, 57 f. It may be added that the particular users of the concept of a series of "repercussions" who were discussed by Robertson and Pigou—namely, Lescure and Bagehot, respectively—are by no means the only ones who could be cited from the earlier literature. In addition to the references to Marshall and Cournot given below, p. 349, n. 6, and p. 351, n. 10, see, for example, the discussion of the "propagation" of initial "shocks" in F. A. Walker, *Money in its Relations to Trade and Industry*, 126 ff.; and for further references to earlier discussion of the concept of "repercussions," see Koopmans, "Zum Problem des 'Neutralen' Geldes," *loc. cit.*, 289 ff. The fact that the list of earlier writers who have made use of the concept of "repercussions" (at all levels of precision in analysis) could be very greatly extended is of obvious interest for the history of the range of ideas associated with the concept of the "multiplier," in its various forms.

[5] See, for example, the comment by Robertson in his *Study in Industrial Fluctuation*, 205, to the effect that "those writers who, like M. Lescure, find the cause of general depression in a 'repercussion' from constructional industry, while their reasoning is, as a rule, unscientific and nebulous in the last degree, would seem to have hit instinctively upon an important truth"; also the argument of Pigou, *Industrial Fluctuations*, 63 (sec. 13). It should be clear, particularly in the light of what is said above, pp. 143 f., and n. 6 thereto, concerning the relative merits of an exposition running in terms of "real" demand, on the one hand, and one running in terms of money receipts and expenditures, on the other, that the statement in the text is not to be interpreted as implying approval of all aspects of the "old" Cambridge method of dealing with "repercussions" of the type indicated. Yet it should also be clear that none of these differences would warrant serious modification of the statement in the text with respect to "old" Cambridge's recognition of, and attempt to deal with, the problem indicated. Cf. also what is said on this matter below, p. 349, n. 7.

cial disorganization spreads: the disorganization of one trade throws others out of gear, and they react on it and increase its disorganization." [6] The point of our Proposition XVI, therefore, is merely that market demand and supply schedules of the Marshallian type are *in themselves* intended to deal only with discrete situations, and do not *in themselves* describe the "repercussions" growing out of each discrete situation in terms enabling us to trace the process whereby we pass from one discrete situation to another.

XVII. The bridging of this gap in our understanding of the pricing process as it unfolds itself in time is provided by the fact that the prices realized in discrete situations are part and parcel of the process of *receiving and expending money in time.*[7] For, in a fully developed money economy, it is the receipt and expenditure of *money* (the "flow" of money) which keeps the economic process functioning. If, for example, in a fully developed money economy, none of the money received in the form of the realized prices of articles

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[6] See Marshall's *Principles,* 710 f., and cf. Lavington, *The Trade Cycle,* 23 f. On the relation of passages of the type quoted from Marshall to the general analytical scheme underlying the great work of which his *Principles* was intended to be only the first volume, see what is said above, p. 75, and especially n. 59 thereto. In view, moreover, of the loose statements made in recent years with respect to the baneful influence of "Say's Law," or similar propositions, in preventing the emergence of a theory of changes in the volume of employment and output (cf., for example, Ohlin, "Some Notes on the Stockholm Theory of Savings and Investment, II," *loc. cit.,* 231), it may not be out of place to observe that Marshall himself regarded the propositions quoted in the text as following directly from Mill's version of that "Law," when the "Law" itself is so stated as to take account of the fact that "though men have the power of purchase they may not choose to use it" (Marshall, *Principles,* 710; cf. also Marshall's comment on the "Law" in his "Mr. Mill's Theory of Value" [1876; p. 130 of the *Memorials of Alfred Marshall*]).

[7] It is this fact which has tended to be obscured by the practice, followed by a number of representatives of "old" Cambridge, of describing the relevant processes in terms of the consequences of changes in the "real" demand for particular commodities (cf. above, p. 348, n. 5). Yet it must again be observed that this does not necessarily convict "old" Cambridge either of positive errors of analysis or of addiction to an analytical system inferior to that of writers who, despite their claim to have effected a new synthesis between the "general" Theory of Value, on the one hand, and the Theory of Money and Prices, on the other, have made no serious effort to grapple with the *problems discussed by* the members of "old" Cambridge under the heading of the consequences of changes in the "real" demand for particular commodities.

sold were subsequently expended, the economic process would cease to function altogether.[8]

XVIII. The concept of a connection between prices received from the sale of commodities and services and the *subsequent* flow of expenditure was, to be sure, implicitly recognized in economic theory as soon as it was established that the price of a given commodity sold is resolvable into income-"shares," which are then, of course, available for subsequent expenditure.[9]   The point was also recognized

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[8] Cf. the similar comment by Fleeming Jenkin on what would happen to "the whole process of buying and selling" in his imaginary "little community of say five families," *"so long as the transactions were all conducted on the cash down principle,"* if the "sixpence" which he assumed to be "the whole coin of the realm" "did not continually return to each person who paid it" (*The Graphic Representation of the Laws of Supply and Demand, and Other Essays on Political Economy,* 143).   The proviso which I have italicized is of course the equivalent of the proviso "in a fully developed money economy" of our Proposition XVII.   On the nature of the "closed circuits" that would be "still a necessary feature of the trade" in a *barter* economy ("when the sixpence was locked up"), see pp. 143 ff. of the work of Jenkin just cited.   On the other hand, the "introduction of the system of credit" to which Jenkin later referred (p. 151) does not change our Proposition XVII in any essential respect.   For under such a system, "money" must be said to be "received" when one individual is "credited" with the proceeds of a sale and "expended" when he assigns this "credit" to another member of the .community.   It is this second assignment of "credit" which is then necessary if the economic system is to continue to function.   See also the reference given below, p. 357, n. 19, to Jenkin's further use of the analogy of a "closed circuit."

[9] The concept in question is of course at least as old as Quesnay's *Tableau Economique* (see below, p. 352).   The *locus classicus,* however, for the statement of the concept in the terms used in the text is Adam Smith's *Wealth of Nations,* Book I, Chap. VI, "Of the Component Parts of the Price of Commodities"—the central thesis of the chapter being that "the price . . . of all the commodities which compose the whole annual produce of the labour of every country, taken complexly, must . . . be parcelled out among different inhabitants of the country, either as the wages of their labour, the profits of their stock, or the rent of their land" (p. 52). From the argument of the present work, it should be clear that the far-reaching consequences of Smith's proposition can be fully appreciated only when it is associated with the functioning of the monetary mechanism and with *clock-time-dated* expressions of the general form $(PT)_I$ (cf. Volume I, 383, of the present work).   It is striking,. therefore, that writers of our own day should have objected to the fact that "Smith's theory of distribution . . . is inserted in the middle of the chapter on prices as a mere appendage or corollary of his doctrine of prices," and particularly to the fact that a series of "intermediate chapters" on "money and prices" should have been inserted between the chapters on the division of labor, on the one hand, and "wages, profit, and rent," on the other (so E. Cannan, *A*

with considerable explicitness by Cournot.[10]   The essential purpose, however, of market demand and supply schedules of the Cournot-Marshall type *as such* is not that of tracing the process of the generation and utilization of money income.

XIX. If there is any part of the "general" Theory of Value which is directly relevant to the problem of the generation and utilization of income, it is that part which is summed up by the Walrasian concept of a "circuit flow," in

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History of the Theories of Production and Distribution in English Political Economy from 1776 to 1848 [1893], pp. 186, 230, of the second [1903] edition).   Others, on the contrary, would rejoice at the fact that Smith's example was not entirely lost on the generation that followed him. See, for example, Say's *Treatise,* Book II, Chap. I (p. 284 of the American edition of 1836); and see also Senior's *Three Lectures on the Value of Money,* 24 ff., where the transformation of realized prices into the "monied incomes" of the sellers of the commodities to which these prices attach was introduced into the discussion of a specifically "monetary" problem.   The use, by Simon Newcomb, of the proposition which he called his "Second Law of Income" (*Principles of Political Economy,* 365 ff.), but which amounted essentially to a restatement of the proposition of Smith quoted above, is particularly striking in the light of Newcomb's place in the history of concepts essentially equivalent to the Walrasian "circular flow," and especially the monetary aspects thereof (see below, p. 357).

[10] See Cournot's *Researches,* 127 ff.   The pages in question have been characterized, even by so great an admirer of Cournot as Irving Fisher, as of "only an historical interest," and as "the most unsatisfactory in the book" ("Cournot and Mathematical Economics," *Quarterly Journal of Economics,* XII [1898], 121, 130); and they have been criticized, on varying grounds, by writers of the standing of Pareto ("Di un errore del Cournot nel trattare l'economia politica colla matematica," *Giornale degli economisti,* IV [1892], 1 ff.); Edgeworth ("The Theory of International Values," *Economic Journal,* IV [1894], 627 ff. [*Papers Relating to Political Economy,* II, 49 ff.]); and Viner (*Studies in the Theory of International Trade,* 586 ff.).   Yet none of the criticisms advanced by these writers, all of which have to do with certain of Cournot's applications of his general device to specific problems, such as those of international trade, contradict the statement that Cournot was clearly aware of the nature of the relation between the type of analytical device represented by the demand schedule for a particular commodity, on the one hand, and the problem of the generation and utilization of money income, on the other.   Specifically, Cournot undertook to establish the nature of the relation between expressions of the type pD_p (representing the money received from the sale of a given commodity), on the one hand, to (1) the essentially Smithian proposition that "all the elements into which this price can be decomposed are distributed into various branches of the social income" (Cournot, *Researches,* 128); and (2) the concept of a cumulative generation or contraction of income as the result of changes in the magnitude of the money sums represented by the individual pD_p's, on the other hand.

which the money proceeds realized from the sale of con-
sumers' goods, for example, are regarded as being returned,
through entrepreneurial purchases of the services of the fac-
tors of production, as incomes to these factors, these incomes
being then expended in the purchase of consumers' goods,
and so on.

Not even the most devoted admirers of Walras would pretend that
he was the first to introduce into economics the type of picture of a
functioning economic *process* which is represented by the concept of a
"circular flow," of the kind just indicated. If any one writer on
economics deserves that honor, it is, as Professor Schumpeter has
argued so eloquently, François Quesnay.[11] Nor can it be said that
the concept of a "circular flow" disappeared so completely from eco-
nomic literature after the decline of the Physiocratic influence that it
had to be discovered entirely anew by Walras. Even the later mer-
cantilists, for example, included, in the person of J. G. Büsch (1780),
a writer whose insistence upon the importance of the rôle of *money*
in the functioning of the economic "circuit" was justified not only in
itself, but also in the light of later tendencies to sin at least as greatly
in the direction of *underestimating* the rôle of money in the functioning
of the circular flow of goods and services as Büsch's analysis can be
said to have sinned in the direction of overestimating it.[12] And no

No one, to be sure, could pretend that Cournot himself provided a
detailed solution of the problem of the relation between the determination
of money prices and the generation of money income; and certainly no
one could pretend that he made a serious contribution toward an under-
standing of the relation of the functioning of the monetary mechanism
to the problem in hand. That even the little of a detailed constructive
nature which he offered was, nevertheless, of importance may be seen by
comparing the implications of Cournot's proposition that the measure of
a change in "income" is provided by the expression $p_1 D_1 < p_o D_o$, on the
one hand, with the implications, on the other hand, of a literal acceptance
of Newcomb's statement that "when the price of goods rises, it is certain
that some one concerned in their production is receiving an increased in-
come for his services, and *vice versa*" (Newcomb, *Principles,* 367, 542).
On the importance of distinguishing between the two statements, see D.
H. Robertson's criticism of J. A. Hobson, in the former's *Study of Industrial
Fluctuation,* 236.

[11] See sec. II ("The Discovery of the Economic Circular Flow" [*wirt-
schaftlichen Kreislaufs*]) of Schumpeter's "Epochen der Dogmen- und
Methodengeschichte," *loc. cit.,* 39 f., especially p. 40.

[12] On the aspects of Büsch's argument which may fairly be characterized
as representing an overemphasis upon monetary factors, see, for example,
the comments by Roscher, *Geschichte der Nationalökonomik in Deutsch-
land,* 567, and Cossa, *Introduction to the Study of Political Economy,* 276;
and cf. also the general comments with respect to mercantilist ideas on
"circulation," on the one hand, and the "social product" and its distribu-
tion, on the other, in Schumpeter, *Epochen der Dogmen- und Method-*

adequate survey of the history of the concept of a "circuit flow" in eighteenth-century economic literature could fail to mention that grossly neglected "adversary" of both Physiocratic and mercantilist excesses, A. N. Isnard, whose work has been highly rated by the few competent historians of doctrine acquainted with his *Traité des Richesses* (1781).[13] For there can be little doubt that, in addition to "standing on the shoulders" of the Physiocrats insofar as the concept of a "circular flow" is concerned, and correcting their picture of the productive process at

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engeschichte, loc. cit., 44. For Büsch's discussion of what he himself called "the circle of monetary circulation" *(der Zirkel des Geldsumlaufs)* or "the circular flow of labor and money" *(der Creislauf der Arbeit und des Geldes)*, see his *Abhandlung von dem Geldsumlauf*, I, 73 ff., 78 ff., 352 ff., 355 ff., 358; II, 83 ff., 108 f., 541 ff. Büsch was, of course, by no means the first writer to use the simile of the "circle" or the term "circuit" in speaking of the flow of money payments. See, for example, the quotations from Petty and Locke given by M. W. Holtrop in his "Theories of the Velocity of Circulation of Money in Earlier Economic Literature," *Economic History* (supplement to *Economic Journal*), 1929, pp. 503, 506, and also the comments on Cantillon on pp. 506, 508, 510 of the same article. Cantillon's discussion, indeed, is of particular importance, not only because of its probable influence on Büsch, despite the latter's none too generous treatment of Cantillon (see the references to Cantillon in Büsch's *Abhandlung* cited above, p. 310, n. 187), but also because of Cantillon's undoubted influence on Quesnay. On the latter point, see the comments by Jevons in his paper "The Nationality of Political Economy" (pp. 354 f., 359 of the paper as reprinted in Higgs's edition of Cantillon's *Essai*); and cf. also the comment by Professor Rist in the latter's *Histoire des doctrines relatives au credit et à la monnaie*, 98. Yet there can be little doubt that Büsch's emphasis upon the importance of monetary factors in the "circular flow," with all its exaggerations, did point to the necessity for a type of analysis to which less than justice is done by Holtrop's severe comments on Büsch's treatment of monetary "velocity," and by the former's comments on what he calls "non-monetary" theories of velocity generally, with their emphasis on the relation between the flow of money payments and the movement of goods "from producer to consumer" (Holtrop, "Theories of the Velocity of Circulation of Money," *loc. cit.*, 511 f., 521).

[13] Cf. Schumpeter in his "Epochen der Dogmen- und Methodengeschichte," *loc. cit.*, 114 n.—one of the few instances, in addition to that of Morgenstern, cited in the following note, in which anything like justice has been done to Isnard. The general rule in histories of economic doctrine, in those instances, from Blanqui to Cossa, in which Isnard is mentioned at all, has been to dismiss him with a supercilious classification as an essentially unimportant "adversary" of either the Physiocrats or the mercantilists, or both, or to include his name without comment in a list of early writers on economics making use of mathematical symbols. See, for example, Cossa, *Introduction to the Study of Political Economy*, 307; also Palgrave's *Dictionary of Political Economy*, II, 460, and the reference to Jevons's bibliography of mathematical economics there given (cf. the Bibliography of Mathematical Economics appended by Irving Fisher to his translation of Cournot's *Researches*, 174).

the point at which it was weakest, Isnard, of all eighteenth-century writers, comes nearest to deserving to be characterized as having "anticipated the idea of [general economic] interdependence developed a century later by Walras." [14]

The concept of a "circular flow" might, to be sure, have appeared more articulately in the writings of the "classical" economists than it

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[14] Cf. O. Morgenstern, in the *Encyclopaedia of the Social Sciences*, V, 365; and for Isnard's position with respect to the aspects of Physiocratic doctrine indicated in the text, see, in addition to the comment by Schumpeter cited in the preceding note, Isnard's *Traité*, I, pp. xiii f., 15 n., 39 ff., 51 ff., 55, 93 n. It may be observed that the similarity of Isnard's "system" to that of Walras goes far beyond the fact that Isnard himself repeatedly characterized his analysis as being concerned with a "*système* de richesses," or a "*système d'un grand nombre de marchandises*" (*Traité*, I, 26 f., 39, 93 n.), for which it would be necessary to establish "*as many equations as there are commodities*" (p. 19). Indeed, this similarity goes considerably beyond even the facts, striking as they are, (1) that, like Walras, Isnard may be regarded as having built his analytical structure on the Physiocratic concept of the economic process as involving "periodic or continuous reproduction" (*Traité*, I, 60); (2) that this process was described in terms of a circuit involving the conversion of "productive expenditures," as realized "expenses of production," into the incomes of the factors of production, who then return to entrepreneurs the funds thus received, by purchasing these entrepreneurs' products, the "value" of which, in relation to the magnitude of incurred expenses, determines the amount of entrepreneurial "income" (*Traité*, I, pp. xv, 34 ff., 37, 41 n., 42, 50, 93), the whole process being oriented by the demands of consumers as gauged by the "intermediary" producers and purchasers (I, 27); and (3) that the fundamental element in the establishment of "equilibrium, not only between the enterprises of agriculture and of industry in general, but also between all individual enterprises," is the flow of resources in response to differences in the rates of return obtainable in these various enterprises (I, 49), since production, while it proceeds ultimately in response to consumers' demand, will cease, or be diverted elsewhere, whenever expenses of production are not covered (I, 96 f.). Nor is the similarity to Walras ended even with that recognition of "the idea of interdependence" which was involved in Isnard's insistence that "the prices of things depend not only on their own abundance or scarcity, but also on the abundance or scarcity of other products which have relations of utility or homogeneity with them" (I, 28). On the contrary, this similarity extends even to such details as the statement of price relations first in terms of pairs of commodities, then in terms of three commodities, and finally in terms of one commodity chosen to act as what Isnard himself called "a *numéraire* or common measure" (I, pp. xiv, 18 f., 21 f.), as well as to other details that cannot even be summarized here. It need be observed only that, so far as the monetary aspects of Isnard's "system" are concerned, he not only introduced money at the same stage of analysis and in the same way as did Walras (see Isnard's *Traité*, pp. xiv, 22 f.), but was quite aware that it was of the utmost "interest" for the whole circulatory process that money should not be hoarded, if the "amount of money proper to the circulation" was not to reach very large heights (I, 267 f.).

did if Adam Smith had shown himself more sympathetic to Quesnay's *Tableau* than he was.[15] Yet, as Professor Cannan observed, one of the central ideas of the *Tableau* (and therefore of the idea of an economic "circuit") came into the Smithian system by way of Smith's own concept of the price of commodities as resolvable into income-"shares" which then become available for subsequent expenditure.[16] Much the same thing must be said of J. B. Say. For if Say's praise of Quesnay was not explicitly directed toward the ideas underlying the *Tableau*, it is none the less true that at least two of Say's contributions to economic theory are to be regarded as implicit extensions of the *Tableau's* central idea: namely, (1) the concept of the *entrepreneur* as the central agent in the "mechanism of the distribution of incomes"; and (2) that aspect of the "Law of Markets" which, by insisting that "all sellers are buyers," amounted, when properly interpreted, to a reminder that production generates incomes, and is in turn generated by the disbursement of incomes.[17]

If, finally, these examples of an implicit recognition of the concept of a "circular flow" are regarded as involving too forced an interpretation of the "classical" economists, there are other examples that can be cited to indicate that the immediate followers of these "classical economists" would have had no difficulty in recognizing the resemblance of the concept of a "circular flow" to the framework of "classical" analysis. For it was Francesco Ferrara, characterized in our own day as "one of the last great classical economists," who not only (1) described

[15] See the well-known reference to Quesnay's *Tableau* in Smith's *Wealth of Nations,* Book IV, Chap. IX (p. 643); and cf. the comment by Schumpeter, "Epochen der Dogmen- und Methodengeschichte," *loc. cit.,* 44.

[16] See Cannan's Introduction to his edition of the *Wealth of Nations,* p. xxxiii (p. xli of the Modern Library edition); and see also pp. 185 ff. of the same author's *History of the Theories of Production and Distribution.*

[17] Again it should hardly be necessary to emphasize that the defense of the Law of Markets implied in this proposition is not to be taken as a defense of all the uses (and abuses) that have been made of the Law, in all the varied formulations and equally varied contexts in which it has appeared. For Say's estimate of· Quesnay, see Say's *Cours,* 567 ff.; and for his treatment of the rôle of the entrepreneur in the "mechanism of the distribution of incomes," see, in addition to Part V, Chap. II of the *Cours,* Say's comments on the entrepreneur as the "principal agent of production" (p. 47) and as the purchaser and seller of "productive services" (p. 55). The place of Say's treatment of the entrepreneur in the historical development of the concept of the "circular flow of economic life" may be judged when it is remembered that it provides a link (1) backward to Cantillon (cf. Higgs's edition of the latter's *Essai,* 388 f.), who in turn had influenced Quesnay (see above, p. 353, n. 12); and (2) forward to Walras, whose treatment of "the function of the entrepreneur" was characterized by Edgeworth as "his next most important contribution to the stock of economic ideas" after his "discovery" of the principle of marginal utility. See the references to Edgeworth given on p. 183, n. 77 of my article, "The Monetary Aspects of the Walrasian System," *loc. cit.*

the "succession of exchanges" as "a compact system in which each element depends on the other like the links of a closed chain," but (2) literally reënforced his repeated use of the word "circle," in describing the process of exchange, by a series of diagrams in the actual form of a "circle," or a series of "circles"; the substance of the argument, as Ferrara himself suggested, being also (3) capable of representation by a system (*complesso*) of algebraic "equations" which it is not too far fetched to conceive of as having adumbrated the later Walrasian model.[18]

[18] See Ferrara's Preface (1864) to Volume IV of the *Biblioteca dell' Economista*, Second Series, pp. xiii ff., especially pp. xiv and xviii (pp. 86 ff., 90 of the *Oeuvres Economiques Choisies* of Ferrara, as translated and edited [1938] by G. H. Bousquet and J. Crisafulli). On the similarity of Ferrara's sketch of the economic process to the Walrasian picture of "general" economic equilibrium, see the comments by Bousquet on pp. 26 f., 35, 38, 40, 90 n., 91, n. 2, of the *Oeuvres Economiques Choisies; also* the often quoted statement by Pareto (*Manuel d'économie politique,* 240) to the effect that none of the "non-mathematical economists" had gone as far as Ferrara in the direction of stating the conditions for "general" economic equilibrium. No one, of course, could affirm that Ferrara's "circles" correspond in all details to the picture of the Walrasian system given by Walras himself. One misses, for example, the emphasis upon the central rôle of the entrepreneur which was so characteristic of the Walrasian "system" (see the references given at the end of the preceding note; and cf. Bousquet's comment on Ferrara's treatment of the entrepreneur in the *Oeuvres Economiques Choisies,* 28, n. 1). Yet it would be equally impossible to deny that Ferrara's exposition was remarkable in several respects on its own account. Striking, for example, in view of Schumpeter's later use of the Walrasian "circular flow" as a starting point for analysis of the *cyclical* process, is the fact that Ferrara presented his picture of an economic "circuit" in "equilibrium" as a first step toward an understanding of the disturbances in the economic process which may lead to "crises" (*Biblioteca dell' economista, loc. cit.,* pp. xii ff.; *Oeuvres Economiques Choisies,* 84 ff.). Striking, also, in view of the irresponsible statements made in recent years with respect to the rôle of *money* in "general" equilibrium analysis, is Ferrara's explicit introduction of money into his economic "circuit" (*Biblioteca dell' economista,* p. xviii; *Oeuvres Choisies,* 90 f.), with the result that he was able to discuss without difficulty the interpretation and the consequences of the fact that the money may be held up in its progress through the circuit (*Biblioteca dell' economista,* pp. xxx ff.). And equally striking, finally, are those aspects of Ferrara's exposition which have led even admirers of the Walrasian system to suggest that, by emphasizing the mutual interdependence of economic phenomena in *time,* Ferrara actually went beyond "the school of static economic equilibrium" (so Bousquet, pp. 40, 91 n., of the *Oeuvres Economiques Choisies*). Cf., for example, the remarks by Ferrara on his "circuit" as connecting "the present with both the past and the future," *Biblioteca dell' economista, loc. cit.,* pp. xxviii f.; though see also what is said concerning the relevant aspects of the Walrasian system below, p. 359 ff., 417 ff. On Ferrara as "one of the last of the great classical economists," see Bousquet, *Essai sur l'Evolution de la Pensée Economique* (1927), 98; and cf. the remarks by the same author on pp. 48 f. of the *Oeuvres Economiques Choisies.*

It was in a paper designed to popularize certain elementary "truths" of the classical "political economy," likewise, that Fleeming Jenkin presented his own picture of the "closed circuit" of economic life, and illustrated it with a quaint diagram prophetic of Simon Newcomb's later picture of the flow of "societary circulation."[19]   The latter, indeed, is of particular interest for our present purpose; for it was Newcomb (himself, despite Mr. Keynes's suggestion to the contrary, an avowed defender of traditional economics as "an established body of principles") who presented, both diagramatically and in words, a picture of the "flow of the *currency*" within a "social organism," the merits of which Professor Schumpeter has summarized in his own chapter on "The Circular Flow of Economic Life" by the statement that "the circular flow of money is nowhere more clearly described" than it is in Newcomb's work.[20]

Yet when all is said, it is Léon Walras who must be given credit for having presented the best picture—"the most complete in its simplicity

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[19] See Jenkin's *The Graphic Representation, etc.,* 143 ff. The diagram referred to is on p. 150. Cf. Newcomb, *Principles of Political Economy,* 318. The similarity between the two would have been even more marked if (1) Jenkin, in providing his quaint representation of "little rudimentary people standing upright," had provided a graphic representation of his earlier suggestion that we "imagine lines from man to man with arrowheads to show which way the goods travelled" (p. 144); and if (2) he had been more explicit in suggesting that his "circuits" were also intended to represent the flow of *money payments,* or (as Edgeworth interpreted Jenkin's use of "the physical metaphor of a 'closed circuit'") were intended to show how "money travels in one direction, goods in another" over the circuit (cf. Edgeworth's article on Jenkin in Palgrave's *Dictionary of Political Economy,* II, 473).

[20] See Schumpeter's *Theory of Economic Development,* 46, n. 2, and the reference there given to Newcomb's *Principles.* For a judgment of Newcomb's attitude toward the "classical" economists, it should be sufficient to consult (1) Newcomb's own Preface to his *Principles;* (2) his comments on those who suggest that "we must either reject or completely reconstruct the science" of economics (p. 33); and (3) passages such as the first paragraph in his Appendix to Book IV, on "The Relation of Demand for Commodities to Demand for Labor," in which Newcomb called attention to his general purpose to present "only such a body of doctrine as is generally accepted by all economic reasoners who have completely mastered the subject" (p. 434). Contrast the characterization of Newcomb's *Principles* by Mr. Keynes (*Treatise,* I, 233 n.) as the type of work which may be expected from a mind "not perverted by having read too much of the orthodox stuff." It should hardly be necessary to add that the concept of a "circular flow of money" is as common in the writings of avowed heretics—N. Johannsen's *Kreislauf des Geldes* and Foster and Catchings' "circuit flow of money" are examples—as in the writings of the avowedly "orthodox." The point made in the text is merely that the concept *was* to be found, even before the rise of the Walrasian influence, in writers avowedly, or generally regarded as being, in the "classical" tradition.

and the most grandiose"—of a functioning economic process of the type which Professor Schumpeter has called the "circular flow of economic life." [21]   It is the more necessary to stress the validity of Walras's claim, in this connection, because of the misrepresentation, or lack of appreciation of the relevant implications of his analytical system, even by writers generally regarded as followers of Walras.[22]   Specifically:

1. It is clear that justice is not done to the implications of the Walrasian system for the problem of the generation and utilization of money income when the discussion of the "income" aspects of the system is confined to a derivation of the magnitude of "income" from (1) a consideration of the value of the "quantities [of all commodities] which the individual possesses," or the "money value of the *wealth* of the individual," at the beginning and the end of a given act of exchange; and (2) the use of the resulting expression, "Value of goods purchased = Income." [23]   For such a derivation conveys virtually no conception

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[21] The characterization of the Walrasian system in the terms quoted in the text is that of Bousquet, in his Introduction to the *Oeuvres Economiques Choisies* of Ferrara, p. 26.

[22] It should hardly be necessary to emphasize that not *all* economists "generally regarded as followers of Walras" have been guilty of the type of misrepresentation or lack of appreciation of the implications of the Walrasian system which is illustrated in the following paragraphs of the text. See especially, in this connection, what is said above, pp. 111 f., concerning Schumpeter's treatment of the Walrasian "system." It remains true, nevertheless, that even writers who have shown an adequate appreciation of *some* of the particular implications of the Walrasian system which are most important for the purpose in hand, have been blind to others of these implications. Thus, from Pareto's description of "production and circulation" as forming a "circle" (see, for example, Pareto's *Manuel*, 376), it is clear, to be sure, that he was aware of the fact that the Walrasian system did describe the functioning of a *process in time* (cf. above, pp. 111 f., and paragraphs (2) and (3) of the text, below). Similarly, on behalf of Cassel's paraphrase of the Walrasian system, it must be said that, by emphasizing the relation between the determination of *prices*, on the one hand, and, on the other, the determination of the *incomes* of the sellers of the products and services to which these "prices" attach, it emphasized the time-"process" aspect of the system (see, for example, Cassel's *Theory of Social Economy*, 147 f.). Yet it is also true that neither writer showed a real understanding of the "*monetary* aspects of the Walrasian system." See my article under the later title, *loc. cit.*, 152 ff., 185 f., and the references there given; also what is said above, p. 339, concerning the unfortunate consequences, for an understanding of the rôle of money in the Walrasian system, of Cassel's conception of the monetary element as a "multiplicative factor."

[23] See Schultz, *The Theory and Measurement of Demand*, 29 ff., 39 ff. From the context, it is fairly clear that Schultz was really interested, not in the effects of price formation *upon income-formation* (that is, upon the generation of income), but in the effect of *income*, however generated, upon price-formation. This, of course, is merely another way of saying that Schultz's method of deriving the equation *Value of goods purchased =*

of what is in many respects the central idea of the Walrasian circuit flow: namely, the fact that "the money proceeds realized from the sale of goods are regarded as being returned, through entrepreneurial purchases of the services of the factors of production, as incomes to these factors, these incomes being then again expended in further purchases of goods." [24] Still less does it convey an appreciation of the fact that the derivation of the magnitude of the "income" generated by the "circular flow" is possible on the basis of a direct *summation* of the results of each clock-time "period," without the introduction of the concept of an "average or typical period," or "average" of situations in all of which certain functions are held to be invariant.[25]

2. It should be equally evident that a clear apprehension of the implications of the Walrasian system would have made unnecessary the suggestion that "a period is a stream of moments in time." [26] For while

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Income is such as to obscure the simple facts with respect to the generation of income which are revealed when the "Income" in question is the income of the *seller,* rather than of the *purchaser,* of the goods thus "purchased."

[24] See above, p. 352.

[25] Contrast Schultz, *Theory and Measurement of Demand,* 30. Actually, of course, if we begin with the nth of a series of clock-time "periods" each of the length t, the "income" of an individual over a clock-time period of the length xt, for example, is given by the expression

$$(PT)_{I \cdot t_n} + (PT)_{I \cdot t_{n+1}} + (PT)_{I \cdot t_{n+2}} \ldots + (PT)_{I \cdot t_{n+x-1}} .$$

(It should be pointed out that the simple idea thus involved (namely, that of *summing* the realized results of a given clock-time period in order to obtain a picture of the realized results over a *longer* clock-time period) has been presented in terms involving the use of "time period" subscripts, by some of the best-known sponsors of "equations of exchange" of the Fisherine type. In a sense, of course, it is implicit in the necessary specification of the "time period" to which such equations are held to apply. In this connection, see the references to Lubbock, Norton, Evans, and Roos given in Volume I, 65 n., 69, of the present work. For a particularly explicit use, however, of time-period subscripts of the kind indicated above, see Fisher's *Purchasing Power of Money,* 355 ff., 358 ff.) This is not to say, of course, that Walras himself never assumed the invariance of certain magnitudes in dealing with certain *particular problems*. See, for example, his *Eléments,* 215, 259 f. It is worth noting, however, that he was quite explicit in insisting that one could pass from a "static" to a "progressive" analysis by a consideration of the interrelations of successive time-"periods," between which the significant changes in data may be assumed to have occurred (*Eléments,* 260). On the relation of "clock-time periods" to those types of "period" used in "sequence analysis" which rest upon the assumption of constancy in certain of the data, see below, pp. 373 ff.

[26] So E. S. Shaw, "False Issues in the Interest-Theory Controversy," *Journal of Political Economy,* XLVI (1938), 839, n. 3. The author was, of course, not discussing the implications of the *Walrasian system* for the particular problem with which the article was concerned; and there is no suggestion here that the specific use made of the definition of a "period" quoted in the text is unjustified for the purposes of the particular problem

this statement can certainly be justified as a method for dealing with certain types of economic problems, it does not convey that concept which, it is here argued, represents one of the essential reasons for regarding the Walrasian system as complementing analysis of the Marshallian particular-demand-schedule type. [27] This concept, it may be repeated, is that the passage from one discrete, or "momentary," situation to another is effected by a stream of money payments, representing the payment and receipt of those prices which may be said to be arrived at on the basis of economic calculations undertaken, to be sure, at different "moments," but linked to each other by one central fact of experience: namely, the fact that the prices realized at these different "moments" are themselves related to each other in time as components of related streams of money receipts, on the one hand, and expenditures out of these receipts, on the other. [28]

with which the article in question was concerned. The point is merely that a similar definition of a "period" must be regarded as having been implicit in the argument of writers who have not faced squarely the problem of relating discrete analysis of the type associated with devices such as the Marshallian particular demand schedules, on the one hand, to "process analysis" of the type for which the concept of a "circular flow" can be shown to be useful, on the other hand. In this connection, cf. the comments of Lundberg, *Studies in the Theory of Economic Expansion,* 2 f. Since, moreover, "equilibrium" analysis has sometimes been identified with analysis referring only to a "moment" of time, attention should be called also to what is said below, p. 410, n. 10, with respect to the proposal to "treat a process of change as consisting of a series of temporary equilibria."

[27] It should hardly be necessary to stress the fact that the concept described in the following sentence of the text is not the *only* reason for regarding the Walrasian "system" as complementing analysis of the Marshallian particular-demand-schedule type. There is, after all, the much more familiar type of argument which is summarized above, pp. 166 ff., as to the relation between the two bodies of analysis. Yet there are grounds for believing that the usual emphasis upon this more familiar argument has obscured the type of complementary relation indicated in the text. See, for example, what is said on this matter below, p. 362, n. 32, in connection with the statement that "the prices explained in general equilibrium theory refer only to a single moment."

[28] In monetary terms, the relations involved are of course those indicated by expressions of the type presented in Volume I, p. 383, n. 88, which establish the relation between the "realized" prices of the "period" t_n and those of the "period" t_{n+1} by two distinct analytical steps. The first is the summation of these prices into aggregates representing, respectively, (1) payments *into* the income (or other money receipts) of period t_n (that is, $(PT)_{I \cdot t_n}$ or $(PT)_{NI \cdot t_n}$), and (2) payments *out* of this income (or other monetary receipts) in the period t_{n+1} (that is, $(PT)_{i \cdot t_{n+1}}$ or $(PT)_{ni \cdot t_{n+1}}$. The second analytical step is the establishment of an eco-

3. Our interest in the nature of the calculations held to lie behind the market actions of economic individuals has sometimes led to a type of exposition according to which, "seizing a moment when the two streams [of money and of goods, respectively] are running smoothly and steadily (corresponding to the condition of static equilibrium), we imagine them, in effect, to be suddenly congealed." [29]   This does not mean, however, that the usefulness of the Walrasian concept of a "circular flow" is dependent either (1) upon its application to "the condition of static equilibrium," or (2) upon the light it throws upon the calculations we impute to economizing individuals on the assumption that the two streams are suddenly "congealed." For (1) the concept of a "circuit flow," in the sense indicated by our Proposition XIX, is completely independent of any assumption with respect to whether the system is or is not in "static equilibrium." [30]   And (2) the conceptual "congealing" of the two streams is easily supplemented by a type of analysis designed to deal with the kind of economic calculation likely to be undertaken in the face of the fact that the "streams" in question are not "congealed," in the real world, but are continually changing in magnitude and direction.[31]

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nomic relation between the price-aggregates of the two clock-time periods by means of factors such as the administration of cash balances within the period t_{n+1} .

[29] The passage quoted is from A. A. Young, "Some Limitations of the Value Concept," *loc. cit.*, (p. 208 of Young's *Economic Problems New and Old*). On Walras's own interpretation of his occasional use of the device of supposing that the flows involved in the economic process are suddenly "congealed" (Walras's word was *arrêté*), see Walras's *Etudes d'économie politique appliquée*, 336; and cf. W. Jaffé, "Unpublished Papers and Letters of Léon Walras," *Journal of Political Economy*, XLIII (1935), 205.

[30] In this connection, see what is said above, pp. 112 f., 113; and cf. Walras's own comments on the "static" and "dynamic" aspects of his system, in his *Etudes d'économie politique appliquée*, 336. The reader may be reminded that Walras himself made use of the particular type of analysis indicated on p. 118 of the present volume: namely, that which would relate a "dynamic" monetary process, such as that typified by "forced saving," to "events within the 'circular flow' as the latter may be supposed to function" before the "dynamic" process of "forced saving" is inaugurated. In this connection, see what is said above, p. 315, concerning the place of Walras in the history of the concept of "forced saving," and the reference given in n. 201 thereto; and cf. also what is said below, pp. 428 ff., 433 ff., concerning the relation of the concept of an "equilibrium of the *system*" to the analytical apparatus outlined in the present work. The reader may be reminded also that Francesco Ferrara made use of what amounts to the concept of a "circular flow of economic life" precisely in connection with the analysis of the "dynamic" phenomena regarded as leading to "crises." See above, p. 356, n. 18, and the references to Ferrara there given.

[31] The element in these "calculations" most stressed in recent years is, of course, the element of "expectation" in the face of the "uncertainty" that is bound to exist in a changing world. See, for example, Myrdal,

The suggestion, in short, that analysis running in Walrasian terms must necessarily be such as to "neglect" the "time factor" is extremely misleading.[32]   And the same thing must be said of all those statements with respect to the substance of "process" analysis which obscure the fact that

Prisbildningsproblemet och Föränderligheten, 21, and *Monetary Equilibrium*, 45 ff.; Lindahl, *Studies in the Theory of Money and Capital*, 36 ff.; and Lundberg, *Studies in the Theory of Economic Expansion*, 6, 246. It may be observed, however, that one of the things stressed by Francesco Ferrara in his discussion of the way in which emphasis upon the "circular" nature of the economic process may be regarded as providing a "re-tying *(rannodamento)* of the present to the future and the past" was precisely the fact that the "expectations" *(aspettative)* which might prevail at the end of a given "period of circulation" might be "disappointed" *(deluse)* in a "following period." See the *Biblioteca dell' Economista, loc. cit.,* pp. xxviii ff.; and cf. the reference given above, p. 356, n. 18, to Bousquet's comment on Ferrara's treatment of the "mutual interdependence of economic phenomena in *time.*" For an example of a description of phenomena "mutually interdependent" in time in which, as in the case of Ferrara, room is left for the factor of "anticipations as to the immediate future," but the main emphasis is put upon the "mutually interdependent" relations of flows of *realized money expenditure,* see Schumpeter, *Business Cycles,* 549; and on the general question as to the rôle to be assigned to "expectations" in "process" analysis, and their relation to other elements in such analysis, see what is said below, pp. 382 ff.

    [32] See, for example, E. Lindahl, "Prisbildningsproblemets Uppläggning från Kapitalteoretisk Synpunkt" ("The Pricing Problem from the Point of View of Capital Theory"), *Ekonomisk Tidskrift,* XXXI (1929), 35 *(Studies in the Theory of Money and Capital,* 277; see also p. 33 of the latter work). Cf. also Myrdal, *Monetary Equilibrium,* 16, where it is stated that "the prices explained in general equilibrium theory refer only to a single moment," and Lundberg, *Studies in the Theory of Economic Expansion,* 19, where it is implied that "an equilibrium system of the type described by Walras" is concerned only with the "question of general interdependence during *one* period." It may be added that the confusion to which such statements are likely to give rise is not lessened by the fact that they by no means always refer to the same things. In some cases, for example, the statement that "the complications due to the time factor have been neglected" is taken to mean no more than that phenomena such as "saving" and those associated with "capital" generally are ignored, or that the introduction of such phenomena represents an "inconsistency" in the analytical system (see, for example, Lindahl, *Studies in the Theory of Money and Capital,* 277 n.). In other cases, the alleged "abstraction from the time factor" refers to nothing more than an exclusive concern with "a *reiterating* process." (So Lindahl, *op. cit.,* 33 [italics mine]. On the sense in which such a *"process"* can be said to "abstract" from, or "eliminate" the "time factor," see what is said above, p. 112, n. 53.) In still other cases (as, for example, in the statement that "the prices explained in general equilibrium theory refer only to a single moment"), all that would seem to be involved is an emphasis upon *one* aspect of the Walrasian system, at the expense of the particular aspect in which we are interested here (on this matter, see above, p. 360, n. 27, and the backward references there given). It should hardly be necessary to labor

the Walrasian system was itself a description of an economic "process" unfolding itself in time.[33]

4. The statement that "Walras tells us nothing concerning the monetary circulation in its relation to enterprise" is completely misleading, if not entirely false, as a description of the substance of the Walrasian system as Walras himself presented it.[34] What is more important, however, is that statements of this type are extremely likely to block the way to an adequate appreciation of the nature of both (1) the crucial rôle played by money payments, in a fully developed money economy, in bridging the gap between otherwise discrete pricing situations; and the nature of (2) the analytical devices, provided by a monetary theory deriving ultimately from Walras, by which we are enabled to go much further than he himself went in constructing a picture of the generation and utilization of money income that will do justice to the complexities of the real world.[35]

the point that such statements have little in common beyond the fact that they all contribute to a misapprehension of the rôle that can be played by the central idea underlying the Walrasian concept of a "circular flow of economic life" in the construction of an analytical system designed to describe economic processes as they unfold themselves in *time*.

[33] Again it must be pointed out that the very concept of a *"process,"* whether it is conceived of as a "reiterating" process or a "cumulative" one, necessarily involves the concept of *time*. It is difficult, indeed, to believe that this fact could have been obscured if there had been wider acceptance of Professor Schumpeter's characterization of the Walrasian system as describing, among other things, the "circular *flow* of economic life," with all that the very concept of a "flow" necessarily involves with respect to the use of time *periods*. (See above, p. 112, n. 54; and cf. also Schumpeter's use of the concept of an "economic period" [*Wirtschafts-periode*] in his discussion of the "economic circular flow" as the latter appears in the Physiocrats ["Epochen der Dogmen- und Methoden-geschichte," *loc. cit.*, 39].) It is equally difficult to believe that this fact could have been obscured if more writers could have been induced to refer to the "Walrasian concept of the economic *cycle* of equilibrium" (so, for example, G. H. Bousquet, on p. 90, n. 2, of his edition of Ferrara's *Oeuvres Economiques Choisies*). The whole question involves, of course, a very large number of the issues usually discussed under the head of the relation between "statics" and "dynamics," and particularly the conception of "dynamics" as *including* the whole of economic "statics." On these matters, see what is said below, pp. 450 ff. Yet if anything is certain, it is that Walras himself did not regard the difference between "statics" and "dynamics" as residing in the fact that only the *latter* is concerned with the analysis of a "process" unfolding itself in time. See again Walras's *Etudes d'économie politique appliquée*, 336.

[34] The statement quoted is from G. H. Bousquet, *Institutes de Science Economique*, III (1936), 110. For the evidence, on the other hand, upon which my own statement in the text is based, see my article, "The Monetary Aspects of the Walrasian System," *loc. cit., passim,* but especially the references to Walras's *Théorie du crédit* given on p. 184, nn. 79 and 80, of the article cited.

[35] See especially, in this connection, Proposition XXI, below (pp. 365 f.).

XX. From the very fact that the Walrasian "circuit flow" is essentially a description of a process whereby realized prices are resolved into money incomes or other money receipts, which are then expended in the realization of further prices—the receipt (realization) of which again amounts to the generation of expendable "income" or other money receipts—it follows that our whole picture of the Walrasian process *is capable of translation into a series of "stream" equations of the general form* $MV = PT$.[36] The point to be observed here, moreover, is that the successive steps of the Walrasian process not only are *capable* of translation into these terms, but *must* be translated into these terms, if justice is to be done to the complexities of the process of the generation and utilization of money incomes as the process unfolds itself in the world we know.[37] For it is no belit-

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[36] This follows directly, of course, from our Proposition IV (above, p. 263). See especially, in this connection, above, p. 266, n. 99, and the references to Walras there given. The particular "stream" equation representing the "realization" of the price of a particular commodity (A), for example, would be of the form $D_A = (MV)_A = p_a q_a$, in which D_A and $(MV)_A$ would represent the realized money demand for commodity (A). The "dating" of these realized transactions would then be accomplished by specification of the period $(t_n, t_{n+1},$ and so on), in which each transaction was realized, so that we obtain a series of equations of the form $D_{A \cdot t_n} = (MV)_{A \cdot t_n} = (p_a q_a)_{t_n}$. And the total transactions in commodity (A) "realized" within a clock-time period of the length xt would be obtained by summing the corresponding terms in the equations for each period, in the manner indicated above, p. 359, n. 25. The equations, on the other hand, for the expenditure of a particular *individual* or firm upon a particular commodity or group of commodities are obtained by the use of subscripts of the type employed by Fisher and Evans (see above, p. 320, n. 5). When, moreover, account is taken of the possibility, provided by an intelligent use of "partial" equations of the general Fisherine form, of relating these expenditures by individuals or firms to the forces determining the administration of cash balances held by these individuals and firms, as consumers and "traders," respectively (in this connection, cf. the reference to Walras given in Volume I, p. 406, n. 46), the reader will understand why I am prepared to accept gratefully the characterization of the analytical apparatus presented in this work as a combination and extension of "the classical theories of Fisher and Walras" (cf. Charles Rist, in the *Révue d'économie politique*, LIII [1939], 598). On the specific devices to be used for representing that part of the Walrasian system which is concerned with the "description of the process whereby realized prices are resolved into money incomes," and for representing the process of the generation and utilization of money income generally, see Proposition XXI (below, pp. 365 f.).

[37] The general proof of this contention is provided by our Propositions VII to XI and XIV to XIX (above, pp. 280–318 and 323–363), as well

tlement of the Walrasian achievement to point out that the general map which Walras provided of that process was one in which only the most general contours of the economic landscape were presented.[38]

XXI. No framework for the close study of the *monetary* aspects of this landscape, and in particular of the factors determining the magnitude and the direction of the flow of money-spending power through this landscape, has yet been provided which compares in scope with "stream" equations of the general form $MV = PT$, when these equations are subjected to the further elaboration and development of which they can be shown to be capable.[39]   Of these elaborations, the most important, apart from the subdivision of the "total transactions equation" into "partial" equations of the type indicated above under Propositions XII and XIII, are

as by Propositions XXI and XXII below. The multiplicity of ways in which the significance of this general proof can be *illustrated*, however, can be gauged only upon the basis of a consideration of further applications of the type indicated below, pp. 470 f., 509 ff., as well as on the basis of the treatment of specific problems involved in the generation and utilization of money income, of a kind that I hope to present in the near future.

[38] Cf. the letter of Walras to D'Ocagne of May 10, 1891: "I consider my work . . . simply an incomplete sketch. I hope that in the near future it will be superseded by other work more complete and better done" (quoted by Jaffé, "Unpublished Papers and Letters of Léon Walras," *loc. cit.*, 201). No true disciple of Walras could ever wish his own work to be regarded in any other light than as an "incomplete sketch," which he, too, hopes will be "superseded by other work more complete and better done." Since, however, I have referred to the Walrasian achievement as a map giving the "general contours of the economic landscape," I venture to refer the literal-minded reader with a flair for studying "economic landscapes" to the kind of relief map of the pricing aspects of the "economic landscape" which is provided by the physical model of the pricing "mechanism" sketched below, pp. 479 ff.

[39] This statement will be accepted or rejected according to whether one accepts or rejects that part of the analysis presented in Volume I of this work which was designed precisely to evaluate the relative merits, for this purpose, of the familiar Quantity Equations and developments thereof, on the one hand, and the various "frameworks" that have been proposed as alternatives to these equations, on the other hand. Here, therefore, it is necessary to make only two further observations. The first is that the fitness of formulations of the general Fisherine type for dealing with "time sequences," on the one hand, and the *structure* of prices and output, on the other, must be judged upon the basis of "elaborations" of the type described in the following sentence of the text, and not merely in terms of "the *simple* forms of the quantity theory [read: quantity equations]" which had been presented by earlier writers (contrast B. Ohlin, in his otherwise suggestive article, "Till frågan om penningteoriens upp-

represented by (1) the distinction between payments into income and payments which do not enter income; (2) the distinction between payments into income or other money receipts, on the one hand, and the subsequent disbursements out of income or other monetary receipts, on the other, which is made possible by the use of "clock time period" subscripts; and (3) recognition of the fact that the relation between payments *into* and *out of* income or other money receipts, in terms of both magnitude and timing, is given primarily by a study of the forces determining the administration of cash balances—that is, is a problem of Fisherine "velocity." [40]

XXII. The fact that the "time period subscripts" thus employed are *"clock"* time period subscripts which are attached to magnitudes actually *realized* in the market means that the apparatus here proposed is not subject to the lim-

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läggning," ["On the Question of the Formulation of Monetary Theory"], *Ekonomisk Tidskrift,* XXXV [1933], 62 f.). The second observation is that one of the principal reasons for the superiority claimed in this work on behalf of stream equations of the general form $MV = PT$ as a "framework" is precisely that, properly understood, such a "framework" *includes* the best of the alternative "frameworks." This will be appreciated at once by those sympathetic to the argument of Volume I of this work with respect, for example, to the relation between the framework provided by the Quantity Equations, on the one hand, and, on the other, by the "cash balance approach" of "old" Cambridge and the various "income" approaches to the Theory of Prices, including the masterly synthesis of Hawtrey (on the latter, in particular, see Volume I, pp. 407 f.). Indeed, the only important type of alternative "framework" which was regarded in Volume I as *not* capable of inclusion within a framework of the type provided by an adequate understanding of the meaning and purpose of the familiar Quantity Equations was that represented by formulations of the type of the Fundamental Equations of Keynes's *Treatise;* and this was because of the fatal limitations which I believe to attach to this type of formulation (see Volume I, 109 ff., 127 ff., 271 ff., and also what is said below, pp. 436 ff. On the framework proposed by Mr. Keynes in his *General Theory,* see below, Chapter Fourteen.

[40] See above, p. 114, n. 59, and the forward references there given. The statement that the "velocity" involved is the "Fisherine" velocity is of course intended to remind the reader that the development of the particular analytical devices indicated in the text was itself an outgrowth of a critical examination of the concept of *"income velocity"* when the latter is regarded as capable of direct application to problems requiring close examination of processes involving decisions of economizing individuals as those decisions are effected within a given institutional setting. On this matter, see Volume I, 364 ff., of the present work, as well as what is said below, pp. 694 ff,

itations upon the usefulness of so-called "period analysis" which are alleged to follow from the definition of a "period" either (1) in terms which do not make clear the relevance of the phenomena envisaged by the "period" to *realized events,* or (2) in terms which require the *holding constant* of processes other than the one selected for study.[41] In each "period," every factor that in any way affects the magnitudes determining the amount and the direction of realized money expenditure is given the value which it obtains as the result of any number of realized processes that may be unfolding simultaneously.

There can be no question of presenting here a detailed examination of all the issues raised in recent years in connection with the general concept of "period analysis"—if for no other reason, because such an examination would involve a detailed consideration of problems which are either largely factitious in themselves (as in the case of the problem of the relation between something called "Saving," on the one hand, and something called "Investment," on the other), or would require for their adequate treatment an excursion into areas only remotely connected with the problems with which we are here concerned.[42] What follows, therefore, must be regarded as merely an attempt to clarify some of the

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[41] It should be clear that this simple statement is not to be taken as implying the issuance of an *ukase* against the attachment of "time period subscripts" to other than *realized* magnitudes, or to indicate other than "*clock*-time" periods. See, for example, the ingenious system of notation used by A. G. Hart, "Failure and Fulfillment of Expectations in Business Fluctuation," *loc. cit.,* 71, and the same author's "Consumption Markets," *American Economic Review,* XXVIII (1938), Supplement, 123 ff. The point of our Proposition XXII, and of the discussion which follows in the text, is merely that the apparatus here outlined is such as to bring the whole of our analytical equipment to bear upon the explanation of that *phase of economic reality which is represented by the realization of market events in clock ("historic") time;* and that it is therefore not open to a type of criticism which might fairly be directed against examples of "period analysis" of which this cannot be said.

[42] An incidental advantage of the procedure here followed, it will be observed, is that it avoids identifying the question of the usefulness of something deserving to be called "period analysis" with that of the usefulness of a particular *type* of "period analysis" introduced by a given author in connection with a particular type of problem. In this connection, see what is said below, pp. 382 ff., under (4), concerning the relation of the so-called "method of expectations" to "period analysis." The same thing could also be said, obviously, of any discussion of the meaning of "period analysis" which would identify it only with the range of problems discussed under the head of the relation between Saving and Investment.

simplest of the methodological issues involved, by way of justifying the substance of our Proposition XXII:

1. *The history of "period analysis."* If one were to take literally the statements made in recent years with respect to the history of "period analysis," one would suppose that nothing deserving to be called "period analysis" can be found in economic literature prior to the writings of D. H. Robertson and the "younger" Swedish economists.[43] The absurdity of such a suggestion is best seen, however, if one substitutes for the expression "period analysis" either (1) the expressions which have been regarded by the younger Swedes themselves as the equivalent of "period analysis" (namely, "sequence analysis," or "process analysis"); or (2) Mr. Robertson's own expression, "the step-by-step method of analysis."[44] For the latter expressions bring out much more

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[43] See, for example, A. P. Lerner, "Saving Equals Investment," *Quarterly Journal of Economics,* LII (1938), 309. In justice to Mr. Robertson, it must be said that he himself has nowhere, to my knowledge, asserted that the substance of his "period analysis" is essentially "new." On the contrary, he has on occasion gone so far (even if one suspects in the comment a sly animadversion upon the facility with which "revolutionary" methods are discovered these days) as to characterize the "methods of thought" underlying his own "more explicitly temporal method of analysis" as "old fashioned" ("Notes on Mr. Keynes' General Theory of Employment," *loc. cit.,* 172). Similarly, it must be said, on behalf of some of the Swedish economists, that by presenting certain aspects of Wicksell's analysis of a "cumulative process" as an example of "period-" or "sequence analysis," they have opened the way to a more adequate appreciation of how long the history of "period analysis" really is. See, for example, Lundberg, *Studies in the Theory of Economic Expansion,* 45 f., 52 ff., and Lindahl, *Studies in the Theory of Money and Capital,* 166 ff.; and cf. also the somewhat less explicit references to Wicksell in Ohlin, "Some Notes on the Stockholm Theory of Savings and Investment," *loc. cit.,* I, 55, 58, and II, 234. Yet it must be admitted that the taking of *Wicksell* as the starting point has hardly done enough to dispel the general impression that, apart from Wicksell as interpreted by the "younger" Swedish economists, "period analysis" is essentially a product of our own generation. In this connection, see also the reference to Lange given below, p. 370, n. 47.

[44] The expression "sequence analysis" is that of Lundberg (see, for example, his *Studies in the Theory of Economic Expansion,* 45 ff., 51 ff.). The expression "process analysis" is that of Ohlin ("Some Notes on the Stockholm Theory of Savings and Investment," *loc. cit.,* I, 58 ff., II, 234 ff.), though he also makes use of the term "period analysis" (see, for example, *op. cit.,* I, 55). The expression "period analysis," on the other hand, seems to have been due to Myrdal (*Monetary Equilibrium,* 43 ff.). For Mr. Robertson's characterization of his own method of analysis as "the step-by-step method," see his "Saving and Hoarding," *Economic Journal,* XLIII (1933), 413, and his "Notes on Mr. Keynes' General Theory of Employment," *loc. cit.,* 186. I may add that, some years before the appearance of any suggestion that "revolutionary" significance must be held to attach to a concern with something called "sequence analysis," I had ventured to characterize the whole of the relevant sector of received monetary theory

clearly the basis for a proposition that is fundamental to any attempt to specify the substance of "period analysis": namely, that what it undertakes to provide is a description and explanation of the successive "steps" involved in any sequential process in time.[45]

When the problem is put in these terms, we see at once that the subject matter of "period analysis" is nothing more nor less than the subject matter of those branches of economic "dynamics" which have been concerned precisely with the tracing of successive steps in *any* economic time-consuming "process." [46] And this, in turn, can mean only that "period analysis," in a significant and important sense of the term, is as old as a very large part of economics itself.[47] It is to be

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as being concerned with problems of "mechanism and *sequence.*" See my article, "Hawtrey's *The Gold Standard in Theory and Practice,*" *Quarterly Journal of Economics,* XLII (1927), 141, 144.

[45] It is of some importance to observe that what is involved is not *only* "description," in the sense of an account which would fail to establish the causal links between the successive steps in a given process, but also "explanation," in the sense of a detailed account of *why* the successive steps in a given realized process are what they are, in the face not only of what has happened but also of what is expected to happen. The matter is of importance, if for no other reason, because of the not infrequent suggestion that close interest in the sequence in which realized events are "registered," particularly when this "registration" is stated in terms of "clock" time, can result only in a type of description which "explains nothing." See, for example, Ohlin, "Notes on the Stockholm Theory of Savings and Investment, I," *loc. cit.,* 58. Actually, of course, the provision of an accurate description of the sequence of registered events (amounting, as it does, to a close concern with the *mechanism* of economic processes) not only is perfectly *consistent* with an attempt to provide a reasoned explanation of why the economic process functions as it does, but represents an essential ingredient in such an explanation. In this connection, see the comment of Professor Ohlin himself, cited below, p. 371, n. 49, on the theory of *international capital transfer,* which may be taken as a model of the type of "sequence analysis," running in terms of successive "registrations" of realized events, to which reference is made in the text. On the relation of the general apparatus presented in this work to "causal" explanation of the successive steps involved in a given "process," see also what is said below, pp. 473 ff.

[46] It may be observed that Mr. Robertson himself has regarded his "step-by-step method," which, as we have seen, he has characterized as representing an "explicitly temporal method of analysis," as typical of " 'dynamic' monetary analysis." See his "Notes on Mr. Keynes' General Theory of Employment," *loc. cit.,* 172, 186, n. 7. On the fitness of the general apparatus presented in this work for dealing with problems of "dynamics" generally, see what is said below, pp. 451 ff.

[47] In a fundamental sense, indeed, it may be said to be considerably older than those parts of economics which have been regarded as particularly "classical": namely, those parts which have been concerned with the establishment of the conditions for, and the description of the functioning of a system in, a state of "equilibrium." In a day, therefore, in which so much is made of the necessity for "new" construction within the

found, for example, in all those exercises in "monetary dynamics" which undertook to trace the successive steps in the process whereby an addition to the stock of money may be expected to work out its effects upon the scale and structure of prices and output.[48]   It forms, likewise, the

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field of what is loosely called economic "dynamics," it is well to be reminded that one of Comte's major generalizations with respect to what he regarded as the unsatisfactory state of the "sociology" of his own day (including, of course, the economics of the "classical" economists) was precisely that the greater interest attaching to "dynamic" problems had resulted in their being "better understood" than the problems of economic "statics," which Comte regarded as by far "the more difficult and uncertain." See, for example, Comte's *Cours de philosophie positive,* IV, 278, 289, 294. (It is not without interest, moreover, to observe that, to Comte, the central idea of "dynamics" was essentially that which has been assigned to "period-" or "sequence-analysis": namely, to conceive of each situation in a sequence [*"succession"*] of situations "as the necessary result of the preceding situation and the indispensable motive force [*moteur*] of the following situation"; so that we may say that "social dynamics studies the laws of sequence" [*Cours,* IV, 292; cf. also IV, 326 of the same work].) Actually, of course, Comte's judgment implies as great an overstatement with respect to the relative amount of attention given by the "classical" economics to "dynamic" analysis as judgments frequently expressed by other writers imply an understatement of it; yet it does throw light on the "age" of economic "dynamics" generally, and therefore on the "age" of "sequence analysis," in any satisfactory sense of the latter term. Contrast, for example, the discussion of the work of Lundberg by O. Lange in *Economica,* New Series, V (1938), 246, where Lange supports his own contention that "the idea that economic theory should pre-occupy itself with a study of time sequences rather than of equilibrium positions . . . has already a small tradition behind it" by citing writers none of whom antedates Walras or Marx; and contrast also Lerner, "Some Swedish Stepping Stones in Economic Theory," *loc. cit.,* 589, where it is suggested that "the only way so far discovered of conducting real dynamic analysis" is that of "treating the dynamic process as a series of equilibrium positions."

[48] In this connection, see the comments on the substance of "monetary dynamics" in Volume I, 84, n. 30, 155, n. 35, 159 f., 307, 501, and the references there given. In the light of these examples of substantive "dynamic" analysis in the past, it is clear that Professor Ohlin is right in including not only Mr. Robertson, who has made *explicit* use of the concept of an economic "period" in tracing the successive steps involved in certain monetary processes, but also Mr. Hawtrey, who has not couched his "process analysis" in just such terms (Ohlin, "Some Notes on the Stockholm Theory of Savings and Investment, II," *loc. cit.,* 234. It may be observed that a similar attribution to Mr. Hawtrey of an important place in the history of "process-" or "sequence-analysis" is implied by writers such as Lundberg, even though Lundberg has not explicitly included Hawtrey's name in a list of authors who made use of the device of a "model sequence." For if the claim of *Wicksell,* for example, to be included in such a list rests upon his "analysis of *cumulative processes*" (Lundberg, *Studies in the Theory of Economic Expansion,* 52; italics mine), then an equal claim must be entered on behalf of Mr. Hawtrey,

very heart of the theory of the mechanism of international capital transfer.[49]   And it is represented by all those contributions to the theory

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who has not only been consistently concerned with the *substance* of analysis of "cumulative processes," but has actually made use of the *term* "cumulative process" to describe the subject matter of his major interest. See, for example, Hawtrey's *The Art of Central Banking,* 167). It should be equally clear, however, that Professor Ohlin is wrong in excluding the whole of "the Mises-Hayek school" from his list of those who have presented a "process theory" (*op. cit.,* 236). For the analysis, by members of "the Mises-Hayek school," of the "successive steps in the process whereby an addition to the stock of money may be expected to work out its effects upon the *structure* of prices and output" is nothing if it is not an example of "process analysis," in the sense indicated in the text. Professor Ohlin's suggestion, therefore, that the members of this school were prevented from developing a "process theory" by their addiction to "orthodox equilibrium constructions" is unjust not only to members of this "school" other than Hayek, in whose version alone a special "equilibrium construction" plays a marked rôle, but also to Hayek himself, in view of the fact that the "equilibrium construction" in question was employed precisely as a means for bringing out the contrast between the type of situation envisaged by such an "equilibrium construction," on the one hand, and the type of "process" in whose description Hayek was particularly interested, on the other. On this aspect of Hayek's argument, see also what is said below, p. 372, n. 50; and on the relation of "equilibrium constructions" generally to the type of analytical apparatus outlined in the present work, see what is said below, pp. 427 ff.

[49] The comments by Professor Ohlin on "the theory of international capital movements," in the *Economic Journal,* XLVIII (1938), 498 ff., are extremely instructive in this connection. Starting (p. 498 n.) from a definition of "dynamic" analysis as that which "is relative to time, i.e., deals with *successive* stages," he cites, in laudatory terms, as attempts "to do something approaching" what he has in mind in the field of international trade theory, the work of Professors Viner and Taussig (p. 500, n. 1); and he adds the obvious comment that "the two outstanding conservative proponents of the orthodox theory of international trade" have thus "proved themselves very 'modern.'" What is difficult to understand, however, is the relevance of Professor Ohlin's further comment that the two authors cited "have in these investigations made very little use of their static concepts, methods, and conclusions." For, apart from the accuracy of this statement in itself, what is really relevant is whether those parts of their analysis which Professor Ohlin rightly regards as admirable examples of "dynamic" or "process" analysis are or are not based upon elements in the received ("classical") theory of international capital transfer. For if they are so based, these older ("classical") elements automatically take their place in the history of the construction of an adequate "dynamic" or "process" theory. And on the question, surely, of the relation of their own analysis to the older analysis, neither of "the two outstanding conservative proponents of the orthodox theory of international trade" has left any doubt. The same thing must be said of M. Fanno's *Normal and Abnormal International Capital Transfers* (1939), in which the concern shown throughout with the "successive stages" involved in the "process" of international capital transfer justifies completely the inclusion of the

of the trade cycle which have been concerned with the process by which
we pass from a state of depression to one of prosperity and again to
depression.[50]   For in' all these cases the description runs in terms of a

monograph in a series of *Studies in Economic Dynamics* (cf. the Editors'
Introduction, p. vii), even on Professor Ohlin's own description of the
substance of "dynamic" analysis.

[50] It is, indeed, a striking fact that virtually all of those who have
regarded "sequence analysis" as something of a novelty, or as having at
best only a "small tradition" behind it, have agreed that business-cycle
theory has been concerned, explicitly or implicitly, with precisely the type
of analysis now designated as "sequence analysis." See, for example,
Ohlin, "Some Notes on the Stockholm Theory of Savings and Investment,
II," *loc. cit.*, 230, 234 f.; and also the reference to O. Lange given above,
p. 370, n. 47.   By this very statement, obviously, the substance of "sequence
analysis" is made at least coeval with that of business-cycle analysis.   The
reader need only be reminded, therefore, that all of the abler writers on
the history of business-cycle theory have pointed out that the attempt to
explain the phenomenon of the cycle goes back at least to the beginning
of the nineteenth century; so that the charge that "economic theory" has
made no attempt to provide a type of analysis whch would deal with the
"successive stages" of the cyclical process and their interrelation in time
is on a par with the charge that "economists" generally have made no
attempt to deal with the *monetary* aspects of the economic process.   For
in both cases the charge rests upon a demand that a body of analysis
designed to explain *one* aspect of economic reality be applied directly in
problems for whose solution it was not intended; and in both cases it
has involved the absurd contention that if the further analytical devices
required for the explanation of other aspects of economic reality are
brought into the picture, their "incidental" introduction means either (1)
that the authors concerned regarded these additional devices as of little
importance in themselves; or (2) the intrusion of such devices into a setting
in which they have "no business to turn up."   On these matters, see what
is said above, pp. 6 ff., 74 ff., and especially the reference to Keynes's
General Theory on p. 76, n. 61.   It must be admitted, to be sure, that
economists have occasionally been guilty of claiming more explicative
value for "equilibrium theory" than is warranted.   In our own day, for
example, Hayek may be charged with having been guilty of just this
practice (see, for example, his *Monetary Theory and the Trade Cycle*,
28 f., 33 n., 42 ff., 52 ff., 59 n., and especially 68 ff., 76 ff., 81, 83 ff., 95 ff.).   It
should be observed, however, that while the comment just made convicts
Professor Hayek of having dismissed, too cavalierly, partial explanations of
the cyclical process other than his own, it does not convict him of having
himself provided an explanation of that process which uses only the weapons
of equilibrium analysis as the latter appears in treatises on "general" eco-
nomic theory (cf. what is said on this matter above, p. 371, n. 48).   It is
certainly not true, at any rate, that all other writers have been equally
guilty in claiming more explicative value for "equilibrium" analysis than
is warranted.   On the suggestion that the very existence of a special body
of "cycle analysis" demonstrates the uselessness of "equilibrium" analysis
for explaining the sequential processes realized in the world we know, see
what is said below, pp. 424 ff.

sequence of events in which each event is the result of the events of the preceding "period" and a factor determining the events of the "period" which follows.[51] To be "skeptical," therefore, of the "usefulness" of analysis running in terms of a description of the steps involved in a process realized over "successive periods," on the ground that such analysis necessarily tends "to complicate or confuse matters rather than clarify them," is to express skepticism with respect to the usefulness of areas of economic analysis which are so firmly established that one can only ask (1) in precisely what way "analysis in terms of successive periods" has tended to "complicate or confuse" rather than "clarify" the issues involved even in the incomplete list of examples specified above; and (2) what sort of analysis, if not "analysis in terms of successive periods," it is proposed to substitute as a method for dealing with the specific subject matter indicated by these examples.[52]

2. *Period analysis and the constancy of the data.* From the description of the substance of "sequence analysis" just presented, it follows that there is no inherent reason why the very concept of "period analysis" should be associated with a "holding constant," within any given "period," of processes other than the particular process chosen for study. For if it be asked what "period analysis" is, if it is not a device for keeping certain factors "constant" in tracing a process in

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[51] See the characterization of "sequence analysis" given by Lange, in *Economica* for May, 1938, p. 244; and cf. Robertson's description of his own "step-by-step method" of "analysis of processes of change" as a method which amounts to "starting again at each point in the light of all that has gone before" ("Notes on Mr. Keynes' General Theory of Employment," *loc. cit.,* 186). With both of these statements should be compared Comte's description of the content of "dynamics" (quoted above, p. 370, n. 47) and Francesco Ferrara's insistence upon a type of analysis which would "re-tie" the present with both the past and the future (see above, p. 356, n. 18, and p. 362, n. 31).

[52] The expression of "skepticism" quoted in the text is from A. P. Lerner, "Saving Equals Investment," *loc. cit.,* 309. Cf. also the same author's "Some Swedish Stepping Stones in Economic Theory," *loc. cit.,* 591, where the hope is expressed that "somebody" who is "secretly ahead of the Anglo-Saxons" may have "discovered how to apply process analysis to real problems." It should be clear that expressions of "skepticism" of this type have little in common with the often quoted exclamation of Mr. Robertson ("Notes on Mr. Keynes' General Theory of Employment," *loc. cit.,* 186, n. 7) as to "whether much practical fruit can ever be hoped for" from "sequence analysis" of the type for which he is so well known. For Mr. Robertson's modesty should not be allowed to obscure the challenge implied in his further question whether "anything can exempt us" from "sequence analysis" of *some* kind "if we are bent on a thorough analysis of processes of change" *(loc. cit.);* and the contention in the text is that Mr. Robertson's critics have neither met this challenge nor evidenced any awareness of the range of problems that would have to be covered in any attempt to show that we can in fact get along without "sequence analysis" of some kind in our attempt to account for the processes of economic life.

time, the only answer that can be given on the basis of what has been said under (1) is this: that "period-" or "sequence-analysis" consists, or should consist, precisely of tracing and accounting for the development of realized processes in time by (i) *identifying the successive steps* in these realized processes; (ii) establishing the relation of each step to what has preceded and what follows; and (iii) in doing so, using all the weapons of economic analysis which help us to explain why the individual decisions which condition each step in the process are what they are.[53]

Our first task, therefore, in attempting to establish the relation of the "holding constant" of certain magnitudes to "period analysis," must be to ascertain the basis for the general impression that in fact the essence of "period analysis" is precisely the "holding constant" of certain processes for given "periods" of time. As it happens, one finds no explicit suggestion to this effect in the Appendix to Chapter Five of Robertson's *Banking Policy and the Price Level*, which must certainly be regarded as one of the primary documents for the "period analysis" of recent years.[54] On the contrary, the Robertsonian "day," which was then defined merely as a "finite but indivisible atom of time," seems to have been intended to represent merely what is called below an " 'analytical' *ex post* period," without any necessary assumptions with respect to the holding constant of certain factors.[55] The same thing must be said of Mr. Robertson's use of the concept of a "day" in his review of Keynes's *Treatise;* for the very fact that he was there prepared to translate his argument into the variables of an equation of the general Fisherine form, all of which refer to "clock" (historic) time, may be taken to indicate that his "period analysis" was intended to involve the "holding constant" of certain magnitudes only in the inevitable and fundamentally innocuous sense indicated below.[56]

If, therefore, Mr. Robertson is to be held responsible for the im-

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[53] On the importance of point (iii), in particular, for any judgment as to the usefulness of the analytical system outlined in the present work, see what is said below, pp. 471 ff. It should be clear, also, that the point involved bears directly on the question of the extent to which a concern with the sequence in which realized events are "registered" is consistent with an attempt to provide a "causal" explanation of the economic process. Cf. what is said on this matter above, p. 369, n. 45, and the forward reference there given.

[54] See pp. 61 ff. of the first edition of the work cited (cf. pp. 59 ff., of the third [revised] impression [1932]).

[55] See below, p. 394. From the discussion there presented, it will be observed that the "analytical" character of an " 'analytical' *ex post* period" does *not* reside in the fact that certain elements are assumed to have been "held constant" over the period. It resides, rather, simply in the fact that the criterion of the length of the "period" is not a fixed amount of "clock" time, but the amount of clock time necessary to complete certain processes.

[56] See p. 378, below; and cf. Mr. Robertson's comments in the *Economic Journal*, XLI (1931), 402 f.

pression that "period analysis" necessarily involves the "holding constant" of certain processes for the duration of a given "period," it must be because of the usage to be found in his article "Saving and Hoarding." [57]   Even here, to be sure, this impression need not have been conveyed by the definition of a "day" that he gives.   For although this definition of a "day" ("a period of time . . . so short that the income which a man receives on a given day cannot be allocated during its course to any particular use") might be said to have involved formally the assumption of constant (in this case, *zero*) expenditure on a given "day" of funds received as income for that "day," any candid examination of Mr. Robertson's argument must result in the conclusion that he really meant no more than this: that income must first be *received* by person A *before* that same income can be expended by A.[58]   It happens, however, that Mr. Robertson, in order to "simplify" his analysis, proceeded to make further statements which, as Mr. Hawtrey insisted and Mr. Robertson himself admitted, involved the assumption of a constancy in certain of the quantities entering into the processes with which Mr. Robertson was concerned.[59]   And it may well be that, despite

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[57] *Economic Journal*, XLIII (1933), 399 f.

[58] See, for example, the concluding sentence of "Saving and Hoarding," *loc. cit.*, 413.   It may be observed, in passing, that the issues involved are among those involved in Hawtrey's distinction between "consumers' *income*," on the one hand, and "consumers' *outlay*," on the other, the difference between the two being determined chiefly by the "consumers'" administration of their cash balances.   See Volume I, 354 ff., and the references there given.   It will be seen, therefore, that the differences between Mr. Hawtrey and Mr. Robertson, in the discussion to which Mr. Robertson's article was a contribution, turned, not upon the importance of the distinction itself, nor even upon the question of the usefulness of "sequence analysis" as such, but solely upon (1) the extent to which it is necessary to formalize the point in question in the particular way in which it was formalized by Mr. Robertson, as well as upon (2) the extent to which the particular degree of "abstraction" involved in Mr. Robertson's assumptions is really necessary for the purposes of analysis of the functioning of the economic process generally, and is even permissible for the purposes of analyzing the particular processes with which the discussion was concerned.   Cf., in this connection, the comments by Mr. Hawtrey himself, in the *Economic Journal*, XLIII (1933), 706.

[59] In justice to Mr. Robertson, it must be remembered that he himself had admitted, in his original article, that the particular definition of a "day" there given was such as to "preclude us from considering the possibility of an increase in the velocity of circulation of money against output above that from which we happen to start" ("Saving and Hoarding," *loc. cit.*, 399).   The reason for this, of course, was that Mr. Robertson now explicitly *identified* his "day" with a period such that "velocity," in the sense indicated, would always be equal to 1 within any given "period"; and this, in turn, involved a specific assumption with respect to the rate at which "income velocity" would vary over a given "clock" time period of a length sufficient to permit the occurrence of the other events (such as changes in the volume of output) whose occurrence was traced over

Mr. Robertson's explicit abandonment of some of these assumptions at later stages of his argument, this accident may have contributed to the general impression that the essence of "period analysis" is the holding of certain elements "constant." [60]

It cannot be denied, on the other hand, that this impression has been strengthened as a result of certain modes of expression used by several members of the "younger" Swedish school, including some of those who have been generally regarded as outstanding exponents of "period analysis." We have been told by Myrdal, for example, that "period analysis . . . must assume most of the world unchanged and the rest changing in a very regularized way." [61]   Similarly, we have been told by

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several Robertsonian "periods." Mr. Hawtrey's main emphasis, on the other hand, was upon the fact that Mr. Robertson's *argument* (as opposed to his definition of a "period") assumed that "there is no accumulation of unsold output": in other words, that what has been called in this work the "rate of sale" of goods is equal to, and constant at, 1, not only within any given "period" as defined by Robertson, but also as between any pair of successive periods (cf. Hawtrey's comments in the *Economic Journal,* XLIII [1933], 703 ff., 708). Actually, of course, Mr. Robertson's argument, "simplified" as it was, involved certain other assumptions, such as that " 'Investment,' defined as expenditure on new instrumental goods, is zero, all net output consisting of consumable goods" ("Saving and Hoarding," *loc. cit.,* 401).

[60] For examples of Mr. Robertson's abandonment of certain of his assumptions in later stages of his argument, see his abandonment of the assumption of constant (= zero) expenditure in the purchase of new instrumental goods during the processes studied, and also his abandonment of the assumption that a "day" is always a period of such length that within it income velocity, in the sense in which he uses the latter concept, is always equal to 1 ("Saving and Hoarding," *loc. cit.,* 403 f., 405 f.).

[61] Myrdal, *Monetary Equilibrium,* 43 f.   Despite certain statements that have been made with respect to Professor Myrdal's attitude toward "period analysis" (see, for example, Ohlin, "Some Notes on the Stockholm Theory of Savings and Investment," I, *loc. cit.,* 55, 58), it should be pointed out that, so far as one can judge from Myrdal's published writings, such "skepticism" as he has expressed regarding the usefulness of "period analysis" has not been directed against the very idea of "period analysis" as such (on the ground, say, indicated by the proposition quoted in the text). On the contrary, he himself has characterized his emphasis on "the importance of clearly recognizing the period implicit in monetary analysis" as one of the "chief contributions" of his *Monetary Equilibrium* (cf. p. 47 of that work). Myrdal's published criticisms of "period analysis" seem, rather, to have been directed against the particular *type* of "period analysis" which leaves "dynamic problems . . . unsolved," by "concealing the changes between the timeless demarcation points between the periods" selected for purposes of analysis, in contrast with "dynamic analysis proper which refers to the *causal development in time* up to the next point studied." (*Monetary Equilibrium,* 44 f. [italics Myrdal's]. See also p. 122 of the same work; and cf. J. Åkerman, *Ekonomisk Kausalitet* [1936], 42 f.) It may be pointed out, moreover, that Professor Myrdal's comment with respect to the necessity for assuming "most of the world unchanged"

Lindahl that "in order to analyze . . . a dynamic process, we imagine it to be subdivided into periods of time so short that the factors *directly* affecting prices, and therefore also the prices themselves, can be regarded as *unchanged in each period*." [62] It is not altogether surprising, therefore, that to some commentators on the nature of "period analysis," it should appear that "the general methodological principle underlying the choice of a unit-period is that no significant change should happen within

carries *fewer* implications with respect to the supposedly necessary connection between "period analysis" and the holding of certain elements "constant" than does, say, the statement of Lindahl quoted in the following sentence of the text; for Myrdal's proposition, unlike that of Lindahl, does not *define* a "period" in such a way as to associate *it* with the absence of change. It would seem clear, in any case, that Myrdal's suggestion with respect to the assumption of "most of the world unchanged" is not to be taken as a criticism of *all forms* of "period analysis." On the contrary, it would seem to argue no more than is argued in the following paragraph of the text with respect to the rôle played by the holding of certain elements "constant" in those examples of "period-" or "sequence-analysis" which occupied a very substantial place in economic literature before the formalization to which the problem has been subjected in recent years; though the rest of Professor Myrdal's comments with respect to other forms of "period analysis" than that which he would seem to favor do raise the question whether all of this formalization has really served in every instance to clarify the major issues involved (see also, in this connection, the comments of J. Åkerman, *loc. cit.*). On the possibility of establishing a substantive reconciliation between Myrdal's position and that of Lindahl, see what is said in the following note.

[62] Lindahl, *Studies in the Theory of Money and Capital*, 158, 318. Cf. also pp. 52 and 62 f. of the same work, where the criterion for the ending of a given "period" is a change in "plans," which are thus assumed to remain unchanged *within* a given period; and for an example of a similar type of analysis, see Hicks, *Value and Capital*, 122 ff. (though see also what is said on this matter below, p. 392, n. 95). Any attempt to justify this type of "period analysis" must be prepared, of course, to meet objections such as those of Myrdal, cited in the preceding note. It may be observed here, however, that the difference between the two conceptions of "period analysis" largely disappears as soon as a method such as that of Lindahl is related to "clock" time periods (as it must be, if the argument of the present work is sound), in the manner suggested below under (5) (p. 384). For in that case, the "end" of a given "period" of the Lindahl type will necessarily be *dated* as a matter of "clock" time, instead of being "timeless," in Myrdal's sense of the term (see Myrdal, *Monetary Equilibrium*, 44). This very fact, in turn, will mean that the process of change, in a system such as that of Lindahl, instead of being "shut up within a timeless moment" (Myrdal, *loc. cit.*), will be registered by a *series* of realized changes, each of which will be "dated" in terms of "clock" time; so that the relevant stage in the particular realized "process" under examination can be represented as having occurred *within* a "clock" time period of a length sufficient to register that "*causal development in time* up to the next point studied" which Myrdal (quite rightly, in my opinion) regards as "dynamic analysis proper."

the period." [63]   And it may well be that statements of this kind have in fact seemed to "complicate or confuse matters rather than clarify them" by failing to bring out clearly the relation of the "holding constant" of certain magnitudes to the general purpose underlying "period analysis" altogether.

What this relation is, however, will easily be seen if we ask what significance attaches to the undoubted fact that, even in the earlier examples of "period analysis" indicated under (1), many of the authors concerned did hold certain magnitudes "constant" either explicitly or implicitly.   For the answer to this question can be only that (i) the very nature of *analysis* involves considering the effect of only one factor, or of only a comparatively small group of factors, at a time; but that (ii) it would be nothing less than absurd to suggest that these earlier writers would have denied that *after* analysis must come *synthesis,* which in this case means nothing more than that an attempt must be made to discover which of the several factors that "analysis" shows are *capable* of affecting an economic situation did *in fact* operate in a given concrete situation, in such wise as to give us a result representing the effect of a number of factors operating simultaneously.[64]   It can be fairly

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[63] So, for example, O. Lange, in his review of. Lundberg's *Studies in the Theory of Economic Expansion,* in *Economica* for May, 1938, p. 244.   (The particular passage in Lundberg's book on which Lange's generalization is based is presumably that on p. 49, instead of p. 79, as stated in Lange's review.)

[64] I am of course merely stating here in other terms the principle which Marshall characterized as "the foundation of the victory of analytical methods in many fields of science": namely, "that a study of the tendency to change, resulting from each several disturbing cause, might be made the starting point for a broad study of the influence of several causes acting together" (*Industry and Trade,* 678; cf. also Marshall's *Principles,* 380 n.). In effect, moreover, the two propositions stated in the text have been accepted by some of the writers whose names have been most closely associated with the use of *"ceteris paribus"* assumptions in the "period analysis" of our own day.   In connection with (i), see, for example, the comment of Lindahl, *Studies in the Theory of Money and Capital,* 59, on why "in general it is impossible for the economist to give a complete analysis of a complicated course of development *in one and the same exposition"* (italics mine); and cf. also the comment of Lundberg, *Studies in the Theory of Economic Expansion,* 170, on the rôle of *"ceteris paribus"* assumptions in business-cycle analysis generally.   (Both quotations, it may be observed, should dispose of the suggestion that the use of *"ceteris paribus"* assumptions are a peculiarity only of "static" analysis, and are not permissible in "dynamic" analysis, in any useful meaning of the latter term.   The fact that such assumptions are common to *both* "statics" and "dynamics" is correctly pointed out by Anderson, *The Value of Money,* 554.   Cf. also below, p. 380, n. 69.)   It should be pointed out, moreover, that Lundberg's statement, on p. 47 of the work cited, that "the development of the whole economy cannot very well be *explained* by summing up the results of partial theories" is not to be taken as a denial of the need for, or possibility of, the act of "synthesis" indicated under (ii); for the "partial theories" to

demanded of any analytical system that it be constructed in terms sufficiently detailed and comprehensive to permit observation of the effects of all possible controlling factors, in order that we may be able to account fully for any process actually "realized," in terms of the operation of any one or any combination of these controlling factors.[65]

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which he refers are not "partial theories" of the economic *process* (in the sense of a series of analyses designed to describe the functioning of certain "parts" of the process on the temporary assumption that no change occurs in other "parts" of the process), but refer only to "partial *equilibrium*" theories—which in themselves, it may be observed, are not necessarily identical with those "partial solutions" of economic problems generally, to which Marshall referred in his discussion of the implications of analysis based upon the assumption of *"ceteris paribus"* (see Marshall's *Principles,* 366). On the rôle of "partial equilibrium" analysis in the general analytical system outlined in the present work, see what is said below, pp. 408 ff. Here it is necessary only to point out that Lundberg may be regarded as implying full acceptance of the proposition stated under (ii) in the text when he insists that his own view "does not deny that a development consists of *all* the changes in every element of time" (*Studies,* 47); though it must be admitted that his failure to emphasize this need for ultimate *synthesis* as greatly as he stresses the need for *analysis* provides some excuse for the question, by certain of his critics, as to what the "connecting link" is between the "functional" ("model") sequences, on the one hand, and "the real sequences that one proposes to explain," on the other (so, for example, J. Åkerman, *Das Problem der sozialökonomischen Synthese* [1938], 221).

[65] It will be observed that the problem, as so stated, is broader than that involved in the older claims made for the use of mathematical devices as a means for avoiding the temptation "to treat *variables* as *constants*" (see, for example, J. N. Keynes, *The Scope and Method of Political Economy,* 262, and the reference to Edgeworth there given). For the temptation indicated may in many cases be much less serious than another temptation: namely, the temptation to avoid the labor of finding out, by actual observation of the functioning of the economic process, what the possible economic variables are. It is an open question, certainly, whether the mere setting down of algebraic symbols for a series of variables whose economic character and even identity has not been clearly understood has carried us as far on the road to an adequate understanding of the nature of the forces which make economic magnitudes what they are, as have those "partial" analyses whose very inadequacy to explain the whole of observed reality has in many cases led to the discovery of new variables which economic analysis can show to be capable of affecting the final result. From this point of view, the *ceteris paribus* assumptions of earlier versions of "the quantity theory," even when they were not explicitly recognized as "assumptions," must be regarded as having filled a *historic* rôle, even if their continued use *in this particular sector of monetary theory* can hardly be regarded as having the significance it once had. In this connection, cf. Lambert, *La Théorie quantitative de la Monnaie,* 148 ff., 189, 255 f.; but see also the comments in Volume I, 95 ff., of the present work, on the significance of the successive insertion of additional factors within the familiar "equation of exchange," as the result of recurring dissatisfaction with the

It is claimed, on behalf of the analytical system presented in this work, that it does permit just such observation.[66] · But there is nothing in this fact which would argue either (1) that the conceptual holding of certain elements "constant" for the purpose of constructing "model sequences" is *inconsistent* with a desire to deal with the complex "sequences" actually realized in the world we know; or (2) that the very concept of "period-" or "sequence-analysis" requires the holding of elements constant *at all stages of the investigation,* and becomes an entirely different kind of "analysis" if elements are not held "constant" throughout.[67]

3. *Constancy of data and "equilibrium" analysis.*[68]   It is only natural that the holding of certain data "constant" should have occurred most frequently in cases in which the author concerned was interested primarily in the determination of the *conditions of "equilibrium"; * for it is elementary that the conditions which are sufficient to establish equilibrium after the emergence of a factor creating an *initial* disequilibrium may not be sufficient to do so if a new set of disturbances should arise in the interim.[69]   There is in this fact, however, nothing which requires

"quantity theories," of differing degrees of crudity, that happened to prevail at given epochs in the development of our subject. It goes without saying that these successive elaborations have made our apparatus vastly more "complicated" than it once was. But that the complexities involved become "impossible" when the apparatus is "applied to any practical problem" is not only a statement without foundation in fact, but is one which could be advanced only by those whose ideal for the weapons of economic analysis is a degree of simplicity which could attach only to propositions that are both formally and substantively false. In this connection, cf. Mr. Lerner's comments on "the process method" generally, in his paper, "Some Swedish Stepping Stones in Economic Theory," *loc. cit.,* 589.

   [66] See what is said on this matter below, pp. 474 ff., 515 ff.

   [67] On the concept of a "model sequence," see Ohlin, "Some Notes on the Stockholm Theory of Savings and Investment, I," *loc. cit.,* 54, and especially Lundberg, *Studies in the Theory of Economic Expansion,* 51 ff.

   [68] For a fuller discussion of the rôle of "equilibrium analysis," of both the "partial" and the "general" types, in the analytical system here outlined, see below, pp. 406 ff. That discussion, together with the brief discussion which follows, should provide its own commentary upon the suggestion that "a way of using non-equilibrium *ex ante* process analysis" remains to be "invented" (so Lerner, "Some Swedish Stepping Stones in Economic Theory," *loc. cit.,* 590).

   [69] On this, one of the most elementary methodological principles of "equilibrium analysis," there has been no difference whatever as between protagonists of "partial" equilibrium analysis, on the one hand, and of "general" equilibrium analysis, on the other. Cf., for example, Walras's *Eléments,* 260, with Marshall's discussion of the assumption that "other things are equal," in his *Principles,* 36 f., 366, 379 n. It does not follow, however, that the use of formal assumptions with respect to the lack of variation, or the nature of the variation, of certain magnitudes is *in itself* enough to justify characterizing analysis based upon such assumptions as analysis applicable only to the processes of "a utopian world *in equilibrium*" (cf. J. Akerman, *Das Problem der sozialökonomischen Synthese,* 167). See, on the contrary, what is said below, pp. 427 ff.

that the "periods" with which "sequence analysis" is concerned should make the particular assumptions, with respect to the constancy of data, that are helpful for purposes of "equilibrium analysis." [70] From the argument of Proposition I, 4, above, for example, it should be clear that the entrepreneurial "plans" which are described by "equilibrium" analysis are important for the determination of *realized* prices only insofar as they lead to market action which can be translated into shifts of, or movements along, particular market demand or supply schedules, which in turn will alone be directly related to actually realized prices.[71] And from our earlier discussion of this argument, it should be equally clear that this conclusion is not overthrown by facts such as (1) that an intervening change of data may change the form or position of these market demand or supply schedules, and therefore market action, as between any two instances of realized prices; or (2) that, as a result of this intervening change of data, the original "equilibrium" position may never be attained.[72] Now, the "periods" which are ultimately important in any apparatus for tracing the "sequence" of the steps in *realized* processes are "periods" which record whatever successive changes in data, and therefore whatever realized consequences of successive changes in data, are found to occur in the world we know.[73] Even, therefore,

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[70] There is of course nothing in this statement which can be taken as justifying a literal acceptance of the charge, by Lundberg, that there is something "illogical" in starting from a given change in a given constellation of data and then attempting "to determine the adaptation of the other variables to an equilibrium" (*Studies in the Theory of Economic Expansion*, 44; cf. also pp. 3 ff. of the same work). The basis for such a procedure is in no respect less "logical" than the *"ceteris paribus"* procedure defended by Lundberg himself in connection with his concept of "sequence analysis" (cf. above, p. 378, n. 64). Indeed, from the methodological standpoint, precisely the same kind of procedure is involved in the two cases (see again, in this connection, the comments of Marshall on the implications of analysis based upon the assumption of *ceteris paribus*, cited above, p. 378, n. 64). The shortcomings of the writers criticized by Lundberg in the passage indicated derive, therefore, not from a lack of "logic," but from a possible failure to realize that, instead of having provided a complete explanation of the economic process, they have explained only a part of it. For the explanation of the rest of economic reality, other techniques must be applied, and other phenomena studied. This is all that is implied by the statement in the text.

[71] See above, pp. 231 ff.

[72] See above, pp. 236 ff.

[73] On the suggestion that a concern with the establishment of realized sequences is unnecessary, or futile, on the ground that their establishment "explains nothing," see above, p. 369, n. 45, and the forward references there given. It should hardly be necessary, moreover, to labor the point that the description, given in the text, of the periods which are "ultimately" important for the purpose there indicated does not preclude the use of "periods" within which, as a matter of conceptual construction, certain factors are held constant, in order to study at closer range the effects of a particular factor or particular group of factors which can be shown to be

from the standpoint of those who would insist upon the relevance of the corpus of "equilibrium" analysis for the explanation of *certain* aspects of the economic process, there is no reason why the use of the concept of a sequence of "periods" should prevent our defining a "period" in such a way as to make it helpful in illuminating aspects of the functioning of the economic process *other* than those to an understanding of which "equilibrium analysis" can be shown to have made definitive contributions.[74]

4. *"Period analysis" and "expectations."*   It is not uncommon to find, in recent literature, statements concerning the substance of "period analysis" such as to suggest that the "method of expectations" (or, less formally, an emphasis upon the importance of introducing into economic analysis a discussion of the effects of "expectations" and changes therein), on the one hand, and "period analysis," on the other, are essentially one and the same thing.[75]   If, however, one accepts the

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capable of affecting the final result. Cf. the argument presented on p. 378, above.

[74] From this proposition, as well as from the preceding note, it should be clear that an adequate apparatus for dealing with the causes and consequences of economic change must be prepared to make use not only of "periods" of differing *length,* but also of different *kinds,* in the sense that they are intended to illuminate different aspects of the general problem of the explanation of economic change. See what is said below, p. 384, under (5).

[75] It may be observed that Mr. Robertson himself has not gone beyond the tentative suggestion that his own "step-by-step method" "seems to *bear some relation to* the more elaborate Swedish 'method of expectations'" ("Notes on Mr. Keynes' General Theory of Employment," *loc. cit.,* 186 n.; italics mine). And indeed in some cases the usage of certain of the Swedish writers whose names have been associated with the device of "period analysis" has been such as to suggest either (1) that they regard their work as representing a *combination* of Robertsonian "period analysis" with an emphasis upon the importance of expectations (see, for example, Ohlin, "Some Notes, etc., I," *loc. cit.,* 55, and cf. the similar comment by J. Åkerman, *Das Problem der sozialökonomischen Synthese,* 155); or (2) that they regard their emphasis upon the element of expectations, in the face of "uncertainty," as constituting "*a necessary preparation for* period analysis" (so Myrdal, *Monetary Equilibrium,* 45; italics mine). See also below, p. 383, n. 77. On the other hand, it cannot be denied that the special interest in the element of "expectations" evidenced by the Swedish writers has often led other writers, avowedly under their influence, to use a mode of exposition such as to suggest that the sole *raison d'être* of "period analysis" is to be found in its association with the so-called "method of expectations." See, for example, Hicks, "Mr. Keynes' Theory of Employment," *loc. cit.,* 241; and cf. also E. S. Shaw, "False Issues in the Interest-Theory Controversy," *loc. cit.,* 856. Indeed, the excessive enthusiasm thus born of a discovery (or, more accurately, rediscovery) of the importance of "expectations" for economic analysis has even led some of these writers to imply an identification of the "method of expectations" (particularly as applied to situations in which "uncertainty" prevails), not only with

statement made under (1) as to the substance of "sequence analysis" in earlier economic literature, it becomes clear that this identification represents at best an unwarranted identification of the part with the whole. Insofar, to be sure, as expectations and changes therein condition and are conditioned by the changes registered by the successive steps in realized economic processes, they are *part* of a body of "sequence analysis" which would undertake to explain why these successive steps are what they are.[76] They are not the *whole* of that body of analysis, however.[77] The considerations, therefore, which may recommend the

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the whole of "period analysis," but actually with the whole of "Economic Dynamics"! See, for example, B. Thomas, *Monetary Policy and Crises* (1936), 68, 102. On the relation between "period" analysis and *"ex ante"* analysis, see below, pp. 389 ff.; and on the reasons for refusing to regard "expectational" analysis as coextensive even with "ex ante" analysis, see above, p. 178, n. 71, and p. 180, n. 73.

[76] For examples, chosen in order to illustrate the fitness of the analytical system here outlined to deal with the influence of "expectations" at all stages in the pricing process, see below, pp. 454 ff.

[77] It is again only fair to the Swedish group as a whole to observe that they have been less guilty of a type of exposition calculated to convey the impression that analysis in terms of "expectations" constitutes the *whole* of "sequence analysis" than have other writers, such as those cited above, p. 382, n. 75. In addition to the references to Ohlin and Myrdal given in that note, see the comment by Lundberg on the necessity for linking "anticipations" with *realized* processes (or, as he puts it, "economic results" [*Studies,* 6]), with all that this must imply with respect to the necessity for tracing these "economic results" in terms of "unit-periods" the length of which will be determined by such "real" facts as, for example, the "long time of construction" of capital goods (*op. cit.,* 49). Yet it must again be recorded that even some of the Swedish writers have sometimes pushed a justified claim on behalf of the introduction of the element of "expectations" so far as to imply an unwarranted rejection of other types of sequence analysis than their own, when these other types happen to be concerned precisely with the "registering" of the successive events involved in the unfolding of an economic process; and they have done so on the ground that a tracing of these realized sequences "explains nothing, for it does not describe the causal or functional relations." In addition to the reference given above, p. 369, n. 45, see Ohlin, "Some Notes, etc., II," *loc. cit.,* 237; also, and especially, Lundberg's criticism of the analysis of Robertson and Durbin on the ground that "the sequence of changes in effective demand derived in this way cannot be regarded as a dynamic theory or as an explanation of a process," since, it is argued, "the successive changes are not causally explained in any way but are assumed from the outset to follow a given pattern"; so that "this method can only lead to a *registration* of events at dates given by the passing of the 'days' or 'transaction periods,'" the "real problems" being "excluded" (Lundberg, *Studies,* 67 f.). Actually, of course, the specific criticisms made by both Ohlin and Lundberg of the rôle played by "the 'injection of new money' idea" and the concept of "velocity," in the realized processes traced by "period analysis" of the Robertsonian type (cf. Ohlin, "Some Notes, etc., I," *loc. cit.,* 69; Lundberg, *Studies,* 127 ff., 132 ff.) evidence not

definition of a given "period" in terms of a given expectancy-horizon are not sufficient to support the implication that the very concept of an economic "period" must derive from a given type of analysis with respect to "expectations" and only from that type of analysis.[78]

5. *The various types of economic "period."* The considerations raised under points (3) and (4) suggest a proposition which should always have been regarded as self-evident: namely, that the "periods" involved in economic analysis are of several *types,* and that each type has its use.[79] This proposition, it may be observed, is much broader than the

only (1) a serious underestimation of the importance of tracing these realized changes in the "quantity" of money and in the rate and direction of its use; but also (2) an almost complete lack of understanding of the relation of this "tracing" analysis to the body of explicative doctrine which is always at hand to explain *why* these realized changes are what they are. Again, therefore, it must be insisted that this further body of doctrine, while it certainly *includes* the element of "planning" and "expectation," includes a very much more extensive area of analysis, whose full substance is not even remotely suggested by the mere statement that we must introduce into the discussion an "analysis of plans of producers and consumers with regard to their acts of production and consumption" (Lundberg, *Studies,* 67). There is considerable point, consequently, to the criticism of "the modern theory of anticipations" by J. Åkerman, *Das Problem der sozialökonomischen Synthese,* 153, on the ground that while it has dealt at length with the problem of "how a future pricing situation affects an earlier one through anticipations," it has not dealt adequately with the problem of "how one pricing situation passes over into another"; or, to put the same point in another way, that it has avoided the task of "explaining how the path between *ex ante* and *ex post* is established" (Åkerman, *op. cit.,* 154. Cf. also p. 270 of the same work; and see the similar comment by Haberler in the 1939 edition of his *Prosperity and Depression,* 189 n. [last paragraph]). For examples of the way in which such a "path" is established in the analytical system outlined in the present work, see, in addition to the argument presented above, pp. 224 ff., what is said below, pp. 392 ff.

[78] Contrast Hicks, in the passage cited above, p. 382, n. 75. It is perhaps not out of place to observe that the argument presented here against the identification of "period analysis" with the "method of expectations" should help to explain simultaneously (1) why Mr. Hicks should have feared, in the article cited, that the "whole method" of period analysis is in "danger, when it is applied to long periods, of . . . petering out"; and (2) why this conclusion by no means follows when "period analysis" is *not* identified with the "method of expectations," but is regarded as including the whole of that received body of "sequence analysis" the nature of which is indicated above (pp. 368 ff.), under (1).

[79] On the various types of economic period, see, for example, the discussion by O. Morgenstern, "Das Zeitmoment in der Wertlehre," *loc. cit.,* 440 ff., of the distinction between, and the mutual relations of, the "income period," on the one hand, and what is called the "economic period," on the other; and see especially F. Machlup, "Period Analysis and Multiplier Theory," *Quarterly Journal of Economics,* LIV (1939), 2, 6. To be sure, Professor Machlup's excellent analysis of the question of what "periods"

proposition that our "periods" must "be chosen of different *length* in dealing with different problems." [80]   For the latter proposition, in itself, might mean no more than that "periods" which are of the same *type* might be of different "lengths." [81]   It is of some importance, therefore, to establish with some precision both the *nature* of the various types of "period" used in economic analysis and the *relation* between these various types of period.

i. *"Analytical" time periods and "clock" time periods.*   No obscurity

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are "significant" for economic analysis makes no attempt to classify the "periods" involved into *methodological* "types." It is easy, however, to regroup his "significant periods" in terms of the classification here suggested. His "transaction" and "income" periods, for example, would seem to be what are called below (p. 394), "analytical *ex post* periods." His "planning periods" and "plan adjustment periods" would seem to be what are called below (p. 389) "analytical *ex ante* periods"; while his "equilibrium adjustment period" is either an analytical *ex ante* period, or an analytical *ex post* period, depending upon whether it is defined (as on p. 6 of the article cited) as "the time interval during which certain (predicted) adaptations to certain changes in data are *expected* to work themselves out" or is treated (as would seem to be the case on pp. 11 ff. of the same article) in terms of the time actually required to effect certain *realized* processes which are regarded as having led to the attainment of a position held to be one of "equilibrium." (The fact, it should be observed, that an "equilibrium period"—or "equilibrium adjustment period"—may be *either* an "analytical *ex ante* period" or an "analytical *ex post* period," depending upon how it is defined, means that the "method" underlying the concept of an "equilibrium period" does not *necessarily* represent "a theoretically inadmissible mixture of *ex ante* and *ex post* analysis." Cf. Myrdal, *Monetary Equilibrium*, 122, and the reference given in n. 9 thereto, to his earlier Swedish monograph. The same fact should make clear the misleading character of the suggestion that "a form of argument . . . which concentrates on the examination of final 'equilibrium' positions . . . can legitimately be criticized as too much *ex-post* and insufficiently *ex-ante*" [so Lerner, *"Ex-ante* analysis and Wage Theory," *loc. cit.,* 449].) Both the "plan adjustment periods" and the "equilibrium adjustment periods" of Professor Machlup, moreover, are in certain respects *ceteris paribus* periods (see below, p. 389); although this does not mean that *all* "conditions" are supposed to remain unchanged in either case (see, for example, pp. 5 and 22 of the article cited).

[80] So, for example, Myrdal, *Monetary Equilibrium*, 44; italics mine.

[81] It should be noted, moreover, that even the latter statement is capable of more than one interpretation. It might mean, for example, that a given type of "analytical *ex post* period" may be of differing "clock" time length under different circumstances (see below, p. 387). Or it might mean that there may be different *types* of "analytical *ex post* period." For examples of the latter proposition, see Koopmans, "Zum Problem des 'Neutralen' Geldes," *loc. cit.,* 301, n. 3; J. Åkerman, *Ekonomisk Kausalitet,* 44 (under point V); Lundberg, *Studies in the Theory of Economic Expansion,* 47, and the summary statement on p. 87 of the same work, with respect to the kinds of unit-periods represented in the particular "model sequences" subjected to examination.

exists as to the meaning of a "clock" time period—or, for that matter, of any of the expressions which have been proposed as equivalents of "clock" time, such as "historical" time, or "formal" time, or "conventional" or "calendar" time.[82] If there are difficulties in connection with the concept of an economic "period," these have attached to the concept of a "period" which is defined in terms of some kind of "time" other than "clock," or "historical," or "conventional," or "formal," or "calendar" time. In cases in which the writers concerned have shown an awareness of the fact that the "periods" used by the writers themselves or others are to be measured in some sort of "time" other than "clock" time, they have used phrases such as "imaginary" time, or "theoretical" time, or "operational" time, or even (paradoxically enough) "real" time.[83] It is clearly not sufficient, however, to define

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[82] For an example of the use of the term "clock time" in a context similar to the present one, see R. Opie, "Marshall's Time Analysis," *Economic Journal,* XLI (1931), 199 ff. For an example of the use of the term "historical" time, in a context in which this "historical" time is contrasted with "theoretical" time, and the latter is thought of as "a time which serves as an axis for a logical (and not merely historical) sequence of events," see Schumpeter, *Business Cycles,* 72, 138 n. The expressions, "conventional time" and "formal time," are used as equivalents of "clock" or "calendar" time by J. Åkerman, *Das Problem der sozialökonomischen Synthese,* 51 f.; while F. Machlup, in his "Period Analysis and Multiplier Theory," *loc. cit.,* uses both the term "clock time" (p. 7) and the term "calendar time" (p. 17).

[83] For examples of a use of the expressions "theoretical time" and "operational time," respectively, see the references to Schumpeter and Opie in the preceding note. The expression "real time" is used in a context in which it is differentiated from "formal" or "clock" time by J. Åkerman, *Das Problem der sozialökonomischen Synthese,* 52 f. "Real" time is apparently that which applies to what Åkerman calls "economic periods" (*op. cit.,* 52, 285), which in turn seem to correspond to what are called below (p. 394) "analytical *ex post* periods," the adjective "real" apparently being intended to connote the fact that the processes involved in these "economic periods" are "realistic" in the sense of being actually *realized* processes (see, for example, *op. cit.,* 52 f., 147, 173). A further distinction is implied by Åkerman as between these "real" periods, on the one hand, and entrepreneurial *estimates* with respect to the state of a given process in terms of these "real" periods, on the other (see, for example, Åkerman, *op. cit.,* 52 f.): in other words, this second distinction would seem to be essentially that which is here drawn between analytical *ex post* periods and analytical *ex ante* periods. The term "imaginary time," on the other hand, is used by Åkerman only in contrast to time measured on a "realistic" time scale (pp. 146 f., 262). It may be observed in passing, however, that "time" in the Böhm-Bawerkian sense, which Åkerman himself cites as an example of "imaginary time" (*Das Problem der sozialökonomischen Synthese,* 262; cf. the same author's *Om det Ekonomiska Livets Rytmik* [1928], 8) is *capable* of translation into Åkerman's "real time," if the Böhm-Bawerkian "time" is divorced from those contexts in which the statement that a given process is more "time con-

such "imaginary" or "theoretical" or "operational" or "real" time by the purely negative criterion that the "time" involved is *not* "clock" time. For the most that can be said for such a negative criterion is that a clear recognition of the fact that the "time" involved is *not* "clock" time is an obvious first step toward the attainment of clarity as to the meaning of the particular "periods" used, as well as toward establishment of a further, and quite fundamental proposition: namely, that every user of a type of time "period" other than a "clock" time period must be prepared to show precisely how the type of analysis based upon the use of other than "clock" time periods helps to explain events registered in "clock" time periods, which alone provide a record of the economic process as it functions in the world we know. It is necessary, therefore, to establish, in addition, a *positive* criterion for the definition of a "non-clock" time period (or, as it will be called in what follows, an "analytical" time period).[84] By way of establishing such a criterion, and at the same time providing a basis for relating these "analytical" time periods to "clock" time periods, the following definition of an "analytical" time period is proposed: an "analytical" time period is one whose length is taken *not* as a *fixed amount* of clock-time, but as of *whatever clock-time length is required for the completion of the particular process envisaged by the analysis.*[85]

It will be observed at once that this definition of an "analytical" time period does not make its "analytical" character depend upon whether or not certain factors are "held constant" during the "period" in question; and this is as it should be. For it is just as easy to state an argument involving the assumption of "other things equal" in terms of "clock" time as in terms of "analytical" time.[86] And if we are to have a basis

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suming" is intended to say no more than that more "capital" is being used. In this connection, see what was said by Akerman himself in his earlier *Om det Ekonomiska Livets Rytmik,* 29, 89, 154, 159, and *Konjunkturteoretiska Problem* (1934), 77, with respect to the relation of the Böhm-Bawerkian time "period" to Aftalion's "period of gestation," which Åkerman himself now cites as an example of a "realistically determined" period (*Das Problem der sozialökonomischen Synthese,* 147), in *contrast* to the "unrealistic" periods of Böhm-Bawerk.

[84] The term "analytical period" is used by J. D. Sumner, "Public Utility Prices and the Business Cycle," *Review of Economic Statistics,* XXI (1939), 102, though no definition is there given beyond the statement that such "periods" are "introduced simply as thinking devices."

[85] Cf. Opie, "Marshall's Time Analysis," *loc. cit.,* 199, where it is suggested that "operational" time should be defined "in terms of the economic forces at work." Cf. also R. H. Blodgett, *Cyclical Fluctuations in Commodity Stocks* (1935), 107, where "time" is said to be "used in the operational sense" when the "periods of time" involved "are described as being long enough for certain things to be accomplished."

[86] Cf., in this connection, Opie, "Marshall's Time Analysis," *loc. cit.,* 208, where certain parts of Marshall's analysis involving the use of "operational" (or, as we should say, "analytical") time periods are characterized as reducing merely to the "warning": "Take care to weigh all the forces

for differentiating a "clock" time period from an "analytical" time period, we should invoke a criterion which does not cut across the two types of economic "period." To be sure, we may speak, if we wish, of

whose aspects are observable in the clock-time under consideration." It is, in fact, quite easy to show that if any differences do arise as between a *"ceteris paribus"* statement in terms of "clock" time, on the one hand, and a statement, on the other, involving no specification of a given amount of clock time, this can be only because it is implicitly assumed to be safe to make certain factual assumptions with respect to the constancy of certain data in "clock" time periods of a given length, whereas such assumptions are unwarranted in the case of propositions which must be regarded as claiming more general validity precisely because they include no specification with respect to the period of clock time involved. Thus, empirical support might be found for the statement that, if there are no changes in the V and in the non-output components of the T of the Fisher equation of exchange, then changes in prices will be strictly proportional to changes in the quantity of "money" over a very short clock-time period (say, a calendar day); for such a period may in fact be too short for output to be increased. A statement, on the other hand, which would leave out the limitation with respect to clock time would often be found to be out of accord with the facts. The reason for the inadequacy of the second type of statement, however, is not that it is a "timeless" proposition (contrast J. Åkerman, *Economic Progress and Economic Crises* [1932], 15; and see also the similar statement by the same writer, in his *Das Problem der sozialökonomischen Synthese*, 167, with respect to the requirements of "analysis based upon the economics of time" [*zeitökonomische Analyse*] in dealing with the problems raised by the "acceleration principle"). The reason is simply that the statement itself is inaccurate, since it fails to take account of all the variables that may be involved over *all possible* periods of "clock" time. Actually, of course, the "timelessness" of a *correct* statement which tells us what may be expected to happen if certain elements are "held constant," but which does *not* specify the clock-time period involved, instead of providing a ground for criticism, is a ground for praise, for precisely the same reasons which justify us in characterizing as "timeless" a truth so profound that the passage of time cannot diminish its validity in any way. In this connection, see what is said in Volume I (p. 98) of the present work, concerning the historical development of the familiar Quantity Equations as registering the successive conversion of propositions "at best true only under definite assumptions" and at worst "generally and literally false," into propositions "that can be shown to be capable of the widest possible application and of passing the most exacting scientific scrutiny"—or, as we may say here, can be shown to be both true and useful over clock-time periods of any conceivable length. It will be observed, moreover, that these successive improvements were such as to make it unnecessary to *assume* that the "periods" analyzed with the help of the equation are "homogeneous" in institutional or other respects (cf. Åkerman, *Das Problem der sozialökonomischen Synthese*, 155, 183); on the contrary, the elaborations were such as to take account of the new elements whenever these new elements could be shown to affect the economic process, without invalidating the use of the equations for periods *prior* to the emergence of these new elements.

"*ceteris paribus* clock-time periods" and "*ceteris paribus* analytical-time periods," with "*ceteris paribus* clock-time periods," in particular, being contrasted with clock-time periods in which all magnitudes are allowed to vary freely.[87] We are still left, however, with the necessity for distinguishing between "clock" time periods and "non-clock-time," or "analytical time" periods; and for this purpose the criterion proposed above will be found to serve quite satisfactorily.

ii. *"Clock" time periods and "analytical" **ex ante** periods.* If, however, confusion is to be avoided in the use of "analytical" time periods; and if, at the same time, a satisfactory *modus vivendi* is to be established between "analytical" time periods, on the one hand, and "clock" time periods, on the other, then it is necessary to distinguish between two types of "analytical" period.[88] The first of these periods may be characterized as an "analytical *ex ante* period."

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[87] The use of "*ceteris paribus* analytical periods" is illustrated in the "model sequences" of the type referred to above, p. 380, n. 67 (cf. also the comment on Machlup's "equilibrium adjustment period," above, p. 385, n. 79). For the "periods" there used are not clock-time periods, and *are* "analytical time periods" in the sense that their length is determined by the amount of clock time required for the completion of particular processes (or of particular steps in particular processes) which are selected for study. They are also "*ceteris paribus*" periods, however; since in most cases definite assumptions are made with respect to the constancy, throughout the whole process, of factors other than the ones whose movements are selected for particular study. For an example of the use of a "*ceteris-paribus* clock-time period," see the preceding note. A "non-*ceteris-paribus* analytical time period" would of course be one in which several processes would be assumed to be going on concurrently. (See, for example, Professor Machlup's comment on the relation of his "transaction or income periods" to "assumptions regarding fixed plans or fixed propensities on the part of the persons involved" ["Period Analysis and Multiplier Theory," *loc. cit.*, 5].) But the length of these periods would be determined, not as a fixed amount of clock time, but as the amount of clock time required for the completion of any one of those processes in which the analyst happens to be interested: with the result, of course, that the various "analytical" periods may be expected to overlap each other in terms of clock time. The reader will observe that, by our Proposition XXII, the "periods" which are ultimately fundamental in the system proposed in the present work are "non-*ceteris-paribus ex post* clock-time periods"; and that therefore the system itself is one which may be called upon to perform the complicated tasks assigned to any analytical apparatus purporting to provide a genuinely comprehensive "causal analysis" of *realized events* (cf. Åkerman, *Das Problem der sozialökonomischen Synthese,* 273).

[88] It may be pointed out that, as a strictly formal matter, not only "analytical" time periods, but also "clock" time periods, may be either *ex ante* or *ex post* periods. See, for example, what is said on this matter below, p. 391, n. 92. As is argued below, however, our real problem is to relate all the periods used in our analysis to events *registered* within *ex post* clock-time periods; and the *method* of doing so (on which see below, p. 393) is the same in principle, whether the "*ex ante*" periods are supposed to refer to "clock" or to "analytical" time.

Such a period is involved whenever economizing individuals are said to make their "plans" with respect to a shorter or longer "period," the length of which is determined not as a fixed amont of clock time, but by the amount of clock time necessary, in the estimation of the planner, to bring about whatever conditions he is supposed to envisage.[89] Thus, it is possible for an entrepreneur to envisage a "long run demand schedule" for his product, in the sense of an expected response of quantity demanded to a series of assumed prices, given enough time for consumers to become accustomed to the product, to make whatever changes may be necessary in their expenditures upon other articles, and in general to overcome whatever obstacles may exist to the ultimately expected increase in consumption at a lower price.[90] Similarly, it is possible for an entrepreneur to envisage a "long run supply schedule" for his product, in the sense of a determination to carry out production plans of varying scope in response to assumed price-situations, given time enough to effect whatever adjustments may be necessary in order to increase production to the desired extent.[91] In both cases, the "long run" schedules may differ markedly from the relevant "short run"

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[89] The *formalization* of the concept of an "analytical *ex ante* period," or its equivalent, is of recent date. In this connection, see, for example, the reference to Professor Machlup's "planning periods," "plan adjustment periods," and "equilibrium adjustment periods" (in one of the aspects of the latter) given above, p. 385, n. 79, as well as the references to the use of the equivalent of Machlup's "plan adjustment period" by Lindahl and Hicks given below, p. 392, n. 95. Cf. also Haberler, *Prosperity and Depression*, 188, n. 2, of the second edition; and also what is said above, p. 386, n. 83, with respect to one aspect of J. Åkerman's "real" periods. The "equilibrium periods" to which Åkerman refers in his discussion of certain writers of the "Anticipations School" (*Das Problem der sozialökono-mischen Synthese*, 155) are likewise to be regarded as "analytical *ex ante* periods," though they would seem to be the equivalent of Machlup's "plan adjustment periods" rather than his "equilibrium adjustment periods," even when the latter are considered in their *ex ante* aspects. But while the *formalization* of the concept of an "*ex ante* analytical period" may be attributed to the influence of "the 'Anticipations School,'" the substance of the concept was certainly implicit in the argument of earlier writers. See, for example, the discussion by H. Mayer of the varying "dispositions" made by economic individuals in the face of given expectations with respect to the timing of relevant "periods of wants" *(Bedürfnisperiode)* in his "Untersuchung zu dem Grundgesetz der wirtschaftlichen Wertrechnung," *Zeitschrift für Volkswirtschaft und Sozialpolitik*, N. F., II (1922), 13 ff.

[90] On this matter, see what is said above, pp. 197 ff., 238 ff.

[91] In this connection, see what is said below, p. 395, concerning the concept of "reaction speeds." It should be observed, however, that the "reaction speed" involved here is an "*ex ante*" concept, in the sense that it is one which enters into the calculation of the entrepreneur, rather than an "*ex post*" concept relating to actually realized "reaction speeds." It follows, therefore, that the general comment made in the following note with respect to the relation of analytical *ex ante* periods to "clock" time applies to these *ex ante* "reaction speeds" also.

schedules, in the sense that the entrepreneur may expect the quantity-price relation to be different over different periods of clock time. But in both cases, as well as in cases involving "short run" schedules, the criterion for the length of the "period" to which these schedules are understood to apply will be, not a definite amount of clock time, but whatever length of clock time may be necessary (in the estimation of the planners) for the attainment of the particular price-quantity relations envisaged in whatever schedule is taken for examination.[92]

It will again be observed that an analytical *"ex ante"* period, of the type indicated, does not necessarily involve the assumption of "other things being equal," if by this is meant that the entrepreneurs must be supposed to assume that all conditions will remain unchanged over the period during which it is expected that the desiderated adjustment of quantity to price will be effected.[93] All that is required for our purpose is that the entrepreneur should have "long run" plans of *some kind;* it is neither necessary nor always reasonable to suppose that the plans made by entrepreneurs will, in the real world, rest upon the assumption of a constancy of all relevant data. It is perfectly possible, for example, to conceive of entrepreneurs who either (1) will have taken account of the possibility of changes in data in making their plans; or who (2) will be prepared, for any one reason or a combination of several possible reasons, to carry through their plans even in the face of an "unexpected" change in data.[94] The real problems involved in relating analytical

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[92] It should be unnecessary to labor the point that this statement is not to be taken to mean that the "planners," in making their "plans," will not have in mind a fairly definite estimate of the amount of "clock time" that will probably be required in order for the result envisaged in their plans to be actually realized. On the contrary, the data for such an estimate may be regarded as provided not only by the fact that certain types of "operation" are in fact geared to fixed amounts of clock time (see below, p. 396, n. 106), but also by the facts (1) that expectations may be presumed to be based largely upon experience; and (2) that "experience" includes experience with respect to the amount of clock time required for the accomplishment of certain types of process. It is, indeed, such considerations which justify the use of the concept of an *ex ante* "clock time" period (cf. above, p. 389, n. 88). It should be clear, however, that even in these cases it is the fact that a given amount of clock time is associated with the expected completion of a given *process* that makes the clock-time period economically significant; and this fact alone must necessitate the use of *analytical* as well as of fixed clock time *ex ante* periods. In this connection, see also what is said below, p. 392, with respect to the problem of relating *ex ante* periods of *any* kind—even when the "time" involved in such *ex ante* periods is "clock" time—to actually realized processes.

[93] In this connection, see again Machlup, "Period Analysis and Multiplier Theory," *loc. cit.,* 5, on the consistency of the concept of a "plan adjustment period" with the fact that certain conditions may change within a given "plan adjustment period."

[94] The "reasons" referred to in the text under (2) will include such

ex ante periods, of the type indicated, to "clock time" periods are, therefore, of a kind entirely different from that suggested by considerations with respect to the supposed necessity for the "holding constant" of certain objective data, or even holding constant the expectations of entrepreneurs with respect to these data.[95]

Specifically, the difficulties arise entirely from the fact that the particular "analytical" periods involved are, after all, *ex ante* periods. For this, by definition, means that these "periods" in themselves do not directly refer to processes which have as yet been *actually realized*.[96]

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things as (1) contracts negotiated, prior to the "unexpected" change in data, upon other than a contingent basis; (2) the fact that an unfinished project will be less remunerative than a finished project, even if it is necessary to finish the project under conditions less favorable than those originally "expected"; (3) the fact that, bad as the situation may be as a result of the "unexpected" change of data, it may be "expected" to become still more unfavorable to completion of the project at a later date; and so on. It is, of course, just such considerations which would justify the use of a "non-*ceteris-paribus ex ante* analytical period." In this connection, see Lindahl, *Studies in the Theory of Money and Capital,* 45, on the distinction between "alterable" and "unalterable" plans, as well as that between "conditional" and "unconditional" plans.

[95] If we include in entrepreneurial "expectations" the entrepreneurial "plans" themselves (as we may, on the ground that these "plans" represent the entrepreneurs' "expectations" as to how *they* will act on their own account), it follows that this statement would apply also to any limitations which might be held to attach to analysis involving the use of what Professor Machlup has called "plan adjustment periods," the criterion of whose length is that "plans remain unchanged" over the period in question (Machlup, "Period Analysis and Multiplier Theory," *loc. cit.,* 5; analogues to the concept of a "plan adjustment period" are to be found in Hicks's "planning interval" of a "week" [*Value and Capital,* 123 ff.] and Lindahl's "periods with fixed relevant plans" [*Studies in the Theory of Money and Capital,* 50, 52, 54 f.]). Indeed, it should be observed that, once it is admitted that the idea of a "planning period" does not necessarily include the assumption of a constancy of all data other than the "plans" themselves within the "plan adjustment period," the very concept of a "plan adjustment period" must be regarded as evidence of a desire to deal, not with processes assuming a constancy of data, including a constancy of "plans," but with processes involving *change* in these data: in this case, the change in "plans." For if "plans" were actually assumed to remain unchanged throughout the process taken for examination, there would be no reason to break up the description of the process into "periods" the length of which is determined precisely by a fact of *change*.

[96] It is of the greatest importance to observe that the "realized processes" to which our *ex ante* analytical periods must be directly related are processes *yet* to be realized, rather than the processes that have *already* been realized. The connection between the *ex post* of period t_n, on the one hand, and the *ex ante* of period t_{n+1}, on the other, is of course an important relation. The problem with which we are here concerned, how-

If they did, it would be a very easy matter, as we shall see below under (iii), to relate them to the clock-time periods to which all our statistics of recorded events refer.[97] The problem, therefore, is (1) to relate the processes envisaged by these *"ex ante"* periods to "clock time" periods by introducing the former as direct determinants of processes actually realized within "clock" (or "historic") time, and to do so (2) without depriving the *ex ante* periods of their *"ex ante"* character.[98]

As it happens, the solution to this problem is provided by the substance of our Proposition I, 4, v.[99] According to this proposition, long run *"ex ante"* demand and supply schedules of the type indicated above are relevant to the determination of *realized* prices only insofar as they can be shown to affect the market attitudes prevailing at the moment particular prices are "realized." [100] Once, however, they *can* be shown to affect these market demand and supply schedules, the problem is solved. For, despite the fact that market demand and supply schedules are of an *"ex ante"* character, they *are* directly involved in the determination of "realized" prices. Now, *realized* prices are prices realized (and therefore "dated") in "clock" time.[101] It follows, therefore, that the successful accomplishment of the task of relating *ex ante* "long period" schedules to the market demand and supply schedules which are alone directly relevant to the determination of "realized" prices will

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ever, is the relation of the *ex ante* of period t_{n+1} with the *ex post* of period t_{n+1}, when the *ex ante* of period t_{n+1} is regarded as *conditioning* the *ex post* of period t_{n+1}. In this connection, see the comment by J. Åkerman cited above, p. 384, n. 77, on what he regards as the weakness of much of the "modern theory of anticipations."

[97] See below, p. 396, and also the following note.

[98] It will be observed that the first condition thus indicated for the solution of the problem (namely, that the *ex ante* analysis must be brought in as a determinant of processes *realized* within a given clock-time period) makes the problem much more serious than that of merely relating an *ex ante* analytical period to an *ex ante* clock-time period. On this matter, see what is said above, p. 389, n. 88.

[99] See above, pp. 238 f.

[100] It should be clear that this proposition is nothing more than an application—albeit an application of the utmost importance—of Lindahl's proposition that "planning" which refers "only to more remote periods," (and the fact of *change* in such "planning") "is of importance only in so far as it facilitates the understanding of the plans actually put into practice" (*Studies in the Theory of Money and Capital*, 47 f.).

[101] It should thus be clear that the argument here developed removes all sting from whatever formally correct interpretation could be given to the suggestion that an analysis of the course of prices which runs in terms of successive impacts of market demand and supply curves has no relation to any "actual occurrence," because the "course" of the "supposed curves of supply and demand" involved is "timeless." See J. Åkerman, *Economic Progress and Crises*, 14.

automatically provide the solution of the problem of relating "analytical" *ex ante* periods, long or short, to the "clock time" periods involved in the analytical apparatus here presented.

It may be added, finally, that the mere fact that the "plans" embodied in the "long run" schedules may change before the price-quantity relations embodied in them may be actually realized in the market, in no wise impairs either the case for the use of *"ex ante* analytical periods" of the type involved in these "long period schedules," or the validity of the method proposed for relating these *"ex ante* analytical periods" to the clock time in which the successive steps of all realized economic processes are actually registered.[102]   For it will still be true (1) that the longer-run plans of entrepreneurs, if they are to affect realized prices at all, must affect them by affecting the alternatives for market action which are summarized by market demand and supply schedules; (2) that a change in these long-run plans may in fact be expected to affect these market demand and supply schedules, so that whatever is happening with respect to the "length" of the analytical *ex ante* periods involved in these long-run plans is bound to exert a continuing influence upon the conformation and position of the market demand and supply schedules; (3) that all realized prices are to be conceived of as resulting from the particular market demand and supply schedules prevailing at the moment a given price is realized; (4) that all "realized" prices are "realized" in clock time; and (5) that therefore the establishment of a relation between (*a*) the plans associated with *ex ante* analytical periods of varying degrees of "length" and (*b*) the market attitudes summarized by the market demand and supply schedules associated with particular realized prices, amounts automatically to the establishment of a relation between these *ex ante* analytical periods and the clock-time periods within which prices are actually "realized."

iii. *"Clock" time periods and "analytical" ex post periods.* Unlike *ex ante* "periods," an *ex post* "period," by definition, encompasses processes conceived of as having been already *realized.* It is, nevertheless, extremely common to find use made, in recent examples of "period analysis," of *ex post* "periods" which are "analytical" in character, in the sense that the criterion for the determination of the length of such *ex post* periods is not the lapse of a given amount of "clock" time, but the completion of a given *process,* or a given step in such a

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102 The argument summarized in the following sentences of the text is of course in all essentials identical with that presented above, pp. 234 ff., with respect to the heuristic value of "equilibrium" analysis involving the use of weapons of the type represented by Marshallian demand and supply schedules (cf. also what is said on this matter below, pp. 408 ff.). It also provides a justification for "period analysis" which makes use of *ex ante* analytical periods of the type designated by Professor Machlup as an "equilibrium adjustment period," *provided* that it is made clear, in every case, just how the weapons used in the tracing of the process of "equilibrium adjustment" can be applied to the explanation of the processes realized in the world we know.

process, which is held to be relevant for whatever problem is taken for examination.[103]   Thus, if the "period" with which we are concerned is a "period" long enough for production to be adjusted in response to a given relation between costs and selling prices, actual and expected, upon the basis of which entrepreneurs have determined to produce or refrain from producing, it is clear that the "period" will be longer or shorter in terms of "clock" time, according to the conditions of production prevailing in the particular case taken for examination.[104]   Or, to put the same proposition in somewhat different terms, the "analytical" period will be long or short in terms of "clock" time, according to whether the "reaction speeds," or "time coefficients," or "velocities of adjustment" involved in each sector of the process are greater or smaller in terms of "clock" time.[105]

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[103] For examples of such a usage, see the references given above, p. 374, n. 54 and p. 375, n. 57, to Robertson's "day"; on p. 386, n. 83, to J. Åkerman's "real," "economic" periods (cf. also what is said below, p. 396, n. 106); and on p. 385, n. 79, to Machlup's "income" and "transaction" periods. Cf., in addition, D. Hammarskjöld, *Konjunkturspridningen,* 53 ff.; also the use of the concept of a "period of registration" in Lindahl, *Studies in the Theory of Money and Capital,* 53 ff.

[104] One has, of course, only to state the problem in these terms to realize that the concept of an "analytical *ex post* period" (and, for that matter, of an "analytical *ex ante* period"), instead of being a particular contribution of contemporary advocates of "period analysis," was precisely the type of "period" envisaged by Marshall in his use of the distinction between the short and the long run. Compare, for example, with the statement in the text, the following summary of "Marshall's Time Analysis" by R. Opie, in his article under that title (*loc. cit.,* 199 f.): ". . . A time was long or short according as it involved modifiability or fixity in some chosen forces on the supply side. The greater the modifiability of the supply forces, *the longer the period of time under discussion, irrespective of the length in clock-time*" (italics mine).

[105] It is worth observing that expressions such as "reaction speeds," or "time coefficients," or "velocities of adjustment" made their formal appearance almost simultaneously in discussions of certain aspects of the "general" Theory of Value, on the one hand, and in monetary and trade cycle theory, on the other. For examples of the appearance of such expressions in the former context, see P. N. Rosenstein-Rodan, "Das Zeitmoment in der Theorie des wirtschaftlichen Gleichgewichtes," *Zeitschrift für Nationalökonomie,* I (1929), 131 ff. (cf. the same author's "The Rôle of Time in Economic Theory," *loc. cit.,* 78, 88 ff.); M. Fasiani, "Elementi per una teoria della durata del processo traslativo dell' imposta in una società statica," *Giornale degli economisti,* LXIX (1929) (*Review of Economic Studies,* I [1934], 82, 94); N. Kaldor, "A Classificatory Note on the Determinateness of Equilibrium," *loc. cit.,* 132 ff. (see especially p. 135, where a "day" is defined in such a way as to make it, for all practical purposes, the methodological equivalent of the "days" of contemporary monetary and trade cycle theory); and O. Morgenstern, "Das Zeitmoment in der Wertlehre," *loc. cit.,* 437. For examples, on the other hand, of the use, in *monetary and trade-cycle theory,* of concepts such as the "velocity," or "speed," of "adjustment" or "change"; "reaction times," "reaction coef-

It has frequently been implied that the problem of relating "analytical" periods of the type indicated to "clock time" periods of the type involved in the apparatus here outlined is one of virtually insuperable difficulty. Yet it should be clear that the solution of this problem is much easier than that of relating these "clock time" periods to the "analytical" *ex ante* periods discussed above under 5, ii. For again it must be observed that the "analytical" periods with which we are here concerned have reference to *realized* processes; that they are "analytical" only in the sense that their length may vary in terms of "clock" time periods.[106]   It is therefore a matter of extreme simplicity to relate

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ficients," or "speeds" or "rapidity" of "reaction"; "velocity of incremental growth" *(Zuwachs);* or "time coefficients"—see Myrdal, *Monetary Equilibrium,* 44; E. F. M. Durbin, *The Problem of Credit Policy* (1935), 44 ff.; J. Åkerman, *Ekonomisk Kausalitet,* 48, 50 (cf. the same author's *Das Problem der sozialökonomischen Synthese,* 166, 267); Ohlin, "Some Notes on the Stockholm Theory of Savings and Investment," *loc. cit.,* I, 66 ff., II, 238 (cf. the same author's earlier "Till frågan om penningteoriens upplägning," *loc. cit.,* 54, on the different "supply reactions" of different commodities, and his *Penningpolitik, Offentliga Arbeten, etc.,* 63, 65, 103 ff.); Lundberg, *Studies in the Theory of Economic Expansion,* 52, 139, 187 ff.; F. A. Lutz, "The Outcome of the Saving-Investment Discussion," *Quarterly Journal of Economics,* LII (1938), 599; E. S. Shaw, "False Issues in the Interest Theory Controversy," *loc. cit.,* 846 f.; Machlup, "Period Analysis and Multiplier Theory," *loc. cit.,* 5. It should hardly be necessary, of course, to labor the point that the mere appearance of *expressions* of the type indicated is no criterion for dating the first appearance of the *type of analysis* involved. See, for example, the classic discussion by J. E. Cairnes, in his *Essays in Political Economy,* 60 ff., of the importance of attending to "the facilities for extending the supply of different kinds of commodities . . . and the facilities for contracting it," in any attempt to "ascertain the laws" determining the degree of differential price change; and on the general problem of relating these "velocities of adjustment" to the supply curves of the "general" Theory of Value, see below, pp. 430 ff., 489, n. 64, 511, 555.

106 It may again be observed, also, that in some cases the nature of the "operations" or "processes" encompassed by a given "analytical" time period may be such as to cause it to evidence a fair degree of consistency in terms of periods of fixed clock-time length. In this connection, see the suggestive discussion by J. Åkerman, *Ekonomisk Kausalitet,* 65 ff., 72 ff. (cf. also his *Das Problem der sozialökonomischen Synthese,* 173) of the calendar *year* as a period significant for the timing of certain economic operations. (It will be recalled that Cournot regarded the *year* as "the natural unit of time, especially for researches having any connection with social economy," on the ground that "all the wants of mankind are reproduced during this term, and all the resources which mankind obtains from nature and by labour" [*Researches,* 52].) From Åkerman's discussion of what he calls "activity periods," however (see, for example, his *Ekonomisk Kausalitet,* 145 ff., 150, and his *Das Problem der sozialökonomischen Synthese,* 285 f.; and cf. also the references to his use of the concept of "real" or "economic" periods given above, p. 386, n. 83), it would seem clear that he would not deny the proposition advanced above,

these "analytical" periods to "clock time" periods of constant length. For, given the facts with respect to the amount of clock time actually consumed in the accomplishment of any given realized process, one has only to *sum* a series of "clock" time periods of constant length, and therefore the results registered in such a series of periods, into a series of longer clock time "periods." Each of these longer "periods" may itself include a different number of "clock time" periods of constant length; but each will nevertheless correspond in each case to an "analytical" period the clock-time length of which will be the amount of clock time required for the actual accomplishment of the particular process whose completion is the criterion for the definition of the analytical "period" in question.[107]

The procedure involved is best illustrated if we take, as an example of an "analytical *ex post* period," Aftalion's "period of gestation." There is, of course, no *a priori* reason why the period of gestation of any one type of machine need remain constant in terms of clock time, nor why the period of gestation of *different* types of machines should be the same in terms of clock time. Yet no one can doubt that if we take the

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p. 391, n. 92: namely, that it is the fact that a given amount of clock time is associated with the completion of a given *process* (or "activity") that makes the clock time period of a year, say, economically significant; and this is sufficient to establish the reality of the problem of relating what have been called above "analytical *ex post* periods" to periods of *clock* time. The reality of the problem indicated becomes still more evident, moreover, when one takes account of the further facts (1) that there is no *a priori* assurance that any one type of "analytical *ex post* period" will always have the same clock-time length; and (2) that there is every reason to believe that *different* types of "analytical *ex post* periods" will be of *different* clock-time lengths.

[107] Thus, if the facts were that a given process required for its accomplishment clock time of the length t, in one instance; twice as much clock time (that is, $2t$), in a second instance; and three times as much clock time (that is, $3t$) in the third instance; then t, $2t$, and $3t$ would each represent a *single* "analytical *ex post* period," although the clock time length of these periods would be found to vary. Suppose, for example, that the process in which we are interested involves a given increase in output sold, which would be registered in changes in the T of the Fisher equation. Suppose, further, that the facts show that the first such increase was registered in the period t_n, but that the second increase was not registered before the period t_{n+3}, and the third increase was not registered before the period t_{n+5}. Given, then, the series $\Delta T_{t_n} + \Delta T_{t_{n+1}} + \Delta T_{t_{n+2}} + \Delta T_{t_{n+3}} + \Delta T_{t_{n+4}} + \Delta T_{t_{n+5}}$, in which each ΔT represents the increase in output sold in each of the periods indicated (over the amount of output sold in the preceding period), we have, by hypothesis, $\Delta T_{t_n} = \Delta T_{t_{n+1}} + \Delta T_{t_{n+2}} = \Delta T_{t_{n+3}} + \Delta T_{t_{n+4}} + \Delta T_{t_{n+5}}$. It follows that the "analytical" period involved corresponds to the clock time period t_n in the first case; to the clock time period $t_{n+2} + t_{n+3}$ (or $2t$) in the second case; and to the clock time period $t_{n+3} + t_{n+4} + t_{n+5}$ (or $3t$), in the third case,

calendar month as our unit of clock time, the clock-time length of the period of gestation of a given machine can be determined by observing the amount of such machines completed in each calendar month after the beginning of the period, as dated in terms of clock time.[108]   In this case, the amount of such machines completed will be zero in each calendar month until the machines are finally finished; the increase in the amount of such machines completed will be dated in terms of clock time by being placed within the calendar month in which the completion is registered; and the clock-time length of the period of gestation will be the number of calendar months elapsing between the date at which the production of these machines is begun and the date at which it is completed.   This, after all, is the logic underlying the procedure whereby Mr. Robertson, following the lead of Aftalion and others, was induced to translate the "period of gestation" into definite periods of clock time.[109]   Each of the "periods of gestation" thus established was, to be sure, of different clock-time length, in the sense that each contained a different number of calendar months.[110]   Yet to argue, for this reason, that such a result either deprives an "analytical *ex post* period," such as the period of gestation, of all "realistic" meaning, or makes its relation to "clock" time in any sense ambiguous, would be like denying that there is any "realistic" meaning to the concept of a business cycle,

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[108] It is clearly not necessary to raise here objections of the kind raised in connection with the "period of production" generally: for example, the objection that the "beginning" of the period cannot be dated, since it goes far back into the remote past.   The "beginning" of the period of gestation of a given machine can be dated at whatever stage in the productive process we choose to select: in the case of an automobile, for example, at the stage at which the automobile factories, having purchased their materials, begin to make automobile parts and bodies.   Regardless of what may be said of certain other uses of the concept of a "period of production," it is clear that the "dating," in terms of clock time, of the beginning of a period such as that indicated offers no difficulty whatever.

[109] See, for example, Robertson's *Study in Industrial Fluctuation,* 15 ff., 21 ff., and the references to Aftalion given on p. 14, n. 3, of the work cited; also Robertson's later paper, "The Slump in Shipping and Ship-Building" (1922; pp. 115 f. of Robertson's *Economic Fragments*).   Actually, of course, estimates of the length of the "period of gestation," such as those of Mr. Robertson, have thus far had to be obtained indirectly (see, for example, pp. 20, and 21, n. 1, of Robertson's *Study*), because of the absence of data going beyond loose statements of the kind which Robertson quotes (*op. cit.,* 15, n. 1) from G. H. Hull, and particularly because of the absence of published data with respect to the date of the beginning of construction.   Indeed, from Robertson's very definition of the "period of gestation"—namely, "the length of time necessary to construct and prepare for use the requisite instruments of production" (*op. cit.,* 13)—it is clear that his own method for arriving at concrete measures of the "period of gestation" is to be regarded as a *pis aller,* to be abandoned in favor of a direct measurement of the type described in the text, whenever the data become available.

[110] See, for example, the figures given on p. 21 of Robertson's *Study*.

on the ground that the processes called "cyclical" have been found to have differing durations in terms of clock time.[111]

Given, indeed a clear understanding of the procedure just outlined, it is easy to show that there is no real basis for certain objections that have recently been urged against the type of "period analysis" which makes use of what are here called "analytical *ex post* periods." It has been suggested, for example, that "periods" of the Robertsonian type do not "take the reaction-times of the various elements in the situation as determinants of the length of the period"; that, indeed, "it is impossible to do this, given the fact that all the elements have different velocities of reaction"; and the latter statement, in particular, has been seized upon by writers entirely out of sympathy with "period analysis" to support their contention that the "usefulness" of such periods "is vitiated by the impossibility of fitting all the relevant time lags into the Robertsonian 'day.' "[112]   In fact, however, the statement quoted with respect to the rôle of "reaction times" in determining the length of the analytical *ex post* periods is itself misleading; and the conclusion drawn from it is entirely without foundation.

For it should be clear that the "reaction time" of *one* particular "element in the situation" is precisely what *does* determine the length of

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[111] The "realistic" basis for the concept of a business cycle, and therefore the usefulness of analysis making use of such a concept, has, of course, been denied on just such grounds as those indicated. In this connection, cf. the well-known argument of Irving Fisher with respect to the justification for speaking of "business cycles," in view of the lack of evidence for strict periodicity in business fluctuations, in his article, "Our Unstable Dollar and the So-Called Business Cycle," *Journal of the American Statistical Association*, XX (1925), 193. Actually, of course, it is quite easy to indicate the nature of the answer to Professor Fisher's challenge to provide any meaning for the concept of a "business cycle," once the criterion of strict clock-time periodicity is abandoned, beyond the simple statement that business "does *fluctuate* above and below its average trend" (p. 191 of the article cited). Such an answer is provided, of course, by the claim of business-cycle analysis to have provided grounds for believing that there are traceable *processes*, called "cyclical," which *account for* the "fluctuation of business above and below its average trend," and which are of such a nature as to require, for their completion, periods of clock time indicated by the recorded "average" of cycles of both the Kitchin and Juglar forms—that is, clock-time periods of from three and a fraction years to two- or three-fold multiples of this figure. From this point of view, indeed, the "business cycle" is itself an example of what is here called "an analytical *ex post* period."

[112] For the first two of the statements quoted, see Lutz, "The Outcome of the Saving-Investment Discussion," *loc. cit.*, 599. For the third statement quoted, see A. P. Lerner, "Saving and Investment: Definitions, Assumptions, Objectives," *Quarterly Journal of Economics*, LIII (1939), 615 (cf. also p. 618 of the same article, where the author argues that "the process analysis that the Robertson approach attempts . . . seems to be stalemated by . . . the multiplicity of time lags which have to be considered").

what is called here an "analytical *ex post* period."[113]   It should be equally clear, moreover, that there can be as many analytical *ex post* periods (Robertsonian "days") as there are "reaction times" for "the various elements in the situation"; and that each of these periods is capable of translation into a given amount of clock time.[114]   The mere fact that these analytical *ex post* periods overlap each other in terms of clock time is no more decisive against the use of such periods than the fact that seasonal processes are accomplished within a length of clock time different from that in which processes underlying the Kitchin cycle, the Juglar cycle, or longer fluctuations are accomplished, is decisive against a use of the concept of a "'period" of business fluctuation.[115] The statement, therefore, that the usefulness of analytical *ex post* periods of the Robertsonian type is "vitiated" by the difficulty in question is as clearly without foundation as would be the suggestion that the analysis of industrial fluctuations in terms of the processes which are held to give rise to these fluctuations is "vitiated" by the fact that some of these processes require only a year for their completion, whereas others require less or more than a year.

It may be pointed out, finally, that there is nothing in the argument here presented which would deny that considerations of convenience in *theoretical analysis* may advise the making of arbitrary assumptions with respect to the length of the "reaction times" of elements in the situation other than those elements whose effect it is particularly desired to study—to the point, for example, of assuming that the "reaction times" of these other elements will be zero within any period taken for examination.[116]   When this is done, we may be said to be making use of what might be called *"ceteris paribus* analytical *ex post* periods."[117] Yet on this head nothing need be added to what was said above with respect to the rôle of *ceteris paribus* assumptions in "period analysis" generally, and particularly with respect to the need for passing from analysis based upon the use of *ceteris paribus* assumptions to a *synthesis* in which no such assumptions are made.[118]   Nor is it necessary here to

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[113] In the context provided by one of Mr. Robertson's uses of his concept of a "day," for example, the "reaction time" involved was the amount of time required before the recipient of income would "react" to the receipt of such income by *spending* it; and it was precisely the length of this "reaction time" which established the length of the Robertsonian "day."

[114] For an example involving the "reaction time" of an element other than the administration of income, see above, p. 397, n. 107.

[115] Cf. what is said on this matter above, p. 399, n. 111.

[116] Or, alternatively, one can conceive of all the various "reaction times" involved as being *equal*. See Rosenstein-Rodan, "Das Zeitmoment in der Theorie des wirtschaftlichen Gleichgewichts," *loc. cit.,* 130, and "The Rôle of Time in Economic Theory," *loc. cit.,* 90. On the significance of this assumption for the results to be expected from "equilibrium analysis," see below, pp. 401 f., nn. 120 and 121, and the forward references given in n. 121.

[117] Cf. what is said on this matter above, p. 389, n. 87.

[118] See above, p. 378.

provide a further detailed argument in order to deal with those particular *ceteris paribus* assumptions which may be involved in "equilibrium analysis." [119]   For if such analysis is not asked to carry more of a burden in explaining reality than it should be asked to bear, it is a matter of altogether secondary importance that in fact the arbitrary assumptions made in such analysis with respect to the similarity of reaction-times in various elements in a given situation do not correspond to what happens in the real world.[120]   What does matter is that the sponsors of the relevant sectors of equilibrium analysis should be prepared to demonstrate that the type of consideration invoked by their analysis is capable of affecting the market action of individuals in the real world.[121]   For, given a demonstration of the rôle played by these

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[119] On this matter, see what is said above, pp. 380 ff., and cf. also the following note.

[120] The rôle to be assigned to "equilibrium analysis," "if such analysis is not to be made to carry more of a burden than it should be asked to bear," is discussed further below, pp. 406 ff. Here it is necessary to remind the reader only that one of the functions properly to be assigned to certain types of "equilibrium analysis" is that of indicating what goals may present themselves as reasonable to the economizing individuals whose decisions make realized processes what they are (see above, pp. 236 ff.). It is hardly conceivable, to be sure, that in setting themselves "goals" of the type with which the relevant sectors of "equilibrium analysis" should be concerned, entrepreneurs operating in the real world will make their calculations upon the basis of the assumption that the "velocities of adjustment" of all the variables involved will be equal. It follows, therefore, that any "equilibrium analysis" with claims to some degree of sophistication must take the form of analysis designed to establish the probable nature of those goals which are likely to appear reasonable to entrepreneurs when an equality of "velocities of adjustment" is *not* assumed. In fact, however, this is precisely what "equilibrium analysis" of the Marshallian type has undertaken to do by means of its distinction between the short-term and the long-term aspects of a given problem—the "fundamental supposition that supply and demand are equal" (Rosenstein, "The Rôle of Time in Economic Theory," *loc. cit.*, 94) thus having a meaning, in Marshallian analysis, which can be established only when we are told what the time period is to which "supply" and "demand" are held to refer. If, therefore, certain propositions in "equilibrium analysis" have in fact been based upon the "assumption" of equal "velocities of adjustment," it can be only because such an assumption may serve, at very elementary levels of analysis, to elucidate certain principles which are valid even when the assumption is dropped. To ask, of analysis at this level of abstraction, that it should explain the whole of economic reality, is absurd on the face of things; but it would be equally absurd to assert that none of the results obtained by "equilibrium analysis" even at this level has any heuristic value whatever in explaining some *part* of economic reality.

[121] See the preceding note. It should be observed again that *if* it can be shown that the type of calculation described in the relevant sectors of "equilibrium analysis" does affect the market action of individuals in the real world, then the mere fact that the *initial* goals indicated by such analysis may change as the result of changes brought about by the very

sectors of equilibrium analysis in the explanation of realized processes, we obtain simultaneously a statement of the relation of such analysis to the *clock* time within which realized processes necessarily take place. It is seen, therefore, that the analytical system outlined in the present work is perfectly capable of dealing with all types of "period" that have been found useful in the "period analysis" of recent years, without any of the obscurity that has been alleged to attach to the very conceptual nature of certain of these periods because of ambiguities in the statement of (1) their relation to "clock" time and therefore (2) their relation to the actually realized processes whose evolution it is the task of economic analysis to explain.[122]

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attempt to attain these original goals, argues, not for an abandonment of "equilibrium analysis," but for its use in such a way as to show how the changes in question lead to new goals and therefore to new market actions. From this standpoint, "equilibrium analysis" *as such* is not invalidated by the mere fact that there may be no possibility of attaining a position which *would* be one of "equilibrium" if the conditions originally prevailing would change, if they changed at all, in a way rigidly specified in advance. What is invalidated, if anything, is an *abuse* of such analysis. Such an abuse of "equilibrium analysis" would be represented by the employment of the concept of an "equilibrium adjustment period" (cf. above, p. 385, n. 79) *in an "ex post" sense, without careful description of the conditions which must prevail over the period of "adjustment" if a full "equilibrium adjustment" is actually to be effected in the real world.* In this connection, I venture to refer again to what is said below, pp. 446 ff., with respect to the extent to which the assumption of a "tendency toward equilibrium" is involved in an analytical system such as that outlined in the present work.

[122] For the sake of those who would welcome a schematic presentation of the various types of "period" discussed above, the following table is provided, with references, in each case, to passages in which examples are given of types of analysis that can be easily subsumed under the categories here indicated.

I. *Clock-time periods.*
    1. *Ex ante* clock-time periods.
        a. Ex ante ceteris-paribus clock-time periods (p. 391, n. 92).
        b. Ex ante non-ceteris-paribus clock-time periods (p. 391, n. 92; p. 391, n. 94).
    2. *Ex post* clock-time periods.
        a. Ex post ceteris-paribus clock-time periods (p. 387, n. 86).
        b. Ex post non-ceteris-paribus clock-time periods (p. 387, n. 86; p. 389, n. 87; p. 396, n. 106; p. 397, n. 107; p. 398, n. 109).
II. *Analytical-time periods.*
    1. *Ex ante* analytical periods.
        a. Ex ante ceteris-paribus analytical periods (p. 377, n. 62; p. 390, n. 89; p. 392, n. 95; p. 393, n. 100; p. 394, n. 102).
        b. Ex ante non-ceteris-paribus analytical periods (p. 384, n. 79; p. 386, n. 83; p. 390, n. 89; p. 391, n. 93; p. 391, n. 94; p. 395, n. 104).
    2. *Ex post* analytical periods.
        a. Ex post ceteris-paribus analytical periods (p. 375, n. 59; p. 380, n. 69; p. 389, n. 87; p. 395, n. 103).

     b. Ex post non-ceteris-paribus analytical periods (p. 384, n. 79;
        p. 386, n. 83; p. 395, n. 103; p. 396, n. 106; p. 397, n. 107; p.
        398, n. 109; p. 399, n. 111).

It will again be observed that the "periods" to which the apparatus out-
lined in the present work is held to be directly relevant, and to which,
therefore, all the other types of "period" must be related, are periods of
the type I, 2, b: *ex post non-ceteris-paribus* clock-time periods.

CHAPTER EIGHT

Corollaries and Vistas: I

IT IS FAIR to ask, of the sponsors of a given work of ana-
lytical construction, what analytical problems, heretofore
incompletely solved, they believe their own system has
helped to bring nearer to solution; and to ask them, further,
how that system can be applied to the concrete solution of
that broader problem which after all represents the ulti-
mate goal of economic analysis: namely, the explanation
and (within the limits permitted by the nature of the data)
the prediction of economic processes as those processes are
realized in the world we know.[1] In a fundamental sense,
obviously, the answer to these questions, if an answer to them
has been given at all in the present work, must be regarded
as being implicit not only in the 22 theses presented in Chap-
ters Five to Seven of this volume and the further theses
presented below in Chapter Eleven, but throughout the
whole of the work. Yet something is to be said for making
some of these implications more explicit by restating the
results obtained in such a way as to bring out their rela-
tions to problems other than those in connection with which
these results were originally obtained.

It is to this task that the present chapter and the one
following are devoted. Specifically, it is proposed to indi-
cate, in brief outline, the relation of the analytical system
offered in the present work to a series of problems whose
perennial recurrence is attested by the fact that their very
statement involves the use of expressions which have be-
come veritable catchwords in recent discussions of the scope
and method of economic analysis. The rôle of "equilibrium

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[1] The "prediction" of economic processes is of course to be understood
in the sense indicated in Volume I of this work (see p. 45, n. 19, and the
references there given).

analysis," both "partial" and "general," in the explanation of the processes actually realized in a changing world; the relation of "statics" to "dynamics," and the rôle of both in explaining the real world; the rôle of *money* in the economic process; the rôle of "institutions" in the determination of money prices, and the relation of "institutional" analysis (in one sense of the term "institutional") to a theory of economic choice; the relation of analysis of *mechanisms* to "mechanical" analysis; the rôle of what has been called "micro-economic" analysis, on the one hand, and of "macro-economic" analysis, on the other, and their mutual relations; the rôle of statistics in the solution of the broad general problem of explaining the functioning of the economic process; the "usefulness" of a given analytical system in the solution of problems of economic policy: all these problems, for all their antiquity, have been thrown up with increasing frequency in recent years.[2]   No one aware of the frequency with which earlier, and in many cases quite adequate, solutions to economic problems have been ignored by later "revolutionaries" could believe that all, or even most, of these problems have really gone unsolved, and that the true "solution" is to be found only in the particular method of salvation which a given writer presents as his own "contribution."   No such absurd pretense is made here.   All that can be said on behalf of chapters such as the present one and the one following is that they undertake to indicate the extent to which the *particular analytical apparatus* presented in this work can be regarded as throwing light on the problems summarized above.[3]

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[2] A full bibliography of even the most recent examples of discussion of the problems indicated would itself be of monographic dimensions. References to the more important of these discussions, however, are given in the following notes, in connection with each of the problems discussed.

[3] In view of the fact that I have undertaken, throughout this work, to compare the particular solution of a given problem which I myself support, with the solution or solutions presented by Mr. Keynes in his various writings, I have not thought it necessary, except in a few cases (see, for example, below, pp. 436 ff., 452, 454, 462, n. 1; 464, n. 4; 468, n. 14; 475 ff., 500 ff.), to provide a separate evaluation of the various Keynesian solutions in terms of the criteria suggested by the problems stated in the text. See, however, J. Åkerman, *Das Problem der sozialökonomischen Synthese,* 94 ff., where Keynes's *General Theory* is tested, and found want-

I

THE RÔLE OF "EQUILIBRIUM ANALYSIS"

It is a striking characteristic of recent economic discussion that in it the usefulness of the whole of "equilibrium analysis," and even of the very concept of "equilibrium," has been brought into serious question—and not only by writers whom some might dismiss as hopeless addicts of either an extreme empiricism or misty "sociological" tendencies.[4] Unfortunately, however, the renewed posing of

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ing, from the standpoint of its failure to provide (1) an adequate explanation, or even an adequate description, of the reactions of economic individuals in terms that would do justice to the changing ("dynamic") character of economic reality; (2) an apparatus capable of tracing processes in terms of "the relation between different variables in time" ("time lags"); (3) an apparatus permitting the use of statistical time series in such a way as to illuminate (or "verify") the analytical relations discussed; (4) an apparatus free from the type of incompleteness which comes from confining the analysis to the effects of too few variables; (5) an apparatus capable of dealing with "cumulative processes," in contrast with one concerned only with what has been called "comparative statics"; (6) an apparatus in which the proposals for policy follow only after the presentation of an adequate *analysis* of the nature of the processes which it is proposed to control; and (7) an apparatus in which adequate specification is made of the *premises,* including the "institutional" premises, on which the argument is constructed. I would add only that I should myself prefer Professor Åkerman's defense of certain aspects of the argument of "the most important of the classicals" (p. 96) against the corresponding sectors of Keynes's argument, to Åkerman's characterization of Keynes's *General Theory* as "ultra-classical" in its general method of posing the problems involved.

   4 For examples of a questioning of the usefulness of "equilibrium analysis" by writers who have been criticized for an alleged addiction to an extreme empiricism, see W. C. Mitchell, *Business Cycles: The Problem and its Setting,* 186 ff.; and S. Kuznets, "Equilibrium Economics and Business Cycle Theory," *Quarterly Journal of Economics,* XLIV (1930), 381 ff. For an example of a similar questioning by a writer who has regarded sociology as being in a better state than economics precisely because of its freedom from obsession by the "fetish" of "equilibrium" analysis, see A. Loria, "Feticci economici," *Economia politica contemporanea: Saggi di economia e finanza in onore del prof. Camillo Supino* (1930), I, 3 ff. For an example, on the other hand, of an expression of equally great skepticism as to the usefulness of "equilibrium analysis" by a writer not usually regarded as guilty of either of the two "sins" indicated in the text, see B. Ohlin, "Some Notes on the Stockholm Theory of Savings and Investment, II," *loc. cit.,* 235 ff. Mention may be made also of the critical observations by J. Åkerman, whose general attitude toward received analytical tech-

the problem can hardly be said to have been accompanied by an increase in the degree of *precision* with which it has been posed.   In particular, there has been altogether too great a willingness to lump together, under the head of the general question of the usefulness of something called "equi-librium analysis," a series of questions that deserve separate treatment.   There is, for example, the question of the use-fulness of *"partial* equilibrium analysis," on the one hand, and that of the usefulness of *"general* equilibrium analysis," on the other.[5]   And there is also the question of the rela-tion between the concept of "general economic *equilibrium,"* on the one hand, and that of "general economic *interde-pendence,"* on the other.[6]   The reasons for separating these

niques is perhaps more adequately described as selectively unconventional rather than as one of either outright opposition or of general adherence. See, for example, Åkerman's "Quantitative Economics," *Weltwirtschaft-liches Archiv,* XXXV (1932), 35 f. (though see also pp. 39 f.), and his chal-lenging *Das Problem der sozialökonomischen Synthese,* 139 f., 143 ff., 172 f., 179 f., 209, 212, 222.

   [5] No stronger argument for separating the two questions is needed than that provided by the fact, for example, that most of those who have seriously questioned the usefulness of "equilibrium analysis" generally in accounting for events in the real world have in fact admitted, explicitly or implicitly, the relevance of "partial" equilibrium analysis in some form, even if the term "partial equilibrium analysis" is not introduced into the discussion.   This is true, for example, of the discussion by Wesley Mitchell of "The Alleged 'Planlessness' of Production" *(Business Cycles: The Problem and its Setting,* 169 ff.) ; for it is clear that any analysis intended to describe how businessmen, "spurred to efficiency by hope of gain, and deterred from recklessness by fear of loss" (p. 170), undertake to bring about an "effective coordination of effort within each business enterprise" (p. 172), must include a large part of the analysis generally referred to as "partial equilibrium analysis."   See also the remarks by Kuznets, "Equilib-rium Economics and Business Cycle Theory," *loc. cit.,* 394, on the lack of "methodological objection" to a "concept of equilibrium" that undertakes "to give only a schematic exposition of how each single price arises from the numerous economic factors which underlie the surface of social eco-nomic phenomena," and on "the main field of application" of "equilibrium economics" as being "the analysis of each instantaneously given economic-social event into the underlying individual acts and attitudes" (p. 400). See also the references given below, p. 411, n. 14, to J. Åkerman's treat-ment of the use of what amounts to "partial equilibrium analysis" in ac-counting for the "business administration" *(betriebswirtschaftliche)* calcula-tions of entrepreneurs.

   [6] On the necessity for breaking down still further the fields of analysis connoted by both the concept of "general economic equilibrium" and that of "general economic interdependence," see below, pp. 412 ff., 424 ff.   Here it is necessary only to reënforce the argument for separating the problem

questions with some degree of sharpness, as well as the nature of the limits to the usefulness that can be claimed for each type of "equilibrium analysis," may best be indicated by a summary of the rôle assigned to each of these types of "equilibrium analysis" in the particular analytical apparatus outlined in the present work.

1. *The Rôle of "Partial" Equilibrium Analysis.* That the substance of "partial" equilibrium analysis is included within an analytical system such as that outlined in the present work follows from (1) the defense presented in Chapter Four, of this volume, of demand schedules of the Marshallian type as devices useful for accounting for the events of the world we know; and (2) the positive argument of Chapter Five with respect to the relation between these demand schedules and the prices actually realized in that world.[7] From the argument there developed, moreover, it should be clear that the inclusion of particular demand and supply schedules in our set of analytical tools is not a matter of blind loyalty to what has justly been regarded as one of the most important sectors in the whole field of received

of the usefulness of the concept of "general economic *equilibrium*," on the one hand, from that of the usefulness of the concept of "general economic *interdependence*," on the other, by pointing out that some of the writers who have been most emphatic in expressing their skepticism with respect to the usefulness of the former have been equally emphatic in insisting upon the necessity or the usefulness of what amounts to the latter. See, for example, the discussion by Wesley Mitchell of the concept of a "System of Prices" (*Business Cycles: The Problem and its Setting*, 108 ff.), with its emphasis upon the "organic character of the relationships among different parts of the system," and especially its treatment (pp. 113 ff.) of "The Interrelations Among Prices," and its explicit references (p. 115 n.) to Walras, Cassel, and H. J. Davenport (cf. the latter's *Economics of Enterprise*, 113 ff., 274). There is obviously all the more reason for objecting to the practice of *identifying* the concept of "general economic interdependence" with the concept of "general economic equilibrium" (cf., for example, Kuznets, "Equilibrium Economics and Business Cycle Theory," *loc. cit.*, 395 f., 400, 413, and the quotation from A. Löwe given on p. 389), particularly when it is later asserted that, regardless of what may be said of the usefulness of much of "equilibrium economics," we must still "retain, even though in a qualified form, all relations of dependence of which equilibrium economics speaks" (Kuznets, *op. cit.*, 415).

[7] See above, pp. 164 ff., 274 ff. The reader is again reminded that the "positive argument of Chapter Five" with respect to the relation between "*demand* schedules and the prices actually realized" in the world we know is supplemented by the further argument, in Chapter Eleven (below, pp. 549 ff.) with respect to the relation of *supply* schedules to these prices.

economic analysis.   It is, on the contrary, a matter of in-
eluctable necessity.[8]

For the purpose of these demand and supply schedules,
and of the analytical techniques which they summarize, is
to provide a description of the type of calculation carried
on by entrepreneurs and other economizing individuals; and
if we are to reject these techniques *in toto*, we must be pre-
pared to show what the calculations of entrepreneurs, for
example, *are* concerned with if they are not concerned with
a set of possibilities with respect to the response of quantity
demanded to changes in selling prices, on the one hand, and
with respect to cost of production in relation to selling prices,
on the other.[9]   For it is precisely with these possibilities that
the subject matter of "partial" equilibrium analysis is con-
cerned.

The question of the "reality" of the calculations described
by "partial equilibrium analysis," it may again be observed,
is entirely independent of the question of the "reality" of
the actual *attainment* of the particular "equilibrium" posi-
tions envisaged by such analysis.[10]   It is enough to prove

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[8] In addition to the summary argument which follows in the text, see
what is said above, pp. 234 ff.

[9] Both sets of possibilities have to do, of course, with the broader fact
that entrepreneurial calculations are concerned with the possibility of mak-
ing "profits."   That entrepreneurial calculations *are* concerned with this pos-
sibility is, in turn, not only not denied, but is actually insisted upon with
the utmost possible emphasis by writers who have often been regarded
as opponents of "equilibrium analysis" in all of its ordinary forms.   See,
for example, Mitchell's *Business Cycles: The Problem and its Setting*, 42 ff.,
105 ff., 162, 172 f., 182 f., 471.   Yet it must be clear that what is contributed
by the sectors of "partial equilibrium analysis" to which reference is made
in the text is a *more detailed* description of entrepreneurial calculations
than is contained in the simple statement that these calculations are
dominated by "profit" considerations.   To deny, therefore, that the sub-
stance of "partial equilibrium analysis" is of any use in explaining the
actions of entrepreneurs in the real world is possible only if one is pre-
pared to deny that the particular *details* involved in this type of analysis
(namely, "details" with respect to the conditions of demand for particular
commodities and "details" with respect to their conditions of supply, in-
cluding "details" with respect to conditions of cost of production) do not
in fact enter in any way into entrepreneurial calculations with respect to
"profit" possibilities.

[10] See what is said on this matter above, pp. 236 ff.   From the argu-
ment there developed, and summarized again in the text above, it should
be obvious that in order to be able to "use equilibrium analysis in the

(1) that such calculations, or calculations of which they
represent a formalization, are made; (2) that these calcula-
tions are conditioned by considerations which justify the
characterization of these particular positions as "equilib-
rium" positions; and (3) that the calculations themselves
can be shown to affect the determination of prices which *are*
actually "realized," by affecting the market action involved
in the realization of these prices. If the assumption of a
"tendency to equilibrium" is involved in this argument at
all, it is involved only in the sense that the market actions
of entrepreneurs, for example, may be assumed to be condi-
tioned by the particular goals described by the substance
of the relevant sectors of "partial equilibrium analysis." [11]

dynamic field," it is not necessary to "treat a process of change as con-
sisting of a series of temporary equilibria" (Hicks, *Value and Capital,* 127;
cf. also Lindahl, *Studies in the Theory of Money and Capital,* 68 ff.), if
by this is meant that we must think of the steps involved in realized
processes as representing in each case the actual attainment of positions
that deserve to be characterized as successive positions of "equilibrium,"
in any meaning of the word "equilibrium" other than the hardly illuminat-
ing sense indicated above, p. 232. It should be clear, rather, that the
ex ante character of the calculations described by "partial equilibrium
analysis" is in itself sufficient to make the usefulness of "equilibrium
analysis in the dynamic field" *not* dependent upon the possibility of treat-
ing "a process of change as consisting of a series of *realized* temporary
equilibria."

[11] The statement that the "market actions of entrepreneurs may be as-
sumed to be *conditioned* by these particular goals," it should be observed,
does not mean that they are *exclusively determined by* these goals. See
what is said on this matter above, pp. 236 f. Here it is necessary to point
out only that any criticism of the substance of "partial equilibrium
analysis" on the ground that it has too often implied that entrepreneurial
calculations, for example, are dominated *exclusively* by the desire for
profit, is, if anything, less valid than such a criticism would be against
the argument of many writers who have been generally regarded as
unsympathetic to "equilibrium analysis," as such. For the statement, by
the latter writers, that "economic activity in a money-making world . . .
depends upon the factors which affect present or prospective profits" (so,
for example, Mitchell, *Business Cycles: The Problem and its Setting,*
107; cf. also the comments on this aspect of the work of Mitchell and
Veblen in J. Åkerman, *Das Problem der sozialökonomischen Synthese,*
234 f.) is presented as a simple statement of *fact;* whereas in the better
versions of "partial equilibrium analysis" the statement indicated is taken
as an assumption preliminary to a determination of how businessmen
would be expected to act *if* they were prepared to take into account *only*
"factors which affect present or prospective profits." If, in any case, by
"the pecuniary aspect of economic activity," we mean the fact that "eco-
nomic activity . . . depends upon the factors which affect present or

This will be true, clearly, as long as these goals *remain* goals, and are not replaced by a different set of goals. And even when they are so replaced, the location of these new goals will again be impossible without the use of the substance of "partial equilibrium analysis." [12]

From the argument presented in the preceding chapters, finally, it should be clear that no one would pretend that the substance of "partial equilibrium analysis" is sufficient *in itself* to provide an explanation of the functioning of an economic "process" in time.[13] But unless we are prepared to assert that there is no basis whatever for the contention that entrepreneurs do base their market actions upon calculations with respect to the probable reaction of quantity demanded to changes in selling prices and upon the relation of costs to selling prices, it should be equally clear that we must be prepared to incorporate the description of these calculations into our theory of the functioning of the economic "process." In sum: the common antithesis between "equilibrium analysis," on the one hand, and "process analysis," or "time analysis," on the other, is a false antithesis, so long as nothing more is meant by "equilibrium analysis" than *"partial* equilibrium analysis" of the Marshallian type. For, to paraphrase one recent writer, all that this analysis amounts to is a contribution toward the description of the "business-administration" (*betriebswirtschaftliche*) calculations of those individuals whose market decisions make the processes realized in clock time what they are.[14]

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prospective profits," it is certainly true that emphasis upon this "aspect of economic activity" does *not* run "counter to one of the traditions of economic theory" (cf. Mitchell, *op. cit.*, 106; though see also what is said above, p. 36, n. 99, and p. 73, n. 54, on the ambiguity of the word "pecuniary" in this context). On the contrary, it has been one of the cardinal elements in economic theory from the very beginning.

[12] On the matter of *changing* "goals," see what is said above, pp. 236 f.

[13] See especially what is said above, pp. 347 ff. (in connection with our Proposition XVI), concerning the limitations upon such analysis as a result of its essentially *discrete* character.

[14] See J. Åkerman, *Das Problem der sozialökonomische Synthese,* 156 f. I should not be prepared to deny Professor Åkerman's further comment (p. 157) that "it is often very difficult, in the case of individual authors, . . . to decide whether the cost-concept that is used is a social *(nationalökonomisch)* or a business-administration *(betriebsökonomisch)* concept." It is clear, on the other hand, that the mere fact that most of

2. *"Partial" equilibrium analysis and the concept of general economic interdependence.* There could be no clearer evidence of the desirability of distinguishing between the concept of "general economic *interdependence*," on the one hand, and "general economic equilibrium," on the other, than that which is provided by recognition of a fact which is borne in upon us before we even raise the question of the rôle of the concept of "general economic *equilibrium*" in an analytical system such as that outlined in the present work. This fact is simply that any use of "partial" equilibrium analysis to explain market facts which does not bear in mind continually the general *interdependence* of economic variables is exposed in advance to the possibility of egregious error.[15]

the tools of "partial equilibrium analysis" were *originally* developed in abstraction from certain elements stressed by business-cycle theory (cf. Åkerman, p. 157) does not mean that these tools are not *applicable* to the study of changing "business-administration" decisions and acts *over the period of a cycle,* and therefore to the study of "dynamic" processes (Åkerman, p. 159). On the contrary, the argument presented here with respect to the rôle of "partial equilibrium" analysis in the explanation of "dynamic" processes amounts to an illustration of the procedure recommended by Åkerman himself as a method for dealing with analysis at a "point of time" *(Punktanalyse),* on the one hand, and a study of the "time-process" *(Zeitverlauf),* on the other: namely, "to put ourselves, from the very beginning of the analysis, at a point in time which is tied up with the type of activity covered by economic concepts, and simultaneously to make the problem of causality start from a psychological interpretation of the acting groups and from the institutional framework that surrounds these groups" *(Das Problem der sozialökonomischen Synthese,* 125 f. On the treatment, in the analytical apparatus here outlined, of individuals' "psychological" decisions, on the one hand, and the relevant "institutional framework," on the other, see what is said below, pp. 462 ff.). From the argument summarized below, pp. 479 ff., moreover, it should be clear that the tools of "partial equilibrium analysis" are capable of an application which makes them directly relevant to that explanation of statistically recorded *time series* which Professor Åkerman rightly regards as a major objective of economic study *(Das Problem der sozialökonomischen Synthese,* 178; see also the references given below, p. 506, n. 108).

[15] It may be observed that writers who have criticized the concept of general economic "interdependence" in some *other* sense of the term virtually admit the propriety of the particular use of the concept of general economic interdependence indicated in the text, when they insist that a use of "equilibrium analysis" which would consist of an interpretation of "every materialized price in the market" as implying "a balance of supply and demand" would be dangerous "if not all the factors are included which have participated in bringing about the single given price"

This follows, indeed, from the familiar argument, restated in Chapter Four of the present volume, with respect to the possible effects, upon the demand schedule for any given commodity, of changes in the prices of other commodities.[16] For our present purpose, however, it is more important to stress a further proposition: namely, that from the argument in Chapter Four it follows also that the very demonstration of the validity of the case for a continued use of demand and supply schedules of the Marshallian type simultaneously involves an acceptance of the necessity for continued emphasis upon the general *interdependence* of economic variables.[17]   A characterization of the type of "interdependence" thus indicated as "timeless," therefore, instead

(so, for example, Kuznets, "Equilibrium Economics and Business Cycle Theory," *loc. cit.,* 394). On the other hand, I cannot see the relevance of Dr. Kuznets's further point (*op. cit.,* 400) that the part of "equilibrium economics" represented by an emphasis upon the fact of general economic interdependence, in the sense indicated, "runs the danger of neglecting important factors, because it is satisfied with establishing a system of equations giving a determinate solution, without being able to check the solution." The trouble with many uses of the concept of "general economic interdependence" (again in the sense indicated), I should have said, is not that the "equational method of solving economic problems" (cf. Kuznets, *op. cit.,* 415) encourages the "neglect of important factors," or that the "solution" given by the equations used cannot be "checked." It is rather that the fear of being charged with having "neglected important factors" has too often led to a solution that is "correct" enough, but is couched in such "general" terms as to be helpful only in the sense that it provides a continued warning to the investigator to see to it that all relevant factors *have* been included in the analysis. See what is said on this matter above, p. 379, n. 65, and also below, p. 656, n. 62.

   16 See above, pp. 166 ff.

   17 In this connection, see the remarks by Wesley Mitchell in his *Business Cycles: The Problem and its Setting,* 108 and 113, on the necessity for taking into account, in any description of cyclical processes, the relations between the prices of *substitute goods,* and his references (cited above, p. 408, n. 6) to the significance, for the problem in hand, of the Walrasian system and H. J. Davenport's long-hand translation of the particular aspect of that "system" which is here in question. It should be observed also that Dr. Kuznets's concession ("Equilibrium Economics and Business Cycle Theory," *loc. cit.,* 415) that any analytical system adequate for explaining the processes of the real world would have to retain "all relations of dependence of which equilibrium economics speaks," itself throws light upon the validity of any rejection of the "equational system of solving economic problems" when the particular purpose of such an "equational system" is precisely to express the "relations of dependence" which exist between the prices, say, of substitute and complementary goods at all moments in time. Cf. also the following note.

of implying a minimization of the importance of an empha-
sis upon such "interdependence," should be taken as imply-
ing its crucial importance for economic analysis. For the
only sense in which the type of "interdependence" under
discussion can be said to be "timeless" is that this interde-
pendence necessarily holds true at all moments of time, re-
gardless of the degree of *change* evidenced within processes
functioning in time.[18]   Again, therefore, it must be said
that if by "equilibrium analysis" we mean the principle of
general economic *interdependence,* in the sense in which

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[18] In the light of this proposition, it should be clear also that the
continued applicability of the particular concept of "interdependence"
which is here under discussion is in no way affected by the fact that *other*
aspects of the Walrasian system happened to involve the assumption of
"certain imaginary conditions" which would make the system incapable
of direct application to "real life." (Cf. Mitchell, *Business Cycles: The
Problem and its Setting,* 115; and see also J. Åkerman, *Das Problem der
sozialökonomischen Synthese,* 147, where the usefulness of the concept of
"general interdependence at a moment" is discussed on the assumption
that the "momentary" situation involved is one of "equilibrium," which
can be thought of as realizable only after the lapse of "an imaginary or
infinitely long period of time.") It should be equally clear that the
concept of "interdependence" defended in the text requires no assumption
of *"persistent* relations between factors" (cf. Kuznets, "Equilibrium Eco-
nomics and Business Cycle Theory," *loc. cit.,* 395), if by this is meant
that the *particular* relations prevailing between, say, two complementary
commodities at one moment may be expected to prevail at the next
moment. All that is assumed to be "persistent" is the *fact* of interde-
pendence between goods which are in any way complementary or competi-
tive. It will be granted without question that the "interrelations" between
the factors may "change with every single change of every single price
on the market" (Kuznets, *loc. cit.*); and it is granted by virtually every-
body these days that this is a proposition which is important chiefly as
providing a warning against error in the use of "partial" equilibrium analysis.
But there is certainly nothing in these statements that would warrant
an attack upon the "concept of . . . fundamental interdependence," *in the
sense in which that concept is here used,* on the ground that the very
concept of such "interdependence" is based upon "assumptions" which
"break down" in real life; or that the introduction of the element of *time*
does away with the "strict interdependence of economic quantities," in
the sense in which the concept of "strict interdependence" is here used
(cf. Kuznets, *op. cit.,* 395 f., 400, 413). On the contrary, it must be ob-
vious, particularly in the light of the statements of Dr. Kuznets himself
quoted above, p. 412, n. 15, and in the preceding note, that he must mean
by "the concept of . . . fundamental interdependence" something differ-
ent from what has been meant by that concept in a context which would
make it relevant to the way in which "partial equilibrium analysis" is to
be used in accounting for market events realized in historic time.

that term is used here, there is no foundation whatever for the proposition that there is a conflict between "equilibrium analysis," on the one hand, and "process analysis" or "time analysis," on the other.[19]

3. *General economic interdependence in successive time periods.* It is of the utmost importance, however, to establish a third proposition: namely, that the type of "interdependence in time" indicated under (2) is by no means the only, or even the principal, way in which the idea of "general economic interdependence" is retained within an analytical system, such as the present one, in which the major emphasis is placed upon the nature of the forces which determine the magnitude of economic variables in time. A further kind of "interdependence in time" is established by the system presented here when one points out, for example, that the system leaves full room for a use of the element of "expectations" which would insist that the expectations and "plans" prevailing in the *present* may be regarded as connecting the events of the *past* with the events of the present and the *future:* for the expectations *now* prevailing are partially conditioned by the events of the past, and both condition and are conditioned by the events of the future.[20]

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[19] For an example of outright identification of the "concept of equilibrium" with a "system of interdependence," with the result that the latter is rejected because of dissatisfaction with the former, see Kuznets, "Equilibrium Economics and Business Cycle Theory," *loc. cit.,* 395 f., 400, 413. Again, however, attention should be called to the quotation from Kuznets's article given above, p. 413, n. 17, with respect to the treatment to be accorded to the "relations of dependence of which equilibrium economics speaks."

[20] As was pointed out above, p. 362, n. 31, the formal suggestion that the element of expectations *(aspettative)* provides a kind of "re-tying" *(rannodamento)* of the present to the future and the past is to be found in economic literature at least as early as the day of Francesco Ferrara; and it was pointed out also that in our own day this suggestion of Ferrara has been characterized as one of the reasons for regarding the latter's argument as being concerned with a kind of "mutual interdependence of economic phenomena in *time*" (cf. the reference to Bousquet given above, p. 356, n. 18). Such a claim is likewise implicit in the argument of those sponsors of an emphasis upon the element of "expectation" who have regarded this type of emphasis as constituting the very essence of "period-" or "sequence-analysis" (see the references given above, p. 382, n. 75, and p. 383, n. 77). Indeed, an attempt has even been made to formalize the proposition indicated, by writing expressions in which the economic quantities belonging to any given "period" are regarded as having func-

Even this, however, does not exhaust the list of ways in which a kind of "interdependence in time" is established by the system presented here. On the contrary, the most important type of "interdependence in time" which the system here outlined may be regarded as describing is that which is concerned with the *realized processes* whereby the events of any one time period are connected with those of the period which follows.

The key to this description, it will be remembered, is provided by our emphasis upon the necessity for studying the *flow of money payments* in time.[21] As we have seen, it is this flow of money payments which, in a fully developed money economy, prevents the economic system from ceasing

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tional relations not only to the prices of that period, but also to the prices of later periods—the claim being that this type of formalization makes it possible to "extend" the "Walras-Cassel system of equations" in such a way as to make them valid for "dynamic" as well as "static" conditions, on the assumption that, in these "dynamic" situations, "the future is completely foreseen by all individuals." In this connection, see E. Lindahl, "Prisbildningsproblemets upplägning från kapitalteoretisk synpunkt," *loc. cit.*, 63 ff. (cf. the same author's *Studies in the Theory of Money and Capital*, 10, 321 ff.). As we have seen, however, the proposal to formalize the proposition that the prices, for example, of a given period may be assumed to be affected by expectations with respect to the economic magnitudes of later periods, by explicitly inserting a series of algebraic terms for these "expected" magnitudes, is not a novelty of our own day (see the references to Auspitz and Lieben given above, p. 189, n. 97); and the *substance* of the proposition, despite the claims for novelty made on behalf of this type of analysis in recent years, is very much older. It is doubtful, moreover, whether these later formalizations add enough to the substance of the proposition itself to warrant incurring the risk of claiming more for the formalization than is justified. In this connection, see Lindahl's own reservations with respect to his own contribution on the point in question, in his *Studies*, 11, 33; and cf. also above, p. 377, n. 61. For our purposes, it is sufficient to point out that the influence of these "expectations" upon *realized* prices is already taken into account by our Proposition I, 3 (see above, pp. 225 ff.); for, according to this proposition, these "expectations" must be shown to affect the conformation and position of those market demand and supply schedules which are alone directly relevant for the determination of *realized* prices. One can accept this proposition without reservation as establishing one kind (though by no means the *only* kind) of "general economic *interdependence* of economic magnitudes in time," and still avoid all those questions, with respect to the precision and accuracy of the "expectations" involved, as well as with respect to their mutual compatibility, which inevitably arise whenever the concept of "interdependence" is not divested of its association with the concept of "equilibrium" in one of the more ambitious senses of the latter term.

[21] See above, p. 349 (Proposition XVII).

to function altogether.   It is, moreover, the description of
the flow of money payments from producer to consumer
and again to producer which establishes the direct relation
of the concept of a "circular flow of money" to those pic-
tures of an interdependent economic process that have been
provided in economic literature from Quesnay's *tableau
économique* to the "system" of Léon Walras.[22]   For, again,
it is nothing less than a caricature of the Walrasian "system"
to represent it as being concerned with the establishment
only of the proposition that economic magnitudes are gen-
erally "interdependent" at any given "moment." [23]   That

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[22] See above, pp. 351 ff. Cf. also the comments of E. Petersen, *Macro-
Dynamic Aspects of the Equation of Exchange,* 9, on the relation of the
system of "stream equations" outlined in that work to Quesnay's *tableau
économique.* In view of the absurd statements made in recent years with
respect to (1) the "habits of mind" supposedly engendered by the use of
equations of the general Fisherine form (see, for example, N. Kaldor, in
the *Economic Journal,* XLIX [1939], 497), as well as with respect to (2)
the supposed uselessness of constructions of the Walrasian type in any
attempt to explain fluctuations in output and employment, particular
attention may be called to Dr. Petersen's comment (p. 11) to the effect
that any adequate "theory" based upon the use of a system of stream
equations of the type indicated, instead of being "a pure monetary theory
in the most restricted sense of the term," will "also be a theory of produc-
tion and output and thereby of employment."

[23] From the argument of paragraph 2, above (pp. 412 ff.), it should be
clear that the *protection against* any *error* that might result from forgetting
this fact of continuing "interdependence" at every "moment of time" is
itself enough to clear Walras himself of the charge of having constructed
a system which could only be "sterile" of positive results. The "sterility"
of the work of Walras's immediate successors (on which, in addition to
the reference to Lange given above, p. 170, n. 55, see J. Åkerman, *Das
Problem der sozialökonomischen Synthese,* 105 f., 262, and W. L. Valk,
Conjunctuurdiagnose [1935], 331) arose, not from an acceptance of this
proposition, but from (1) an *abuse* of it, as when all "partial" equilibrium
analysis was rejected as unsound in principle; and (2) a failure to see the
possibilities for further construction upon the Walrasian foundation which
are provided when his "system" is envisaged, in the manner of Schumpeter,
as a picture of the "circular flow of economic life" unfolding itself in clock
time (see above, pp. 111 ff., and also below, pp. 429 ff.). It is only fair
to the later Walrasians, moreover, to point out that if they have been
blind in this second respect, so have the authors of those skeptical com-
ments with respect to the "usefulness" of the Walrasian system, who have
read more into the concept of a "simultaneous" interdependence of eco-
nomic magnitudes at every "moment" than need have been read into it.
It is clear, for example, that the kind of "simultaneous interdependence"
discussed above under 2 (pp. 412 ff.) is capable of complete conceptual
separation from another kind of "simultaneous interdependence": namely,

the Walrasian system, on the contrary, presented an imperishable picture of an economic system functioning in "time" is seen as soon as one considers the Walrasian description of the mutual influence upon each other of the realized actions, in time, of the streams of payments which bind together consumers, entrepreneurs, "capitalists," and again consumers.[24]

It is, therefore, certainly a misapplication of whatever validity there is in a distinction between relations of "interdependence," on the one hand, and "irreversible relations of dependence," on the other, to insist that what we are concerned with here is the latter, and not the former.[25]

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that involved in the assumption that the *adjustments* that would be necessitated in order to realize the Walrasian picture of an *equilibrium* position, given the "interdependence" of consumers, capitalists, and "entrepreneurs," will be made "simultaneously," within a timeless "moment." In addition to the quotation from J. Åkerman given above, p. 414, n. 18, see, for example, the comments upon the concept of "simultaneous interdependence" in Lundberg, *Studies in the Theory of Economic Expansion*, 17, 19 ff., 45 f. From the nature of the comments of Lundberg just cited, it is clear that the concept of "simultaneous" interdependence, or "general interdependence during *one* period," which Lundberg rejects, like "the complete interdependence in a simultaneous adaptation process," which he believes to be "invalid" (and therefore, he believes, should be eliminated in an analysis of "cumulative processes"), has nothing in common with either the "general interdependence at each moment" discussed on p. 412 ff., under 2, or the concept of "interdependence in time" which has been associated with the Walrasian system in its "circular flow" aspect.

[24] It should be clear that nothing prevents the inclusion, within this system, of *government* as one of the agencies the effect of whose realized actions may be traced in a system of "stream" equations of the type indicated. The matter is obviously of importance for a tracing of the processes involved in the generation and utilization of money income.

[25] On the distinction itself, see Rosenstein-Rodan, "Das Zeitmoment in der mathematischen Theorie des wirtschaftlichen Gleichgewichtes," *loc. cit.*, 142. In justice to Dr. Rosenstein-Rodan, it should be noted that in his later paper, "The Role of Time in Economic Theory," *loc. cit.*, 94, the distinction between the "reversible" and the "irreversible" is confined to the distinction between "reversible functions" and "irreversible functions": as applied, for example, to the question of the nature of the functional relations between price and quantity demanded. The distinction, that is, was not employed, in the later paper, in such a way as to argue that, in the real world, "there is no general interdependence," but only "irreversible relations of dependence," or that there is no room for the concept of "interdependence" in any "theory of open cycles"; nor was there, in the later paper, any approving quotation from A. Löwe to the effect that "the assumption of different time-spans for the reactions of the separate elements destroys the general interdependence." (Contrast Rosenstein's "Das Zeitmoment, etc.," *loc. cit.*, 142. The passage from Löwe is taken from the latter's "Wie ist Konjunkturtheorie überhaupt möglich?"

For the only effect of such an application of the distinction indicated is to substitute mere logomachy for an illuminating discussion of the central issues involved. It is worth noting, for example, that Simon Newcomb, who can hardly be accused of ignorance of the mathematical meanings to be assigned to the word "interdependence," used the word *"inter*dependence" to describe precisely the type of *dependence in successive time periods* which is here under discussion.[26] In order, moreover, to be convinced of the entirely factitious nature of the distinction between "dependence" and "interdependence" as applied to the problem in hand, one has only to consider what could possibly be meant by a *"reversible* relation of *inter*dependence" when the latter is applied to the particular aspect of the Walrasian system with which we are here concerned. When Walras described the consumers as directing streams of payments to producers, who were in turn described as "returning" these payments to the consumers, who would then "return" them to the producers, and so on, he would have been talking utter nonsense if he had meant to suggest that the process was "reversible" in the sense that it "reverses" itself in terms of clock time.[27] If he

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loc. cit., 184. On the substance of the passage itself, see the comments by R. W. Souter, "Equilibrium Economics and Business Cycle Theory: A Commentary," *Quarterly Journal of Economics,* XLV [1930], 51.) Unfortunately, however, the damage had already been done. See, for example, the approving comments on Rosenstein-Rodan's earlier formulation of his position in Kuznets, "Equilibrium Economics and Business Cycle Theory," *loc. cit.,* 403; also J. Åkerman, "Quantitative Economics," *loc cit.,* 54 n. (though see also Åkerman's later *Das Problem der sozialökonomischen Synthese,* 273, where the author's reference to the *inter*dependence of "cumulative processes" is capable of an interpretation that would imply an abandonment of his earlier acceptance of the suggestion that, in dealing with "open" systems, the relations involved are relations of "dependence," rather than "interdependence"). It may be added, finally, that the introduction, into the discussion, of the distinction between the "reversible" and the "irreversible" can hardly be said to have brought more illumination to the issues involved, in view of the fact that the distinction in question has often meant no more than that between an upward movement followed in time by a downward movement, on the one hand, and an unceasingly upward or downward movement, on the other. (See, for example, R. Frisch, "Propagation Problems and Impulse Problems in Dynamic Economics," *Economic Essays in Honour of Gustav Cassel* [1933], 205.) It is clear that the latter meaning of the distinction can have no application to the particular issues with which we are here concerned, since the writers cited above have regarded both wave-like movements and steady movements upward or downward as impossible within a "closed," "interdependent" system.

[26] See Newcomb's *Principles,* 367.

[27] It is for this reason that the greatest care must be taken in providing an interpretation of a statement to the effect that in a "system" such as that of Walras "all the variables belong to the same point of time" (so, for example, Frisch, "Propagation Problems and Impulse Problems in Dynamic Economics," *loc. cit.,* 172). If the statement is taken to mean no

spoke of an *"inter*dependence" between the payments of consumers and producers, in cases of the kind indicated, he could have meant only what more recent writers have appeared to mean by the statement that what is involved is an "irreversible relation of dependence." [28]

Nor can it be said to have been entirely helpful, for the special purpose in hand, to describe the Walrasian system as a "closed" system, when the purpose of such a characterization is that of contrasting a "closed" system with the "open" type of system that is held to be alone capable of application to a description of "cumulative processes." [29]   As

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more than is meant when one says, for example, that the variables in "stream" equations of the Fisherine type all refer to the "same period of time," it is of course correct. But it is not only not correct, but is complete nonsense, if what is meant is that the successive steps in a given Walrasian circuit flow are conceived of as happening "at the same point of time." When, for example, it is said that the consumers direct streams of payments to producers and that at the same "moment" the producers are directing streams of payments to the consumers, both sets of "streams" may be said to refer to the "same period of time"; but the streams of money and of objects sold for money in the two markets are certainly not the *same* streams of money and objects sold, respectively (cf. Schumpeter, "Das Sozialprodukt und die Rechenpfennige," *loc. cit.,* 675 [last sentence]). On the contrary, in order to trace the course of any given segment of either the money or the "goods" streams, respectively, through the "circular flow," one is forced to move "irreversibly" in a *forward* clock-time direction.

[28] It follows, also, that any serious attempt to understand what Walras was really attempting to do would put little store by a distinction between (1) the concept of "mutual interdependence," on the one hand, according to which "it is as true to say that receipts determine the payment of costs as that production expenses determine receipts"; and (2) a "dynamic analysis," on the other hand, in which these "relations" are stated in terms of a "series of *causes* and *effects* linked together over certain time intervals" (so Lundberg, *Studies in the Theory of Economic Expansion,* 171; italics mine). To be sure, a good part of the blame for overemphasizing the distinction between the concept of "mutual interdependence," on the one hand, and "cause and effect," on the other, must be put on the shoulders of those mathematical economists who have regarded the former as having "superseded" the latter; but the over-emphasis is still objectionable, even when it comes from those for whom the latter represents a more advanced position than the former.

[29] On systems of the Walrasian type as "closed" systems, and on the implied incompatibility of an "open" system with anything deserving to be called an "interdependence" of its various sectors, see Löwe, "Wie ist Konjunkturtheorie überhaupt möglich?" *loc. cit.,* 173; and cf. also Kuznets, "Equilibrium Economics and Business Cycle Theory," *loc. cit.,* 387. The comment by J. Akerman, *Das Problem der sozialökonomischen Synthese,* 120, on the relation between "closed systems" and "interdependence" is much less objectionable, since it seems to leave room for the possibility that "open systems," instead of being *incompatible* with the phenomenon of "interdependence," may *include* all that is assumed with respect to the latter in "closed systems," the only difference being that the description

was pointed out above, the adjective "circular," as applied to the concept of a "closed" "circular flow" can only be intended either (1) to describe forces which act and interact in the *same way* through time; or (2) to describe the *mutual interaction* of different types of expenditure.[30] The Walrasian system, in other words, is not a "closed" system in the sense that it provides no description of a process which carries within itself the means for its continued functioning, or even (when the concept of "interdependence" is divested of its "equilibrium" connotations) the means for developing cumulative tendencies upward or downward.[31] There is nothing, therefore, in the concept of an "interdependence" in time, of the kind envisaged by the concept of a "Walrasian circuit flow," which creates anything of the *impasse* between the theory of "general economic interdependence," on the one hand, and business-cycle theory, on the other, which has seemed to some writers to be of crucial importance for an understanding of the issues involved in any attempt to establish a proper relation between the two bodies of theory.

One of the main purposes of the analytical system outlined in the present work, to be sure, has been to fit further

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of the particular type of "interdependence" involved is *"complemented* by" a description of various types of "disturbance." It should be added, however, that the very conception, as in Löwe's discussion, of a "closed" system as one which is insulated against "outside" disturbances, has itself helped to obscure the precise nature of the relation between the "closed" character of the system, on the one hand, and the fact of "interdependence" in time, on the other. For, quite apart from the confusion that has arisen as a result of attempts to decide whether a given factor is to be classified as an "inside" ("endogenous") factor or as an "outside" ("exogenous") factor, the type of "interdependence in time" which is characteristic of the Walrasian "circuit flow" is capable of complete conceptual separation from any discussion of the place of "endogenous" and "exogenous" elements in the causation of the business cycle.

[30] See above, p. 113.

[31] On the contrary, the Walrasian "circular flow" is precisely the kind of "closed system" described by Frisch, "Propagation Problems and Impulse Problems in Dynamic Economics," *loc. cit.*, 174, as a starting point for a proposed study of "the complete macro-dynamic problem." Professor Frisch's proposal, in other words, is based upon an analytical foundation in all essentials identical with that represented by the Walrasian system in its "circular flow" aspect—despite the fact that Professor Frisch, on the ground that in the Walrasian system "all the variables belong to the same point of time," had himself rejected such a system as "basically different" from the kind of system he proposed to construct in order to be able to "explain how one situation grows out of the foregoing" (p. 172). The most notable example of a use of the Walrasian circuit flow as a basis for description of *"cumulative* processes" is provided, of course, by the work of Schumpeter. On this matter, see what is said above, pp. 113, 118, 315, as well as the reference given above, p. 315, n. 201, on Walras's own treatment of the particular type of "cumulative process" later discussed under the head of "forced saving"; and see also below, pp. 429 ff.

essential details into the Walrasian picture of the "circular flow of economic life." [32]   In particular, an attempt has been made to show precisely how virtually the whole of monetary theory, as well as the whole of "partial" equilibrium analysis, may be made to contribute to an explanation of why these streams of realized money payments are of the dimensions they are and have the direction of impact that they have, and thus give to money prices the general "scale" and structure that they have.[33]   Yet at bottom the processes

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[32] Cf. above, p. 71, n. 48. I need hardly emphasize that I would not wish to exaggerate any claims that the present work may have to "originality" either in the posing of the problem or in whatever contributions it may contain toward its solution. I hope, to be sure, that I have done something to establish with more clarity the "vague outlines of a dynamic theory of price" which my own teacher, Allyn Young, envisaged in his paper on "Some Limitations of the Value Concept," published in 1911: a theory which would "analyze the forces controlling the volume and rates of flow of particular kinds of commodities, and the volumes and rates of flow of the parts of the money stream to which they are equated in the market" (*Economic Problems New and Old*, 208). But the reader of this volume, as well as its predecessor, will be under no illusions as to the amount that every individual worker in the field must owe to his predecessors and to contemporaries such as Hawtrey and Schumpeter. So far as the broad statement of the problem is concerned, I might refer particularly to the desideration by E. Petersen, *Macro-Dynamic Aspects of the Equation of Exchange,* 102 ff., of a system of "stream equations" which would do justice to all usable elements of "partial" equilibrium analysis, in particular (cf. also what is said concerning Petersen's work above, p. 417, n. 22); and I might refer also to the emphasis upon the importance of constructing a system of money streams and "goods" streams which runs throughout W. L. Valk's *Conjunctuurdiagnose* (see, for example, pp. 27 ff., 44 ff., 65 ff., 322 ff., and 360 ff., of the latter work).

[33] The fact, moreover, that the "streams of realized money payments" are all capable of representation in "equational" form (specifically, in the form of a series of "Fisherine" equations) itself provides a commentary upon the suggestion that the difficulties raised by the writers cited earlier in this chapter are inherent in the use of *any* "equational system of solving problems" (see, for example, Kuznets, "Equilibrium Economics and Business Cycle Theory," *loc. cit.,* 415). For, quite apart from the factitious difficulties raised by the association of *all* "equational treatments" with the concept of *equilibrium* (see, for example, Kuznets's comment [*op. cit.,* 399 n.] upon the Fisherine "equation of exchange" as involving an "application of the equilibrium idea," and contrast what is said on this matter in Volume I, 85 ff., of the present work), it is clear that the use of "stream" equations of the type proposed in this work does not involve a failure to recognize "the importance of the time element" (Kuznets, *op. cit.,* 415). The direct contrary is demonstrated by the facts (1) that all the variables in our "stream" equations are dated in terms of clock time; and (2) that clock-time dating underlies the whole of our use

thus described are processes which have been described, ever
since the days of the Physiocrats, in terms that led later
writers to regard the authors of these earlier descriptions as
sponsors of the concept of "general economic interdepend-
ence." [34]   To reject the received descriptions of these proc-
esses as "unrealistic," therefore, on the ground that they
involve the "unrealistic" concept of "general economic *equi-
librium*," is merely to strengthen the case for refusing to
regard as identical the concept of "general economic *inter-
dependence,* on the one hand, and "general economic *equilib-
rium,*" on the other.[35]   For if anything is certain, it is that

of the distinction between payments into and payments out of money
income or other money receipts, respectively, in combination with the use
of the clock-time-conditioned element of cash-balance administration as
the principal link between the "streams" of two successive clock-time
periods (see again Volume I, 382 f.). It may be observed, finally, that
the "equational system" proposed in the present work, although it bears
little formal resemblance to that proposed by Professor Frisch in the paper
to which frequent reference has already been made, does satisfy completely
the criteria set up by Professor Frisch for the construction of "an essentially
dynamic theory, that is to say, . . . a theory that explains how one situa-
tion grows out of the foregoing." For our system *is* a system in which
"we consider not only a set of magnitudes in a given point of time and
study the interrelations between them, but we consider the magnitudes of
certain variables in different points of time, and we introduce certain
equations which embrace at the same time several of the magnitudes be-
longing to different instants" ("Propagation Problems and Impulse Prob-
lems in Dynamic Economics," *loc. cit.,* 171; for examples of equations of
the latter type, see Volume I, 382, n. 85, and 383, n. 88). It should be
clear, moreover, that the possibility of applying equations of this type
to the description of the Walrasian "circuit flow" provides a further com-
mentary on the suggestion that the type of analysis desiderated by Pro-
fessor Frisch "is basically different from the kind of analysis that is
represented by a system of Walrasian equations."

  [34] On this matter, see what is said above, pp. 352 ff. In view, more-
over, of the fact that the idea of a Walrasian circular flow has been traced
to the Physiocrats not only by the writers there cited, such as Schumpeter
(cf. also the comment on the line of descent from the Physiocrats to
Walras in Schumpeter's *Business Cycles,* 36), but also by a writer so
generally critical of the concept of "general economic interdependence"
as A. Löwe ("Wie ist Konjunkturtheorie überhaupt möglich?" *loc. cit.,*
175), it is worthy of note that Professor Frisch, despite his categorical
rejection of the Walrasian system, in all its aspects, as unsuitable for the
purpose in hand, refers to his own model as representing a kind of
"Tableau Economique" ("Propagation Problems and Impulse Problems
in Dynamic Economics," *loc. cit.,* 173 f.).

  [35] Contrast the description of Löwe of "all systems of economics since
the Physiocrats" as being based upon the concept of an "interdependent
system," which is alleged to be virtually identical with "the concept of

the type of interdependence in time which is established by a careful tracing of the process of money expenditure in time, instead of being in conflict with "realistic" "process analysis" or "time analysis," is of the very essence of such analysis.

4. *The Rôle of the Concept of General Economic Equilibrium.* To insist, however, upon the necessity for distinguishing sharply between the usefulness and "realism" of the concept of general economic *interdependence,* on the one hand, and the usefulness and "realism" of the concept of general economic "equilibrium," on the other, is not in itself to insist that a demonstration of the usefulness and realism of the former concept amounts to a demonstration of the uselessness and lack of realism of the latter. It is still necessary to ask just how, if at all, the concept of general economic *equilibrium* is to be incorporated into the analytical system outlined in the present work. For this purpose, however, it is again necessary to draw a sharp distinction —in this case, a distinction between the mere *concept* of a system "in equilibrium," on the one hand, and, on the other, a thesis with respect to *the existence, in the world we know, of a general "tendency toward equilibrium."*

i. *The concept of "general economic equilibrium."* There can be no clear basis for refusing to admit into economic analysis a concept for which nothing more is claimed than that it provides a description of one conceivable ("ideal") type of functioning economic system with which other types of functioning economic systems can be compared.[36]

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equilibrium" ("Wie ist Konjunkturtheorie überhaupt möglich?" *loc. cit.,* 173). It should be clear also that an identification of the two concepts is to be rejected even when the concept of *"equilibrium"* is reduced to the equivalent of "an income and expenditures statement" in which "successive periods of time link into each other, as they should do for our purposes" (so Mitchell, *Business Cycles: The Problem and its Setting,* 187). For the term "equilibrium," as so used, carries none of the implications with respect to a balancing ("equilibration") of *forces* which it has usually carried (cf. Mitchell himself, *loc. cit.*); and since we already have the expression "interdependence in time" (or *"dependence* in time," if one insists) to describe the type of relation indicated by Professor Mitchell, no good purpose would seem to be served by using the terms "equilibrium" and "interdependence" interchangeably.

    [36] In this connection, see Schumpeter, *Business Cycles,* 69, on "the *concept* of a state of equilibrium" as useful "for purposes of analysis and

To reject the concept of an equilibrium of the "system,"
for example, on the ground that in the world we know the
economic system is usually in a state of "disequilibrium,"
is to stultify the use of the very term "disequilibrium": for
the term "disequilibrium" can have no meaning except in
terms of a previously described state of "equilibrium." [37]

diagnosis, *as a point of reference*" (italics mine; though see also p. 47
of the same work, where Professor Schumpeter argues—as I myself would
not argue—that if the concept of a "state of equilibrium" of the system
as a whole "is to be useful as a tool of business-cycle analysis, the eco-
nomic system must strive to re-establish equilibrium whenever it has been
disturbed"). Cf. also the comment on the usefulness of "static struc-
tures" as *"instruments of analysis"* by Lindahl, *Studies in the Theory of
Money and Capital,* 34 f.; and for an example of the use of a conceptually
constructed functioning system the purpose of which is to provide a
standard of *comparison* with other conceivable types of functioning eco-
nomic system, without any commitment as to the actual existence of a
"tendency" toward the establishment of such a system in the world we
know, see Pigou's description of what he calls "the standard monetary
system" in his *Theory of Unemployment,* 205 ff. Attention should be
called to the identification, in the text above, of the adjective "ideal" with
the adjective "conceivable"; for such an identification makes it possible
to avoid the charge that the very use of the concept of "equilibrium" im-
plies a "penumbra of approbation" of equilibrium situations as such (cf.
Robbins, *An Essay on the Nature and Significance of Economic Science,*
127). That the concept of an "equilibrium" of "the system" *has* often
carried such a "penumbra of approbation" cannot, of course, be denied.
For an early example, see the otherwise remarkable discussion of what
amounts to the conception of a system "in equilibrium," by James Steuart,
An Inquiry into the Principles of Political Economy, Book I, Chap. XIX
(I, 163 f. of the 1805 edition of Steuart's *Works;* though cf. also the more
hesitant statement in Book II, Chap. XV [*Works,* I, 351]). Steuart ac-
tually used the phrase *"in equilibrio"* to characterize a system in which
there would be what he called a "balance between work and demand," his
major concern being with the forces leading to *"vibrations* of this balance
of work and demand, and the *overturning* of it." See Book II, Chaps. X,
XI, XV, XXIII, XXVI, XXVII, XXXI [*Works,* I, 289 ff., 298 ff., 306, 351,
440; II, 31 ff., 52 ff., 223]). But there is no reason why a use of the con-
cept of equilibrium which carries a "penumbra of approbation" should be
regarded as the only possible one; and there is certainly no reason why
it should be regarded as invalidating the *concept* of an "equilibrium of the
system" in and of itself. Finally, it should be clear that, in speaking of
a system "in equilibrium" as a "conceivable" type of functioning economic
system, there is no suggestion that it must be possible to "conceive" of
the actual *realization* of an equilibrium of *our* economic system in the
world we know.

[37] In this connection, see the comment by Professor Schumpeter on the
relation of the *concept* of an "equilibrium" of the system to "such phe-
nomena as overproduction, excess capacity, underemployment, maladjust-
ment," in his *Business Cycles,* 68. The proposition advanced in the text

Nor is the mere fact that, in monetary and business-cycle theory particularly, there has been altogether too much carelessness in defining the conditions of general "equilibrium," in itself sufficient to justify the rejection of the very concept of "general economic equilibrium" as an analytical device.[38]   For what is required, in these cases, is not a rejection of the very *concept* of a system in general "equilibrium," but a greater precision in the use of this concept.   And if it is true that in some cases the use of such a concept has led to a greatly oversimplified description of the causes of general "*dis*equilibrium," the fault is again to be found not in the use of the concepts themselves, but in the particular applications of these concepts of which certain of their sponsors have been guilty.[39]

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also applies, obviously, to the position of all those who would reject the *concept* of "equilibrium" on the ground that in the world we know there *is* no "tendency toward equilibrium" (see below, p. 446, and the references given in note 98 thereto). For, obviously, the categorical statement that "there is no tendency toward an equilibrium of the system" takes on meaning only upon the assumption that the sponsors of such a statement are making use of a definite *concept* of an "equilibrium of the system." It may be observed, further, that I do not deny the possibility of discussing the *issues* involved in the question of the reality of a tendency of the system to "equilibrium" without using the *terms* "equilibrium" or "disequilibrium."   Yet it is a striking fact that writers most critical of the usefulness of the concept of an "equilibrium" of the system have usually ended by advancing propositions which amount only to a loose translation of what can be shown to have been meant by writers who have made use of the terms "equilibrium" and "disequilibrium," as applied to the "system."   See, for example, Mitchell, *Business Cycles: The Problem and its Setting*, 187: "We know that the modern system *does not function smoothly* when the aggregates of the opposing items in certain pairs [such as expenditures and receipts] get *too much* out of *balance*" (italics mine). Clearly, an attempt to secure greater precision in the specification of what is meant by a "smooth" functioning of the system and "too much" "unbalance" in the "aggregates of the opposing items" would lead to attempts of the same *kind* as, though they might be of different substance from, those of earlier writers who have spoken of an "equilibrium" or "disequilibrium" of the system.

   [38] For examples of such carelessness in defining the conditions of "general ['monetary'] equilibrium," see Volume I, 72 ff., and the references there given. A particularly notable example of such carelessness was provided by Mr. Keynes in his *Treatise*. In this connection, see Volume I of the present work, 76 ff., and the references to Keynes's *Treatise* given in nn. 11–13 thereto; also 107 f., and the reference given in n. 14 thereto.

   [39] In this connection, see the comments of Kuznets, "Equilibrium Economics and Business Cycle Theory," *loc. cit.*, 399, where it is argued that

It can reasonably be expected of any analytical system, therefore, that it should be capable of representing a state of affairs characterized as one of "equilibrium," as well as a state of affairs regarded as one of "disequilibrium." This, indeed, is a simple corollary of the proposition of Marshall and Pantaleoni that "statics" can be conceived of as a special case of a general and inclusive "dynamics." [40]   It is

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it is the use of the concept of an equilibrium of the system that has been "largely, if not exclusively, responsible for those theories which attribute business cycles to one factor mainly:" for "whenever the concept of equilibrium is in the background of the discussion, it is sufficient for the solution of the problem of business cycles to point out one factor which accounts for the appearance of the oscillations." I should not myself go so far as to accept the generalization that the particular reason cited is *the* one which has been "largely, if not exclusively, responsible for" theories of the type rightly rejected by Dr. Kuznets; and I would certainly not accept his statement with respect to "the application of the equilibrium idea in the equation of exchange" (see above, p. 422, n. 33). On the other hand, I fear that Dr. Kuznets's proposition does apply to certain aspects of the argument of F. A. Hayek, particularly as that argument was developed in Hayek's *Monetary Theory and the Trade Cycle* (see, for example, pp. 42 ff., 65 ff., 86 ff., 92 ff., 95 ff., 101 ff., 139 ff., of that work; and cf. what is said on this matter below, p. 458. I should repeat, however, that there is nothing in this fact which can be taken as invalidating the *whole* of Hayek's argument in its *positive* aspects (see what is said on this matter above, p. 372, n. 50); and I should certainly defend the proposition stated in the text with respect to the alleged inevitability of this type of abuse of the concept of an equilibrium of the system.

[40] Cf. the references to Marshall and Pantaleoni given in Volume I, p. 120, n. 50, of the present work. See also Marshall's *Principles,* 366, n. 2, where "Statics" was held to be "really but a branch of Dynamics"; and cf. Lindahl, *Studies in the Theory of Money and Capital,* 31 ff., on dynamics, in a "broad sense" of the term, as *including* "the static problem." It may be observed that whenever the term "dynamics" is used in this "broad" sense, it is convenient to have some other term for that part of this broadly conceived "dynamics" which is *not* "statical" in character. I have used the clumsy term "non-statical analysis" in my first volume (see, for example, pp. 78 ff. of that volume); though it is of course clear that any adequate classification would break the general category of "non-statical analysis" down into subdivisions, of which the theory of economic "development," for example, would be one; a description of the "frictional" elements involved in the passage from one equilibrium position toward another would be a second; and so on. It may be observed further that the proposition that "Statics" is "really but a branch of Dynamics" may be interpreted in a sense different from that indicated in the text, where it is assumed to mean that a "statical" ("equilibrium") *situation* is a special case within the range of possible situations capable of analysis by the use of a truly "general" analytical apparatus. The proposition in question may mean, for example, that this general apparatus will include analytical *devices* (such as demand and supply schedules of the Marshallian

therefore of some importance to observe that the analytical framework here outlined can be used as easily for a description of an economic system functioning in a state described generally as one of "equilibrium" as it can be used for a description of the processes by which such a state can be disrupted, or, in general, of processes which can be characterized as "cumulative" in nature.[41]

type) which were originally developed in connection with the "static" problem of determining the conditions of equilibrium. Yet it is clear from the context surrounding the passages from Marshall cited above that the interpretation given in the text is the one which Marshall himself had in mind chiefly.

[41] From the illustrations which follow, it should be clear that it would be possible to accept only with the most serious reservations any statement to the effect that the reason why "equilibrium economics" is not "suited" to serve as an "instrument of causal analysis" is that such economics "cannot follow realistic processes occurring in time and cannot explain such processes" (so J. Åkerman, *Das Problem der sozialökonomischen Synthese,* 145). For if what is in question is that part of "equilibrium economics" which has undertaken to describe a system functioning in a state of "equilibrium," the examples provided below show that such a system is to be conceived of as functioning "in time" quite as much as systems which are in a state of "disequilibrium." On the "realism" of an "equilibrium economics" which would insist that the world we know is characterized by a "natural order" under which the actions of economizing individuals have "a tendency to lead [the system] to a position of general equilibrium" (Åkerman, *op. cit.,* 139), see what is said below, pp. 446 ff. The contentions here are merely (1) that the justification for the use of the *concept* of a "system" in "general equilibrium" does not depend upon the "real" existence of a "tendency" of the type just indicated; and (2) that if limitations attach to the concept of a system functioning in a state of "equilibrium," these limitations do not derive from the alleged fact that such a system cannot be conceived of as functioning in "time." On the other hand, it is clear that serious limitations do attach to any method, designed to deal with the problems of the real world, which would carry its concern with the statement of the conditions for "equilibrium" so far as to leave no room for the study also of "processes which can be characterized as 'cumulative' in nature" (cf. the text, above). This must, indeed, be borne continually in mind in any attempt to judge the usefulness of any proposed methods, designed to deal with the problems of economic "dynamics," which would undertake "to treat a process of change as consisting of a series of temporary equilibria." (See again Hicks, *Value and Capital,* 127; and cf. also Pareto, *Manuel,* 147. It should be added, however, that Pareto regarded "the study of successive equilibria" as only *one* of the *two* "dynamic parts" of economic theory, the other "dynamic part" consisting of a study of "the movement of economic phenomena," conceived in such a way as to leave room for the study of "cumulative processes": though it can hardly be claimed that Pareto himself made serious contributions to this second of the "dynamic parts" of economic theory.)

A complete demonstration of the possibility of using this analytical framework for the description of a system in a state of *equilibrium* is impossible here for a very simple reason: namely, that the descriptions of such a state have themselves been of so varied a character that a translation of *all* of these descriptions in terms of the analytical apparatus here presented would take very much more space than can be devoted to it in the present work.[42] On the other hand, it is anything but clear that all of the descriptions that have been presented of the characteristics of a "static" or "equilibrium" situation are of sufficient heuristic value in themselves to warrant serious attention. The most that can be done, therefore, is to take, as an example, an instance which is notable not only for the care with which the "static," or "equilibrium" situation was itself described, but also because this description was undertaken specifically as preliminary to a description of how this "statical" situation may pass over into a "non-statical" one. The instance in question is provided by Professor Schumpeter's description of what he calls "the stationary flow," and its relation to the inauguration of those particular "non-statical" processes which he himself regards as constituting "the phenomena of economic evolution." [43]

The stationary flow, according to Professor Schumpeter, is to be regarded as represented by "an unchanging economic process which flows on at constant rates in time and merely reproduces itself." [44] It will

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[42] The divergence between the various descriptions of a system in "equilibrium" is of course characteristic not so much of the "general" Theory of Value (in which the Walrasian statement of the conditions for equilibrium has come to be regarded as more or less standard) as it is characteristic of monetary and business-cycle theory, and particularly of attempts to state the conditions for what is characterized as *"monetary* equilibrium." For examples, see the references given in Volume I of the present work, p. 11, n. 7; pp. 72 ff. (together with the references given in nn. 1–15 thereto); 86 f.; 108 f.; 113; 128 f.

[43] See, for example, Schumpeter's *Business Cycles,* 35 ff. On the continued usefulness of the analytical framework presented here for the tracing of the mechanism of "non-statical" processes *other* than those stressed by Professor Schumpeter, see what is said below, p. 435, and in n. 62 thereto.

[44] See, for example, Schumpeter's *Business Cycles,* 35 f. It is only to be expected that there should be differences in detail as between Schumpeter's description of the "stationary flow" and corresponding constructions by other writers, in view of the fact that, as Marshall put it (*Memorials,* 413), each writer is "sole autocrat" when it comes to "statical constructions." It should be observed, however, that the Schumpeterian construction is anything but a freak, in the sense of bearing little resemblance to other "statical constructions." Cf., for example, Marshall's acceptance of "the case of a spinning top" as a metaphor for describing that "conception of 'steady motion'" which he held to be characteristic of the "static state" (*Memorials,* 315). On the interpretation of Schumpeter's suggestion that, in constructing a picture of the "stationary flow," no "reference to time" need be involved, since "rates" of flow may be "replaced" by "absolute quantities" (*Business Cycles,* 45), see what is said above, p. 112, n. 53. Cf. also Lindahl, *Studies in the Theory of Money*

be remembered, however, that our own analytical system is concerned precisely with "rates of flow," of both money and objects sold for money, in clock time. In order, therefore, to represent this first aspect of the "stationary flow," we have only to write a series of "stream" equations the characteristic of which is that the magnitudes relating to each clock-time period correspond in every essential respect to the magnitudes of every other clock-time period. In terms of *aggregates*, this would of course mean that $(MV)_{t_n} = (PT)_{t_n} = (MV)_{t_{n+1}} = (PT)_{t_{n+1}}$, and so on. But the same type of notation may obviously be used to express the constancy of all individual components of the monetary and "goods" flows over a series of clock-time periods. It is clear, that is, that we may write not only $M_{t_n} = M_{t_{n+1}} = \ldots M_{t_{n+x}}$; $V_{t_n} = V_{t_{n+1}} = \ldots$ $V_{t_{n+x}}$; $T_{t_n} = T_{t_{n+1}} = \ldots T_{t_{n+x}}$; and so on. We may write similar expressions making use of the concepts indicated in this work by the notation $(PT)_I$, $(PT)_{NI}$, $(PT)_i$, $(PT)_{ni}$, and so on. We may also write $(D_a)_{t_n} = (p_a q_a)_{t_n} = (D_a)_{t_{n+1}} = (p_a q_a)_{t_{n+1}}$, and so on, in which q_a and D_a refer to a particular commodity purchased and the particular stream of realized money demand involved in such purchase, respectively, in the time-periods indicated. In all these cases, that is, the failure of the "variables studied" to "change their values with the lapse of time" will bring it about that "the corresponding time curves" (that is, time series) of the "variables studied" will have "the nature of straight lines parallel with the time-axis.[45] It will be observed also that it would be very easy to express, in the terms indicated, that particular condition which is sometimes regarded as a condition of "equilibrium" of the system: namely, that there be *constancy of output and employment* over the period for which equilibrium is held to exist.[46] For, since both output (O) and employment (say, N) are components of the T of a "total transactions equation," we have only to write out these components in the form indicated above, so that $O_{t_n} = O_{t_{n+1}} = \ldots O_{t_{n+x}}$, and $N_{t_n} = N_{t_{n+1}} = \ldots N_{t_{n+x}}$.[47]

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and Capital, 31 f.: "Properly interpreted, static theory also has for an object economic developments taking place in time, only the variables studied do not change their values with the lapse of time." This, after all, was the meaning of Marshall's famous statement that while, in the "Stationary State, . . . the general *conditions* of production and consumption, of distribution and exchange remain motionless, . . . yet it [the Stationary State] is full of movement; for it is a *mode of life* (*Principles,* 367; italics mine).

[45] So Lindahl, *Studies in the Theory of Money and Capital,* 32. On the rôle of time-series in the analytical system here presented, see also what is said below, pp. 480 ff., 506.

[46] See, for example, the reference to Keynes's *Treatise* given in Volume I of the present work, p. 77, n. 13.

[47] At the same time, it would be perfectly possible to use the apparatus indicated to represent an "equilibrium" situation in which both employment and output would be changing in absolute magnitude, but in which

In the "stationary flow," moreover, as Professer Schumpeter goes on
to point out, just as in a *non-*"stationary" situation, the choices of pro-
ducers (assumed, in the case of the *"stationary* flow," to be *unchanging*)
will result in definite production functions for each firm (each of these
production functions again being assumed, in the case of the "stationary
flow," to be unchanging).[48] In our system, production functions are

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the criterion of "equilibrium" would be that there would be *full use* of
available labor and other resources, the absolute amount of such available
labor and other resources increasing from period to period (cf. Volume I
of this work, p. 76, n. 13). No one could deny that certain very serious
"logical difficulties," which too many writers have been inclined to "slur
over," are involved in the construction of such a picture of "'balanced'
or 'equilibrated progress'" (cf. Schumpeter, *Business Cycles,* 37). But the
apparatus here outlined can certainly be used to draw a picture which
will be no more ambiguous than any description of the type of situation
indicated which does *not* make use of the notation suggested here; and
such a picture will certainly be *less* ambiguous than those versions which
fail to bring out clearly the distinction between *constant* output and em-
ployment, on the one hand, and "full" employment and maximum out-
put, on the other (cf., in this connection, the comments on the relevant
parts of the argument of Keynes's *Treatise* in Volume I of the present
work, p. 42; p. 76, n. 13; and p. 201). For although, under the conditions
indicated, we now have $O_{t_n} < O_{t_{n+1}} < O_{t_{n+2}}$, and so on, and the same
statement holds for most of the other magnitudes of our "stream" equa-
tions, the constancy as between successive periods would be found in a
series of coefficients which would themselves indicate the degree to which
output and employment would represent "full" utilization of resources in
each period. Thus, if $\Omega_{t_n} = O_{t_n}/O_{m \cdot t_n}$, in which O_{t_n} represents actual
output and $O_{m \cdot t_n}$ represents maximum output obtainable from a "full"
use of resources in the period t_n, then the condition of "equilibrium" in-
dicated would be represented by the expression $\Omega_{t_n} = \Omega_{t_{n+1}} = \ldots$
$\Omega_{t_{n+x}} = 1$.

    [48] Schumpeter, *Business Cycles,* 38 ff. It may be observed that, for our
present purpose, no difficulty is created by any disputes that might arise
as to what is to be regarded as a "change" in production functions (cf.
Schumpeter, 39 f.). For the proposition advanced in the following sen-
tence of the text makes it clear that in any case the relevant production
functions, if they are to be related to realized market events altogether,
must be related to such events through the use of market supply and
demand schedules, in our sense of the term. Any dispute, therefore, with
respect to what is to be regarded as a "change" in production functions
and what is not, would mean simply a difference of opinion as to how
much variation in these market supply and demand schedules (and there-
fore in the realized market events to which such variation may lead) is
compatible with the particular degree of "stationariness" which is at-
tributed to the "stationary flow." (Cf. also, in this connection, Macken-
roth, *Theoretische Grundlagen der Preisbildungsforschung und Preispolitik,*
116, where it is pointed out that it is possible to neglect, in the analysis

introduced by our proposition that any factors held to affect realized market actions, and therefore realized money prices, must be related to a given market supply or demand schedule, in our sense of the term.[49] Specifically, these production functions will help to determine (1) the rate (in clock time) at which the entrepreneurs in question will add to market supply (the q's of our formulation), and the supply prices at which this supply will be offered in the market; as well as (2) the direction and level of the realized money *demands* (the D's of our formulation) which these entrepreneurs, in effecting the processes pointed to by their chosen production function, will direct against the resources they require.[50] The picture may be painted in as much further detail as is required, with the introduction of monopoly, quasi-rents, or any other features that may be desired.[51] For in all cases the *plans* of the entrepreneurs, monopolist or otherwise, are capable of relation to market *action* by the use of market demand and supply schedules, in our sense

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of a *stationary* process, such "changes" as are represented by changes in functions which do not affect the range of values within which the currently *realized* [*wirklichen*] value of a variable lies; although it is pointed out also that it would obviously *not* be safe to neglect such changes in data in the analysis of a *"non*-stationary" process, even if the "nonstationariness" itself resulted initially from a change in data elsewhere in the system). A dispute on such matters would not affect the fitness of the apparatus here outlined for representing events within the "stationary flow," whatever *degree* of stationariness may be assumed in the construction of the latter. The same statement applies, obviously, to any possible dispute as to whether or not "saving" is or is not to be regarded as consistent with "stationariness" (cf. Schumpeter, *Business Cycles*, 40). For net saving which results merely in a shift in the proportions of a fixed amount of aggregate money expenditure devoted to the purchase of consumers' and producers' goods, respectively, will be represented by compensating shifts in our market demand schedules; while net saving which is held to bring about "decisions about [the use of] monetary funds" (Schumpeter, 75) of such a kind as to change, for example, the dimensions of the aggregate stream of money expenditure, will be taken care of by those variables in our "stream" equations which are designed precisely for the purpose of tracing such changes, *as well as* by changes in our market demand schedules. In both cases, clearly, our apparatus permits the representation of any degree of "stationariness" that is held to characterize the so-called "stationary flow."

[49] See above, pp. 238 f., and also below, pp. 533 ff., 553 ff.

[50] In this connection, see especially what is said above, pp. 395 ff., and p. 397, n. 107, with respect to "reaction speeds" on the side of supply, and the representation of a constancy (or an absence of constancy) in such "reaction speeds" ("rates of flow") in terms of the T's of our formulation, and their components, when those T's and their components are provided with clock-time period subscripts. On the relation of the q's (or T's) of our formulation to "supply curves" in general and to the concept of "elasticity of supply" in particular, see the forward references given at the end of n. 105 to p. 396, above.

[51] Cf. Schumpeter, *Business Cycles*, 40 f.

of the term; and these schedules, in turn, are capable of relation to the market actions of entrepreneurs by being related, in the manner indicated by our Proposition IV, to realized streams of money and of objects sold for money, and therefore to the realized prices whose explanation is one of the essential objects of the analytical apparatus here proposed.[52] The same thing can be said of the plans and actions of *consumers:* in all cases the elements of choice involved in consumers' *plans* (as summarized, say, by the familiar apparatus of indifference curves) are related to the market *actions* of consumers by being related first to the market demand schedules of these consumers, and then, by our Propositions II to IV, to the streams of money expenditure which are effected whenever prices are "realized" at the point indicated by the intersection of these market demand schedules with the corresponding market supply schedules.[53]

All this, it will be observed, is true in *any* situation, whether the situation considered corresponds or does not correspond to that described by the Walrasian-Schumpeterian "stationary flow"—the case of the "stationary flow," as Marshall might have said, being merely a special case within the complete range of "dynamic" possibilities.[54]   The criterion distinguishing this special case represented by the "stationary flow" is merely that there shall be no change in production functions, consumers' choices, or any of the elements which would prevent the economic process from turning out, "year after year, the same kinds, qualities, and quantities of consumers' and producers' goods," with every firm employing "the same kind and quantities of productive goods and services," all these goods and services being "bought and sold at the same prices year after year." [55]   Now, however, we introduce into the situation, in the manner of Professor Schumpeter, elements which will upset these "stationary" practices of entrepreneurs and consumers.[56]   By the terms of Professor Schumpeter's argument, the main-

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[52] For Proposition IV, see above, p. 263.   The reader is again reminded that any system designed to account for the determination of realized *prices* necessarily involves a concern with the forces determining the quantities of the *object sold* at these prices.   He is again referred, therefore, to the discussion presented below in Chapters Ten and Eleven, of the rôle played by the concept of "elasticity of supply" in the Theory of Prices.   See also what is said concerning the representation of "quantities sold" at realized prices in the mechanical "model" described below, pp. 479 ff.

[53] For Propositions II to IV, see above, pp. 240 f., 263.

[54] Cf. the references given above, p. 427, n. 40.

[55] See Schumpeter, *Business Cycles,* 41.

[56] The reference here is to that part of Professor Schumpeter's argument which is summarized on pp. 87 ff. of his *Business Cycles.* It should hardly be necessary to labor the point that use of the Schumpeterian argument with respect to the mechanism of the introduction and spread of "innovation" does not necessarily imply agreement with all parts of Professor Schumpeter's argument with respect to the "upsetting" rôle of other factors than entrepreneurial innovation (see, for example, *Business*

spring of this "upsetting" process is to be sought in entrepreneurial "innovation." And (again by the terms of his argument) "innovation" is to be thought of as involving a variation in the form of the production functions, and therefore a change in the conformation and position (a "shifting") of cost curves.[57] In order, however, for these "shifts" in cost curves to affect realized market processes, they must be related, in the manner suggested above, to entrepreneurial action with respect to (1) supply price, in which case market supply schedules, in our sense of the term, will be affected; and (2) realized entrepreneurial *demand* directed against the elements in the system now desired in "new combinations," in which case market demand schedules, in our sense of the term, will be affected.[58] By the Schumpeterian argument, these initial shifts in entrepreneurial supply and demand curves will be "imitated," elsewhere in the system, by lesser entrepreneurs (or "managers").[59] Each of these types of "shift"—both the initial shift and the "imitative" shifts—will be dated in terms of clock time. And each of them, as long as they are held to be related to realized processes, will be associated with changes in the dimensions and direction of those streams of money payments and of objects sold for money, the explanation of whose magnitude represents an essential part of the subject matter of the analytical system outlined in the present work.

There is, moreover, no limit to the detail in which any one of these stream-embodied processes in time may be treated in terms of our analytical system. In all versions of Professor Schumpeter's argument, for example, great stress is put upon the importance of entrepreneurial borrowing from commercial banks as a determinant of the dimensions and directions of these streams.[60] It should be observed, therefore, that

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Cycles, 72 ff.). On the contrary, it would be easy to show that the mechanism of the processes engendered by the intrusion of *any* kind of "upsetting" factor can be traced by the use of the general apparatus here presented. See, for example, what is said below, p. 435, n. 62.

[57] See Schumpeter's *Business Cycles,* 87 ff. It will be observed that the "shifting" of cost curves may, and usually will, imply a change in the *conformation,* as well as the position, of these curves. The terminological usage here employed is in accordance with Professor Schumpeter's own. See, for example, *Business Cycles,* 91, 93, 96, 97.

[58] On the meaning assigned by Professor Schumpeter to his concept of "New Combinations," see his *Business Cycles,* 88. Again it should be observed, however, that the apparatus here outlined is quite capable of dealing with the phenomenon of "new combinations" even when the latter fall within the categories excluded by Professor Schumpeter in the passage cited.

[59] See, for example, Schumpeter's *Business Cycles,* 100 ff.

[60] See, for example, Schumpeter's *Business Cycles,* 109 ff. It is again clear, of course, that the usefulness of the apparatus here outlined for tracing the steps involved in monetary expansion and contraction in no sense involves acceptance of the particular part of Professor Schumpeter's argument which insists upon "credit creation" as "the monetary complement of innovation" (*Business Cycles,* 111 f.). See, for example, what is said below, p. 435, n. 62.

the openings for the introduction of this element are provided by two simple facts. The first fact is that each of our expressions for realized "demands" is reducible to the components (1) the absolute volume of cash balances (and particularly the absolute volume of such balances in the form of "bank money" [M']); and (2) the rate at which these balances are disbursed ($V = 1/K$), the changes in both M' and $V (= 1/K)$ being in all cases dated in terms of clock time. The second fact is that in each case the symbols for these components call attention to the body of analysis, within "monetary" theory and trade-cycle theory, which is to be invoked in order to explain why the streams of money expenditure are as large as they are.[61] So one could go on, the translation of any description of the "dynamic" process being extended to the contraction phase, with all that this description may involve with respect to further shifts in market demand and supply schedules and changes in the dimensions and direction of the money stream (or streams), including the effect, upon the latter, of possible movements in the rate of interest.[62] The important thing is that we have, in the

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[61] It must be remembered that an essential part of the apparatus thus indicated is its emphasis upon the possibility of dealing with a "plurality" of "price levels," and therefore with a "plurality" of streams of money and of objects sold for this money, respectively. It should be observed, also, that the inclusion of *trade cycle* theory" as well as "monetary" theory, in a narrower sense of the latter term, makes it possible to do full justice to elements which, although themselves of "non-monetary" origin, can nevertheless be shown to *affect* the dimensions and the direction of monetary streams. Specifically, for example, they can be shown to affect the *rate of profit expected* from a bank loan, and therefore the quantity of bank money (M'). Or they can be shown to affect the scale of investment opportunities open to holders of "surplus" cash, and therefore the rate at which existing cash balances are spent ($1/K = V$). Indeed, the introduction into our system of the element of "expected profit" is one of the ways in which theories of "economic development"—in other ways as disparate as those of the "classical" economists, Marx, and Schumpeter, respectively—can be shown to converge, in the sense that they all contain elements which, however divergent otherwise, can be shown to involve the same kind of impingement upon that system of money flows which mirrors the functioning of modern capitalist society. See also the following note.

[62] It is unnecessary to labor the point that this possibility demonstrates at once the applicability of the general system here outlined to a tracing of other types of process in time. It is applicable, for example, to the type of process envisaged by what might be called the "Mises-Hayek effect," the essence of which is a series of changes in the dimensions of the given sectors of the aggregate money stream in response to changes in the rate of interest, with all that this is held to imply with respect to the phenomenon sometimes known as "forced saving" and the ultimate shifts in the proportions of goods of "higher" and "lower order" produced (the "structure of production") in response to these shifts in the money streams. It should be equally clear, however, that the use, for this purpose, of the analytical system here outlined would involve none of the

analytical system here outlined, a framework which is as perfectly fitted for description of the processes inaugurated by the introduction of "dynamic" factors as it is for a description of the processes characteristic of a system functioning in a state of "equilibrium" before these "dynamic" factors were introduced.[63]

The only further comments, indeed, that are called for here have to do with the rôle played in a system such as that outlined, by the type of emphasis upon the relation between *costs* and *selling prices* which was to be found in the Fundamental Equations of Keynes's *Treatise*.[64] These comments are as follows:

1. Proof that the analytical system here outlined is perfectly capable of taking account of the effects of changes in the level of *costs* is provided by the argument stated above with respect to the relation of this system to a series of production functions for individual entrepreneurs. For, as we have seen, changes in these production functions are held to affect the system of realized prices and entrepreneurial action based upon this system of prices by way of their effects upon (i) entrepreneurial supply curves; and (ii) realized entrepreneurial demands, as controlled by the new cost-price situation in which each entrepreneur finds himself as the result of a change in the relevant production function.[65]

2. The argument for the use of a system such as that outlined in the present work is strengthened, rather than weakened, by the fact that this cost-price situation may be affected *not only* by changes in "production functions" of the kind usually discussed within the "general"

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claims for the universality of this "effect" as a cyclical phenomenon that have sometimes been read into the exposition of Hayek, in particular. On the contrary, the apparatus here outlined could be applied equally well to the type of process envisaged by what might be called the "Hawtrey effect," with its emphasis upon the importance, for the level of output and prices, of expansion and contraction in "general (money) demand" ("consumers' outlay"), and its tracing of these movements to successive "releases" and "absorptions" of cash—all of the steps in the latter, it should be observed, being perfectly capable of translation into the variables of our "stream" equations. Precisely the same thing may be said, of course, with respect to the type of effect upon the level of investment expenditure which, by the argument of Mr. Keynes's *General Theory*, is to be attributed to significant changes in the "propensity to consume" or "liquidity preference."

[63] The reader who is inclined to conclude from this proposition that the very "generality" claimed for the analytical system here outlined deprives it of all "usefulness," is advised to read what is said below, pp. 494, 515 ff.

[64] On the treatment of the relation between prices and costs (particularly "wages") in Keynes's *General Theory*, see below, pp. 563.

[65] It is of some importance to observe that the "cost-price situation" which is held to condition the level and direction of realized entrepreneurial demands is of an *ex ante* character. See, for example, what is said under (5) below (p. 438 f.), with respect to the relation between *realized* and *"expected"* costs; also what is said under (6) below (pp. 439 ff.), with respect to the relation between "costs" and the stream of money payments.

Theory of Value (namely, changes associated with *non-monetary* factors), but also by changes of a monetary character. For our market supply and demand schedules are regarded in all circumstances as subject to change in either position or conformation as the result of monetary, as well as non-monetary factors.[66]

3. In contrast with the type of apparatus represented by the Fundamental Equations of the *Treatise,* our emphasis upon the necessity of relating cost-price calculations to the relevant market supply and demand schedules, in our sense of the term, implies a continuing emphasis upon the importance of studying the cost-price positions of *individual entrepreneurs,* rather than, in the first instance, the cost-price position of the "system as a whole." Thereby we avoid all the objections to the Fundamental Equations of the *Treatise* which were based upon their alleged obscuring of the importance of studying cost-profit relations in *different parts* of the system.[67]

4. This does not mean, however, that a system such as that outlined in the present work implies a lack of interest in, or is incapable of dealing with, movements in *aggregates.*[68] The possibility, stressed in earlier parts of this work, of *summing* the terms of the various "partial" stream equations into significant aggregates or sub-aggregates, proves the direct contrary.[69] And there is obviously no reason whatever why, even without an actual summation of these more microscopic magnitudes into more macroscopic ones, it should be impossible to generalize, with

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[66] See our Proposition XI (above, p. 304), and also what is said below, pp. 597 ff. Included, of course, in the list of "monetary" factors affecting the position or conformation of market supply or demand schedules, in our sense of the term, would be not only (1) those deriving from the fact that "when increased or decreased purchasing power in the form of money, seeking to realize itself in actual purchases, comes into, or is withdrawn from, the market, the increase or decrease (as the case may be) is not spread evenly and proportionately over the various buyers"; but also (2) all those consequences resulting from institutional obstacles to *general* price flexibility during periods of monetary expansion or contraction. See Volume I, pp. 501 and 504, and the references there given; also what is said above, p. 159, n. 33, on Mr. Keynes's failure to translate these factors into terms of demand and supply schedules.

[67] See Volume I, 277 f., of the present work, and the references to Pigou and Hayek given in n. 26 thereto. Cf. also the comments by Schumpeter, *Business Cycles,* 43, on the use of "aggregative quantities," including the "net total of profits," in Keynes's *Treatise.*

[68] For a further discussion of the rôles accorded to an emphasis upon "aggregates" and upon "partial" magnitudes, respectively, in the analytical system here outlined, see below, pp. 501 ff.

[69] See, for example, what is said on this matter in Volume I, pp. 512 and 516, and also what is said above, pp. 323 ff. It should be observed that since the "summation" involved applies to the summation of *realized* magnitudes, it is not open to the objections that have been raised to a mechanical summation of "expected" magnitudes (see below, p. 503, and n. 101 thereto).

respect to the cost-price situation prevailing in the system "as a whole" at any given time, upon the basis of a study of the situations confronting a majority of individual entrepreneurs at that time.[70]

5. An argument for retaining the latter method in many cases is provided by a further fact which was adduced, in Volume I of the present work, against the usefulness of the Fundamental Equations of the *Treatise* as a weapon for dealing with the type of situation characterized as one of general "disequilibrium": namely, their failure to distinguish adequately between realized "costs" and *expected* "costs," of which the latter are certainly the more important in determining whether the situation is one of stability or is about to be transformed into a cumulative movement upward or downward.[71]   In the system outlined in the present work, the expectations of entrepreneurs are regarded as affecting (1) the position and conformation of market supply or demand schedules, in our sense of the term; and (2) separate components of our stream equations, such as the quantity of bank money, the rate at which cash balances are spent, and the rate at which stocks of commodities are moved toward or held back from the market.[72]   In other words,

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[70] In this connection, compare what is said above, pp. 333 f., with respect to the possibility of obtaining a picture of a "general" movement of prices by plotting the individual prices in a "swarm" of prices, and then "generalizing concerning the movements of the 'swarm' on the basis of the picture of the movement of individual prices thus obtained." The phrase "a *majority* of individual entrepreneurs," used in the text, is of course to be understood in a sense in which the number of individual entrepreneurs confronted by favorable and unfavorable cost-price situations, respectively, would be *weighted* in accordance with (1) the *quantitative* importance of the individual firms as *disbursers of money funds;* and (2) their *strategic* importance, in the sense that certain firms whose money disbursements are themselves quantitatively less than the money disbursements of other firms, may be located at so "strategic" a point in the economic process that expansion or contraction of their total money disbursements may be expected to bring in its train a *subsequent* expansion or contraction of the larger disbursing units (the case, for example, of Hawtrey's strategically located "traders"). On the possibility of constructing an actual weighted *index* of "profits" of this general type, see Myrdal, *Monetary Equilibrium,* 72, 76 f., 80 f. The problem thus raised, of course, is the ancient one, raised by the "classical" economists (though by no means always with successful results), of the possibility of deriving a *general* upward or downward movement in business from a situation in which the gains of one group in the community may be "cancelled" by the losses of another. Its adequate discussion must therefore be deferred to a publication which will deal explicitly with the effect of monetary expansion and contraction upon "output as a whole."

[71] See Volume I, pp. 278 f., and the references to Hawtrey, Myrdal, and Ohlin given in n. 27 thereto (the corresponding passage in Myrdal's *Monetary Equilibrium* is to be found on pp. 32 f. of the latter work); and cf. the comment of Mr. Keynes himself in his *General Theory,* 77.

[72] From our Propositions II to VII (above, pp. 240, 241, 263, 274, 280), it should be clear that an emphasis upon (1) and an emphasis upon (2),

while all possible emphasis is put upon the necessity for tracing in all necessary detail the steps involved in *realized* processes, these realized processes are at all points regarded as related, as both cause and effect, to the element of "expectations," without any of the confusion that is necessarily engendered whenever there is uncertainty as to whether the cost-profit situation under discussion is being described in terms of *realized* or *expected* magnitudes.[73]

6. The real element of novelty in the Fundamental Equations of the *Treatise,* as was pointed out in Volume I of the present work, was their attempt to include in a *single brief expression* a statement of the relation between *costs, incomes,* and *outlay* from incomes upon output.[74] We saw, in Volume I, that the particular type of relation described by the Fundamental Equations, instead of being one of universal validity, was one which might be expected to hold only under a highly special set of conditions.[75] It may be observed here, on the other hand, that the analytical system outlined in the present work is at once such as to avoid the assumption of a special set of conditions and such as to permit the representation of the particular cases in which the assumption of such conditions *would* correspond to the facts of the real world. Specifically:

(*a*) The fact that our "stream" equations include only actual money payments makes it clear that they do not involve, as the Fundamental Equations of the *Treatise* involved, the assumption that *all* elements of "cost" are simultaneously elements in the flow of money payments, and particularly in the flow of money incomes.[76] As is pointed out below under (*b*), the cases in which elements of "cost" *do* enter into the flow of money payments and money incomes are perfectly capable of representation within the analytical system here outlined. In cases in which they do not enter, the "costs" involved are introduced in such a way as to avoid the assumption that "costs of production and money incomes are really only two different aspects of the very same thing."[77] Spe-

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instead of being antithetical, are strictly complementary. For while any realized price is given by the intersection of the market demand and supply schedules indicated under (1), we know, from Proposition V (p. 274), in particular, that the *explanation* of why these market demand and supply schedules have the position and conformation they do have, can be provided only by a *combined* use of what is offered by *both* the "general" Theory of Value and the type of contribution provided by "monetary" theory which is indicated under (2). The point made here is merely that in both bodies of theory the element of *expectation* necessarily bulks large.

[73] See what is said above, pp. 225 ff., 382 ff., on the relation between "realized" and "expected" magnitudes in the analytical system here outlined; and cf. also what is said below, p. 678, n. 2, concerning certain aspects of the argument of the *General Theory* on the matter of the relation between "realized" and "expected" magnitudes.

[74] See Volume I, 124 ff., of the present work.

[75] See Volume I, 128 ff.

[76] See Volume I, 127 ff., 271 ff.

[77] Cf. Volume I, p. 130.

cifically, those costs the incurring of which does not involve a corresponding movement in the stream of (money) cost *payments* are regarded as elements that may condition some type of realized action which *in turn* may affect the generation and utilization of money incomes.[78] The precise *nature* of these relations between the "costs" in question and the generation and utilization of money incomes, that is to say, is left for specific investigation in the concrete case, instead of being assumed to be of the unequivocally simple character implied by Mr. Keynes's earlier Fundamental Equations.

  (b) This does not mean, however, that the apparatus here outlined is *incapable* of being used to represent the special case in which all costs may be regarded as entering directly into the flow of money payments and particularly the flow of money incomes.

  Let us suppose, for example, that we wish to present, as the Fundamental Equations of the *Treatise* purported to present, a method for determining the amount of "profit" actually *realized* as a result of a difference between realized "costs" and realized selling prices.[79] Our

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  [78] The point may be illustrated by the two examples of incurred "costs" which do not generate money income given in Volume I (cf. p. 128, n. 63, and 129, n. 64): namely, passed "dividends" on preferred stock and certain forms of "depreciation." The failure to cover the preferred "dividend" and depreciation quotas may affect adversely the plans of the firm involved, either (1) with respect to borrowing from commercial banks (and therefore the quantity of bank money, M'); or (2) with respect to the "investment" of "surplus" cash balances ($V = 1/K$). In both cases, it should be observed, the high level of "costs" (relative to selling prices) will mean a *lower* level of money incomes for a good part of the period over which the level of the particular "costs" involved remains high. On the general problem of the "Classification of Costs with Regard to their Income-Generating Effect," see Lundberg, *Studies in the Theory of Economic Expansion,* Chapter VII, especially pp. 156 ff.; and on the relevant aspects of the argument of Keynes's *General Theory,* see what is said below, pp. 611 ff.

  [79] It is evident, of course, that the computation of "realized" *costs* is itself impossible without the introduction of some element of "expectation," whenever the "realized" costs involved are not unequivocally identifiable as realized money *payments*. Depreciation allowances, for example, necessarily involve some estimate with respect to the degree of obsolescence, and so on, that may be expected in the future (see, in this connection, Myrdal, *Monetary Equilibrium,* 60 f. [under (2)]; also 93 ff., of the same work; and cf. F. H. Knight, "Professor Hayek and the Theory of Investment," *Economic Journal,* XLV [1935], 80 n.). It should be equally evident, however, that there is nothing in this fact which would warrant a denial of the validity of the distinction between *ex ante* cost-estimates, on the one hand (corresponding to the " 'expected' costs" discussed above, pp. 438 f., under [5]), and "realized" costs actually charged to the business in a given period, on the other. Nor, certainly, is there in this fact anything that would argue against a formulation which would distinguish between those "realized" costs which are accompanied by money payments, on the one hand, and those, on the other, which are not accompanied by

first problem is to allow for the fact that all "costs" not only do not necessarily enter into money *incomes*, but also do not even necessarily enter into the flow of money *payments*. In all strictness, therefore, we should include in the figure for "realized" costs not only "costs" involving actual money payments, but also "imputed" costs, such as certain forms of depreciation, and interest charges payment of which is deferred.[80]   The desired figure for "realized" profits, in other words, would be given by the expression $(PT)_S - (PT)_x - X_u$, in which $(PT)_S$ represents the amount of money payments received by sellers of a given commodity, $(PT)_x$ the amount of money payments which they had previously disbursed as the costs of producing that commodity, and X_u the amount of "imputed" costs incurred in producing the commodity, but unaccompanied by money payments.[81]   An analytical system such as that outlined here, which encompasses money payments of all kinds, must necessarily yield the required figures for $(PT)_S$ and $(PT)_x$.[82] Necessarily, therefore, it will also yield a figure for "profits" (Q), in the sense indicated, *in the special case in which X_u has a value of zero.*[83]

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such payments. On the contrary, the very fact that the difficulty indicated above applies specifically to "cost" elements of the latter type, argues for a segregation of these "costs" from costs the magnitude of which is made unequivocal precisely *because* they are accompanied by a corresponding amount of money cost-payments.

[80] It may be observed that the fact that these "deferred" interest payments may *never* be actually made would constitute a reason for segregating them from interest charges which *are* being paid currently, even if there were no objections on other grounds to defining "money income" in such a way as to include the amount of *claims* accruing to income recipients, when what is involved is the construction of a system of "stream" equations of the type outlined in the present work. On this last point, see what is said in Volume I, 375 ff., and the references there given.

[81] The matter of the *time relations* between $(PT)_S$ and $(PT)_x$ is of course a matter of very great importance. Since, however, the dating of all money payments in terms of clock-time by the use of time-period subscripts is an essential feature of the analytical system here outlined, the difficulty is adequately met. See what is said on this matter below, p. 445 ff., under (*c*). On the element of "expectation" involved in certain of the "imputed" costs included in X_u, see what is said above, p. 440, n. 79.

[82] The essential methodological point involved, of course, is that every money payment may be classified and subclassified in as many ways as is called for by the analytical problem chosen for investigation, the payments falling within any one of these subclassifications being then capable of summation in the case of each category of payments whose magnitude is regarded as significant for a given problem. For an example of the application of the method indicated, see Volume I, p. 383, of the present work.

[83] It is clear, of course, that in cases in which the magnitude of X_u for any given commodity, while greater than zero, bears a constant relation to the $(PT)_x$ for the same commodity, the expression $(PT)_S - (PT)_x$, although it will not give a figure for "profits" in the strict sense of the

The system here outlined, that is to say, is perfectly capable of deriving a figure for realized profits from a comparison of the money cost-disbursements and the money sales-receipts of individual firms, *in those cases in which such a derivation is permissible;* and an algebraic summation of these figures for realized profits will give us the figure for profits in our economic group "as a whole," *in those cases in which a figure of the kind indicated would in fact represent "profits," in a significant sense of the term.*[84] It will not derive such figures, however, through the uncontrolled use of a device, such as that represented by the Fundamental Equations of the *Treatise,* which claimed to perform simultaneously the two-fold task of explaining (1) why the total *stream of money payments* is as large as it is; and (2) why the *relation between costs and selling prices* is what it is. On the contrary, it will insist upon giving primary place to the former task (interpreted in such a way as to include also the task of explaining why the *individual* streams of money payments have the dimensions and directions they have, and, therefore, why individual realized prices are what they are); and it will leave the task of accounting for the divergences between costs and selling prices for subsequent analysis, which will make use of weapons that will do justice not only to the institutional and frictional elements that necessarily enter into the problem, but also to the necessity for taking into account cases other than that in which $X_u =$ zero.[85]

Precisely the same kind of procedure would be followed if we were to pass to the next phase of the problem. We may assume that we are dealing with a case in which $X_u =$ zero. It does not follow, however,

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term, will provide a satisfactory indication of *variations* in these profits. It should be equally clear, however, that there is no *a priori* reason why the ratio $(PT)_x/X_u$ should remain constant in the world we know.

[84] On the limitations on the use of such a figure for profits in the system "as a whole," see what is said above, p. 437, under (3); though see also what is said on p. 438, n. 70.

[85] More specifically, it should be observed that any attempt to explain why individual realized prices are what they are would include also attention to the *supply* side of the problem, in the case of each commodity. On this matter, see especially Chapter Eleven below. What this means, in particular, is that any adequate account of the reasons for the conformation and position of the *supply curves* for individual commodities would include a consideration of the "institutional and frictional elements" which make these supply curves what they are, and therefore help to determine the relative degrees of price change evidenced by different categories of "prices," including the relative degree of change in those prices which are "costs" to a "majority" of entrepreneurs (see above, p. 438, n. 70), on the one hand, and those, on the other hand, which are "selling prices" to a "majority" of entrepreneurs. It must be evident that the result most likely to emerge from an investigation of this type is that all generalizations with respect to the course of "profits" during a period of monetary expansion and contraction must rest upon an empirical basis and therefore are subject to check and possible modification as the result of continuing empirical studies.

that we may assume that the total of money cost-payments—that is, $(PT)_x$—enters in its entirety into money *incomes*. On the contrary, a large part of these payments which are elements in cost to the disburser of the payments may be merely "traders' receipts" to the recipient of the payments: that is, $(PT)_x = (PT)_{x \cdot I} + (PT)_{x \cdot NI}$.[86] In order, therefore, to be able to assume that all cost disbursements represent payments into money income, we must assume that $(PT)_{x \cdot NI} = $ zero.[87] Clearly, the analytical system here outlined is perfectly capable of representing the special case in which this assumption would be a valid one. It is to be observed also, however, that this analytical system shows that the case in question *is* a special case, and not one having the degree of general validity suggested by an easy identification, as in the case of the Fundamental Equations, of "costs" with payments into incomes.

It can be shown, likewise, that the analytical system here outlined is entirely capable of representing a further series of special cases in which "costs" may be identified with "incomes," without involving any of the arbitrary implications as to the universal validity of such an identification which were involved in an apparatus such as that represented by the Fundamental Equations of the *Treatise* or their equivalent.[88] There are special cases, for example, in which it would be true to say, not (1) that all "costs" represent money payments into incomes; but (2) that all money *incomes* received necessarily represent *costs* to the disburser of these "incomes." The two propositions, it should be observed, are anything but identical.[89] Consider, for example, the case in which

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[86] Again the general method involved is that indicated above on p. 441, n. 82. Cf., for example, equation (3) on p. 383 of Volume I of the present work.

[87] On the case in which $(PT)_{x \cdot NI}$ is greater than zero, but bears a constant relation to $(PT)_{x \cdot I}$, compare what is said above, p. 441, n. 83.

[88] An "equivalent" of the essential concept underlying the Fundamental Equations of the *Treatise* is to be found, for example, in one of the propositions characterized by A. P. Lerner ("The Relation of Wage Policies and Price Policies," *American Economic Review* Supplement for March, 1939, p. 159) as "Keynes's Law": namely, "the costs incurred in the production of any commodity constitute the incomes out of which comes the demand for all the other commodities." On this matter, see also what is said below, p. 444, n. 91 and p. 445, n. 93, and also below, pp. 611 ff.

[89] If, for example, we let $(PT)_{nx \cdot I}$ represent the amount of payments into income which do not represent costs to the disbursers of these payments into income, then we may write $(PT)_I = (PT)_{x \cdot I} + (PT)_{nx \cdot I}$. We have already written $(PT)_x = (PT)_{x \cdot I} + (PT)_{x \cdot NI}$. The proposition designated as (1) in the preceding sentence of the text would then represent the special case in which the $(PT)_{x \cdot NI}$ of the latter equation would be equal to zero; whereas the proposition designated as (2) would represent the special case in which the $(PT)_{nx \cdot I}$ of the first equation would be equal to zero. In other words, in order to be able to use any proposition based upon the assumption that $(PT)_I = (PT)_x$, we should have to assume (1) that both $(PT)_{nx \cdot I}$ and $(PT)_{x \cdot NI}$ are equal to zero; or (2)

money incomes are generated by way of a dole financed by the creation of money *ad hoc*. The disbursements into income thus involved could be said to represent "costs" to the disbursing agency, in a sense comparable to that in which a private agency would be subject to certain "costs" of "production," only if we were prepared to accept a series of conceptual constructions of the greatest possible degree of tortuousness.[90] Or consider the case, much more important practically, in which the payments into income are payments of "profits" by a corporation to its owners. It was in order to salvage the identification of "incomes" with "costs" that such "profits" (apart from "normal" profits) were excluded from "income" altogether by the definitions underlying the Fundamental Equations of the *Treatise*.[91] No such arbitrary procedure is required, however, when no attempt is made to provide a formulation which would pretend to summarize simultaneously the forces determining the level and direction of streams of money payments (including payments into income), on the one hand, and the relation between costs and selling prices, on the other. On the contrary, the analytical system here outlined would start from the proposition that, of the total of payments into income in any given time period, some would represent "costs" to the disburser of these payments, and some would not; the case in which we could assume that all payments into income would represent costs to the disburser of these payments would

that they are equal to each other. There is clearly no reason whatever for assuming *a priori* that either of these conditions will hold in all possible cases.

[90] It is of some interest to recall that Mr. Keynes himself, instead of attempting to argue that dole payments of the type indicated represent "costs" to the disbursing agency, preferred to fall back upon an even more tortuous construction, according to which a dole was to be regarded as a case of "negative saving" or as a case of "investment." See Volume I, 131, n. 69, of the present work, and the references there given.

[91] It is of considerable importance to stress this fact, in view of the circumstance that the virtual identification of "costs" with "incomes" has been sponsored by followers of Mr. Keynes's *General Theory*, according to which "income" includes not only income to the ordinary factors of production, but also *entrepreneurial* income, which is defined as including not only a "normal" profit, but the whole of the "excess of the value of . . . finished output sold during the period over . . . prime cost" (*General Theory*, 53; cf. also pp. 60 and 77 of the same work). The result, of course, is that propositions with respect to the effect upon "incomes" of reductions in "costs," which might have been formally correct (though substantively misleading) upon the basis of the apparatus of the *Treatise*, become propositions which require the explicit statement of a whole series of supporting assumptions if they are to be made even formally correct upon the basis of the apparatus presented in the *General Theory*. This is true, for example, of that particular formulation of what Mr. Lerner calls "Keynes's Law" which is supposed to allege that "a general reduction of wages would constitute a reduction in costs, *in incomes, and in demand*" (Lerner, "The Relation of Wage Policies and Price Policies," *loc. cit.*, 159; italics mine).

be explicitly recognized as a special case.[92] The very fact, in other words, that our analytical system makes clear that not *all* payments into income are necessarily cost-payments, means that we are protected against commission of the type of fallacy which is encouraged by that identification of costs and incomes which was involved in the Fundamental Equations of Keynes's *Treatise* and has recently been revived by supporters of Keynes's *General Theory* under the heading of "Keynes's Law."[93]

(c) It is, however, when we come to the implications of the Fundamental Equations of the *Treatise* with respect to the *time* aspect of the relations between costs, incomes, and outlay from incomes upon output, that the superiority of the analytical system outlined in the present work becomes clearest.[94] As was pointed out in Volume I of this work, one of the essential assumptions underlying the Fundamental Equations was that the *costs* incurred in the production of a given volume of output would be equal to the volume of expenditure out of income upon that *same output* when the output came to be sold.[95] It was demonstrated, however, that this would be true only under a very special set of "stationary" conditions.[96] These special conditions are, of course, perfectly capable of representation by the analytical apparatus here

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[92] On the notation proposed for the statement of these propositions, see above, p. 443, n. 89.

[93] See the references to A. P. Lerner given above, p. 443, n. 88, and p. 444, n. 91. Again it should be observed that both Mr. Lerner's *statement* of the "Law" and his *application* of it is actually inferior to the corresponding statement of the problem in Keynes's *Treatise*. For while, according to the formal apparatus of the *Treatise*, "profits" (other than "normal" profits) were excluded from "income" by definition, there was no suggestion in the *Treatise* that the total of "demand" would be unaffected by variations in "profits"; nor was there any implication that "profits" would in all cases vary directly with variations in the total of money demand exerted by *wage-recipients*. Contrast the italicized portions of the statement quoted from Mr. Lerner above, p. 444, n. 91.

[94] It may be observed that one of the confusions which is avoided by a correct treatment of the time relations between the magnitudes indicated is that between "income," on the one hand, and *outlay* from "income" (or *"demand"*), on the other. Contrast the statement quoted from Lerner above, p. 444, n. 91. The confusion was, of course, involved in the *Treatise* (see, for example, the references to that work given in Volume I, p. 134, n. 79, and cf. also Volume I, 380, n. 76, and p. 404, n. 39). See also what is said below, pp. 694 ff., with respect to the treatment of the relevant issues in Keynes's *General Theory*.

[95] See Volume I, 130 ff. On the general matter of the time relations involved, see also Lundberg, *Studies in the Theory of Economic Expansion,* 160 f., 163 ff., 168 ff.; and for a notable attempt to salvage certain aspects of the apparatus of the *Treatise* by the more careful statement of the relevant *time relations,* in particular, see D. Hammarskjöld, *Konjunkturspridningen,* 12 ff., 53 ff., and the references to Keynes on p. 12, n. 1, and p. 56, n. 1.

[96] See Volume I, 132, 138.

presented.[97] But it is of the utmost importance to observe that the use of a system of *time-period subscripts,* which date all the magnitudes involved in terms of clock time, and which is an integral part of the analytical apparatus here outlined, not only (1) offers a means for tracing the precise relation between the cost-payments of a given period, on the one hand, and disbursements out of income upon output sold in successive clock time "periods," on the other, regardless of the degree of "stationariness" evidenced by the situation taken for study; but also (2) provides a standing protection against the making of tacit assumptions which must themselves have the effect not only of destroying the claims of a formulation such as that presented in the *Treatise* to specific fitness for "dynamic" analysis, but also of destroying the possibility of constructing a truly "general" apparatus for dealing with the phenomena of the world we know, whether these phenomena are those characteristic of stationary "equilibrium" or not.

ii. *The alleged "tendency" to an "equilibrium" of the system.* When, on the other hand, it is claimed that the concept of a state of general equilibrium is useful not only as a standard of *comparison,* but also as the specification of a goal which the economic system as a whole actually tends to approach, the cogency of the argument for the use of such a concept is very greatly diminished.[98]

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[97] Specifically: if the "period" during which costs are disbursed is t_n, and the "period" which witnesses the sale of the output to which these costs attach is t_{n+1}, then the "special case" indicated would be that in which, assuming $X_u = $ zero, we should have $(PT)_{x \cdot t_n} = (PT)_{I \cdot t_n} = (M_i V_i)_{t_{n+1}}$, and $O_{t_n} = O_{s \cdot t_{n+1}}$. (The expression $(PT)_{x \cdot t_n}$ obviously corresponds to the $W_{t_n} O_{t_n}$ of the exposition presented in Volume I, pp. 135 ff.)

[98] In the light of the argument presented under (i), it should be clear that this proposition by no means involves acceptance of the argument of those who have rejected the use of the very *concept* of an "equilibrium" of the system on the ground that there is no basis for the assumption of a continuous tendency toward such an equilibrium in the world we know. All that it has in common with such arguments is acceptance of the proposition that "we have no more warrant for *assuming in advance* that business processes 'tend' to maintain an equilibrium [of the system as a whole] than to assume that they 'tend' to get out of balance" (so Mitchell, *Business Cycles: The Problem and its Setting,* 187; italics mine). Actually, of course, the refusal to "assume in advance" that "the economic system tends toward a stable equilibrium" or that there is "a tendency [for the 'system'] to move in the direction of some such position" (so Ohlin, "Some Notes on the Stockholm Theory of Savings and Investment, II," *loc. cit.,* 238) has been as characteristic of writers who have regarded themselves as strictly in the "classical" tradition as it has been of those who have attacked the alleged "assumptions" of the "classical" tradition. See, for example, the passage from Marshall's *Principles* (379 n.) quoted by Souter, "Equilibrium

For, in the first place, the demonstration of a "tendency toward" equilibrium within the range of phenomena envisaged by "partial" equilibrium analysis, in the special sense in which such a "tendency" was described above, does not automatically provide a demonstration of a tendency toward an "equilibrium" of the system as a whole.[99] It is entirely reasonable to suppose, for example, that individual entrepreneurs will attempt to avoid loss by attempting to bring costs into a satisfactory relation with selling prices. It does not follow, however, that the particular methods these entrepreneurs may adopt will actually have the effect of bringing the "system" closer to a position that can be characterized as one of "equilibrium": on the contrary, they might have the effect of driving the system still further away from "equilibrium." [100]

Economics and Business-Cycle Theory: A Commentary," *loc. cit.*, 46, as well as the references in the index to Marshall's *Principles* (p. 861) under the entry "Cumulative, distinction between causes whose effects are or are not," and passages such as that on p. 368 of the *Principles,* on what is and what is not "true in the world in which we live." The most notable case, of course, in which a writer avowedly in the "classical" tradition explicitly rejected the assumption that the "adaptation processes" evidenced by the economic *system* which we know necessarily move toward an "equilibrium" of the system is Wicksell. In this connection, see the comments of B. Thomas, *Monetary Policy and Crises,* 87, and Lundberg, *Studies in the Theory of Economic Expansion,* 45 f. But this does not mean that Wicksell made no use of the *concept* of an "equilibrium" of the "system," particularly in its monetary aspects. On the distinction between the two "uses" of the concept of "equilibrium," see Myrdal, *Monetary Equilibrium,* 40 ff., and Lundberg, *Studies,* 8, 27.

[99] On the "special sense" in which a "tendency toward equilibrium" may be assumed to exist within the range of phenomena envisaged by "partial" equilibrium analysis, see what is said above, pp. 409 f.

[100] Much has been made of this fact by writers who, in pointing to "the danger of taking propositions that have been established as true when applied to sections of the economy and illegitimately applying them to the economy as a whole," and in insisting that "what is true of a firm or of a particular industry or of a set of industries need not be true of the economy as a whole," have stressed the importance of Mr. Keynes's contributions to the establishment of these propositions (so, for example, Lerner, "The Relation of Wage Policies and Price Policies," *loc. cit.,* 158). It should be observed, however, that a warning of this type is not only implicit in the proposition underlying the whole of "modern" economic theory to which reference is made below, p. 449, n. 102, but has been made as explicit as one could wish by writers who have pointed out that although complete equilibrium of the "system," in the most rigorous statement of the conditions for such "equilibrium," necessarily implies the attainment of an equilibrium

To be sure, it might be contended that the same argument would apply against the assumption of a "tendency toward equilibrium" even within the range of phenomena envisaged by "partial" equilibrium analysis—on the ground, say, that some of the very measures adopted by individual entrepreneurs as a means of bringing about a satisfactory relation between costs and selling prices might make the attainment of such a relation even more remote than before. It can be shown, however, that this type of contention does not constitute the same kind of decisive argument against the assumption of a "tendency toward equilibrium" within the range of phenomena envisaged by "partial" equilibrium analysis as it does against the assumption of such a "tendency" within the system as a whole.

That this is so will become clear if it is remembered that the argument for retaining the assumption of a "tendency toward equilibrium" in the case of "partial" equilibrium analysis is that, without a specification of the *goals* which individual entrepreneurs may be assumed to set themselves, we are left with an incomplete explanation of why they act as they do. In other words, the "tendency toward equilibrium," in this case, has to do solely with an assumed correspondence between entrepreneurial *actions,* on the one hand, and entrepreneurial *calculations* and *intentions,* on the other—the contentions being (1) that these calculations and intentions must be described if we are to explain why entrepreneurs act as they do, and therefore why market results are what they are; and (2) that the basis for these calculations and intentions is provided by the subject matter of "partial" equilibrium analysis.[101] It is of the utmost importance to observe, however, that in the case of the

position by each individual and firm within that system, the attainment of "equilibrium" by individual entrepreneurs stationed at certain *parts* of the "system" is perfectly consistent with a disequilibrium of the system as a whole. See, for example, Schumpeter, *Business Cycles,* 42 f.

[101] See above, pp. 235 ff. It must again be observed that the "correspondence" thus assumed is in no way shaken by the fact that the attainment of a *given* "goal" may be prevented by the abandonment of *that particular* goal, and the substitution of another for it. All that is needed is the assumption that entrepreneurs will take such action as will, if successful, "tend" to bring them nearer to the particular goal before them *when the action is taken.*

concept of an "equilibrium of the *system*," there is *no agency, under institutions such as ours, which can be assumed to be engaged in a type of calculation, or to cherish a type of intention, involving the conditions for "general" economic equilibrium* in a sense comparable to that in which the calculations and intentions of individual entrepreneurs can be said to involve a consideration of the conditions for "equilibrium" within their own firms.[102] Clearly, this fact in itself removes the main ground for putting the assumption of a "tendency toward equilibrium" of the *system* on a par with the assumption of such a "tendency" within an individual firm, in the special sense in which we have spoken in this work of the existence of such a "tendency."

If, therefore, there is in fact a tendency toward an equilibrium of the "system" under the institutions we know, it is a tendency the reality of which requires a separate demonstration. It is not possible to deny, on *a priori* grounds, that such a tendency may exist under certain conditions.[103] Neither can it be denied, however, that the case for believing in the existence of such a "tendency" is to be put on a basis entirely different from that underlying a belief in a "tend-

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[102] Cf. Mitchell, *Business Cycles: The Problem and its Setting,* 172: "The business economy provides for effective coordination of effort within each business enterprise, but not for effective coordination of effort among independent enterprises." The proposition leaves completely open, obviously, the question of the effectiveness of the Invisible Hand. What it affirms is, after all, only the substance of one of the cardinal principles of "modern" economic theory: namely, that market actions under a system of private enterprise are predominantly the result of economic calculations by *individuals* or individual business firms, and not of economic calculations by a social "organism" or the state-appointed "planning" representatives of such an organism; and that these market actions must therefore find their explanation in the type of economic calculation undertaken by these individuals or individual firms, while we leave for separate examination the question of the extent to which these calculations result in an unintended "coordination of effort among independent enterprises." It should hardly be necessary to emphasize that a proposition such as the one quoted at the beginning of this note applies specifically to a "business economy": what would be true in a completely "planned" economy, or in one in which the monetary authorities, for example, set themselves the specific task of bringing about a state characterized as one of "equilibrium," is a different question altogether.

[103] See, for example, the argument of Schumpeter, *Business Cycles,* 56, 70, 149, 157 n., 206, 219.

ency toward equilibrium" within the range of phenomena
envisaged in "partial equilibrium analysis," when the latter
"tendency" is understood in the sense indicated above.
Much is to be said, therefore, for the use of an analytical
framework, such as that provided here, in which the prob-
lem of a tendency of the "system" to "equilibrium" is left
for empirical investigation, after an analytical specification
of the conditions which must prevail if such a "tendency"
is to be evidenced at all.[104] At the same time, it will be
observed, full place is given to the possibility of using the
concept of an equilibrium of the system as a standard of
comparison.[105] And it will be observed also that full place
is given to the possibility of using the whole of those sectors
of the "general theory of equilibrium" which are concerned,
on the one hand, with the problem of the "equilibrium" of
the individual and the firm, and, on the other, with the fact
of general economic *interdependence,* both at a given mo-
ment and over time, including those illustrations of the
interdependence of economic variables in time which consti-
tute the very heart of the theory of a "cumulative process."

II

THE RÔLE OF "STATICS" AND "DYNAMICS"

The relation between "statics" and "dynamics," and the
rôle played by each in an analytical system making use of
"stream" equations of the general Fisherine form, was dis-
cussed at some length in Volume I of the present work.[106]
Here, therefore, it should be necessary only to summarize
the results there obtained, and to make clear the relation
of the analysis presented thus far in this volume to the gen-
eral question of the rôle played by "static" and "dynamic"
analysis, respectively, in the analytical system here outlined.

It was pointed out, in Volume I, that an initial obscurity

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[104] The "analytical specification" of these conditions must be left for
my later publication on *Money and Production,* in which all the tools de-
veloped in the present two volumes will find an application.

[105] For examples, see above, pp. 429 ff.

[106] See especially Volume I, 40 ff., 78 ff., and the cross-references given
in the Index to that volume (p. 613) under "Statics and Dynamics."

has been introduced into discussions of the "static" or "dynamic" character of a given type of economic analysis by wide disagreement as to what should be understood by the terms "statics" and "dynamics." [107]   As in Volume I, therefore, we must begin by testing the fitness of the apparatus here outlined for "static" and "dynamic" analysis, respectively, when we understand by "static" analysis, first, analysis based upon the assumption of *stationary* conditions; and, second, analysis designed to determine the conditions for "equilibrium." [108]

1. From the first definition of "static" analysis, it follows that "non-statical" analysis ("dynamic" analysis, in one sense of the latter term) will be concerned with the nature and causes of *change* in the specific data which are held to be of significance in economic problems.[109]   The reader need only be reminded, therefore, that the whole of the positive argument of Volume I of the present work was concerned with the nature and the causes of changes in those data for which the terms included in the familiar Quantity Equations are to be regarded as convenient rubrics.   And he may be reminded also that a very large part of this discussion was concerned with those sequential changes in time which have for years been discussed under the head of "monetary dynamics," but which in recent years have come to be discussed under the head of "period-" or "sequence-analysis." [110] From this discussion, moreover, it should be clear that room has been left for the particular type of "dynamic" analysis represented by the attempt to ascertain the causes and consequences of differences in the "rates of growth" or "reaction speeds" of the different variables involved, just as room has in fact been left for such analysis in the treatment accorded to problems of "monetary dynamics" by generations of monetary theorists.[111]

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[107] See Volume I, p. 40.

[108] Cf. Volume I, 43 ff., 72 ff.

[109] Cf. Volume I, 79 ff., 143.   On the distinction between "non-statical" analysis and "dynamic" analysis, when the latter is "broadly" defined, see what is said above, p. 427, n. 40.

[110] See above, pp. 369 f., and the references given in nn. 48 and 49 thereto.

[111] On "dynamics" as being concerned essentially with "an analysis of rates of growth" *(veksthastigheter),* see Frisch, "Statikk og Dynamikk i den

Given this setting of the problem, it follows that while the assumption of a constancy in certain of the data (or of a "stationary" character in such data) may recommend itself for certain types of analysis, the use of "static" analysis, in this special sense of the term, is at best only a step toward the attainment of the more general and "non-statical" solution.[112]   In other words, room is left, in the analytical system here outlined, for both "static" and "dynamic" analysis, in the sense indicated, without any of the restrictions that have been held to attach to rival systems, including those of Keynes's *Treatise* and his *General Theory*, because of their incompletely "dynamic" character.[113]   And it will be

økonomiske Teori," *loc. cit.*, 326; and cf. the comments of J. Åkerman, *Ekonomisk Kausalitet*, 48, as well as the same author's characterization of an emphasis upon "rates of increase" as an example of "the dynamic method of research," in his earlier "Quantitative Economics," *loc. cit.*, 37. See also Harrod, *The Trade Cycle*, 150 f., on "the proper method of dynamic analysis" as being concerned with the causes and consequences of the different "rates of growth" of the particular variables involved in any given problem chosen for analysis (cf. the same author's "Mr. Keynes and Traditional Theory," *Econometrica*, V [1937], 86). On the relation of analysis designed to determine the nature of the "forces controlling the rates of growth and change" to "stream" equations of the general Fisherine form, see Volume I, p. 84, of the present work, and particularly the references to E. Petersen there given. On "dynamics" as being concerned essentially with "reaction speeds," see Frisch, *loc. cit.*; also what is said below, p. 454, n. 114, with respect to the implications of the statement that "statics" is constructed upon the assumption that the variables involved have "infinitely high" reaction speeds. On the method proposed in the present work for dealing with differences in "reaction speeds," as well as on the rôle played by the concept of "reaction speeds" in earlier economic literature, see what is said above, pp. 395 ff., and the references there given (particularly the reference to Cairnes on p. 396, n. 105).

   112 In this connection, cf. what is said above, pp. 373 ff., with respect to the relation of "period analysis," of the type made possible with the aid of the analytical apparatus here outlined, to the assumption of "constancy of the data"; as well as what is said above, p. 427, with respect to the case involving "stationary" conditions as a "special case of a general and inclusive 'dynamics.'"

   113 On the claims made on behalf of the *Treatise*, at the time of its appearance, to a peculiar fitness for "dynamic" analysis, as well as for an examination of the validity of these claims, see Volume I, Chapter Five, of the present work. It should be remembered that Mr. Keynes himself has since asserted that in his *Treatise* "the dynamic development, as distinct from the instantaneous picture, was left incomplete and extremely confused" (*General Theory*, p. vii; though see also what is said in this connection in Volume I, 138 ff., of the present work), in contrast with the account of "the dynamic development" presented in his *General Theory*. The *Gen-*

noted also that the *relation* between the "static" and the "non-static" analysis thus included in the system, instead of being one of mutual contradiction, is of a mutually complementary character.

2. If, on the other hand, by "static" analysis is meant analysis designed to determine the conditions for "equilibrium," the answer as to the rôle played by "static" and "non-static" analysis, respectively, in the system here out-

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eral Theory, in turn, has been hailed, to be sure, as (1) a contribution of profound importance for the future development of economic "dynamics"; (2) as a contribution to the "dynamization of monetary theory"; and (3) for its alleged "freeing of the short-run view from the static mode of viewing the problem" (cf. B. Thomas, *Monetary Policy and Crises,* 74 n.; E. Petersen, in the *Statsøkonomisk Tidskrift* for 1937, p. 266; and H. Peter, in *Jahrbücher für Nationalökonomie und Statistik,* CXLVI [1937], 72). Yet it is a striking characteristic of later comments on the argument of the *General Theory,* including comments by writers otherwise sympathetic to much of its argument, that they have tended to stress more and more its limited usefulness for "dynamic" analysis. See, for example, G. L. S. Shackle, *Expectations, Investment, and Income,* 1 f.; Harrod, "Mr. Keynes and Traditional Theory," *loc. cit.,* 85 f.; J. R. Hicks, "Mr. Keynes and the 'Classics': A Suggested Interpretation," *Econometrica,* V (1937), 159; Ohlin, "Some Notes on the Stockholm Theory of Savings and Investment, II", *loc. cit.,* 235 ff.; A. Kruse, "Zu Keynes' Allgemeine Theorie der Beschäftigung," *Jahrbücher für Nationalökonomie und Statistik,* CXLVI (1937), 83; A. Amonn, "Keynes' 'Allgemeine Theorie der Beschäftigung,'" *ibid.,* CXLVII (1938), 131 ff., 153; J. Åkerman, *Das Problem der sozialökonomischen Synthese,* 94 f., 273; Haberler, *Prosperity and Depression* (1939 edition), 249 ff.; Lerner, "Saving and Investment: Definitions, Assumptions, Objectives," *loc. cit.,* 618; and R. M. Bissell, "Price and Wage Policies and the Theory of Employment," *Econometrica,* VIII (1940), 238. The criticisms leveled against the *General Theory* on this head include criticisms as heterogeneous as those directed against (1) certain aspects of the *General Theory's* treatment of the element of "expectations" (Shackle); (2) its too great readiness to assume "stationary" conditions (Kruse), and its inadequate treatment of the factors affecting "rates of growth" (Harrod); (3) its failure to provide an adequate apparatus suitable for the tracing of *processes* in time (Ohlin, Haberler, Åkerman, Lerner), and its slurring over "all sorts of questions about the timing" of these processes (Hicks); and its general "failure to give due attention to time lags" (Bissell). On the way in which, in the analytical system outlined in the present work, expectations are made to "depend, at any moment, on the comparison which we may suppose business men to make between their expectations of a slightly earlier moment and *what has actually happened in the interval*" (Shackle, *op. cit.,* 2; italics mine), see below, pp. 456 f. On the way in which account is taken of changes in "rates of growth," see above, p. 451, and the references given in n. 111 thereto. And on the relation of the analytical system here proposed to the tracing of *processes in time,* see what is said above, Chapter Seven.

lined, is given by the summary presented in section I of the
present chapter.   Again it may be observed that if the argu-
ment there presented is sound, it follows that the analytical
system here outlined is subject to none of those limitations
which have been held to apply to rival systems, including
that presented in Keynes's *General Theory,* on the ground
that these alternative systems fail to go beyond analysis
which is at best of the "comparative static" type, by virtue
of their primary concern with the description of alternative
positions of equilibrium, rather than, say, with a descrip-
tion and explanation of the successive steps by which these
positions are attained or approached, or with the tracing of
"cumulative processes" generally.[114]   And it may be ob-
served also that the relation between the "static" and the
"non-static" analysis thus included, instead of being one of
mutual contradiction, is again of a mutually complementary
character.

　If, finally, one were to accept that implied definition of
the subject matter of economic "dynamics" which is in-
volved in its virtual identification with the subject matter
of the "method of expectations," it should be clear that the
system here outlined does justice to whatever element of
truth there is in this contention, without involving any of
the excesses that its literal acceptance would necessarily
imply.[115]

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　[114] See, for example, the references to Ohlin, Åkerman, Haberler, Amonn,
Lerner, and Bissell given in the preceding note. It may be observed also
that the apparatus outlined in the present work is to be regarded as equally
applicable in "dynamic" analysis if (1) the distinction indicated in the text
is stated in terms of the amount of attention devoted to the "reaction
speeds" attaching to particular variables; and (2) the substance of "statics"
is described as that body of analysis in which these "reaction speeds" are
assumed to be "infinitely high" (cf. Frisch, "Statikk og Dynamikk i den
økonomiske Teori," *loc. cit.,* 326). For it must be remembered that one of
the outstanding characteristics of the system here outlined is its ability
to describe these "reaction speeds" in terms of differing amounts of clock
time (see above, pp. 395 ff.).

　[115] For an example of a virtual identification of the substance of "Eco-
nomic Dynamics" with the "method of expectations," see the references to
B. Thomas given above, p. 383, n. 75; and for a criticism of this practice,
see, in addition to the argument presented above, pp. 383 ff., the comments
of Harrod, "Mr. Keynes and Traditional Theory," *loc. cit.,* 86, and Haberler,
Prosperity and Depression, 252. The appellation "dynamic" has been applied
by Professor Schumpeter to the type of theorem with respect to "interde-

It may be pointed out, for example, that the element of "expectation" which is necessarily involved in all "ex ante" calculations is included in the whole of that part of *"static"* theory (in the sense of "equilibrium" theory) which is concerned with *"ex ante"* calculations—that is to say, the whole of "partial" equilibrium analysis and that part of the theory of "general economic interdependence" which is to be regarded as providing a continuing check upon the use of the devices of "partial" analysis, such as demand and supply schedules of the Marshallian type.[116]   The formal inclusion

pendence in time" (cf. above, p. 415, n. 20) which is represented by the proposition that "quantity demanded or supplied at any time is not merely a function of the price that prevails at the same time, but also of past *and (expected) future* values of that price." (See Schumpeter's *Business Cycles,* 48. This appellation is suggested by Professor Schumpeter "in deference to Professor Frisch"; though it should be added that the latter's own conception of a "dynamic" theorem as one which includes "in our functions values of variables which belong to different points of time" [Schumpeter, *loc. cit.*] has had to do less with the type of example cited by Professor Schumpeter than with a type involving "the concept of speed of growth or the concept of speed of reaction." See Frisch's "Statikk og Dynamikk i den økonomiske Teori," *loc. cit.,* 325 f.) Readers familiar with the analytical structure developed by Professor Schumpeter will be aware, however, that he himself cannot be interpreted as arguing that the use of such "theorems" constitutes the *whole* of the subject matter of "Economic Dynamics." I am not sure, on the other hand, as to just how far Professor J. R. Hicks is to be interpreted as advancing such a proposition. His description of "Economic Dynamics" as "those parts of economic theory . . . where every quantity must be dated" (*Value and Capital,* 115) is at once so broad and so loose as to forbid the attribution to him of the position indicated, upon the basis of this description alone. His further description, however, of the proposition "that supplies (and ultimately demands too) are governed by expected prices quite as much as by current prices" as "the first main crux of dynamic theory," which "marks the first parting of the ways" (*Value and Capital,* 117), might be taken to imply a greater willingness to identify "Economic Dynamics" with the "method of expectations" than others (including myself) would be prepared to countenance; and this presumption as to Professor Hicks's understanding of the substance of "Economic Dynamics" is reënforced by the comparative lack of interest evidenced in the later chapters of his book in the type of detailed tracing of *realized processes* in time which has played so large a rôle in the "economic dynamics" of the past, and still plays a very large rôle in the writings of Professor Schumpeter, Mr. Hawtrey, and others of our own generation. The question, in short, is whether the sections in Professor Hicks's work devoted to "Economic Dynamics" are regarded by him as a contribution to one branch of that subject, or as providing a survey of the whole field of "Economic Dynamics"; and this is a question that each reader of Professor Hicks's suggestive book must answer for himself.

116 It may be observed that the mere statement that an element of

of this body of "partial equilibrium analysis," within the analytical system here presented, automatically implies, therefore, the inclusion of the particular use of the "method of expectations" which undertakes to do no more than to relate market events to the forward-looking "plans" of economizing individuals, even when these "plans" are not regarded as *subject to change* within the period taken for examination, or when the whole analysis runs in terms which make it directly relevant only for the determination of the conditions of "equilibrium" under rigidly specified conditions.

Within the range of problems, on the other hand, in which the element of *changing* "plans" is held to give the "method of expectations" a "dynamic" character, it should be observed that the emphasis provided in the analytical system here presented should prevent any of the abuses with which the "method of expectations" has been charged, not unfairly, whenever the element of expectation is introduced into the explanatory scheme as an "independent" *deus ex machina*.[117] For we have insisted throughout upon the necessity for accompanying any use of an emphasis upon "expectation" by a tracing of *realized processes* in all possible detail, in order that these realized processes may be related with all possible precision to the expectations which condition them and to which they give rise. And it will be observed that the particular analytical devices which, in the system here outlined, provide the link between expectations and realized results (the "path between *ex ante* and *ex post*") are devices so well established within the corpus of received economic theory as to prevent any suggestion that a "revolutionary" character must be assigned to the "method of expectations"

"expectation" is "necessarily involved in all '*ex ante*' calculations" does not necessarily mean that the very concept of "*ex ante* calculations" connotes the type of "calculations" that has bulked large in certain types of "expectation analysis." On the relations between "*ex ante* analysis" and "expectation analysis," see what is said above, p. 178, n. 71, and p. 180, n. 73. From the examples given above, pp. 180 ff., however, it should be clear that the ablest users of the "*ex ante*" type of analysis to which reference is made in the text have in fact been aware that an "element of 'expectation' is necessarily involved in all '*ex ante*' calculations."

[117] In this connection, see the comments of Schumpeter and Lundberg, quoted above, p. 229, n. 19.

as such.[118] For, on the one hand, the devices involved are the *ex ante* demand and supply schedules of the "general" Theory of Value—the changes in "plans" held to affect market action being in all cases related to movements either in the position or in the conformation of these schedules.[119] And on the other hand, they are represented, within monetary theory, in the narrower sense of the latter term, by (1) the establishment of an articulate relation between the *ex post* fact of changes in "velocity" to the *ex ante* calculations treated under the heading of the "cash balance approach," which from the start has been associated with the element of expectations and uncertainty; and also by (2) the establishment of an equally articulate relation between the *ex post* fact of changes in the quantity of bank money, on the one hand, and *ex ante* calculations, on the other, with respect to the relation between the expected profit to be made by the use of a bank loan and the market rate of interest.[120] Again, therefore, the final result is seen to be one in which different areas sometimes held to be divided by an unbridged or unbridgeable gap—statics and dynamics, on the one hand, and monetary theory and "general" economic theory, on the other—are found to be related to each other in a mutually complementary, rather than a mutually exclusive, way.

[118] For the expression "the path between *ex ante* and *ex post,*" see the reference to J. Åkerman given above, p. 384, n. 77.

[119] See above, pp. 226 ff.

[120] The suggestion has been made that a considerable degree of novelty attaches to the introduction of the element of "expectations" in both cases: that is, to the relating of the holding of cash balances to the element of "uncertainty," and to an emphasis upon the *prospectiveness* of the schedule of "profit rates" ("marginal efficiency of capital") with which the "market rate" of interest must be compared. The comment in the text is obviously relevant, moreover, to the criticisms advanced by those writers who have seen a conflict between an emphasis upon "the 'injection of new money' idea" and "velocity," on the one hand, and, on the other, an emphasis upon "expectations" and a truly "causal" "dynamic theory" (see the references to Lundberg and Ohlin given above, p. 383, n. 77). On the *relation* between an emphasis upon the type of analysis indicated here and the type of analysis indicated in the preceding sentence of the text, see what is said above, p. 438, n. 72; and cf. also what is said below, pp. 466, 555, with respect to the relation between the "rate of sale of goods" of monetary theory, on the one hand, and the supply curves of the "general" Theory of Value, on the other, and with respect to the rôle played in both by the element of "expectation."

III

MONEY AND THE ECONOMIC PROCESS

From the survey presented in Chapters One to Three of
the present volume, it should be clear that to raise the ques-
tion of the relation between received monetary theory, on
the one hand, and "general" economic theory, on the other,
is unfortunately to raise questions other than those which
are strictly relevant to the realistic problem of describing
the effects of money upon the functioning of the economic
process.[121]   Yet it is certain that the latter question—the
only one that is genuinely fundamental—has also been in-
volved as a matter of doctrinal history.   It is of some in-
terest, therefore, to call attention to those aspects of the
analyical system here outlined which can be said to bear
upon this question.[122]

It will be observed, to begin with, that money is *not* in-
troduced into the analysis in the way it has been introduced
by some writers in recent years.[123]   Specifically, it is not
introduced as a factor which alone is capable of disrupting
a system that otherwise tends automatically to a position
of "equilibrium." [124]   Nor is it introduced solely in order
to provide a "multiplicative factor," which leaves the price
structure in precisely the position in which it would have
been if no concrete medium of exchange had been in exist-

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[121] Unfortunately, also, it is possible to see in recent discussion the conse-
quences of a failure to learn the lessons that should have been learned from
the very fact that much of the earlier discussion of the relation between
the two bodies of theory was concerned with issues *other* than that of
"describing the effects of money upon the functioning of the economic
process."   See below, Chapters Twelve to Fourteen.

[122] The reader is again reminded that a complete summary of the rôle to
be attributed to money in the functioning of the economic process must
await the publication of the promised works on *Money and Interest* and
Money and Production, respectively.   Both of these works, however, will
be found to be based upon the same methodological principles as those
outlined in the text above.

[123] The attribution of the positions indicated to "some writers in recent
years" should be contrasted with an attribution of similar positions to
"classical" or "traditional" economic theory generally.

[124] In this connection, see what is said above, p. 427, n. 39, and the
references to Hayek there given; and cf. the comments of J. Åkerman in his
Konjunkturteoretiska Problem, 11 (under point 10) and 79.

ence.[125]   Nor, finally, is it introduced after a preliminary description of the supposedly "fundamental" forces at work in terms of a "barter economy," with the further suggestion that although money may effect temporary divergences from the results that would have been obtained in a "barter economy," the results ultimately obtained are essentially those which would have prevailed in a "barter economy." [126]

On the contrary, the problem has been conceived in the present work as one involving the introduction of money into the theory of pricing at the very start.[127]   This follows directly from the conception of the pricing process which underlies the whole of the present work: namely, a process in which all realized prices are conceived of as resulting from the impact of a stream of money-spending power against a stream of objects sold for that money-spending power.[128] This means, of course, that the whole of those sectors of

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[125] See what is said on this matter above, pp. 339 ff.

[126] The best known construction of this type is represented by that *part* of the argument of Wicksell's *Interest and Prices* which was based upon the definition of the "natural rate of interest" as the rate of interest that would be established if "real capital" were lent *in natura* (see Volume I, 201, of the present work, and the references given in n. 125 thereto). See, in addition, what is said above, pp. 68 f., with respect to the concept of "neutral money"; and cf. also the following note.

[127] Again it may be observed that such a conception of the problem, instead of being "original" with me or a development of our own day, was inherent in the constructions presented by some of the greatest writers in the history of our subject—despite common misrepresentations of the argument of these writers. See, for example, what is said above, pp. 70 ff., with respect to the construction presented by Léon Walras, and the references given in nn. 43 and 44 thereto.   There is, therefore, very little foundation in fact for the statement that, as a matter of doctrinal history, "the whole classical theory was a theory of relative prices of a *barter economy* in a state of equilibrium" (so, for example, Rosenstein-Rodan, "The Coordination of the General Theories of Money and Price," *loc. cit.*, 257; italics mine).   The situation is not greatly helped, moreover, when the term "barter economy" is given a meaning, or a series of meanings, according to which a "barter economy" may still witness "changes in the volume of *money*" (Rosenstein-Rodan, *op. cit.*, 260).   For I cannot help feeling that the only effect of such a usage with respect to the meaning of the term "barter economy" is to make confusion worse confounded; and it is of some importance to establish the fact that it is not a usage which is found in the "classical" writers themselves.

[128] In this connection, cf. the reference to A. A. Young given above, p. 422, n. 32.   On the suggestion that such a conception of the pricing process is necessarily "mechanical" in nature, see what is said below, pp. 471 ff.

monetary theory which explain why the dimensions of these
streams are what they are is regarded as an integral part of
the "general theory of pricing." [129]   It does not mean, how-
ever, that even the whole of monetary theory is *in itself*
sufficient to explain why the dimensions of these streams are
what they are—at any rate, if "monetary theory" is regarded
as having only the narrower content ordinarily assigned to
it.   For a large part of the argument summarized in the
22 theses of Chapters Five to Seven has been designed pre-
cisely to show that it is impossible to answer the question
indicated unless one is prepared to call also upon the whole
of the relevant sectors of what is usually called the "general"
Theory of Value.[130]

All of this argument, in short, can be taken as having
honored the slogan derived from Mr. Hawtrey with respect
to the alleged desirability of tearing aside the "distorting
veil of money." [131]   The game is described, to be sure, in
terms that do full justice to the fact that the net of money
does in fact "set the conditions of play" in the world we
know; and room is left also for a complete description of the
effects of those alterations in the "conditions of play" which
may come from the fact that the net may itself become
warped and shrink or stretch, as the case may be.   But there
is still no implication that the game can be described in terms
which would suggest that the strength and skill of those
who strike the ball are of no importance in determining
where the ball will go, and with what results.   *This* is what
is involved in an adequate "synthesis" of monetary theory
with "general" economic theory; and the results which are

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[129] See our Propositions II, V, VII–XI, XVI–XXII, and the related dis-
cussion (above, pp. 240, 274, 281, 285, 296, 297, 304, 347, 349, 350, 351, 364,
365, 366).

[130] In this connection, see especially our Propositions III, V, VI, XIX,
and the related discussion (above, pp. 240, 274, 351).   Precisely the same
kind of conclusion, it may be said, will be found applicable to the prob-
lems of Interest, and of Output as a Whole, respectively (cf. above, p.
458, n. 122).   It will be argued, that is to say, that no theory of Interest
or of Output as a Whole can be regarded as satisfactory which would regard
variations in either as a "purely monetary phenomenon," and therefore
capable of explanation without introducing "non-monetary" considerations
into the problem.

[131] See above, p. 124, and the reference given in n. 82 thereto.

here outlined, and which have been shown to be based upon
the work of successive generations of economic theorists,
may be compared with those obtained by recent aspirants
to the title of "synthesizer" of the two bodies of doctrine,
whenever these aspirants have done considerably less than
justice to those devices of "general" economic theory, or of
"general" interest theory, or of business-cycle theory other
than the "purely monetary" variants thereof, which can be
shown to be still of the greatest heuristic value in account-
ing for the phenomena of the world we know.

CHAPTER NINE

Corollaries and Vistas: II

I

THE RÔLE OF "INSTITUTIONS" AND THE CALCULATIONS OF INDIVIDUALS

FROM THE PURELY methodological standpoint, it is doubtful whether any antithesis is falser than that which has sometimes been set up as between an emphasis on the importance of a study of economic institutions and their functioning, on the one hand, and an emphasis, on the other hand, upon that type of calculation by "economizing" individuals which represents the subject matter of the core of "traditional" economic analysis. For no economist, at least since the day of Bagehot, ought to be prepared to deny that the whole of our analysis is concerned with a world characterized by a very special set of economic "institutions," which condition at every point the economic calculations of the individuals who live under those institutions.[1]

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[1] Cf. the well-known passages in Bagehot's *Economic Studies* (5 ff., 16 ff., 80 f.) in which "the science of Political Economy as we have it in England" was defined as "the science of business, such as business is in large productive and trading communities." The charge of a lack of awareness that definite institutional assumptions do underlie a very large part of our analysis has been leveled indiscriminately against what is called *"pure* economic theory" by a number of writers in recent years. See, for example, the quotations from Lederer and Carell given by Kuznets, "Equilibrium Economics and Business-Cycle Theory," *loc. cit.,* 386, 391. If, however, one wishes to see what has actually been *said,* with respect to the "institutional" assumptions underlying our analysis, even by writers who have been accused of an excessive addiction to the cause of keeping our economic theory entirely "pure," one may consult Robbins, *An Essay on the Nature and Significance of Economic Science,* 92 ff. It is sufficient here, therefore, to observe that one ·of the works of recent years which has been most sharply criticized on the ground that it pays too little attention to the "institutional" premises on which its argument is constructed is Mr. Keynes's *General Theory.* See, for example, the comments of J. Åkerman, *Das Problem der sozialökonomischen Synthese,* 96. The fairness of a test of

On the other hand, a sufficient defense of the use of that part of "traditional" economic analysis which is here in question is provided as soon as one contemplates the void left in our apparatus for explaining the events of the real world by those who would press their insistence upon the importance of "institutions" to such a point as to deny that "rational" economic calculation does occur, or that the particular type of "rational" calculation described in traditional economic theory does have a counterpart in reality.

Yet to give lip service to both types of emphasis is not to guarantee that both will be represented in any given analytical apparatus claiming to be capable of explaining economic events in the world we know. Something is to be said, therefore, for calling attention to the way in which both types of emphasis enter into the analytical system here outlined.

1. *Institutions and Individuals' Calculations in Monetary Theory.*—*Money* is, of course, itself an "institution." Any emphasis, therefore, such as that described in Chapter Eight of the present volume, upon the way in which money may affect the functioning of the economic process, is itself a contribution toward an understanding of the effect of economic "institutions" upon that process.[2] Yet no one would deny that within the field of monetary theory itself it is

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the validity of the argument of the *General Theory* from this standpoint has been made even more apparent by a later assertion by Mr. Keynes himself: namely, that one of the respects in which his argument represents a more "general" theory than that of his predecessors is its greater freedom from "narrow presuppositions" with respect to institutional conditions, so that it can be more easily applied to a "wide field" of instances involving different institutional situations (see p. ix of the preface to the German version of the *General Theory*). For our present purpose, however, it is more important to point out that even if this claim were justified, we should then have to raise again the old question whether the attempt to secure complete "generality" does not mean abdication of the task of describing with all possible accuracy the effects of the *particular* institutions which are characteristic of the particular world in which we live. See, for example, what is said below, p. 464, n. 4.

   [2] The reader is again reminded that any complete discussion of the effect of the "institution" of Money upon the functioning of the economic process would have to deal in particular detail with the problems raised by topics such as Money and Production, on the one hand, and Money and Interest, on the other. See again what is said on this matter above, p. 458, n. 122.

possible to distinguish types of approach which evidence varying degrees of awareness of the importance of a study of the effect of "institutions" and institutional practices upon the functioning of the economic process.

In Chapters Two and Three of the present volume, for example, we had occasion to comment on the unwarranted exclusivism of those neophytes in monetary theory who would justify the neglect of important sectors of received monetary theory on the ground that the issues there raised are not "sensible and interesting" to a "value theorist." [3] Attention may be called here, on the other hand, to the fact that the very type of analytical framework rejected by these writers might have provided a guarantee against such exclusivism. To raise the question, for example, of the nature of the forces determining the $M' = cM_r$ of our Quantity Equation is to raise at once a whole series of sub-questions with respect to the nature of the forces determining the amount of bank reserves and the magnitude of the internal and external drains; the theory and practice of central bank open market operations; and indeed all those detailed matters of institutional practice which, however "uninteresting" they may appear to a "value theorist," must be an integral part of the analytical equipment of anyone anxious to account for the events of the real world.[4] The same thing must be said with respect to the T of our Quantity Equa-

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[3] See above, p. 83, n. 78, and p. 98, n. 21.

[4] See Volume I, 158 ff., of the present work. It should be observed that Mr. Keynes apparently regards it as a virtue of the *General Theory* that, in the Theory of Money which it presents, and in contrast with the Theory of Money presented in the *Treatise*, "technical monetary detail falls into the background" (*General Theory*, p. vii). It should hardly be necessary to labor the point that a fair evaluation of Mr. Keynes's contributions to monetary theory would recognize that this apparent minimization of the importance of much of the discussion presented in the *Treatise* need not be taken seriously by anyone with enough independence of judgment to take, as a basis for evaluation, something other than Mr. Keynes's own estimate of the "importance" of various parts of his work. I need only state here my conviction that in this respect serious disservice to Mr. Keynes is done by those of his disciples for whom any detailed discussion of large parts of the *Treatise* is made "otiose" by the mere fact that Mr. Keynes himself has evinced little interest in, and even hostility to, these parts of his earlier work since the publication of his *General Theory*. See the references to Whale and Kaldor given above, p. 157, n. 31.

tion; for what is revealed by an analytical breakdown of T is precisely a series of problems, such as those connected with stock market practices and the degree of integration of industry, which are concerned with nothing if they are not concerned with the effects of economic "institutions." [5] And the same thing must be said with respect to those elements in the theory of the determination of the size of cash balances held relative to outlay (the $K = 1/V$ of our formulation) which are concerned precisely with the "institutional" factors involved in that determination.[6]

It is of the utmost importance to observe, however, that at no point in the argument is it implied that the analysis can proceed without simultaneous and detailed reference to the calculations and actions of economizing individuals who live under these institutions, and whose decisions help to make economic events what they are. The theory of the forces determining the magnitude of M', for example, is in very large part a description of the calculations of entrepreneurs with respect to the profitability of borrowing from commercial banks in the face of given facts with respect to individual schedules of expected profit rates and the level

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[5] See the algebraic summary of the breakdown of the Fisherine T which is presented in Volume I, 599, n. 58, of the present work, and the analysis presented, in Chapters Eighteen and Nineteen of that volume, of the elements summarized by this algebraic formulation. The substance of the argument there presented with respect to the importance of dealing with "non-output transactions" (the magnitude of which will depend in such large part upon "institutional" factors) provides a sufficient commentary on the argument of those for whom "it is more natural and less misleading" to speak of "elastic" or "inelastic" *"supply"* as a *substitute for* a discussion of the factors, institutional and conjunctural, which make the Fisherine T as large as it is. On this matter, see also what is said below, pp. 475, n. 28, and 555 ff. For a further example of failure to do justice to the institutional factors involved in the determination of the "non-output" elements in T, see what is said in Volume I, 439 f., 444, 449, of the present work, concerning the proposition, in Keynes's *Treatise,* that "the volume of real-balances depends on the decisions of the depositors."

[6] See, for example, points four to seven of the "tentative list of the forces determining the size of cash balances held relative to outlay" given in Volume I, 482 f., of the present work. It may be added that even points one and two of the list in question necessarily involve a consideration of "institutional" factors; for it is inconceivable that the "time shapes" of individual streams of money receipts and money expenditures should remain unaffected by such "institutional" elements, for example, as the methods of paying wages and dividends.

of "market" rates of interest.[7]   Precisely the same thing
must be said of any adequate theory of the nature of the
forces determining the magnitude of the V of our formula-
tion; for the whole purpose of the expression $V = 1/K$ is
to remind us that no theory of the velocity of circulation of
money can be regarded as satisfactory unless it is at all
points consistent with the methodological implications of
the "cash balance approach": implications which, as we
have seen, are precisely the same as those with respect to
the rôle of individuals' calculations which may be said to
underlie "modern" value theory generally.[8]   And the same
thing, finally, must be said with respect to elements in-
cluded in the T of our Quantity Equation, such as the r
in the expression $v = m + r$ (in which v represents the
"velocity of circulation of goods," m the "number of middle-
men's sales," and r the "rate of sale" of goods); or such as
that component of T which is represented by the volume
of sales of securities (the T_{sc} of our formulation).[9]   For if
anything is clear, it is that, in the determination of the mag-

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[7] Particular attention should be called to the phrase "in very large part,"
in the statement made in the text. The statement itself, moreover, repre-
sents a generalization intended to apply under the institutional conditions
prevailing over most of the western world prior to that large-scale substitu-
tion of *governmental* borrowing for borrowing by private business which
has been so characteristic of recent years. It should hardly be necessary to
labor the point, however, that the statement continues to apply with
undiminished force to that *part* of borrowing from commercial banks which
is still undertaken by private business. One could, indeed, wish for no
better example than that which is here provided of the necessity for pre-
senting an apparatus which would do justice to *both* (1) "institutional"
factors and (2) the element of individuals' calculations of profit—the for-
mer helping to determine how *much* of the borrowing will be subject to
these calculations, and the latter telling what these calculations will be.

[8] Cf. Volume I, 417 f., of the present work. It is something of a com-
mentary upon the slowness with which analytical devices take hold that
we should still be compelled to read criticisms of the use of the concept of
"velocity" which show a complete failure to understand what is involved
in the "cash balance approach" to the problems which that concept sum-
marizes. See, for example, the comment on the relation of the concept of
"velocity" to the "explanation" of realized events by Ohlin, "Some Notes
on the Stockholm Theory of Savings and Investment, I," *loc. cit.*, 69; and
see also the characterization of the concept of "velocity" as a "most abstract
and bloodless concept" by J. Åkerman, *Konjunkturteoretiska Problem*, 84.

[9] On the expression $v = m + r$, see my "The 'Rate of Sale' and the
'Velocity of Circulation of Goods': A Comment," in *Economica*, VI (1939),
450 ff.   On T_{sc}, see Volume I, 599, n. 58, of the present work.

nitude of r, entrepreneurial decisions (in this case, decisions with respect to the administration of inventories) play a major rôle.[10]   And it is equally clear that changes in the volume of trade in securities will in all cases be the result not only of institutional factors, but also of the decisions of holders of such securities with respect to the profitability of trading or refraining from trading in such securities.[11]

It would be a very serious mistake, moreover, to suppose that an item such as the r of our formulation is the *only* element in our T_g whose magnitude will be what it is as the result of entrepreneurial calculations, the nature of which it is the function of an adequate analytical equipment to describe.   There is, after all, the element of *Output*, which will in all cases be what it is as the result of the calculations and the decisions of entrepreneurs confronted by given conditions of demand for and supply of their respective products.[12]   Even in advance, therefore, of the more detailed discussion of the supply side of the problem presented below in Chapter Eleven, it is of considerable importance to point out that the argument of our Chapter Five with respect to the relation of the supply and demand curves of the "general" Theory of Value, on the one hand, and "stream" equations of the Fisherine type, on the other, constitutes at the same time a demonstration that there is included in our analysis of the forces determining T the whole of that sector of the "general" Theory of Value which is concerned with calculations of entrepreneurs in general, and the effects of such calculations upon "supply," in particular.[13]

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[10] In this connection, see also what is said below, p. 555, with respect to the relation of the r of our formulation to the concept of "elasticity of supply" and to "supply curves" generally.

[11] It is clear that any adequate description of the nature of the forces affecting these decisions would have to take account of the type of factor treated, in Keynes's *Treatise*, under the head of the "bearishness" and "bullishness" of the public (cf. Volume I, 77, n. 14; 466, n. 20; 472, n. 36; and the references to the *Treatise* there given), and, in Keynes's *General Theory*, under the head of "liquidity-preference due to the speculative motive" (cf. the *General Theory*, 173 f.).

[12] Cf. Volume I, 449, of the present work; and see also what is said on this matter in Chapters Ten and Eleven, below.

[13] See below, pp. 549 ff.

2. *Institutions and Individuals' Calculations in the "General" Theory of Value.* When this is seen, however, we see an additional fact which itself constitutes a commentary upon the frequent antithesis between an emphasis upon "institutions" and "institutional" practice, on the one hand, and the economic calculations of individuals, on the other. For if there is any "institution" whose importance for the pricing process cannot be ignored, it is the "institution" of *Monopoly*.[14] What this must mean, in turn, is that the whole of that part of the "general" theory of pricing which is concerned with the calculations of monopolists or quasi-monopolists is a contribution to our understanding of the effect of the "institution" of monopoly upon the pricing process.[15]

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[14] The fact that our theory of monopoly price and monopoly practice generally is almost entirely a contribution of "traditional" economic theory itself provides a commentary both upon the assaults encouraged by, and upon the positive achievements of, that type of "institutionalist" theory with which "traditional" economic theory is often contrasted. It is hardly pleasing, therefore, to find Mr. Keynes among those who, in defiance of all the facts of doctrinal history, persist in characterizing "the orthodox theory" of "production and distribution" as having been constructed entirely on the assumption of "free competition." See the preface to the German version of the *General Theory*, p. ix. In view of the fact that the alleged addiction of "orthodox theory" to the practice of confining itself to the case of "competition" is cited by Mr. Keynes as one of the reasons why the argument of the *General Theory* is to be regarded as more "general" than that of "orthodox theory," it may not be out of place to call attention to the oblique way in which the element of monopoly is disposed of in the *General Theory* itself. See, for example, pp. 268 and 270 of that work; and cf. also what is said on this matter below, p. 561, n. 19.

[15] It should hardly be necessary to labor the point that to say that a "contribution" is involved is not to say that the "contribution" itself represents a complete and unperfectible body of analysis. The opposite is indicated by the two following paragraphs of the text. But surely there are few economists who would agree with the implications as to the significance of the contribution itself which are contained in Professor Hicks's suggestion that "a general abandonment of the assumption of perfect competition, a universal adoption of the assumption of monopoly, must have very destructive consequences for economic theory," since what is threatened is a "wreckage" of "the greater part of economic theory" (*Value and Capital,* 83 f.). For one thing, there is no clear reason why "a general abandonment of the assumption of perfect competition" should be regarded as necessarily equivalent to "a universal adoption of the assumption of monopoly." For another, there is no clear reason why the term "economic law" should be reserved only for propositions constructed upon a basis making it possible to establish "determinate" "stability conditions"

For our present purpose, however, it is of more importance to stress a further fact: namely, that the argument of Chapters Five to Seven of the present volume with respect to the relation between demand and supply schedules of the Marshallian type and "stream" equations of the Fisherine type takes on particular significance when it is applied to the special problem of the effect of monopoly upon the *functioning of an economic system in time*. For the formal theory of monopoly and monopolistic competition is almost entirely a theory running in terms of Marshallian ("partial equilibrium") supply and demand curves.[16]   It follows, therefore,

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(*Value and Capital*, 83 f.). And it is to be doubted, finally, whether many economists would accept Professor Hicks's extraordinary suggestion that the criterion of the usefulness of a given analytical apparatus is not its fitness for dealing with a setting in which specific institutions, such as monopoly, occupy a position of importance, but its fitness for establishing "determinate" "stability conditions" in a setting in which such institutions did not exist.

[16] It should be observed that, by "Marshallian" curves, I do not intend to mean that the curves involved in the theory of monopoly and monopolistic competition are to be regarded as *"ceteris paribus"* curves, in any sense of the term which would contrast such curves with what have been called *"mutatis mutandis"* curves. See above, pp. 173, n. 60, and 174, n. 62. On the contrary, the suggestion in the text is to be interpreted in the light of the argument of our Chapter Four, with its insistence that the use of "Marshallian" ("partial equilibrium") demand and supply schedules is not invalidated by the fact that they are subject to change as the result of changes elsewhere in the price structure, or of expectations of such changes (cf. the references to Marshall himself given above, p. 192, n. 104). On the basis of the argument of our Chapter Four, also, it should be clear that this proposition does not contradict the further proposition that "Marshallian" ("partial equilibrium") analysis "is continually subject to the limitations imposed upon it by 'general equilibrium' analysis of the Walrasian type" (above, p. 166). For it was shown (1) that the abler sponsors of "partial equilibrium" analysis have always recognized this limitation (pp. 167 ff.); and (2) that Walras himself, in contrast with some of his more fanatical disciples, frankly recognized that his own emphasis upon the general interdependence of economic variables did not mean a denial that the devices of "partial equilibrium" analysis were themselves extremely useful (see above, p. 171, n. 55). In the light of these facts of doctrinal history, it should be clear that the statement in the text with respect to the relation of "the formal theory of monopoly and monopolistic equilibrium" to "Marshallian supply and demand curves" should not be taken as implying a denial of the fact that some aspects of the theory of monopolistic competition necessarily involve the use of certain theses with respect to general economic interdependence. Cf. R. Triffin, *Monopolistic Competition and General Equilibrium Theory*, 3 f., 9 ff., *et passim*. If one were inclined to quarrel at all with Mr. Triffin's suggestive treatment, it would be only to question (1) whether the "canyon which has for years separated these two

that any conclusion developed originally within the "general" Theory of Value with respect, for example, to the conformation and position of supply curves under monopoly, *including conclusions with respect to the probable rigidity of such curves,* can be applied directly to an explanation of monopoly or monopoloid prices actually realized at a given point in clock time, or successively over a given period of clock time.[17]

This, however, is by no means all. For it would be easy to show that no description of the *consequences,* for the functioning of the economic system as a whole, of rigidity in given parts of the price structure can be regarded as satisfactory unless it is prepared to trace the effects of such rigidity upon *the rate at which the streams of money receipts,* as modified by the conditions with respect to price rigidity, *are subsequently disbursed, and upon the direction of such disbursement.*[18] This, clearly, is a problem which can be solved only by the use of an apparatus of the type outlined in the present work, an essential element in which is the provision of a special set of devices for tracing those

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schools of theoretical thought" (p. 3) has really existed in the case of instructed members of each "school"; and (2) whether only the advent of the theory of monopolistic competition can be said to have begun "to bridge the canyon" (p. 3).

[17] On the suggestion that, under conditions of monopoly, there *is* no market "supply curve," see what is said above, pp. 257 ff. It goes without saying that the very application of the type of analytical apparatus represented by these curves to the explanation of prices historically realized in clock time may be expected to lead to an enrichment of the analytical apparatus itself as the result of the contact thus necessitated with the substance of *monetary* theory. The rigidity of contract stipulations, for example, in the face of monetary expansion and contraction, is a commonplace of monetary theory; and it can hardly be doubted that a fuller exploration of the effects of such rigidities upon monopolistic pricing policy during periods of monetary expansion and contraction will yield significant additions to the theory of monopolistic pricing as we now have it.

[18] The vistas opened by the possibilities inherent in an examination of this problem on both the analytical and the empirical sides are so vast that it is hardly surprising that work in the direction indicated must be regarded as being still in its infancy. In addition, however, to suggestions such as those contained in Haberler, *Prosperity and Depression* (for example, p. 401), see D. Wallace, "Monopoly Prices and Depression," in *Explorations in Economics: Notes and Essays Contributed in Honor of F. W. Taussig* (1936), 348, 353 ff., and Sumner, "Public Utility Prices and the Business Cycle: A Study in the Theory of Price Rigidity," *loc. cit.,* 101 ff.

changes in the amplitude and direction of the money stream which can be shown to result from decisions made with respect to the spending of money previously received.[19]   And it will be observed that the analytical system here outlined makes it possible to discuss this problem without at any time loosening the connection previously established between the magnitude of these streams of realized money *receipts* and realized money *expenditures,* on the one hand, and those realized *prices,* on the other, whose determination is to be accounted for by the full use of the relevant sectors of both monetary theory and the "general" Theory of Value, both of which, in turn, are given full play in the analytical system here proposed.

II

Mechanisms and "Mechanical" Analysis

We already had occasion, in Volume I of the present work, to deal with the suggestion that the use of "stream" equations of the general Fisherine form necessarily commits the user to what is characterized as a "mechanical" view of the pricing process, in some undesirable sense of the word "mechanical." [20]   Specifically, the suggestion is that the use of these equations makes impossible or unlikely a close attention to the motives and calculations of those economizing

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[19] The general form of the expressions involved is that given in Volume I, p. 382, n. 85, and p. 383, n. 88.   Since the money *receipts* of the monopolist are represented by a (PT) with appropriate subscripts (those used, for example, in Volume I, p. 383, n. 88 represent only a single possibility in the very wide range of possibilities opened by a further subdivision of prices and money payments into significant sub-groups), it is made clear that these money receipts will be as large as they are because the price and sales policies of the monopolist are what they are.   It is also made clear, however, that what happens thereafter will depend upon the decisions of the monopolist with respect to the holding of cash, on the one hand, and the direction of such expenditure as is undertaken, on the other. It is evident that the effect of monopoly upon the amplitude and direction of expenditure out of money previously received can be traced empirically in all necessary detail, and can then be compared with the results that one would expect to come about under various hypotheses with respect to different types of pricing policy and the resulting differences in the distribution of money receipts.

[20] See Volume I, 160, n. 6; 173; 178; 493, n. 20; also what is said on this matter above, pp. 100 f.

individuals whose decisions make economic events what
they are.[21]   It should be clear, however, from the argument
of the preceding section of this chapter, that no characteri-
zation of the subject matter of Volume I which is there sum-
marized could have less foundation in fact.   On the con-
trary, one of the major purposes of the apparatus outlined
in the present work is to show at precisely what points, and
in precisely what ways, these motives and calculations con-
dition the successive steps in the economic process.[22]

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[21] Cf. Lambert, *La Théorie quantitative de la Monnaie,* 81, where, in
discussing the criticism of certain forms of "the quantity *theory*" as "me-
chanical," the author points out that the adjective "mechanical" is here
taken in one of its dictionary meanings: namely, as "operating without the
help of reflection and will."   The references given by Lambert in n. 3 to
the page indicated are interesting as reminding us that the type of issue
discussed in the text has been repeatedly raised (and with the same con-
fusion of ideas and misrepresentation of the views of one's opponents) by
those who insist upon finding an inevitable conflict between "quantitative"
and "qualitative" "theories" of money, or between "quantity theories" and
"psychological theories" of money.   The use of the adjective "mechanical"
as a term of opprobrium has of course not been confined to the issues
raised by "the quantity theory."   On the contrary, it has sometimes been
applied to all economic theory which relies upon the deduction of con-
clusions from premises, and particularly to the use of mathematics in
economic theory.   In this connection, cf. the comments of Wicksteed,
"Jevons's Economic Work," *Economic Journal,* XV (1905; p. 810 of the
article as reproduced in the 1933 reprint of Wicksteed's *The Common
Sense of Political Economy and Selected Papers and Reviews on Economic
Theory*).   It should hardly be necessary to emphasize the point that the
very differences in the nature of the issues involved in the two types of
charge are such as to provide a further warning against an indiscriminate
use of the adjective "mechanical" as a term of opprobrium.   In this con-
nection, cf. the comment on the relation between "Jevons's interest in his
logical machine and in what he called the 'mechanics of industry,'" in
Edgeworth's *Papers Relating to Political Economy,* II, 290; and on Jevons's
use of expressions such as "the mechanics of industry," see below, p.
474, n. 24.
[22] Contrast the comment of A. H. Hansen, in the *American Economic
Review,* XXVIII (1938), 751 f.   It should be clear, for example, that the
apparatus here outlined does precisely what, according to Myrdal (*Mone-
tary Equilibrium,* 124) should be done by any adequate apparatus for
"analyzing the changes in the volume and velocity of the means of pay-
ment": namely, start from a statement of the problem making use of both
"*ex ante* and *ex post* calculations, of which the *ex ante* denote the driving
causal factors, i.e., the tendencies at every point of time, and of which the
ex post denote the outcome as subsequently registered."   It should be
equally clear that while our apparatus is certainly intended to help in
"analyzing changes in the volume and velocity of the means of payment,"
it continues to "derive its whole conceptual apparatus and its statement of

It cannot be too strongly emphasized, on the other hand, that there is another sense of the term "mechanical" which, when applied to a given analytical system, must be regarded as a term of encomium rather than the opposite. For economics, if it is ever to account for the economic processes realized in the world we know, must be prepared to trace in all possible detail the *mechanism* of those processes—in the sense of a series of realized events each of which is what it is because of what has preceded, and each of which itself helps to determine what is to follow.[23]   That an analytical system should be characterized as "mechanical" because it is concerned precisely with *mechanisms* of this type is clearly

the problems from . . . analysis on that deeper level of [the theory of] price formation where the causal relations of demand and supply are studied (Myrdal, *loc. cit.*). What our apparatus does *not* do is to assume that there is any necessary conflict between the latter type of emphasis, on the one hand, and, on the other, analysis designed to establish "how the quantity of money could be affected," or "what happens to the velocity of money" (contrast Ohlin, "Some Notes on the Stockholm Theory of Savings and Investment," I, *loc. cit.*, 56, 67 f.; cf. also what is said on this matter above, p. 383, n. 77, and p. 466, n. 8).

[23] The type of analysis thus desiderated is, of course, nothing more than that which Comte described as analysis designed to establish a "rational filiation in the succession of events, so as to permit, as for every other order of phenomena, and within the general limits imposed by a superior complication, a certain systematic prevision of their ulterior succession" (cf. Volume I, 143, of the present work, and the references given in n. 1 thereto). The concept of a "rational filiation in the *succession* of events" *in time* is of course precisely the concept underlying those "mechanisms" which bulked large in certain sectors of economic theory long before the modern term "sequence analysis" was introduced (see above, pp. 369 ff.). No one with an adequate appreciation of the degree of enlightenment which the provision of these "mechanisms" (or "model sequences") has brought to the explanation of economic processes, could ever have brought himself to suggest that there is a *conflict* between the provision of "mechanisms," on the one hand, and "causal explanation," on the other. On the contrary, he would be more likely to echo the comment of a recent writer: "When one says 'cause', one also says 'time' " (J. Åkerman, *Ekonomisk Kausalitet*, 44); for it is precisely these "mechanisms" operating *in time* which represent an essential contribution to our understanding of the "causal connections" involved (cf. the same writer's *Das Problem der sozialökonomischen Synthese*, 74 f., and also p. 168 of the latter work). In a fundamental sense, therefore, any antithesis between an interest in the "mechanical" aspects of a given process, and the "organic" aspects of such a process, in a sense of the former which would leave out a consideration of the "causes" of the process, is a false antithesis (cf. Hammarskjöld, *Konjunkturspridningen*, 11, 53 ff., and Lundberg, *Studies in the Theory of Economic Expansion*, 86 f.). See also the following note.

a characterization implying praise, and not blame.[24]   The
only question worthy of serious discussion, therefore, is
whether the possibilities inherent in the analytical system
here outlined for tracing these mechanisms are greater or
less than the possibilities offered by rival systems.

It is claimed that the analytical system outlined here is
in this respect at least equal to any of its rivals, and is su-
perior to most of them.[25]   One may cite, in this connection,
the description of the mechanism whereby, given an addi-
tion to the stock of money of ultimate redemption capable
of serving as bank reserves, *changes in the quantity of bank
money are effected*—in contrast with the type of descrip-
tion which would regard the quantity of bank money as
"adjusting itself" to the pecuniary volume of transactions.[26]

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[24] For proof of this fact, as well as of the fact that there is no necessary
conflict between an emphasis upon "psychological" factors, on the one
hand, and the "mechanism" underlying realized economic processes, on the
other, one has only to recall that Jevons, who can hardly be charged with
a lack of awareness of the "psychological" elements in the economic process,
actually proposed to subtitle his projected *Principles of Economics,* which
he intended to make "the work of his life," "A Treatise on the Industrial
Mechanism of Society" (cf. p. v of the fragment of the *Principles* pub-
lished in 1905). It should be added, moreover, that a survey of the range
of topics which Jevons proposed to discuss in his *Principles* hardly sup-
ports Edgeworth's statement that "what Jevons called the 'Mechanics of
Industry' is statical, not dynamical" (see Edgeworth's *Papers Relating to
Political Economy,* II, 56). On the contrary, Edgeworth seems to have
been guilty in this instance of the common practice of basing a generaliza-
tion as to Jevons's understanding of the scope of economics (and therefore
of the study of "the industrial mechanism of society") solely upon the
evidence provided by the fragmentary *Theory of Political Economy.* On
this matter, cf. what is said above, p. 61, n. 24. Actually, Jevons, instead
of advancing, even in his *Theory of Political Economy,* a proposition of
the type quoted above from Edgeworth, stated his belief "that dynamical
branches of the Science may remain to be developed," and merely pointed
out that he himself had "not at all entered" on the consideration of these
"dynamical branches" in the *Theory* itself (cf. p. vii of Jevons's Preface to
the *Theory*).

[25] The examples which follow are, of course, to be regarded *only* as "ex-
amples," and in no sense as a complete list of the instances in which the
analytical system here outlined can be shown to be superior to its rivals.

[26] See Volume I of the present work, 171 ff., 213 ff. In the light of the
references there given to earlier literature on the subject of the nature of
the forces affecting the quantity of bank money and the mechanism whereby
changes in this quantity are brought about, it must remain a matter of
astonishment that it should be possible to find statements in contemporary
literature to the effect that earlier discussions of the problem succeeded only

One may cite, as a further example, the description of the
mechanism whereby the rate of interest, operating as a
"capitalization factor," affects the structure of realized
money prices by way of its effect upon the dimensions and
the direction of use of streams of realized money expenditure
—in contrast with the type of description which would imply
a direct effect upon realized money prices without, or inde-
pendently of, any alteration of the dimensions of the stream
of money expenditure.[27]   Another set of examples is pro-
vided by the breakdown of the T of the Fisherine equation
into components each of which permits the study at close
range of the effect of both institutions and individual cal-
culations upon the mechanism whereby money prices are
actually realized—in contrast with a type of analysis which
would avoid all these questions of mechanism by operating
with a formulation in which room is made only for the
magnitude of "output." [28]   A further set is provided by a

in revealing the "inherent difficulty" represented by an alleged "impossibility
of determining quantitatively the supply of money" (cf. Rosenstein-Rodan,
"The Coordination of the General Theories of Money and Price," *loc. cit.*,
258); or statements implying that nothing more is provided by an intel-
ligent use of the analysis summarized by the variables of the familiar
Quantity Equations than propositions to the effect that "in order that the
total money value of transactions shall be able to rise, MV must rise,"
whereas, it is argued, MV is itself nothing but "the total money value of
transactions" (cf. Ohlin, "Some Notes on the Stockholm Theory of Sav-
ings and Investment, I," *loc. cit.*, 69). On certain aspects of Mr. Keynes's
treatment, in his *General Theory,* of the forces affecting the "quantity of
money," and particularly his treatment of the way in which the "quantity
of money" is alleged to "adjust" itself, see what is said below, pp. 582, 643 f.

    [27] See Volume I of the present work, 258 ff.; and cf. the comments on
the relevant aspect of the argument of Keynes's *Treatise* which are cited
on p. 260, n. 70, and p. 509, n. 67, of that volume.

    [28] See Volume I, 514 ff., 525 ff. It is still possible to find frequent ex-
amples of the influence of Keynes's *Treatise* in encouraging a belief in the
inferiority of a formulation making use of a Fisherine T, which is then
broken down into its several components, to a formulation in which a term
for "output" is virtually *substituted* for the inclusive T, despite the fact
that the volume of *"output"* sold is only a single (albeit very important)
component of this T. See, for example, the suggestion by B. P. Whale, in
Economica for February, 1940, p. 90, that the mere fact that, under certain
circumstances, "the expenditure of new money will almost inevitably in-
crease *production and therefore T,*" means that it is "more natural and less
misleading" to avoid entirely analysis in terms of the Fisherine T and its
components, and to say simply that "P will not rise except in so far as
supply is not elastic" (italics mine). On the treatment of these issues in
Keynes's *General Theory,* see what is said below, pp. 553 ff., 716 f.

comparison of the possibility of tracing in close detail the
steps by which an alleged discrepancy between Saving and
Investment is alleged to affect prices—in contrast with a
system in which the tangible manifestations of such a dis-
crepancy, including all the essential intermediate steps in
these manifestations, are left unanalyzed in terms of real-
ized monetary magnitudes.[29]   And most important of all
is the example provided by the treatment of the steps in-
volved in the mechanism of the generation and utilization
of money income—in contrast with those treatments in
which the problems of mechanism are hurdled, instead of
being resolved, through the unsupplemented use of devices
such as the concept of "income velocity," on the one hand,
or the "multiplier," on the other.[30]   For it is precisely the
unsupplemented use of devices of the latter type that would
make it impossible to relate the successive steps in realized
processes to those intermediate decisions and actions which
are the very antithesis of "mechanical" reactions, in an in-
vidious sense of the term "mechanical." [31]   Yet it is not

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[29] In this connection, see what is said, for example, in Volume I of the
present work (pp. 280 ff.), with respect to the argument of the *Treatise*
concerning the concept of *Investment*, when the problem involved is that
of tracing the operation of "the forces leading to a change in the dimensions
of the stream of money expenditure."   The argument applies, of course,
without significant qualification, to the treatment accorded to "Investment"
in Keynes's *General Theory*.

[30] Particular attention should be called to the fact that it is to an *"unsup-
plemented"* use of the concept of "income velocity" to which objection is
made here, as it was in Volume I (see, for example, pp. 385, 404 ff.).   The
objection, that is to say, is to a use of the concept of "income velocity"
for the purpose of accounting for changes in the generation of money in-
come and outlay from such income which is "unsupplemented" by "close
analysis" of the successive changes in magnitudes, the nature and opera-
tion of which is not revealed by the concept of "income velocity" itself.
On the nature of the apparatus which *is* required for the purposes of such
"close analysis," see Volume I, pp. 382 f., 404 f.   For evidence that I did
not, in Volume I, "reject" the "use of the concept of income-velocity"
(contrast B. P. Whale in *Economica* for February, 1940, p. 91), see, in addi-
tion to the passages cited at the beginning of this note, pp. 408 and 561, n.
103, of that volume.   I call attention particularly to the first of these pas-
sages; for the position of Mr. Hawtrey to which I there refer with approval
is precisely my own position.

[31] On the relevant aspects of the concept of "income velocity," see, for
example, what is said in Volume I of the present work, pp. 368 ff., 372 ff.
With respect to the accusation that the position there defended amounts
to the advocacy of a type of "atomistic" analysis *to the exclusion of* anal-

the least paradoxical aspect of recent discussion that such unsupplemented use should be found precisely in the writings of some of those who have protested against the use of the familiar Quantity Equations, and even of the further elaborations of which these equations can be shown to be capable, on the ground that such use involves an excessively "mechanical" treatment of the subject.[32]

There is, therefore, no need to apologize for the fact that the analytical system outlined in the present work is capable of being represented by a "machine" as rigorously constructed as Irving Fisher's celebrated contrivance for the representation of the conditions for general economic equilibrium; particularly since this representation involves none of the limitations applying to Fisher's "machine" precisely because of the fact that the latter was constructed primarily to show the conditions for the establishment of "equilibrium."[33] For the purpose of such a "machine" as is here

ysis which will provide us with "a view of the process of circulation as a whole," (cf. Mr. Whale in *Economica* for February, 1940, p. 92), see what is said in Volume I, 433 ff., and especially pp. 444 f., concerning the attempts of earlier writers to put upon the "cash balance approach" a burden greater than I believe it capable of bearing.

[32] Since Professor A. H. Hansen has courteously chided me for an alleged addiction to an excessively "mechanical" treatment of the issues involved (see the reference given above, p. 472, n. 22), I may point out that he himself has made "unsupplemented" use of the concept of "income velocity" in the past (see, for example, the reference given in Volume I, p. 360, n. 34, of this work, and also Garver and Hansen, *Principles of Economics,* p. 337 of the second [1937] edition); and, to judge from Professor Hansen's more recent writings (see, for example, *Full Recovery or Stagnation,* 295 f.), he has become increasingly sympathetic to the claims of "multiplier analysis," as against other types of analytical apparatus for dealing with the processes with which "multiplier analysis" is intended to deal. I hope to be able, in a later publication, to deal at length with those aspects of "multiplier analysis" which seem to me open to the charge of excessively "mechanical" treatment, in the sense that they involve a failure to establish with all possible precision, and at every step of the argument, the nature of the decisions by individuals (and of the forces affecting these decisions) which make realized processes what they are.

[33] See Fisher's *Mathematical Investigations in the Theory of Value and Prices,* 24 ff. It should be observed, for example, that, in contrast to the "machine" here described, there is no explicit use of any of the axes in the Fisher "machine" to measure *clock time.* It may be pointed out also that, in contrast with the Fisher "machine," the variables whose dimensions, in our "machine," give the magnitude of the various "cisterns," or the amount of water in these "cisterns" (our "compartments"), refer in all cases to

indicated is to provide a rigorous picture of the mechanisms by which every event realized in clock time is conditioned by the events that have preceded it, and itself conditions the events that follow, while at every step in the process the realized events are brought into direct relation with the calculations which help to make each of these events what it is.

No one could pretend that the construction of analogies from physical mechanics in itself proves anything for economics. On the contrary, all physical "models" and "charts" of the economic process must always remain, from one point of view, within the realm of curiosities. From another point of view, however, they may be welcomed on the ground on which all pedagogical devices should be welcomed: namely, that they may bring out more clearly some aspects of the verbal argument that might otherwise have remained obscure.[34] Only for this reason do I

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realized *events,* and do not include magnitudes such as "marginal utility" (cf. Fisher's *Mathematical Investigations,* 25 ff., 37 ff.), which, in the case of our "machine," is to be imagined as conditioning the form of the *ex ante* market demand and supply schedules which are themselves to be "imagined" as determining every realized price's location with respect to the price (vertical) axis and the quantity (depth) axis.

[34] Contrast the skeptical comments by Mitchell, *Business Cycles: The Problem and its Setting,* 186 f., concerning the possibility of designing "a mechanical contrivance which would work somewhat after the fashion of cyclical business fluctuations," and which would not at the same time be "difficult to understand" and would not leave "uncertain" the "real similarity of its operations to business processes." How "difficult to understand" my "machine" is, and how "similar" its operations are to "business processes," I must allow the reader to judge—though I suspect that "whether it is worth while taking much trouble in getting into these conceptions" will depend, as Edgeworth said of Fisher's "machine" (see Edgeworth's *Papers Relating to Political Economy,* III, 37), on "how much cognate training the reader has already received." I can say, at any rate, that in attempting to provide a picture of a "mechanical contrivance" which will do justice not only to the type of situation envisaged in " 'static' problems," but also in problems which "are the opposite of 'static' " (cf. Mitchell, *op. cit.,* 186), I am only following the example set by writers on economics from the eighteenth century to our own day. For an early instance of the use, in the description of economic processes, of the figure of "a machine, consisting of sundry channels or cisterns of water, connected together by tubes, through which the water should be continually flowing or circulating, whereby those several cisterns should continually *supply* and be *supplied by* each other," see the reference to Samuel Gale given in Volume I of the present work, p. 533, n. 19. The use of this type of analogy is of course more common in writings to be classed as being on the periphery of "received" economic doctrine, rather than at its center—though it should be added that the peripheral character of the contribution has been overemphasized as a result of the typical penchant, of the avowed "heretics" pre-

venture to present the following description of a "machine" constructed in accordance with the outlines of the analytical system presented in this work.

1. The base of the "machine" is measured, along its *width,* in units of *clock time.*[35] Along its *depth,* it is measured in units of *quantities*

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senting these constructions, for claiming complete originality in "picturing closed circuits in three dimensions." (Cf., for example, A. Dahlberg, *Jobs, Machines, and Capitalism* [1932], 105 ff., where the author's method "is advanced as a new scientific method," opening quite revolutionary vistas for "the social sciences." Cf. also the (unintentionally) amusing comments of the same author in his later *When Capital Goes on Strike,* p. xxi, on "the unwisdom of familiarizing oneself too well with prior endeavors," on the ground that an investigator "only grooves his thought if he persists in detailed study and review of others' efforts.") See, for example, the "charts" (which could easily be used as the basis for construction of mechanical models) in the writings of N. Johannsen ("J. J. O. Lahn": cf. his *Der Kreislauf des Geldes und Mechanismus des Sozial-lebens* and his *A Neglected Point in Connection with Crises*); the charts of Foster and Catchings (see, for example, their *Money,* 303 ff.); and the charts in Dahlberg's *Jobs, Machines, and Capitalism,* 50, 58, 64, 70. Such designs, however, have by no means sprung only from the imagination of the slightly "queer." In addition, for example, to Irving Fisher's well-known construction designed to display "The Mechanics of Bimetallism" (*Economic Journal,* IV [1894], 529 ff.; cf. *The Purchasing Power of Money,* 104 ff., 116 ff.)—a construction itself suggested, in part, by Jevons (*Money and the Mechanism of Exchange,* 140; cf. Fisher's *Purchasing Power of Money,* 116 n.)—see the description of a "mechanical model" designed to "represent many of the characteristics of money events," which is presented by Pigou in his *Theory of Unemployment,* 199 ff., 223 ff.; and see also the more loosely conceived "models" presented by J. Akerman in his *Om det ekonomiska Livets Rytmik,* 172, 175, and in his *Konjunkturteoretiska Problem,* 28 ff. (cf. the same author's *Das Problem der sozialökonomischen Synthese,* 126 f., 223). Intermediate between the "models" presented by the "respectable" writers just cited and those presented by the avowed "heretics" listed above is the type of construction presented by G. and E. Guillaume, *Sur les Fondements de l'Economie Rationelle* (1932)—a construction whose "cells" (pp. 154 ff., 186), "tubes" (181 ff.), and "cinémogrammes" (182, 235) are by no means without interest to the economist, but whose effectiveness is very greatly lessened by the authors' unwillingness to translate the elements involved in their construction into the categories of technical monetary and "general value" theory.

[35] The use of three-dimensional constructions in which one of the axes represents *time,* and the other two represent, respectively, (1) quantities bought or sold, and (2) the prices at which these quantities are bought and sold, goes back at least as far as Marshall's use of the concept of a "demand and supply surface." See Marshall's *Principles,* 809, n. 2. (The construction is characterized as a "demand and supply surface" in the index [p. 861, under *"Demand curve"*]. The concept itself was introduced in the third [1895] edition of the *Principles* [p. 516, n. 2]; and reference was given in the fourth [1898] edition [p. 520 n.] to "an article by Cunynghame in Vol. 2 [1892] of the *Economic Journal*" [H. Cunynghame, "Some Improvements in Simple Geometrical Methods of Treating Exchange Value,

of particular articles sold.[36] Along the *height* of the "machine," on the other hand, realized individual *prices* are measured.

2. It is clear that if the prices were *not* plotted in depth, as well as in height, we should have an ordinary graph of a time series of prices.[37] In fact, however, the individual prices can be plotted not only in height and width (dates of realization in clock time), but also in *depth*, according to the *quantity* of the particular article sold at each price and the

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Monopoly, and Rent"]. Actually, however, the suggestion under discussion here—namely, the use of a specific axis in three-dimensional constructions to represent *time*—was purposely avoided by Cunynghame. See p. 39 of the article cited.) The three-dimensional construction, of the type indicated, which is best known in our own day, is that of H. Schultz. See his *Theory and Measurement of Demand,* 187 ff., 195, 199, *et passim.* See also, however, the reference, in the preceding note, to J. Åkerman's *Konjunkturteoretiska Problem.* It should be observed, on the other hand, that Åkerman's construction makes no attempt to represent either the flow of *money payments* or the relation of these money payments to the demand and supply curves of the "general" Theory of Value. The same thing is to be said of the construction, in "three dimensions," presented in G. and E. Guillaume, *Sur les Fondements de l'Économique Rationelle,* 181 ff. Professor Schultz's construction does attempt to represent the shifting of *demand* curves in clock time, although the type of "routine of change" represented bears little resemblance to the type of change envisaged in the construction presented here (see below, p. 482, n. 42). There was, however, no attempt, in Professor Schultz's work as far as he had been able to carry it, to represent the shifting market *supply* curves whose point of intersection with the shifting market demand curves gives us a series of prices actually realized in clock time; and, above all, there was no attempt to represent, by means of analogies from hydrodynamics, the flow of *money payments* in time.

[36] In Schultz's construction (see the preceding note), the corresponding magnitudes (in terms of "consumption") are plotted, perpendicularly to the time axis, in *height,* rather than in depth. His construction is like the one here outlined, however, in that it does make clear the desirability of plotting price and quantity on *different* axes, and not on the same axis. The former, presumably, is what is implied also by Lindahl (*Studies in the Theory of Money and Capital,* 60 f.), when he suggests that "if described graphically, the supply and demand *prices* appear as lines parallel to the time axis, with discontinuous movements at the points when prices are changed," while "the *transactions performed at these prices* appear as lines perpendicular to the time axis at those points of time at which the terms have been agreed"—although the fact that the "prices" indicated in the text are *realized* prices would argue for their representation by a series of *points* rather than by "lines." It should be observed also that the quantities referred to in the text are quantities *sold,* and not quantities *produced.* On the difference between the two, and its significance, see below, pp. 553 ff.

[37] The fact that time series *are* thus utilized is worthy of particular notice, in view of the emphasis placed, particularly in recent years, upon the necessity for developing our conceptual apparatus to a point at which it can be related directly to data in the form of time series. See what is said on this matter below, p. 506.

date of clock time at which such a sale is effected.[38]   We thus have, as before, a time series of realized individual prices; but if we tip the "machine" up so that the quantity-sold axis is now vertical, we see that we also have a time series of *quantities sold*, since all these quantities sold were originally plotted with reference to the time axis as well as with reference to the price axis.[39]   If, now, we turn the model of our "machine" so as to be able to view it from the side of *depth* (the quantity-sold axis), we observe that the co-ordinates of the plane which we now view are precisely the co-ordinates of the type of graph usually used in "partial" equilibrium analysis: namely, *quantity offered for sale* (or demanded) on the abscissa, and *price* on the ordinate.[40]

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[38] On the necessity for specifying, in the case of each realized price, the *quantity sold* at that price, see J. Åkerman, *Das Problem der sozialökonomischen Synthese,* 281, and Schumpeter, *Business Cycles,* 520.   No one familiar with the literature of our subject could be prepared to deny Professor Schumpeter's statement that it has "always been recognized," in "general theory and its statistical complement," that, "taken by itself, neither price nor quantity conveys its full message or in fact any that is definite, and each must be interpreted in the light of its companion."   So clearly is this the case, indeed, that one can regard only with amusement attacks upon the definition of economics as "the science of price" which are based upon no more profound an observation than that economics' is the science not only of *price,* but "also" of the "*quantities* of goods produced and consumed" (see, for example, F. Vinci "Il corporativismo e la scienza economica," *Rivista italiana di Statistica, Economia e Finanza,* VI [1934], 93).   There is no good reason, moreover, why we should regard with any less amusement the suggestion that the very use of a title such as *The Theory of Prices* implies a failure to bear in mind that any analysis of the effects of a "change in the flow of money payments" must take into account the effects not only on "prices," but also on the "volume of goods sold."   (Cf. N. Kaldor, in the *Economic Journal* for September, 1939, 497 f. I may take this occasion to point out that the title *The Theory of Prices* was chosen "in conscious imitation of the practice indicated by Mr. Keynes in his *General Theory,*" and that the area covered is intended to be precisely "that which Mr. Keynes himself has demarcated under the heading 'The Theory of Prices.'"   See Volume I, 2 f., and 5, of the present work.) For a description of the way in which account is taken, in the "machine" here described, of the fact that a "change in the flow of money payments" may be expected to effect *either* "prices" *or* the "volume of goods sold," or *both,* see below, pp. 483, n. 43, and 492 ff.

[39] It should be evident also that we have here a means for determining the "reaction speeds" involved in realized changes in supply.   In this connection, see the comments of J. Åkerman, *Das Problem der sozialökonomischen Synthese,* 166, on the need for dealing, "in empirical economics," with "*actual* velocities of growth"; and on the general question of the treatment of realized "reaction speeds" in the analytical system here outlined, see what is said above, pp. 395 ff.

[40] This idea is of course an essential element in the Schultzian construction to which reference has repeatedly been made.   It will be observed, at the same time, that each of these demand and supply curves is dated in terms of clock time, and the successive shifts of each are likewise dated in

3. If it be remembered that each realized price is to be conceived of as resulting from the intersection of a market demand schedule and a market supply schedule in just such a plane, it follows that there will be just such a pair of curves for every point in clock time at which prices are actually realized—in other words, a series of planes corresponding to each of such points in clock time, each plane containing an intersecting demand and supply curve for each commodity sold at a given price at the corresponding point in clock time. Since, however, our model is designed to represent only *realized* processes, these supply and demand curves are to be *imagined* by the observer, in precisely the way in which all *ex ante* magnitudes are conceived of as being "imagined" by those who base their market action upon their conception of these magnitudes.[41] There is, of course, no reason why these "imagined" curves need be of the same conformation and position as between any two instances of prices realized in successive intervals of clock time.[42]

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terms of clock time. Whatever may be said, therefore, with respect to other limitations of the work done (by Professor Schultz among others) on the statistical determination of demand curves (see what is said on this matter above, pp. 306 ff., nn. 183–185), fairness to Professor Schultz's memory demands at least a partial correction of the implication that in *all* work on this problem "time has disappeared from the system of coordinates" (cf. J. Åkerman, *Das Problem der sozialökonomischen Synthese,* 172). On the contrary, the effect of constructions of the Schultzian type under discussion here, when they are treated with adequate imagination and developed further along lines not explored by Professor Schultz himself, is only to confirm the generalization that "the boundary between an economics of time and equilibrium economics can never be drawn in a hard and fast way, if we wish to make use of empirical results"; for the type of analysis provided by large sectors of "equilibrium economics" is "so fundamental, that it appears in a new form in the explanation of time series, which might seem to have not the slightest connection with the premises" often laid down for purposes of "equilibrium analysis" (cf. J. Åkerman, *Das Problem,* 179 f.). On the possible suggestion that serious limitations are set to the apparatus here described because the "equilibrium analysis" involved is of the "partial" type, see what is said below, p. 484, and the backward references given in nn. 46 and 47 thereto.

⁴¹ This very fact, it will be observed, should be sufficient to demonstrate that the "machine" here described is not the type of "mechanical" contrivance which would suggest that economic processes unfold themselves without being affected at every stage by the *"plans"* and *decisions* of economizing individuals. See also the following note.

⁴² Nor, obviously, is there any reason to suppose that these curves will evidence the simple kind of *"routine* of change" which has so often been assumed in attempts to derive "statistical" demand curves. On this matter, see what is said above, pp. 306 ff., nn. 183–185. On the contrary, the presumption is rather that the business cycle, in particular, will bring about such "irregularities in all economic curves" as to provide a warning against the "mechanical" adoption of oversimplified hypotheses with respect to the "shifting" of these curves (cf. Valk, *Conjunctuurdiagnose,* 152). There is nothing in this proposition, obviously, which would argue against

On the contrary, we must conclude that when these successively realized prices are at different levels, something has happened to either the conformation or the position of the demand curve, or the conformation or position of the supply curve, or both.[43] And the answer as to what actually happened to these "imagined" (ex ante) curves will be obtainable only by independent evaluation of all empirical data that might tell us what probably did happen, and why—the nature of the data in question being indicated by whatever analytical material is available with respect to the nature of the circumstances which could have led to a shift or change of conformation of the demand and supply curves.[44]

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the presentation of "model sequences," even with respect to the business cycle, which would attempt to provide a description of "regularities" in the shifting of these curves that may be expected to follow under conditions rigorously defined in advance. There is every reason to believe, however, that these "shifts" (including, under this head, changes in the conformation as well as in the position of the curves) will be very much more complicated than those pictured in the Schultzian model referred to above, p. 480, n. 35, which pictures a "routine of change" in which not only the changes in the position of the curves are assumed to be of a very regular character, but in which there are also no changes in the conformation of these curves. In this connection, see, for example, Schultz's Theory and Measurement of Demand, 187; and contrast the comments of Lundberg, Studies in the Theory of Economic Expansion, 18, 21.

[43] Precisely the same conclusion holds, obviously, whenever, though the prices "successively realized" are at the same level, the quantities sold at these prices are at different levels. Such a development would be revealed by the fact that our prices are plotted in depth as well as in height (cf. above, p. 481, n. 38); and the obvious explanation of such a development would be found to reside in a simultaneous shifting of both the demand curve and the supply curve.

[44] The "analytical material" indicated is of course all of that included both within the relevant sectors of the "general" Theory of Value (the effects of changes in income, changes elsewhere in the price structure, and so on) and in the relevant sectors of monetary and trade-cycle theory (the differential effects of monetary expansion and contraction upon incomes, and so on). It is of some importance to observe that the very fact that evidence of the actual occurrence of changes in these elements is to be found in available statistics means that there are other (and, as I myself believe, more fruitful) ways of using statistics to account for changes in price as the result of changes in "supply" and "demand" than to attempt actually to construct "statistical" schedules of "demand" and "supply." For the effect of the method here suggested would be to transfer emphasis to the nature and the explanation of the probable changes in conformation and position of these (ex ante) schedules, instead of our being concerned with these changes only in order to be able to present a picture of these schedules as they would have looked if these changes had not occurred (see above, p. 176). At the same time, the proposed method of using statistics as a means of throwing light on the nature of the forces affecting "supply" and "demand" should answer the objections of those who have asserted that the designations "demand" and "supply," as used in the "general" Theory of Value, are "absolutely unrealistic"; that "no

4. Ideally, all realized prices will be plotted in the manner indicated under (2).[45] The resulting picture will represent the *system* of realized prices prevailing at any given point in clock time, and will also record all changes in the system of realized prices over a *period* of clock time, together with the *system* of quantities sold at any given point in clock time and all changes in that system over a period of clock time. It is this system of realized prices and realized sales, and the changes therein, that, together with individuals' *expectations* with respect to the future course of individual prices and the quantities associated with these prices, will condition the conformation and position of each successive pair of plane curves whose intersection gives us each realized price. This, of course, is merely another way of stating two propositions already established by our earlier argument. The first of these propositions is that a "general interdependence" exists as between all prices prevailing at a given moment in time, and that this fact is taken into account in the conceptual construction of any given market demand or supply curve.[46] The second proposition is that one kind of "interdependence in time" is established between the prices realized in successive moments in clock time by the very fact that the "expectations" which condition the market demand and supply curves controlling the realization of prices at a given moment in time are themselves conditioned by the course of prices realized at preceding moments in time.[47] It should be observed, however, that the processes envisaged in the two cases indicated are *mental* processes, and do not yet provide the type of interconnection in time which is given when the actual realization of a price at one moment of time is related, through *realized processes*, to the price realized in the next moment of time.

5. As we know from our Proposition XVII, the latter type of interconnection is given by the *flow of money payments*.[48] The problem, therefore, is to translate our argument with respect to the flow of money payments in terms of our model as described thus far.

The natural mechanical parallel to a "flow" of money payments is

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statistics can exemplify them"; and that "it is unthinkable that statistics could ever give us an unambiguous expression for these semi-psychological, semi-quantitative concepts" (so J. Åkerman, *Das Problem der sozialökonomischen Synthese,* 142).

[45] The word "ideally" has reference, of course, to the fact that considerations of expediency would recommend the construction of a physical model in which only a comparatively small number of realized prices would be represented. Since, however, nothing more would be involved than the question of physical expediency, there could be no more objection to the procedure indicated than there could be to the conventional practice of writing $p_1, p_2, p_3 \ldots p_n$, instead of writing out a complete series of numerical subscripts.

[46] See above, pp. 413 ff.

[47] See above, p. 415.

[48] See above, pp. 349 ff.

provided by hydrodynamics.[49] Let the "planes" described under (3), therefore, be conceived of as the walls of compartments in our "machine," through which water (= money) flows.[50] A "compartment" is set up, and a part of the "stream" of money passes from one such compartment to another, each time a money payment is made. The course of the money stream will then be represented by the dimensions of these compartments. We already have two of these dimensions—the height, given by the price received for each unit of commodity sold at the calendar date at which the "compartment" wall is located, and the depth, given by the number of units of the commodity sold at this price and date.[51] The problem now is to determine the *width* of the

[49] Again it may be observed that in this respect I have very respectable models to follow. See again, for example, the references to Fisher and Pigou given above, p. 478, n. 34; and cf. also the quotation from a letter from A. W. Flux given by Marshall in his article "Distribution and Exchange," *Economic Journal*, VIII (1898), 41 (*Memorials of Alfred Marshall*, 315), as well as the comment on certain passages in Edgeworth's *Mathematical Psychics* (cf. pp. 4 ff. of that work) in J. Åkerman, *Om det ekonomiska Livets Rytmik*, 172 n. The use of analogies from hydrodynamics is naturally found most frequently in those writers who, like Simon Newcomb, made much of the distinction between a "fund" and a "flow" (see, for example, Newcomb's *Principles*, 316); and it is particularly worthy of note that in such cases the analogy was extended specifically to the *monetary* flow, or, as Newcomb called it, the "flow of the currency," which was in turn related to the amount of money in the "reservoirs" represented by cash balances (see Newcomb's *Principles*, 317, 319, and cf. Irving Fisher, "What is Capital?" *Economic Journal*, VI [1896], 526).

[50] Cf. the conception, presented by G. C. Evans in his *Mathematical Introduction to Economics*, 168 ff., of "the economic system as divided into various compartments," the problem being "to discuss the flow of commodities or services through these various compartments," and particularly "the analysis of the compartments relating to money," in terms of the variables of the Fisher equation.

[51] The very fact that we are here interested in representing the effect of changes in the stream of money expenditure upon the structure of *prices* and *quantities sold* at these prices means, of course, that another type of provision must be made for those monetary transactions which are incapable of resolution into "the sale of a specifiable volume of goods at a specifiable price" (cf. Volume I, 57 f., 60 f., 572 ff., 589). By way of making such provision, let an additional area be set aside for such transactions behind the series of compartments whose height and depth dimensions are given in the way indicated in the text. On this area, as on the remaining area, "compartments" will be set up by the flow of money payments. The peculiarity of these "compartments," however, is that we are no longer interested in the relation between their height and depth, respectively. Let us, therefore, assign an arbitrary and constant depth to the area allotted to the new compartments. With the depth thus held constant, all variations in the total volume of money transactions not resolvable into "the sale of a specifiable volume of goods at a specifiable price" will be registered by changes in the *height* of the compartment. The extent to which varia-

compartment, which is measured along a scale of clock time. *This is given by what Wicksell called the "resting time [Ruhezeit] of money"*— that is, the length of clock time money "rests" between its receipt and its subsequent disbursement.[52] Clearly, this "resting time" of money is the reciprocal of Fisherine "velocity."[53] It is therefore equal in magnitude to the K of our formulation $M = K(PT)$.[54] And what this means is that the speed at which money flows on its way to the realiza-

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tions in these compartments due to changes in their height will take water away from the compartments in which we are primarily interested (cf. Volume I, 582 ff., and see what is said below, pp. 494 ff., with respect to the treatment of "income" and "non-income" payments, respectively), will then depend upon the *width* of these compartments. Since these transactions, like those which *are* resolvable into "the sale of a specifiable volume of goods at a specifiable price" are *dated in terms of clock time,* the location of the compartment wall in question (the width of the compartment) will be given by this dating. In terms of economics, of course, it will be given by the facts with respect to the cash-balance administration of those who find it necessary to make the payments in question. See the remainder of the paragraph in the text.

[52] See Wicksell, *Interest and Prices,* 52 ff., and *Lectures,* II, 60. (The word *"Ruhezeit"* is rendered as "interval of rest" in the English translation of the *Geldzins und Güterpreise;* as "period of idleness" in the English version of the *Vorlesungen;* and, quaintly enough, as the "time of recuperation" by Lambert, *La Théorie quantitative de la Monnaie,* 121—though Lambert gives no specific reference to Wicksell in this connection.) For further examples of a use of the term *Ruhezeit,* see J. F. Feilen, *Die Umlaufsgeschwindigkeit des Geldes* (1923), 77, 99 ff., 121 (though the issues involved are very badly treated by Feilen himself), and H. Neisser, *Der Tauschwert des Geldes* (1928), 15. Wicksell's "resting time" is of course in all essentials equivalent to Newcomb's "number of days which money remains in one man's hands" (see Newcomb's *Principles,* 321, 347), and to Fisher's "time of turnover" of money (see Fisher's *Purchasing Power of Money,* 354, 363). It should be clear also that the Robertson-Pigou concept of a "period of circulation" of money (see Robertson, *Banking Policy and the Price Level,* 47, and Pigou, *Industrial Fluctuations,* 136 ff., 152), if it were divested of its relation to *"income* payments" and made to refer to payments in all types of transactions, would be mathematically equivalent to Wicksell's "resting time." (On the treatment of "income payments" in models of the general type here described, see below, pp. 494.

[53] Cf. Wicksell, *Interest and Prices,* 53, and his *Lectures,* II, 60; Newcomb, *Principles,* 321; Fisher, *Purchasing Power of Money,* 354; Neisser, *Der Tauschwert des Geldes,* 15; and (after allowance for the differences due to the fact that the "period of circulation" was designed to refer only to *"income* payments") Robertson, *Banking Policy and the Price Level,* 47 n., and Pigou, *Industrial Fluctuations,* 152.

[54] On the expression $K = 1/V$, see Volume I, 416 f., of the present work; and cf. Pigou, *Industrial Fluctuations,* 136, where it is pointed out that the "circulating period" of money, as Pigou defines the term, "will obviously constitute a fraction of the year equal to the fraction of their real income that people choose to hold in money form"—that is, will be equal to the "Marshallian K" in one of its variants.

tion of money prices will be accounted for by the whole of that, apparatus for explaining the size of cash balances relative to outlay which is provided by the "cash balance approach." [55]

6. It is easy to translate these results into the variables of our Quantity Equations. Every one of these equations, when properly stated, refers to a given period of clock time.[56] If, therefore, it is desired to represent the values of the variables in our Quantity Equations in the form of time series, we must regard each period of clock time within which prices are realized as having its own equation of exchange.[57] When this is done, it is seen that the values for T, for example, will be given by our time series of quantities sold at given points in clock time.[58] The values for P, likewise, will be given by our time series for prices realized at given points in clock time. Since the M of our equation is the average of cash balances held over the period in question, it

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[55] See Volume I, 417 ff. On the rôle of "velocity," when properly defined and properly related to the "cash balance approach," in establishing the relation in time and magnitude between money expenditure and money receipts, see Hawtrey, "Mr. Robertson on 'Saving and Hoarding,'" *Economic Journal*, XLIII (1933), 702, as well as the references to Hawtrey given in Volume I, 354, n. 21, of the present work; and cf. the type of formulation presented in Volume I of the present work, p. 382, n. 85, and p. 383, n. 88.

[56] On this matter, see Volume I of the present work, p. 65, and the references given in n. 69 thereto.

[57] When, of course, the magnitudes (other than M) attained in each of these clock-time periods are *summed*, we obtain an "equation of exchange" for the clock-time period most commonly used to illustrate the equation of exchange—namely, a *year*. See, for example, Fisher, *The Purchasing Power of Money*, 16 ff. It should be equally obvious, however, that the longer the period over which the magnitudes thus involved are summed, the more is obscured with respect to the actual sequence, in clock time, in the variations of the particular prices and other quantities involved. On the treatment of M, the successive values for which, over any given period covered by a single equation of exchange, are to be *averaged*, rather than *summed*, see below, p. 488, n. 59.

[58] Cf. above, p. 481, n. 39, and the backward references there given. When we are dealing with transactions incapable of resolution into "the sale of a specifiable volume of goods at a specifiable price," there will of course be no term for either T or P at the clock-date indicated: there will be merely a single term (the equivalent of Lubbock's E or Copeland's R) which will correspond to the *product PT*. See what is said on this matter above, p. 485, n. 51. When, however, we *are* dealing with "the sale of a specifiable volume of goods at a specifiable price," there is no difficulty in breaking down the "volume of goods" (the T_g of our formulation) into its components, and representing these *components* in the form of a time series; for these components are themselves always dated in terms of clock time. See, for example, my articles, "The Statistical Measurement of the 'Velocity of Circulation of Goods,'" *Quarterly Journal of Economics*, XLVII (1932), 22 f., 27, and "The 'Rate of Sale' and the 'Velocity of Circulation of Goods': a Comment," *loc. cit.*, 454 f.

corresponds to the average amount of water found within our compart-
ments over the period of clock time taken for examination.[59]   A time
series for M, accordingly, is obtained by recording the figures for the
stock of money held as cash balances at a series of calendar dates chosen
in such a way as to show significant movements in the monetary stock.[60]
Since V is the reciprocal of the "resting time of money," it will corre-
spond to the *number of compartments* into which a given time period
may be found to be divided by our "planes," and through which money
passes during this time period.[61]   As a matter of statistical measure-

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[59] On the cash balances involved as *average* balances, see Volume I,
426, of the present work, and the references given in n. 34 thereto.   The
amount of cash held over any *one* interval between the receipt of cash and
its disbursement is of course given by our formula for the "absolute" size
of the cash balance: namely, $M = kPT$ (cf. Volume I, 445, n. 86).   In
terms of our mechanical model, this amount of cash is represented by the
amount of water in any one compartment; and since the dimensions of
each compartment are given by k (width), P (height), and T (depth), it
is clear that the cubic measure of this amount of water (M) is given by
the product kPT.

[60] From this description of the procedure involved, it is clear that
significant changes in the absolute volume of cash balances held over a
given period need not necessarily be obscured by the mere fact that the
relevant cash balances are *average* cash balances.   For all that is necessary,
in order to reveal these significant changes, is to construct equations of
exchange for each of the sub-periods which are held to evidence these
significant changes, and to obtain an average of the cash balances held
within each of these sub-periods.

[61] Thus, if the resting time (K) of a given part of the money stock is
one month, it is clear that there are in one year twelve compartments, each
having the width 1/12 of a year.   Since $K = 1/12$, it follows that V, the
reciprocal of K, or 12, will be equal to the number of compartments, whose
vertical walls mark the successive spendings of the part of the money stock
indicated.   Since there is no reason to suppose that the "resting time" of
all parts of the money stock will be the same, or that the successive
"resting times" of even the same part of the money stock will be equal, it
has been usual for sponsors of the concept of a "resting time of money"
to speak of an *average* "resting time."   See, for example, the references to
Wicksell, Newcomb, Fisher, Robertson, and Neisser given above, p. 486, n.
52.   Again, however, there is nothing in this fact which would justify the
conclusion that all such averages necessarily obscure important differences in
the "resting time" either of different parts of the monetary stock, or of the
same part of the monetary stock in successive "resting times."   Any reason-
ably sophisticated monetary theory would be prepared, for example, to
divide the total monetary stock into different sub-groups, upon the basis of
considerations indicated by an adequate understanding of the issues raised
by the cash balance approach, on the one hand, and by the theory of the
generation of money income, on the other (cf. Volume I of the present
work, pp. 147, n. 9; 320 ff.; 324 f.; 404 ff., 463, n. 10); and there is no reason
why the computation of an "average resting time" (or, what comes to the
same thing, the average ratio to outlay of the balances held against that
outlay) should necessarily go beyond an "average" *for each sub-group* of

ment, the movements of V, in the sense of a time series showing the value of V over each clock-time period taken as a unit, are obtained in the conventional way, by dividing the PT for each of these clock-time periods by the average M for that period.[62]

7. The use of the "machine" just pictured to describe the "stationary flow" of economic life is obviously a matter of great simplicity. In a condition of stationary flow, nothing changes within the internal structure of the machine: the location of the "imagined" intersecting supply and demand curves and the dimensions of the compartments established by the realization of actual transactions remain as before.[63] The "flows" involved would, of course, still be flows occurring in "clock" time; for it is still clock time that is measured along the width axis of our "machine."[64] What remains "stationary" is merely the *form* of the system

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cash balances. Similarly, the fact that successive "resting times" may be of different lengths is merely an argument for taking, as a time period over which these "resting times" are averaged, a period in which the "resting times" are reasonably homogeneous.

[62] Cf. Fisher, *Purchasing Power of Money,* 282 ff.

[63] On the compatibility of *some* of the changes thus excluded from the particular description of the "stationary flow" thus indicated, with *other* conceptions of a "stationary" state, see what is said above, p. 431, n. 48. At all events, in the light of what is said below under (8) with respect to the possibility of using the "machine" here described for the representation of "dynamic" changes, it should be clear that these differences with respect to the degree of "stationariness" assumed in the "stationary flow" are all capable of representation by the apparatus here indicated. See also what is said on this matter below, p. 490, n. 65.

[64] Cf. what is said on this matter above, pp. 479 ff. It may be observed also that the apparatus here described brings out quite clearly the significance of the familiar proposition that, in dealing with the "supply" and "demand" of particular commodities in terms of Marshallian supply and demand schedules, "in reality, supply should be taken to mean not supply absolutely, but rate of supply," and that "before we can form any judgment about the statement" that "a town consumes fifty gallons of water," we "must know whether it is consumed in a day, or a week, or a month" (Jevons, *Theory of Political Economy,* 64 f.; cf. Cournot's *Researches,* 47, 51 f., and Marshall, *Money, Credit, and Commerce,* 282). If, for example, we assume (1) that a given pair of supply and demand schedules remain unchanged in conformation and position over the period of a *year;* and (2) that the interval of clock time elapsing between each realized action of "supply" and "demand" remains unchanged over the year; then it is quite clear that the "supply" and "demand" involved can be stated either in terms of supply and demand per interval of clock time between successive transactions, or in terms of "supply" and "demand" over the period of a year—the procedure in the latter case being simply the summation of the magnitudes involved over the period of a year, as given by our time-series of T (the q's) and of "money demand" (the realized D's [$= pq$'s] or MV's). It should be equally clear, however, that in those cases, envisaged by Cournot (*Researches,* 52), in which the "law of demand may . . . vary" over the period of the year, it is not necessary, in order to retain the concept of a "rate" of demand and supply, to follow Cournot's suggestion that

of compartments, flows, prices, and quantities.[65]   One could, therefore, pipe water that has once run its course back to the beginning of the clock-time scale, and merely change the calendar dates indicated in the scale, to show that the process, while "stationary" in the sense that its *form* remains unchanged, does move forward in clock time.[66]   In all other respects the process reproduced by the "machine" would be the same each time.

8. The really interesting uses of the "machine," however, are provided when we pass to a representation of the kind of change typical of a "dynamic" process.   It is clear, for example, that the depiction of the stages involved in a process of monetary expansion induced by an increase in the quantity of bank money would begin with the introduction of a greater flow of water into a given "compartment" (the first wall of this compartment being located by the point of impact of the first expenditure of the new money upon the price structure), and would

we consider "the curve which represents the function F to be in itself an average of all the curves which would represent this function at different times of the year."   For it will still be true that the interval between any two acts of "supply" and "demand" will be measured in terms of clock time.   This interval of clock time, clearly, will give us all that is required in order to represent demand and supply *per unit of time;* but it still remains for us to proceed, if we wish, to obtain either an *average* of "rates" of "supply" and "demand" over the whole period taken for examination (with the accompanying Cournotian implications with respect to the "average" conformation and position of "all the curves" representing the demand and supply functions "at different times of the year") or to state each individual "rate" of "supply" and "demand" *in annual terms* by multiplying the amount "demanded" and "supplied" in any given interval by a coefficient equal to the number of times the time-interval in question is contained within a year.

[65] Again it should be observed (cf. above, p. 489, n. 63) that it would be a simple matter to represent those types of "stationary flow" in which less rigorous conditions of "stationariness" were assumed.   If, for example, it were assumed that, although the *conformation* of both supply and demand curves, respectively, remained unchanged, their *positions* might shift in such a way that prices would remain unchanged, but the *quantities sold* at these prices would increase, we should have a continuous increase in the *depth* of each compartment, with the height and width remaining unchanged.   It would be very easy to provide a long list of similar illustrations—including, for example, the case in which, with the decrease in selling prices made possible by a successive lowering of supply curves, the cubic content of each compartment (and therefore the amount of money needed to finance an increased volume of goods sold) would remain unchanged despite a continuous increase in the *depth* of each compartment.

[66] On the relation of a "circular" flow, of the type indicated, to forward movement in clock time, cf. what is said above, pp. 113, 419.   The "piping back" of the water, as described in the text, which would mean that the water would pass through "tubes bent round in a circle and closed" (cf. Pigou, *Theory of Unemployment,* 199) would, of course, picture a "closed" system, in one sense of the latter term.   See, however, what is said on this matter above, pp. 420 f., and especially nn. 29 and 31 thereto.

then trace the flow into and out of later compartments.[67] Similarly, the depiction of a process of monetary contraction induced by a decrease in the quantity of bank money would begin with the loss of a given quantity of water through one of the compartments (this "lost" water being prevented from reappearing within any compartment until the process of contraction is reversed), and would then trace the effects of this loss of water upon the flows into and out of later compartments.[68]

For the sake of convenience, we may confine ourselves to the effects of an *increase* in the quantity of money. We begin at the point indicated above: an additional quantity of water (money) has entered one of the compartments, as the result of a payment of the "new" money to a firm or individual.[69] Our problem, first of all, is to estab-

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[67] Again respectable precedents are to be found for this type of analogy. See, for example, Pigou, *Theory of Unemployment,* 199 ff., and Fisher, *Mathematical Investigations in the Theory of Value and Prices,* 28, 45 ff. It will be observed also that the very fact that the "compartments" in our apparatus are related to the prices paid for *particular commodities* makes it possible to represent in all desired detail the effect of the additional money expenditure upon the price *structure,* at the same time that full room is left for the tracing of the effect of these payments upon the generation of money *income* and its distribution. (On the latter point, see below, pp. 494 ff.) In this connection, cf. the comments of Fisher, *Investigations,* 45 ff., on the "effect on various commodities," as well as the effect on the incomes of "different individuals," of an injection of new money which is not equally distributed among all the individuals in the "system." On the treatment of monetary transactions which, not being resolvable into "the sale of a specifiable volume of goods at a specifiable price," cannot be regarded as affecting the *price* structure *directly,* see above, p. 485, n. 51.

[68] It may be observed that the "loss" of water here envisaged is to be regarded as occurring only when money is actually "destroyed" (as in the case of the "destruction" of bank money through the calling of bank loans), or at best (if the apparatus is intended to describe the functioning of the monetary system of a single country having trade relations with other countries), when money is actually *exported.* That is, in contrast with Professor Pigou's model, the "destruction" of money is not to be regarded as occurring when money "is hoarded as savings deposits or in stockings" (*The Theory of Unemployment,* 200). On the analytical issues involved, see Volume I of the present work, 459 ff., and 390 ff. This type of comment is worth making, if for no other reason, because it illustrates again the point that the construction of "machines" of the type here described, instead of resting upon a "mechanical" avoidance of genuine problems of economic analysis, presupposes an adequate grasp of these problems at every step of the way. See also nn. 69 and 70, immediately following, and also below, p. 497, n. 86.

[69] It should hardly be necessary to labor the point that the *explanation* of the increase in the quantity of money thus assumed for purposes of the present argument, instead of being evaded, is to be regarded as provided by the whole of our theory with respect to the causes of changes in the quantity of money, including (particularly in the case of *bank* money) the effect of the rate of interest (cf. Volume I, 154 ff., 158 ff.). Precisely the same thing is to be said with respect to the explanation of the fact

lish what changes, if any, may be expected to occur with respect to the *width* of the compartment which the water has entered: in other words, to establish the length of time this new money will remain unspent. And again the answer to this question is given by the analytical apparatus provided by the "cash balance approach." [70]

It is easy to see, moreover, how changes in the *width* of these compartments will be related to changes in their other dimensions. If, for example, there is no change in the width of a given compartment as a result of an increase in the amount of water (money) within the compartment, there must be a change in either the height or depth of that compartment, or in both.[71] Or, in terms of economics, we may say that there must be a change in either the p's or the q's of the price system, or in both. But since the location of a given p and q is given by the position and conformation of the relevant market demand and supply schedules, this is equivalent to saying that the answer to the

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that the initial expenditure of the new money occurs at one point in the price- and income-structure rather than at another (cf. Volume I, 501 ff.). The discussion in the text, in other words, is designed only to show how, *given* an initial increase in the quantity of money, its diffusion throughout the system is to be represented by the apparatus here described.

[70] See above, p. 487, n. 55. The use of the "cash balance approach" to account for changes, or a lack of change, in the rate at which money received is subsequently spent, obviously applies as directly to the case represented by the picture of a "stationary flow" of economic life, described under (7), as it does to the "dynamic" case here envisaged. That Walras, for example, was aware of this is evidenced by the fact that he introduced the concept of an *"encaisse désirée"* into his picture of the stationary "circular flow." It should be equally clear, however, that the very fact that he did introduce it there shows how easy is the transition from a picture of the *stationary* "circular flow" which would arbitrarily exclude a consideration of the particular "workings of monetary circulation" associated with "changes in money holdings" relative to outlay (cf. for example, E. H. Phelps Brown, *The Framework of the Pricing System* [1936], pp. vi, 14, 47, 95, 124, 129 ff., 135 f., 138, 190 ff.), to the "dynamic" case in which such changes are admitted as a possibility (cf., in this connection, my article, "The Monetary Aspects of the Walrasian System," *loc. cit.*, 186). There is, therefore, considerable justification for the suggestion that a study of the "velocity" with which the flows of money payments can be shown to "connect the reactions of the entrepreneurs, consumers, etc., ought to be the basis for making dynamic a static system" (so Lundberg, *Studies in the Theory of Economic Expansion*, 117; cf., likewise, J. Åkerman, *Ekonomisk Kausalitet*, 71)—though it is clear also that no attempt to "trace the flow of money payments" in the "dynamic" case by studying the factor of "velocity" could hope to be completely successful if it were to use only concepts such as that of "income velocity," or of an "average income period" (cf. Lundberg, *op. cit.*, 118, 125 ff.), or were based upon an entirely inadequate understanding of the implications of the cash balance approach (see, for example, the quotation from J. Tinbergen given in Lundberg, *op. cit.*, 132 n.).

[71] Again this follows from the expression $M = kPT$, in which M represents the amount of "water" in a given compartment, k represents the width of the compartment, and P and T the height and depth, respectively.

question whether the p's or the q's or both will change, and in what
degree, will be given by the whole of that part of our analytical equip-
ment, as represented by both monetary theory and the "general" Theory
of Value, which tells us why the conformation and position of demand
and supply schedules are what they are.[72] What we do know, from
the condition that the width of the compartment is assumed to remain
unchanged, is that the increased amount of water must force a change
in some other dimension of the compartment; that is to say, we do
know that there has been a change in the dimensions of "money demand"
(in one sense of the latter term) and that this must have an effect
somewhere on the structure of prices and quantities.[73]

Every time a new price is realized, moreover, water will flow into
another compartment; and this means that we must now trace the
possibilities of a change in the dimensions of the new compartment.
Considerations suggested by the cash balance approach may tell us,
for example, that the *width* of the compartment in question may be
increased.[74] Indeed, it may even be increased by an amount such as
to force a reduction in the height, or depth, or both, of the new com-
partment, below the height and depth of the typical compartment estab-
lished before the new money was introduced into the system; that is
to say, we may have a decline in both prices and quantities sold as the
result of the reduction in money demand that has come from a failure
to maintain the rate at which money received is being spent. What
will happen in the concrete case can be determined only by the com-
bined use of an *analytical* equipment, on the one hand, which is capable
of describing (1) all things that are likely to happen, (2) under what
conditions they are likely to happen, and (3) *why* they are likely to
happen under those conditions; and, on the other hand, of empirical
investigation designed to establish which of the several possibilities
analysis shows *might* be realized were in fact realized in a given his-
torical period.[75] Our "machine" is capable of reproducing any "model
sequence" that may be set up; it will give an accurate picture of the
functioning of the system whenever sufficient data are provided with

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[72] See above, pp. 274 ff.; also what is said above, pp. 274 ff., with respect
to the effect of changes in production functions, and on pp. 469 f., with
respect to the effect of such elements as monopoly upon the rigidity of
supply schedules. It is of considerable importance to observe that supply
schedules, in the ordinary sense of the term, *are* involved in the determina-
tion of the degree of response of the q's to a given change in "money de-
mand." In this connection, cf. what is said below, pp. 527 ff., with respect
to the concept of an aggregate supply function as used in Keynes's *General
Theory*.

[73] Again it should be observed that full allowance is made for changes
in *quantities sold*, as well as for changes in prices. See above, p. 481, n. 38.

[74] This case would occur whenever the effect of the injection of new
money is counteracted, or more than counteracted, by an "increase in
liquidity preference" (to use the term made fashionable in recent years).

[75] In this connection, see what is said below, pp. 507 ff., with respect
to the rôle of *statistics* in accounting for the economic processes observed
in the world we know.

respect to the particular conditions present—just as the device of a "model sequence" will tell us what will happen only if we have sufficient information with respect to the particular conditions assumed. To ask of either our "machine" or any given "model sequence" that it provide a single description capable of application to all concrete instances is to ask more of either device than any one has a right to ask of devices intended for application to a world evidencing processes of change that cannot be assumed in advance to be identical in all historical cases.[76]

9. The "machine" described above provides an *accurate* picture of the system of money flows which we observe in the world about us. A picture may be accurate, however, as far as it goes, without being detailed enough to show clearly just what happens in certain parts of the economic process in which we may be particularly interested. It may be of some interest, therefore, to show how more elaborate "machines" could be constructed, on the same general model, of such a kind as to bring out more clearly the essential features of aspects of the economic process other than those represented thus far.

The "machine" described thus far is capable, for example, of representing the flow of *aggregate* money expenditure, and the distribution of this aggregate money expenditure in the purchase of particular commodities at particular prices. And if, as has sometimes been implied, all money expenditure were expenditure out of or into someone's *income,* our model would serve perfectly satisfactorily for the representation of the processes involved in the generation of money income.[77] For it would then be literally true that all changes in the level of money incomes would be due either to changes in the quantity of money (the amount of water in our compartments) or in the "resting time," and therefore in the Fisherine "velocity of circulation," of money (the width of the compartments in our "machine," and therefore the number of such compartments over a given length of clock time).

Actually, however, not all money expenditure is expenditure into or out of income. On the contrary, by far the greater amount of money payments effected in a given period of clock time neither enters into income directly, nor is paid out of income directly, within that period. The reason for this is the occurrence of what have been loosely referred

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[76] This, after all, is the answer to those who would suggest that the analytical system here outlined is so very "general" as to be virtually useless in analyzing the processes of economic life (cf. above, p. 436, n. 63, and what is said below, pp. 515 ff.). The important thing to be observed here is that the "machine" here described is as capable of representing a system of "numerous forces which are constantly changing, changing at different rates, and influencing one another as they change," as it is capable of describing a system operating under "static" (stationary?) conditions (cf. the reference to W. C. Mitchell given above, p. 478, n. 34).

[77] On the difference between "expenditure" and expenditure into *income,* see Volume I, 383, of the present work; and see also below, pp. 699 ff,

to as "intermediate transactions." [78] It is, indeed, a matter of comparative ease to show that, other things being equal, the amount of money income generated within a given clock-time period will vary inversely with the amount of these "intermediate transactions." [79] Our problem, therefore, is to devise improvements of detail in our "machine" which will represent this type of effect.

Let a machine be constructed on exactly the same lines as those indicated above, except that the compartments are now intended to receive only payments into *income*. Now let another level be added to this machine, the compartments in this upper level being intended to receive only payments that are *not* payments into income.[80] Assume, further, that whenever a money payment made by an income recipient does not enter directly into someone else's income (but *does* enter, say, into what Mr. Hawtrey has called "traders' turnover"), a chemical change takes place in the liquid representing the money used to effect such a payment, and that this chemical change reduces the specific gravity of the liquid.[81] Assume, also, that a contrary chemical change takes place whenever a "trader" makes a payment out of his "turnover" directly into someone's income.

In such a machine, it is clear that the lower level alone will picture the flow of money into money income. Yet it is obvious that our improved machine also gives a picture of the process whereby the size of this flow is affected by changes in the volume of "intermediate transactions." Suppose, for example, that the new money whose injection into the system was assumed under (8) is paid entirely into the hands of a businessman borrowing the funds from the bank. This will be

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[78] The problem of the treatment of "intermediate transactions" has been discussed for generations—though the discussion can hardly be said to have maintained a uniform level of excellence. For references to the earlier literature, see Volume I, 314, nn. 36 and 37; 326, n. 73; 385, n. 91; 488, n. 9; 593 f., nn. 48 and 49.

[79] See Volume I, 383 f., and 385, n. 91, of the present work.

[80] Cf. Pigou, *Theory of Unemployment,* 199 ff., where, although the mechanical model presented differs in important respects from the one here described, it is also proposed to differentiate payments into income from other money payments by assuming that money becomes income only when it passes through a horizontal "income plane" which represents only one sector of the tubes designed to represent the *totality* of money payments. See also the following note.

[81] In the model described by Professor Pigou, any increase in the amount of "intermediate transactions" is represented by a lengthening of the tubes of his apparatus—the effect of this lengthening being to delay the return of the water in these tubes to the "income plane" (*Theory of Unemployment,* 199). It should be clear, however, that the same idea underlies the two constructions: namely, the representation of a differentiation of "income payments" from "non-income payments" by the use of different *planes,* one of which is the "income plane." On the concept of "traders' turnover," see Volume I, 317, nn. 43 and 44, and 340, n. 114, and the references there given.

represented by an increase in the amount of liquid in a given compart-ment of the *upper* level of our "machine": thus far, no money income has been generated.[82] Suppose, now, that the businessman uses half of his newly acquired money to pay wages, and the other half to pay other businessmen for the raw materials which they sell to him from their stocks. There will be an increase in the specific gravity of that part of the liquid which represents the wage payments and the profits made on the transaction by the sellers of the raw materials. This part of the liquid will pass into the lower level; the rest will remain on the upper level. What happens to the amount of liquid in the lower level thereafter will be the result not only of the factors discussed under (8), but also of the relative amounts of payments into income, on the one hand, and into other money receipts, on the other, both by income recipients and by "traders," over the period of clock time for which the process of income generation is being traced.[83]

It is clear that this model is perfectly capable of representing that aspect of the "circular flow" of money payments which is concerned with the interrelations, in time, of payments made by entrepreneurs to the factors of production, and by the latter to the entrepreneurs in return for the products of industry.[84] Since, however, it is character-istic of our new model, as it was of our first model, that all money payments are dated in terms of clock time, it is clear that it can do much more than has often been done by writers who have made use of the general concepts of "interrelation" and the "circular flow." In many cases, these writers have merely stated that there *are* such things as "interrelation" and a "circular flow," and have made no reference to the amount of clock time involved in the process whereby this "inter-

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[82] In terms of our algebraic notation, the "payment" of the bank to the entrepreneur will be represented by $(PT)_{NI}$; or, more accurately (since the payment is *not* resolvable into "the sale of a specifiable volume of goods at a specifiable price"), by E_{NI}. From the description given above, p. 485, n. 51, of the manner in which such payments are to be represented, it fol-lows that the "upper level" to which reference is made in the text is the "upper level" of the compartments in the rear of those designed to represent "the sale of a specifiable volume of goods at a specifiable price."

[83] See Volume I, 369 f., and 383 f., of the present work.

[84] What this means, of course, is that our model is perfectly capable of representing the "Walrasian" "circular flow" in terms which not only relate each step in the process to definite points in clock time, but also are entirely free from any limitations that might be supposed to attach to the Walrasian picture when it is confined to the representation of "equi-librium" or "stationary" conditions. In this connection, see what is said above, pp. 361 ff., 429 ff. On the Walrasian "system" as a picture of a "circular flow" in the sense of a description of the processes whereby entre-preneurs make payments to the factors of production, and the latter, in turn, expend their receipts in purchase of the entrepreneurs' products, see above, p. 358, n. 22; and cf. the description of what amounts to a simple version of the Walrasian "system" which is given by E. H. Phelps Brown, *The Framework of the Pricing System,* 133 ff., 142 ff.

related" "circular flow" brings it about that money expended out of income will later be found to re-enter into income.[85]   Our own model, on the contrary, can reproduce with all desired accuracy the processes in time by which money expended by income recipients may be prevented from entering immediately into other incomes, and can bring out with considerable clarity the reasons why it takes as long as it does to complete the "circuit" before it *does* re-enter into income.[86]

One factor, for example, will be the width of the compartments in the upper level of our machine; and this will be explained by the details of cash balance analysis, which tell us why the Fisherine V is as large as it is.   Another factor will be the changing specific gravity of the liquid used; and this will be explained by certain parts of the analysis summarized by the T of our Quantity Equation.[87]   In all cases, it will be observed, the variables involved are dated in clock time, and therefore are capable of representation in the form of time series.   The same thing may be said of the figure for "income velocity" (in the sense of the number of times money *enters into money income*) obtained by counting the number of compartments in the lower level through which liquid passes over a given length of clock time; just as a figure for Fisherine velocity will be obtained by counting the number of compartments through which liquid has passed on *both* the upper and the lower levels over a given stretch of clock time.[88]   It is to be observed

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[85] The latter purpose is, of course, the essential one underlying the Pigovian construction to which reference has several times been made.   It will be observed, however, that while our model is perfectly capable of representing the type of process which Professor Pigou's model was designed to represent, it is also capable of representing the effect of the flow of payments upon the structure of *prices* and *quantities sold*, and indeed the whole pricing process generally—matters with which Professor Pigou's model does not pretend to deal.   See also the following note.

[86] In this respect, also, the model here described goes considerably beyond the Pigovian model.   For in the latter, changes in the processes referred to in the text are represented simply by changes in the length of the tubes, without differentiating the case in which such a lengthening is due to changes in the size of cash balances held, relative to outlay, from the case in which it is due to a change in the amount of "intermediate transactions"; whereas in our model the two types of change are sharply differentiated.   The point involved, as so often (cf. above, p. 491, n. 68), shows how essential, in the construction of a mechanical model purporting to represent economic processes, is an adequate appreciation of the analytical issues involved.   On the issues themselves, see Volume I, 368 ff.

[87] Cf. Volume I, 538–600, of the present work.

[88] On the representation of Fisherine "velocity," in the apparatus here described, see what is said above, p. 488, n. 61.   The definition of "income velocity" as the "number of times money enters into money income" is, of course, only one of the many definitions that have been given to "income velocity" (cf. Volume I, 378 ff.).   It would be very easy, however, to show that these other definitions could likewise be represented by our model.   Such representation, indeed, would emphasize a fact of which too many users of some particular variant of the concept of "income velocity"

also, however, that in all cases our "machine" pretends to do no more than provide a picture of the mechanisms involved in *realized processes*, whose ultimate *explanation* involves the use of the whole of the relevant sectors of monetary and "general" economic theory, including those parts of that theory which were built up originally as part of a protest against an excessively "mechanical" view of the functioning of the economic process.

III

THE RÔLES OF "MICROECONOMIC" AND "MACROECONOMIC" ANALYSIS

The *terms* "microeconomic" and "macroeconomic" seem to have been introduced into economic literature only recently.[89] The *issues* involved in the relation between the two types of analysis, however, are very old: indeed, as has recently been suggested, it is possible to write a good part of the history of economic thought in terms of a shifting emphasis upon the one or the other type of analysis.[90] As

have seemed to be unaware: namely, that many of the definitions of that concept which have commonly been regarded as identical are by no means necessarily identical. I prefer, however, to point out again that a careful description of the steps involved in the process of the generation and utilization of money income, such as is provided by the analytical apparatus presented in this work and is pictured by the "machine" described above, in a fundamental sense makes it possible to *dispense* with the concept of "income velocity": whereas the use of the latter concept does *not* make it possible to "dispense" with a careful description of the processes involved. One can say this and still object to the suggestion that the position taken in this work with respect to the concept of income velocity is one of outright "rejection." See what is said on this matter above, p. 476, n. 30.

[89] For examples, see the references to Lindahl given above, p. 342, n. 63, and Haberler, *Prosperity and Depression*, 248. Just who was responsible for the introduction of these terms, I do not know. On the other hand, the terms "micro-dynamic" and "macro-dynamic," as applied to economic analysis, seem to have been introduced by Professor Frisch. See his "Propagation Problems and Impulse Problems in Dynamic Economics," *loc. cit.*, 172 ff.; and cf. also J. Åkerman, *Das Problem der sozialökonomischen Synthese*, 181, 213 ("micro-dynamics" and "micro-statics"), and 85, 95 (the "economic microcosm and macrocosm"); E. Petersen's *Macro-Dynamic Aspects of the Equation of Exchange* (1938); and Schumpeter, *Business Cycles*, 185.

[90] Cf., for example, the comment of J. Åkerman, *Das Problem der sozialökonomischen Synthese*, 85: "The step from the political economy of mercantilism to the economic theory of the nineteenth century was taken when there was a genuine recognition of the interdependence of the economic microcosm and [the economic] macrocosm."

so often, moreover, the net result of the controversies in-
volved has been to show that *both* types of analysis are
necessary, and that trouble comes only when the sponsors
of one type of analysis have pushed their claims for it so
far as to bring them to a position as viciously exclusivist
as that which they themselves attacked. It should be suf-
ficient, therefore, to indicate briefly the grounds for believing
that the analytical system here outlined is free from either
type of exclusivism.

The first great example of an insistence upon the neces-
sity for "microeconomic" analysis is represented by the con-
tention of the protagonists of the "modern" theory of value
that more attention must be paid to those calculations by
individuals which help to make market events what they
are.[91]   A revulsion against this emphasis, on the other hand,
was represented by the position of those, from the members
of the Historical School to the Institutionalists, who have
insisted upon the importance of studying the ways in which
institutions may be expected to condition these market

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[91] Any adequate list of the articulate defenders of this point of view
would have to give a distinguished place to the name of Francesco Ferrara.
See, for example, his preface (1873), to Volume Seven of the *Biblioteca
dell' Economista,* first series, pp. ci f. [*Oeuvres économiques choisies,* 61 f.],
where Ferrara proposed (as an alternative to the conventional division of
the study of economic phenomena into problems of "production, distribution,
and consumption") a method which, starting from "the actor and not the
act," would "consider man—the efficient cause of the economic phenomenon
—first in isolation, then as a member of an aggregate composed of several
men, and, finally, as a member of another aggregate, still more complex,
constituted by several groups of men"; would "consider the synthetic
phenomenon of economic activity in each of these stages"; and would form
"three systems," to which, "in order to preserve an established terminology,"
Ferrara proposed to give "different names: specifically, 'individual' eco-
nomics, 'social' economics, and 'international' economics"—Ferrara's thesis
being that "it is not possible to erect sound and solid theories in the domain
of International Economics without having solidly established in advance
the laws of Social Economics; and it is impossible to study the latter
unless one begins by recognizing all of them—more or less in embryo, but
still in *complete* embryo—in man, regarded as an individual." See also
p. civ f., of the same work (*Oeuvres économiques choisies,* 64), where
Ferrara argued that, if we have provided a complete description of the
"phenomenon of the individual," then "Social Economics cannot present
any principle which is radically new: everything will derive from the in-
dividual as the germ, and will come down to a substitution of forms ob-
viously and exclusively derived from the addition of several individual
elements."

events, by affecting the social ("macroeconomic") milieu
in which these events occur. It is clear, however, that
nothing need be added here to what was said above concern-
ing the mutually complementary relation between an em-
phasis upon the rôle of institutions, on the one hand, and
individuals' calculations, on the other.[92]

The second great example of a supposed clash between
the two types of emphasis is provided by the alleged con-
flict between "microeconomic" analysis of the "particular
equilibrium" type, on the one hand, and the "macroeco-
nomic" analysis of the "general equilibrium," or "system"
type, on the other. On the falsity of this antithesis within
the "general" Theory of Value itself, nothing need be added
to what has been said countless times in recent years; nor
need anything be added here to what was said above con-
cerning the rôle of both types of analysis in the analytical
apparatus here outlined.[93] If a comment is called for at
all, it is one of regret that the defenders of Mr. Keynes's
General Theory should have thought it necessary not only
to accept uncritically Mr. Keynes's implications with re-
spect to what "traditional" economic theory has had to say
concerning the relation between the two types of analysis,
but also to pass over without comment Mr. Keynes's own
rejection of weapons originally designed for "microeco-
nomic" analysis, such as demand schedules of the Mar-
shallian type, which are in fact indispensable if we are to
account for the processes unfolding in the world about us,
including processes involving movements in "Output and
Employment as a Whole." [94]

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[92] See above, pp. 462 ff.

[93] See above, pp. 408 ff.

[94] Typical of the implications to which reference is made in the text
is that contained in Mr. Keynes's suggestion that the "right" dichotomy
in the study of economics is that "between the Theory of the Individual
Industry or Firm and of the rewards and the distribution between different
uses of a *given* quantity of resources, on the one hand, and the Theory
of Output and Employment *as a whole,* on the other hand" (*General Theory,*
293). It may be added that Mr. Keynes's italicization is hardly calculated
to bring out the fact that, in the better statements of received economic
theory, the usage has been such as to make clear that, *within* the theory of
"the rewards and the distribution between uses of a *given* quantity of re-
sources," there has been not only a "Theory of the Individual Industry or

Within the field of monetary theory, on the other hand, the raising of false antitheses between "macroeconomic" analysis, on the one hand, and "microeconomic" analysis, on the other, was typified by the attack, in Keynes's *Treatise*, on Quantity Equations of the Fisherine type precisely on the ground that they are so macroscopic in their coverage as to make them unusable for detailed analysis of precisely those processes "in which we are likely to be interested." [95] As we have seen, however, Mr. Keynes's attack, in this case also, was invalidated for at least two reasons. The first of these was that the attack itself seriously underestimated the possibilities inherent in the use of a *system* of "partial" equations of the general Fisherine form; it failed, that is to say, to do justice to the possibilities inherent in a proper use of these equations for a more "microscopic" analysis.[96] The second reason for the failure of the attack of the *Treatise*, on the other hand, was its failure to appreciate the in-

Firm" (Cournot-Marshall), but also a Theory of the System as a Whole (Walras)—the theory of *variations* in Output and Employment as a Whole being represented by the whole of received doctrine with respect to the trade cycle, on the one hand, and the Theory of Economic Development, on the other. Unfortunately, the comments upon Mr. Keynes's work by those sympathetic to it have not done much to clear up the confusion involved. Cf., for example, Professor Hicks's characterization of the argument of the *General Theory* as "a theory of output in general *vis-à-vis* Marshall, who took into account many of the sort of complications which concern Mr. Keynes, but took them into account only with reference to a single industry" ("Mr. Keynes' Theory of Employment," *loc. cit.*, 238; on the fairness of the implication that Marshall's "system" did not contemplate any extension of the analysis beyond the problems of a "single industry," see what is said above, p. 75, n. 59). On Mr. Keynes's treatment, in the *General Theory*, of the problem of a "plurality of price levels" (or, if one wishes, the problem of the price-*structure*), and all that this should imply with respect to devices, such as the Cournot-Marshall demand and supply schedules for the products of "particular industries," see what is said above, pp. 155 ff., and also below, Chapter Ten; and for a discussion of the ways in which the *General Theory's* treatment of the *supply* side of the problem suffers from a failure to establish a proper relation between "microeconomic" and "macroeconomic" analysis, see below, pp. 539 ff.

[95] See Volume I, 484 ff., of the present work, and the references there given; and cf. what is said above, pp. 155 ff., concerning the differences between the arguments of the *Treatise* and of the *General Theory*, respectively, on the matter of a "plurality of price levels."

[96] It may again be pointed out that these "possibilities," instead of being a discovery of my own, had been envisaged by a number of writers of the highest standing. See above, p. 103, n. 32, and the references there given to Volume I of the present work.

adequacy of any analytical system which would push its
emphasis upon the necessity for the use of "partial" equa-
tions of this type to the point of ignoring the case for the
supplementary use of what has been called in this work an
equation of the "total transactions" type.[97]   In this second
instance, in other words, we have an example of a type of
exclusivism opposite to that which can be fairly charged
against the argument of the *General Theory*.   In the latter,
as we have seen, an enthusiasm for analysis of a more *macro*-
economic character has obscured the importance of prop-
erly conceived *micro*economic analysis as a necessary supple-
ment to analysis of the macroeconomic type; whereas, in
the case of the *Treatise*, an enthusiasm for analysis of a more
*micro*economic character obscured the importance of prop-
erly conceived *macro*economic analysis as a necessary sup-
plement to analysis of the microeconomic type.[98]

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[97] See what is said on this matter above, pp. 323 ff., and the references
to Volume I given above, p. 103, n. 32. It should be clear also that any
characterization of my argument in Volume I with respect to the need
for a "total transactions equation" as amounting to a *"preference* for 'trans-
actions' equations as against 'income' equations" (cf. P. B. Whale, in
Economica for February, 1940, p. 91) is entirely inaccurate, or at best ex-
tremely misleading. For what that argument contends is that the use of
"income" equations must be *supplemented by* the use of a "total transac-
tions" equation (see Volume I, 518 ff.). I not only *"appear* to agree that for
the purposes of monetary theory, a special importance attaches to those
particular money payments which represent income payments" (cf. *Eco-
nomica* for February, 1940, p. 92)—I have said as much in virtually those
very words. See Volume I, p. 364; and cf. also the use of the analogy of
the microscope on pp. 99 f. of that volume, and the forward references
given in nn. 59 and 60 thereto. It is, indeed, statements such as those of
Mr. Whale, cited above, that illustrate precisely the type of exclusivism with
respect to the relation of "microeconomic" and "macroeconomic" analysis
that I attacked in my Volume I and am attacking here.

[98] I am discussing here, of course, only those aspects of the argument
of the *Treatise* in which (as in the case of his insistence upon the necessity
for working with a "plurality of price levels") Mr. Keynes did show an
awareness of the necessity for analysis of a sufficiently "microeconomic"
character. Actually, however, the *Treatise* itself, superior as it is to the
General Theory in the particular respects indicated, showed anything but
an adequate appreciation of the need for adequately "microscopic" analysis.
See, for example, what is said 'in Volume I, 278, of the present work, and
in n. 26 thereto, with respect to the use of concepts such as "the global
amount of 'profits' in a community, or the 'average' amount of profits,"
in contrast with an insistence upon tracing the "effect of price movements,"
for example, "upon the profit position of *particular classes of entrepreneurs
and of particular types of transactions.*" It should be clear also that the
Treatise's insistence upon describing certain of the forces leading to changes

Nor can it be said that the recent swing to an emphasis upon the importance of the use of "macroeconomic" concepts, other than those involved in apparatus of the type symbolized by Fisherine Quantity Equations, has been in all cases an unmixed blessing, even in those instances in which an attempt has been made to *relate* these "macroeconomic" concepts to the conceptions of "microeconomic" analysis.[99] Within the field assigned to the "method of expectations," for example, it is anything but clear that genuine progress has come from the attempt to suggest, in Mr. Hawtrey's words, that "mental processes" of the kind envisaged by the "method of expectations" can "simply be added together like the items in a ledger." [100] There are cogent reasons, on the contrary, for believing that the way to be preferred is that followed in the apparatus here outlined, in which these "mental processes" are introduced only insofar as they can be shown to lead individuals to bring about certain *realized results,* which can *then* be "added together" precisely because these realized results do represent a net, quantitatively measurable social resultant of "expectations," after all allowance has been made for the loose quantitative aspects of the expectations themselves, their essentially contingent nature, and their possible mutual inconsistency.[101]

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in the level of prices and of output in terms of a divergence between Savings and Investment, without describing in all possible detail the *successive steps in the monetary processes by which such a divergence would be manifested,* itself represented an unjustified minimization of the importance of analysis of a sufficiently "microscopic" character.

[99] It should hardly be necessary to labor the point that reservations with respect to the amount of "blessing" provided by the recent emphasis upon the importance of dealing with "macroeconomic" concepts apply *a fortiori* in those cases in which *no* serious attempt has been made to relate these "macroeconomic" concepts to the concepts of "microeconomic" analyysis. A case in point is provided by Mr. Keynes's treatment of his concept of "effective demand." See, on the one hand, what is said above, pp. 203 ff., with respect to the relation of this concept to the "demand" of the general Theory of Value; and see also what is said below, pp. 693 ff., with respect to the relation of a concept such as the "elasticity of effective demand" to concepts (such as those indicated by the variables included in the more elaborate equations of the general "Fisherine" form) which permit a closer examination of the steps involved in the generation and utilization of money income.

[100] "Alternative Theories of the Rate of Interest," *loc. cit.,* 440.

[101] Cf. Hawtrey, *loc. cit.,* and also what is said above, p. 437, n. 69, with respect to the possibility of summing *realized* results. This is not to say,

Much the same thing must be said, finally, of those examples of "macroeconomic" analysis that involve the use of concepts such as Saving and Investment, the "multiplier," and even "income velocity." [102] No one could deny that "macroeconomic" concepts of this type may be salvaged if their use is *accompanied* by more nearly microeconomic analysis.[103] Unfortunately, however, it is precisely the sponsors of the particular concepts indicated who have shown themselves least sympathetic to the use of these more detailed devices; and in taking this position they have evidenced a degree of exclusivism from which it is hoped that the analytical system outlined in the present work is itself free.[104]

IV

THE RÔLE OF STATISTICS

In every important respect, methodological discussions with respect to the rôle played by the use of statistics, on the one hand, and "theoretical" analysis, on the other, in

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of course, that there are *no* circumstances under which it would be perfectly permissible to sum up *ex ante* values. The case of the "total" demand schedule for a particular commodity proves an example to the contrary. (See, for example, Marshall, *Principles,* 99.) Even here, however, the realistic validity of such a "total" demand schedule depends upon its being related in all cases to realized results; and since these results are in all cases "realized" through the actions of *individuals,* it would always be safer to approach the problem from the standpoint of the calculations and the probable reactions of these individuals, leaving for a next step an evaluation of the share contributed by the actions of individuals to the "total" realized result. In this connection, see also what is said below, pp. 539 ff., with respect to Mr. Keynes's treatment, in his *General Theory,* of the relations between his "aggregate supply functions" for (1) a particular firm; (2) a "particular industry"; and (3) *"industry as a whole."*

[102] See above, p. 476, and the references given in nn. 29 and 30 thereto.

[103] See above, p. 476, and n. 31 thereto.

[104] For an example of what must be interpreted as a lack of sympathy for the position that the use of the "macroscopic" concepts indicated must be accompanied by analysis of a more "microscopic" character, see above, p. 477, and n. 32 thereto. With respect, on the other hand, to the suggestion that by my insistence, in Volume I of the present work, upon supplementing the use of a concept such as "income velocity" by analysis that would do justice to the methodological implications of the "cash balance approach," I was guilty of the opposite sin of relying solely upon the use of *microscopic* ("atomistic") analysis, see what is said above, p. 476, n. 31,

the explanation of the processes that unfold in the real world, are to be put on a plane with discussions with respect to the rôle of the study of "institutions" and those calculations of economizing individuals which are the subject matter of the main body of "traditional" economic analysis. In both cases, unjustifiably extreme claims have been made for one type of investigation to the exclusion of the other. In both cases, the controversy has usually ended with both parties paying lip service to both types of investigation. And in both cases the lip service has often served to conceal genuine differences in the conception of what is, after all, the common problem: namely, that of explaining the events of economic life.

In a fundamental sense, clearly, the only way to demonstrate to doubters the genuineness of anyone's protestations with respect to the necessity for combining theoretical analysis with statistical investigation is that which should always be recommended for the demonstration of the fruitfulness of any proposed "method"; and that is to *practice* the method one preaches—in this case, to *use* both methods of investigation.   And indeed I hope to follow up the "theoretical" work here presented, at not too great an interval, with a large-scale investigation, involving a very extensive use of statistical data, the central purpose of which will be to ascertain what magnitudes were actually assumed, over specific historical periods, by variables whose *relevance* to the determination of money prices and of the quantities sold at those prices, and whose general *mode of operation* it has been the task of the "theoretical" analysis presented in this work to discover.[105]   Yet something is to be said for indicating briefly here the reasons for believing that an analytical system of the type here outlined lends itself to statistical "application" in much greater degree, and with much greater hope of success, than many of its rivals.

Of these reasons, the most important is the fact that, from first to last, we have insisted upon the necessity for relating our conceptual apparatus to processes actually *realized* in

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[105] On the general questions of method involved, see what is said below, pp. 507 ff., and 515 ff.

the world we know.[106]   These *realized* processes are the only ones for which "objective" data, in the sense in which the adjective is used by sponsors of "quantitative" analysis, can, in the nature of the case, be found.[107]   The particular data involved, moreover, as we have been continually reminded by these sponsors of "quantitative analysis," are in the form of *time series*.[108]   It is of some importance to remember, therefore, that we have been at some pains to show just how our "theoretical" devices are to be related to the data represented by time series.[109]

It is of the utmost importance to observe, however, that the argument thus advanced on behalf of the analytical system here outlined amounts to much more than the mere contention that this system is a better system because statistics can be found for the measurement of the magnitudes which it includes.[110]   After all, it might still be true that the "measurable" magnitudes thus included would not be worth measuring, for the simple reason that no economic significance would attach to them after we had measured them.   The argument here is rather that the possibility of statistical measurement in this case points to a further fact which is in itself of the utmost heuristic significance: namely, that the processes whose description and explanation are envisaged by the analytical system here outlined are "real" processes, representing that "real" functioning

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[106] See, for example, what is said above, pp. 222 ff., 382 ff.

[107] Cf., for example, the remarks of Mydral, *Monetary Equilibrium,* 47 f., and J. Åkerman, *Das Problem der sozialökonomischen Synthese,* 56, 72, 243.   See also Wesley Mitchell, "Quantitative Analysis in Economic Theory" (*The Backward Art of Spending Money,* 25 ff.)—though, in the light of the argument presented in this work (see, for example, what is said above, pp. 224 ff.), with respect to the need for relating these "realized" processes to the calculations of economizing individuals, it should be clear that I would not be prepared to accept Professor Mitchell's further suggestion that interpretations in terms of such calculations necessarily "smack more of metaphysics than of science" (*op. cit.,* 25).

[108] In addition to the references to J. Åkerman given above, p. 412, n. 14, see the same author's "Quantitative Economics," *loc. cit.,* 36, 39 f., and his *Das Problem der sozialökonomischen Synthese,* 60, 94, 164, 243.

[109] See, for example, what is said on this matter above, pp. 480 ff., 497.

[110] It will be recalled that this was the only "great advantage" which the Keynes of the *Treatise,* along with others, was prepared to see in Quantity Equations of the general Fisherine form.   See Volume I, 62 f., of the present work, and the references given in n. 62 thereto.

of our economic system which it is our task to explain.   In other words, it is the fact that statistical measurability is in this case an evidence of the *realization*, and therefore the "reality" of the processes in question that makes the fact of statistical measurability of considerable importance.

It is of equal importance to observe, moreover, that this contact with statistically measurable "reality" is accomplished without the sacrifice of any of the significant analytical devices which have played so large a rôle in "traditional" economic theory.   On the contrary, it is precisely these devices which play a central rôle in the apparatus here outlined.[111]   And what this must mean is that while no one could deny that a concern with "quantitative" analysis may in fact lead to the development of "qualitative" analysis of a "new" type whose nature it is not yet easy to envisage, there is little foundation for the skepticism sometimes expressed as to the applicability of "qualitative" analysis of the traditional type to statistical data, without a prior radical change in the content of this "qualitative" analysis.[112]

It will be observed, finally, that the function assigned to statistical investigation goes far beyond that of "verification" of the type of "theory" which consists of a guess as to which of the several possible mechanisms that might operate in the world we know, has operated in a given concrete case. No one ought ever to have denied that it is a proper function of statistics to "verify" or "disprove" a specific contention of this kind, when the contention is advanced with respect to a given historical episode; nor ought any one ever to have denied that such verification or disproof may

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[111] See, for example, what is said on this matter above, pp. 224 ff., 408 ff.

[112] Cf., for example, the well-known comments of Wesley Mitchell in his "Quantitative Analysis in Economic Theory" (*The Backward Art of Spending Money*, 23 ff., 33).   One ventures the opinion, for example, that few economists would echo at the present day Professor Mitchell's remarks (*op. cit.*, 23 f.) with respect to the relative "significance" and "relevance" of the type of statistical demand curve developed by H. L. Moore, on the one hand, and "Marshall's qualitative analysis of demand," on the other.   In this connection, cf. what is said above, p. 176, with respect to "statistical demand curves," and also what is said above, p. 483, n. 44, with respect to the more general question of the use of statistics in the study of the phenomena of "demand."

itself lead to the correction and amplification of oversim-
plified accounts of the functioning of the economic proc-
ess.[113]   Yet it should also be clear that one of the major
functions of statistical investigation, quite apart from its
service in the verification or disproof of a given guess as to
what the facts may be expected to show, is to provide *quan-
titative measures of the changes in variables whose* ana-
lytical *relevance to the final result it is the task of "theory"
to establish.*

A simple illustration of the differences between the two
types of use of statistics is provided by the relation of these
uses to the familiar distinction between the Quantity
Theory, on the one hand, and the Quantity Equations, on
the other.   Statistics may be used, to be sure, to "verify"
or to "disprove" the contention that changes in the "price
level" over a given historical period were strictly propor-
tional to changes in the quantity of money over that period.
This type of "verification" or "disproof," however, would
have virtually no interest for contemporary students of
monetary problems who would lay claim to any degree of
sophistication.[114]   Such students would have a very great
interest indeed, however, in a use of statistics to tell us by
how much each of the variables included in our Quantity
Equations varied in a given historical period, so that we
may come closer to understanding why prices attained the
level they did.   The discovery of these variables was a
"theoretical" discovery; the importance of the discovery was
that it told us *what to look for* in the statistics, if we wish
to know why prices were what they were; but it has been

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[113] It is of some importance to observe that, whatever may have been
true of a few overzealous defenders of the rôle of "theory" in economic
analysis, the two propositions stated in the text have been so generally
accepted by defenders of "traditional" economics that Wesley Mitchell
has not hesitated to characterize them as constituting "the classical con-
cept of method" in economics. See his "Quantitative Analysis in Eco-
nomics" (*The Backward Art of Spending Money*, 33), and the reference to
Cairnes there given; and cf. also Robbins, *An Essay on the Nature and
Significance of Economic Science*, 106 ff.

[114] Unhappily, to judge from certain recent developments, one would
therefore have to include among "students of monetary problems" *without*
"any degree of sophistication" writers who themselves would undoubtedly
resent any such characterization most violently.   Cf. the following note.

only by the actual use of statistics that we have been able to transform a "qualitative" proposition into a "quantitative" measure of the relative strength of the forces whose operation contributed to the final result.[115]

It should hardly be necessary to labor the point that the possibilities for further statistical work of precisely this type are virtually unlimited. The light cast upon the problem of explaining movements in the velocity of circulation by the "cash balance approach," for example, is such as to open an endless vista for further empirical work in the field. For what that approach tells us is that if we are ever to explain the recorded movements in "velocity," and in some measure to predict them, we must break the problem down into a study of particular types of balance, classified further according to the external conditions under which the administrators of these balances must make their decisions.[116]

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[115] In this connection, cf. what is said in Volume I, 26, of the present work, with respect to "the true goal of the Theory of Prices," in contrast with attempts to verify or disprove "the Quantity Theory." On the other hand, something of a commentary upon the extent to which supposed "revolutions" in economic theory, as in other aspects of life, turn out to be little more than reversions to a level of primitiveness supposedly long since abandoned, is provided by the fact that recent defenders of Mr. Keynes have thought it wise to defend a concern with the "truth" or "falsity" of the Quantity *Theory* against a concern with the further development of analysis and empirical studies in terms of the variables included in the Quantity *Equations,* and the breakdowns of which these variables can be shown to be capable. See, for example, the comments on this matter by Mr. Kaldor in the *Economic Journal* for September, 1939, p. 497, and by Mr. Whale in *Economica* for February, 1940, p. 90.

[116] On the nature of these "external conditions," see Volume I, 482 f., of the present work. A preliminary breakdown of cash balances into the equivalent of Hawtrey's "consumers' balances," on the one hand, and "traders' balances," on the other (see Volume I, 325, 407, 423, n. 26, and the references given in the Index to that volume [p. 605] under " 'Consumers' Money' and 'Producers' Money' "), would be called for because of the very great probability that "consumers" and "traders" would show different degrees of sensitiveness to certain of these "external conditions" (cf. Volume I, 424, n. 28), even if such a breakdown were not called for in the interest of an adequate tracing of the process of the generation of money income (see Volume I, 323, 333 f., 349 f.). With respect to the matter of "prediction," it should be pointed out (subject, of course, to all the limitations attaching to "prediction" that would go beyond the "power of prediction" claimed for economics by Cairnes [see Volume I, 45, n. 19, and the reference to Cairnes there given]) that any "prediction" with respect to movements in a *global* figure for "velocity" would be arrived at by a weighting of the results obtained with respect to particular types of bal-

The evidence of what these decisions were, and an essential part of the required information as to the external conditions themselves, is provided by the monthly statements issued by all commercial banks to administrators of checking accounts; and supplementary information can be obtained from other records of these cash balance administrators, particularly insofar as they are *business* administrators.[117] And the same thing must be said with respect to possibilities for the use of statistics in connection with such problems as the generation of money income or the effect of a given episode of monetary expansion or contraction upon the price structure. In all cases, the "intermediate" steps in the processes are either represented by, or are analytically related to, *realized events* in time. The "prices" with which we are ultimately concerned are *realized* prices; and the changes which are held to make these prices what they are are likewise *realized* changes. Thus, the changes in the level of "money demand" with which we are ultimately concerned are *realized* changes in this "demand"; and the changes which give this "money demand" the level and direction it has are *realized* changes—changes in the administration of cash balances, as evidenced by the type of data described above; changes in the amount of payments into, and payments out of, income, as well as in the amount of payments involved in the various types of "intermediate

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ance on the basis of information as to the quantitative importance of each type (cf. Volume I, 394 f.).

117 The monthly bank statements, when carefully controlled in order to ascertain how many checking accounts are being administered by the same individual or agency as part of a co-ordinated program of cash administration, would provide, for example, all the required information with respect to the time shapes and size of realized streams of money receipts and money expenditures (cf. Volume I, 482 f.; the same information would obviously be provided by a study of the bank records of the deposits into, and debits of, individual savings accounts). From a series of preliminary studies, as yet unpublished, made by Mr. Emanuel W. T. Weiler of the University of Minnesota, moreover, I am convinced that, even without these monthly bank statements, a considerable amount of useful information with respect to cash balance administration can be gleaned from statements now submitted regularly to the Securities Exchange Commission. Similarly, information with respect to the other elements likely to affect cash balance administration could be provided either by questionnaire or by a series of type studies of the general financial position and the borrowing and investing practices of the agencies involved.

transactions" which theoretical analysis has shown to be relevant to the problem of price determination; and changes in the direction of monetary expenditure, as evidenced, for example, by the dollar volume of realized sales of given types of commodities and services. And the changes, on the "goods" side, which make realized prices what they are, are also *realized* changes—realized changes in the level and structure of output and employment; and realized changes in the "rate of sale" of different types of commodities, as evidenced by data with respect to changes in the inventories held of these commodities, this data being used, wherever possible, in such a way as to yield a measure of the "rate of sale" as defined by our formula for the determination of the latter magnitude.[118]

The records of these realized changes are the stuff of which business records are made.[119] In the light of this

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[118] See again my note on "The 'Rate of Sale' and the 'Velocity of Circulation of Goods' " in *Economica* for November, 1939, pp. 454 f. On the treatment of the "goods side" generally, and particularly on its relation to the concepts of the "general" Theory of Value, see below, Chapter Eleven.

[119] This is true, it should be observed, even in the case of those items which may be expected to occasion the greatest practical difficulties in the quantitative description of the processes involved in the generation of money income: namely, the measurement of the magnitude of payments which are payments "into income" and those which are not payments "into income" (the $(PT)_I$ and $(PT)_{NI}$, respectively, of our formulation). From a study of income and expenditure statements of individual businesses, for example, there should be no difficulty in segregating those payments (such as wage payments) which are indubitably payments "into income." The real difficulties would come in the segregation (from the total of payments made to other "traders," for example) of the amount of these payments which represents *income* ("profit") to these other traders on the transactions involved (see Volume I, 317, n. 45). But to say that a problem is difficult is not to say that it is incapable of treatment such as to increase very appreciably what Marshall called "the area of economic certainties" (see Marshall's *Industry and Trade,* 674). To take, for example, as a measure of "traders' " income on a given volume of transactions his recorded figure for realized profit on a given volume of realized sales may very well mean, as Marshall suggested, that we may in some respects increase "the area of *conscious* economic uncertainties" "in even larger proportions" than we shall have increased "the area of economic certainties." But to refuse to increase the latter area simply because its increase may be accompanied in other respects by as great an increase, or a greater increase, in the former, is to miss the lesson which is taught us by the historical progress not only of economics, but of all the important branches of human knowledge.

fact, to suggest that a desire to develop an analytical apparatus which will enable us to understand why these realized results are what they are, represents a concern with concepts "with little reference to their 'use' other than purposes of logic," or to suggest that the resulting analysis fails to "reach the point at which factual verification could enter the scene" is to raise issues with respect to the "purposes of logic" and the nature and the implications of "factual verification" which are more reminiscent of outmoded controversies on the subject of "method" than they are relevant to the analytical system outlined in the present work.[120]

V

THE "USE" OF THE SYSTEM AND ISSUES OF ECONOMIC POLICY

From the history of the natural sciences, we know it to be a sign of immaturity in a given discipline that its sponsors should worry unduly over the charge that their work is "useless" because no immediate "practical" consequences can be shown to follow from the analytical results obtained.[121] From that history, to be sure, we know also that in some instances—particularly in the earlier historical stages of certain branches of the natural sciences—the search for an immediate "practical" result has led to the discovery

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[120] The phrases quoted are from the review of Volume I of this work in the *Journal of the American Statistical Association*, XXXIV (1939), 193.

[121] In these days of imitation of the *letter* of the claims made on behalf of physical science ("general" theories, as opposed to the older "special" theories, and so on), one could hardly do better than to imitate something of its *spirit* with respect to the "practical" significance of its discoveries, as expressed by one of its greatest exponents. According to the *New York Times* of March 14, 1938, Albert Einstein was asked "whether he believed that recent progress in the release of vast amounts of energy from the uranium atom justified the hope that mankind would be able to tap the enormous stores of energy known to be locked up within the atoms." He replied that "our results so far concerning the splitting of the atom do not justify the assumption of a practical utilization of the atomic energies released in the process." "However," the *New York Times* went on to report Professor Einstein as having added, "there is no physicist with soul so poor who would allow this to affect his interest in this highly important subject." Would economists be prepared to exonerate their brethren, in similar degree, of poverty of soul?

of results which are analytically important.[122]   From the history of both the natural sciences and the social studies, however, we know also that a premature haste to provide a simple solution for pressing problems that were later shown to be of enormous complexity has often led both to a waste of scientific effort and to courses of action that proved to be disastrous.   And from a recognition of all these facts has come the only kind of conclusion that can commend itself as reasonable to the practitioners of a discipline dealing with materials capable of being affected by policy: namely, that while nothing should be said or can be said against rule-of-thumb action in areas in which our understanding of the processes involved is so inadequate as to be incapable of providing a reliable guide to action, everything can be said and must be said against either (1) a type of policy which flies in the face of what established analysis tells us with respect to the functioning of the system one desires to "control"; or (2) an intellectual attitude which would dismiss all analysis which does not include explicit counsels of action as an "intellectual asceticism" which can produce no results of " 'use' for other than purposes of logic." [123]   For if anything is clear from the history of both the natural sciences and the social studies, it is that progress in the understanding of how a given system works, automatically provides guides to "policy" in the form of both positive prescription and warning as to what we may expect and what we may not expect from a given act of economic policy.[124]

The illustrations of this maxim that could be drawn from

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[122] Mr. Keynes reminds us, for example, that "Newton, Boyle, and Locke all played with alchemy."   See the *Economic Journal*, L (1940), 156.

[123] See, for example, the review of Volume I of the present work in the *Journal of the American Statistical Association*, XXXIV (1939), 193; and also the comments, in the review of that volume published in the *Canadian Journal of Economics and Political Science*, V (1939), 266, on the alleged consequences of my supposed "self-denying ordinance against the discussion of practical monetary affairs."   Contrast, on the other hand, the review of the same volume by Professor Bresciani-Turroni cited in the following note.

[124] In this connection, cf. the comments on Volume I of the present work by C. Bresciani-Turroni, "Quelques Aspects de la Pensée Economique Contemporaine," *L'Egypte Contemporaine*, XXX (1940), 528 ff.

the history of monetary theory are so numerous that a complete enumeration of them would require a volume in itself. No one, for example, who was familiar with the truly "classical" theory of the effect of changes in the rate of discount upon the amount of bank money, with its emphasis upon the proposition that a given rate of discount is to be regarded as "high" or "low" only as compared with the height of the rate of profit expected from the use of a bank loan, ought ever to have been led to exaggerate the power of low money rates to bring about an expansion, or to minimize the power of sufficiently high money rates to put a stop to a credit inflation based upon business borrowing from commercial banks.[125] Nor ought anyone aware of the substance of the analysis associated with the "cash balance approach" ever to have sponsored with high hopes a program of monetary "reflation" that took no account of the possibility of occurrence of a type of cash balance administration which might lead us through a period of disappointing response of total expenditure to the increase in the quantity of money, on to a period of over-rapid resuscitation of "dead" balances.[126] Nor, finally, can anyone aware of the complexity of the processes involved in the generation of money income be expected to accept uncritically a justification of a given volume of public expenditure as involving a promise of "secondary" effects of a given magnitude, when the promise itself is based upon the unsupplemented use of a concept of the degree of analytical crudeness which has characterized the "multipliers" thus far proposed by one group of monetary theorists.[127]

In all these cases, it will be observed, the basis for choice between rival analytical systems as guides to policy is pro-

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[125] Precisely the same proposition holds, obviously, with respect to the *details* of the argument, for example, as to why a low bank rate would be expected to lead to a rise in prices. See, for example, what is said in Volume I, 233 ff., of the present work, with respect to the limits within which, and the conditions under which, bank rate could be expected to affect prices by way of the operation of interest as a "capitalization factor."

[126] In this connection, cf. the comments of Bresciani-Turroni, "Quelques Aspects de la Pensée Economique Contemporaine," *loc. cit.*, 531.

[127] On the reasons for the "analytical crudeness" in question, see what is said above, p. 476. The matter will be discussed at greater length in a later study of The Generation and Utilization of Money Income.

vided, not by the degree of confidence with which a particular sponsor of a given analytical system may announce his maxims of policy, but by the relative degrees of precision with which the rival analytical systems are able to describe and explain the functioning of the individual sectors of the economic process.   For what one learns from a study of the fate that has overtaken most of these maxims of policy is that while cases can be imagined in which a given maxim would provide precisely the right prescription for policy, in other cases it might well prove disastrous.   And one learns also that in virtually every case the reason for these overgeneralizations of the applicability of a given maxim of policy has followed from an overgeneralization of the applicability of the *analysis* which is held to justify the particular maxim of policy involved.[128]

In the light of this fact, to fear the charge that no "general" theory of monetary dynamics, for example, can emerge from a painstaking analysis of the forces controlling the action of the separate cogs in the monetary sectors of the economic system, is to fear a chimera.[129]   For if anything

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[128] The examples that could be provided in support of this proposition, as well as of that advanced in the preceding sentence of the text, are legion.   Within the field of monetary policy and monetary analysis, however, it should be sufficient to point to the advocacy of rigid stabilization of the "price level" as a goal of monetary policy, and the critical analysis to which such advocacy has given rise, as well as to the advocacy of alternative goals of monetary policy, such as that of keeping the "quantity of money" constant, and to the critical analysis which the advocacy of these alternative goals led in turn.   Within the broader field of general economic policy, on the other hand, one has only to think of the various attempts in recent years to justify permanent changes in the distribution of the tax burden and therefore in the amount of funds available for private investment, on grounds which assume, tacitly or avowedly, that the obstacles to private investment are due to a virtually "inevitable" and permanent lessening of the number of openings for private investment, rather than, in an appreciable degree, to the maladjustments of a post-war era and the vagaries of governmental economic policy.

[129] What follows in the text provides the obvious answer to the possible suggestion (cf. above, p. 436, n. 63, and p. 494, n. 76) that the apparatus outlined in the present work, precisely by virtue of its applicability to the widest possible range of conceivable dynamic "processes," is too "general" to be of any "practical" use.   At the same time, it will be observed, the very fact that the "generality" of the apparatus thus outlined is obtained as the result of a conscious sacrifice of any claim to have discovered the *single* way in which our system can function in time means that it is not

can be said to have emerged from both the business-cycle
analysis and monetary analysis of recent years, it is that,
in a changing world, the usefulness and indeed the very
possibility of constructing a "general" apparatus designed
to explain the functioning of the economic system depends
upon the extent to which such a "general" apparatus can be
used in the elaboration of what the Germans would call
a *"Kasuistik"* of the processes of economic change.[130]   Of

open to the objections of those who have regarded "dynamical solutions, in
the physical sense, of economic problems" as "unattainable," on the ground
that not only the "quantity," but also the "character" of the "forces at
work" may change (Marshall, "Distribution and Exchange," *loc. cit.*, 39, 42
[*Memorials of Alfred Marshall,* 313, 317]). Nor is it open to the objec-
tions of those who would insist that while we may be able to "construct
pieces of 'theoretical dynamics,'" these "pieces" "can never be combined
into any 'general theory'" (so B. Ohlin, in the *Economic Journal,* XLVIII
[1938], 500). One may refuse to accept such statements, and still recognize
that a full explanation of the *reasons* why the "forces at work" may "change"
in "character" would undoubtedly involve an invocation of findings of fields
other than economics—indeed, very large parts of the whole of "general
sociology" or "historical sociology" (cf. J. N. Keynes, *The Scope and
Method of Political Economy,* 140 f., 147 f., and R. W. Souter, "Equilib-
rium Economics and Business-Cycle Theory: A Commentary," *loc. cit.,* 59)
—which is another way of saying that it would have to invoke the findings
of very large parts of the whole of accumulated knowledge. For there is
no reason whatever why we should not be able to trace, in all required
detail, the *effects* of these changes upon those variables whose relation to
the determination of realized prices it is the task of an analytical apparatus,
such as that outlined in the present work, to establish. See, for example,
what is said above, pp. 433 ff., with respect to the use of this analytical ap-
paratus for the purpose of tracing the type of sequence of events in time
involved in "dynamic" theories of "economic development" of the type
presented by Professor Schumpeter. With respect, on the other hand, to
the contention that while we may be able to "construct *pieces* of 'theoretical
dynamics,'" these "pieces" "can never be combined into any 'general
theory,'" see what is said above, pp. 378 ff., on the rôle of analysis and
synthesis, respectively, in tracing economic processes in time; and see also
what is said in the following note.

     [130] On the need for making use of such a *"Kasuistik,"* see, for example,
Myrdal, *Monetary Equilibrium,* 44, and Ohlin, "Some Notes on the Stock-
holm Theory of Savings and Investment, I," *loc. cit.,* 58. (Cf. also Pro-
fessor Ohlin's earlier "Till frågan om penningteoriens upplägning," *loc. cit.,*
73, and his comment in the *Economic Journal,* XLVIII [1938], 500. The
reader will observe, however, that the antithesis which Professor Ohlin sets
up in the latter passage between the use of a *Kasuistik* of change and
what he calls a "general theory" of dynamics disappears when nothing more
is meant by a "general theory" than a general "apparatus" of the kind de-
scribed in the text. In this connection, cf. the comments of Lindahl, *Studies
in the Theory of Money and Capital,* 24 f., on the relation of "economic

two rival analytical systems, that is to say, that one is to be preferred which lends itself best to a *complete subsuming under its categories of all the elements that could reasonably be expected to affect the final result,* as well as to a careful specification of the time sequence in which these elements can be shown to operate.[131]   Given such a categorical system, we are able to determine what elements to watch as the elements which give a changing process the contours and direction it has, and, so far as the statistical data permit, we are able both to observe the time at which these elements begin to operate and to measure the extent of their operation.

theory" to what he calls "the elaboration of the special theories.") It may be observed also that even Professor Hayek, whose earlier utterances with respect to what may fairly be demanded of a genuinely satisfactory "theory" of the business cycle tended to suggest the possibility of dispensing with a *Kasuistik* of economic change (see, for example, *Monetary Theory and the Trade Cycle,* 89 f., 130, 140 f.), has since deplored attempts "to apply an over-simplified and defective theory to these complicated phenomena" of the business cycle, and thus "to press the problems into the strait-jacket of a scheme which did not really help to solve them"; and, in particular, he has made it clear that he believes that the task of business-cycle theory "is not to construct . . . a detailed scheme which will fit all actual fluctuations, but rather a development of those sections of general theory which we need in the analysis of particular cycles—which often differ from one another very considerably ("Preiserwartungen, monetäre Störungen, und Fehlinvestionen," *Nationaløkonomiske Tidskrift,* LXXIII [1935], 177 f.; cf. the same author's *Profits, Interest, and Investment* [1939], 136, 138).

[131] In a review of Volume I of the present work in the *Economic Journal* for September, 1939, p. 497, Mr. Kaldor draws a distinction between a formulation which *"exhibits* the operation of price determining forces," and one under whose variables "all such forces could be *accommodated";* and he suggests that I claim only the latter for the type of apparatus I sponsor. I can reply only that I can see no possibility whatever for a sensible interpretation of Mr. Kaldor's distinction; and that therefore, even if I *were* to speak (as I have not spoken) of "accommodating" the forces in question under the variables indicated, I should mean only what must be meant when it is said that a given apparatus "exhibits" the operation of these forces. For the argument is precisely that it is by showing how these forces are "accommodated" under movements in given variables that one "exhibits" the functioning of an economic process. The only question worth discussing is that of the relative merits of two categorical systems, one of which is capable of "exhibiting," in all desired detail, the successive steps in such a process, whereas the other does not. The reader who accepts this conclusion will provide his own answer to Mr. Kaldor's question (p. 497): "Why $MV = PT$? Why not $A = B$?" In fact, of course, as any careful reader of this work will hardly need to be reminded, the analytical framework here outlined, while it accepts, without apology of any kind, formulations of the type $MV = PT$ as a *starting point,* goes far beyond such formulations.

Armed with this knowledge, and with a knowledge of the channels by which we pass from one stage in a given process to the stages that follow, we are able in some degree to judge the wisdom of a proposed policy of intervention; without such knowledge, all talk of a "scientific" basis for economic policy is a snare and a delusion.[132]   And this is merely another way of saying that even from the standpoint of those for whom the goal of all economic analysis is the improvement of economic policy, any analytical system, including the one outlined in this work as well as any more nearly adequate system for whose construction it may serve as a foundation, must be judged, from first to last, upon its adequacy, *as* an analytical system, to account for economic events in the world we know.[133]

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[132] It should hardly be necessary to labor the point that I myself do not cherish the "delusion" that the provision of a scientific *basis* for economic policy will itself provide a technique whereby one can select "scientifically" one among a possible series of policy alternatives. See Volume I, 156, n. 36, of the present work, and the reference there given; and cf. also Professor Viner's presidential address, "The Short View and the Long in Economic Policy," *American Economic Review*, XXX (1940), 1 ff. But to say these things is to say nothing against the rôle of "scientific" *analysis* as a *preliminary* to decisions in matters of policy.

[133] Again I must remind the reader that the present work is to be followed by two others—namely, *Money and Interest* and *Money and Production;* and I ask those who are interested solely in those particular problems of policy which have to do with the control, by monetary means, of movements in employment and output, and can see no relevance to these problems in the analysis presented in these two volumes, to await the second, in particular, of the publications indicated.

PART THREE

PARTICULAR SUPPLY CURVES, STREAM EQUATIONS,
AND THE REALIZATION OF MARKET MAGNITUDES

CHAPTER TEN

Elasticity of Supply and the Structure of Money Prices

THE PURPOSE of this chapter is to examine certain aspects of the treatment accorded, in Keynes's *General Theory*, to the problem of the rôle to be assigned, in the Theory of Money and Prices, to *supply* curves of the Marshallian type, with their special property of "elasticity." A parallelism with the argument presented in Chapter Four of this volume with respect to the *General Theory*'s treatment of the rôle to be assigned, in the Theory of Money and Prices, to Marshallian *demand* curves, would be suggested in any case. Such a parallelism is made inevitable, however, by the fact that the treatment accorded to this sector of the Theory of Prices in Keynes's *General Theory* itself parallels the treatment there accorded to particular demand curves in a number of significant respects, although in other respects it differs from the treatment accorded to particular demand curves.

Specifically, the following propositions may be compared with the corresponding propositions advanced in Chapter Four of this volume, with respect to the rôle played in the Theory of Money and Prices by Marshallian "elasticity of demand," when the latter concept is applied to the demand for particular commodities:

1. There is no foundation for the charge that while "economists," so long as they "are concerned with what is called the Theory of Value, . . . have been accustomed to teach that prices are governed by the conditions of supply and demand," and, "in particular," have allowed "the elasticity of short-period supply" to play a "prominent part," they have made no use of the "homely but intelligible concept" of "elasticity of supply" when they "pass . . . to the Theory

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of Money and Prices." [1]   On the contrary, the introduction, into the Theory of Money and Prices, of the equivalent of the *concept* of differing "elasticities of supply" for particular commodities antedated Marshall's suggestion of the *term* "elasticity of supply"; and in the post-Marshallian era the *term*, as well as the concept, was explicitly introduced not only into advanced discussions of the problems of monetary theory, but also into elementary textbooks on the subject, as an essential element in the explanation of differential price change, and therefore of changes in the structure of money prices.

That the *term* "elasticity of supply," as applied to particular commodities, like the *term* "elasticity of demand" as so applied, should have been explicitly introduced by representatives of "old" Cambridge into their discussions of the nature of the forces affecting the level of output as a whole, will, of course, surprise only those who have not undertaken to test on their own account the accuracy of Mr. Keynes's generalizations with respect to the practices of earlier "economists." [2]   A particularly striking demonstration, moreover, that this type of explicit introduction of the concept of "elasticity of supply" was no peculiarity of "old" Cambridge is again provided by the case of Mr. Hawtrey; for Mr. Hawtrey could not have been more articulate in grouping the relative "elasticity of supply" of different commodities

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[1] See the *General Theory*, 292.   Mr. Keynes includes "changes in marginal cost" along with changes in "the elasticity of short-period supply" among the "homely but intelligible concepts" which have "played a prominent part" in the "Theory of Value," but of which, he insists, "we hear no more" when economists "pass . . . to the Theory of Money and Prices."   On the rôle to be assigned to "changes in marginal cost" in the determination of realized money prices, see below, pp. 557 ff.

[2] The concept of "elasticity of supply" is, of course, necessarily *implicit* in the type of portmanteau concept of "old" Cambridge represented by a concept such as that of an "elasticity of demand in terms of effort" (see above, p. 143, n. 6). It is implicit also in the use, by representatives of "old" Cambridge, of the concept of a "period of gestation" for particular commodities.   For, as Mr. Robertson argued, the use of the latter concept "is, in fact, only a development of that doctrine of quasi-rent long familiar to students of Dr. Marshall's work" (*A Study in Industrial Fluctuation*, 14) ; and "students of Dr. Marshall's work" should hardly need to be reminded of the relation between the concept of "quasi-rent" and what Mr. Keynes calls "the elasticity of short-period supply" (see, for example, Marshall's *Principles*, 426). It is unnecessary, however, to rely solely on these passages in which the concept of "elasticity of supply" was merely *implicit*. See, for example, the explicit use of the term "elasticity of supply" by Mr. Robertson in his *Banking Policy and the Price Level*, 28, 31 f. (including the footnote) ; also Robertson's *Economic Fragments*, 187.

along with the relative "elasticity of demand" for such commodities as factors affecting the degree of "sensitiveness of price" shown by individual commodities (and therefore affecting the commodity price *structure*) during periods of monetary expansion and contraction.[3] And again a proof that this practice was in no way regarded as exceptional by writers prior to the appearance of Keynes's *General Theory* is provided by the treatment of the problem which one finds in the textbooks on Money current at that time; for their discussions of the effect of "unequal elasticities of *supply*," as well as of demand, as elements affecting the price structure during periods of monetary expansion and contraction, were characteristically couched in terms explicable only upon the assumption that the authors concerned regarded the point in question as so widely accepted and obvious as to require no particular elaboration or emphasis.[4]

Again also, therefore, the really interesting fact is that an emphasis upon the effect of what amounts to differing "elasticities of supply" for different commodities upon the structure of prices and output during periods of monetary expansion and contraction, should have appeared in economic literature before the Marshallian *term* "elasticity of supply" was itself introduced. It is worth noting, for example, that one of the factors included by Cantillon in his account of "what generally causes Meat to become dearer in proportion than Bread" was the relative ease of expanding the supply, from both domestic and foreign sources, as the price of each commodity began to rise.[5] Tooke, likewise, was as aware of the importance, for the degree of price change shown by specific commodities, of the relative facilities of "increased *supply*" of these commodities in response to an initial change in their prices, as he had shown himself to be of the importance of factors operating on the side of demand.[6]

It was J. E. Cairnes, however, who provided the *locus classicus*, in works published prior to Marshall's introduction of the *term* "'elasticity of supply,'" for discussions of the effects upon the structure of money prices ("the disturbance effected in the relation of prices"), during periods of monetary expansion, which are brought about by the varying elasticities of supply of particular commodities (the varying "facilities for extending the supply of different kinds of commodities and . . . the facilities for contracting it") in response to such initial

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[3] See, for example, *Currency and Credit,* p. 165 of the third edition; and cf. *The Gold Standard in Theory and Practice,* third (1933) edition, 140, 151 f., 175.

[4] See, for example, Edie, *Money, Bank Credit, and Prices,* 82; also p. 85, where the greater "inelasticity of supply of agricultural products" is adduced as an explanation of the more serious fall in price evidenced by these products.

[5] See Cantillon's *Essai,* 229 f., 236 f. (pp. 173, 179 of Higgs's translation).

[6] See, for example, Tooke's *Thoughts and Details,* I, 92; and cf. also II, 23, 29, of the same work. On Tooke's treatment of elasticity of *demand,* see above, pp. 148 ff.

changes in price as might result from the impact of new money-spending power.[7] Not the least significant aspect of Cairnes's discussion, moreover, is that both he and commentators upon his analysis believed that, in arguing as he did, he was merely applying, to the special problem of differential price-change during periods of monetary expansion and contraction, the principles of "supply and demand" which are to be found in the "general theory of value." [8] It is significant, also, that Jevons, who would certainly have been prepared to quarrel with certain aspects of the particular version of the "general" Theory of Value sponsored elsewhere by Cairnes, not only refrained from quarrelling with Cairnes's analysis on the point in question, but actually endorsed it without qualification.[9] And the lack of substantive significance in the fact that Cairnes, writing before Marshall, did not use the *term* "elasticity of supply" will be evidenced if one asks whether Mr. D. H. Robertson can be accused of having failed to see the significance of the Marshallian *concept* of "elasticity of supply" for the problem in hand, simply because, in translating Cairnes's argument, he chose to translate it in terms of Robertsonese "tendencies to dysentery" rather than in terms of Marshallian "elasticities of supply." [10]

2. As in the case of the *General Theory's* treatment of Marshallian "demand schedules for the products of particular industries" (and therefore of the Marshallian "elasticity of demand"), an element of paradox was introduced into that work's treatment of the corresponding supply schedules (and therefore of the Marshallian "elasticity of supply") by the fact that Mr. Keynes did not rest content

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[7] See Cairnes's "Essays Towards a Solution of the Gold Question, II: The Course of Depreciation" (1858), in his *Essays in Political Economy,* 56 f., 60 ff.

[8] It will be recalled, for example, that Cairnes protested against Newmarch's suggestion that prices might be affected, during the processes of monetary expansion or contraction, by some factor which "operates upon prices neither through demand nor yet through supply" (*Essays,* 57 n.; cf. above, p. 271, n. 108). For an example of the suggestion that Cairnes's "results become of vast significance in the general theory of value," see Francis Walker's review (1874) of Cairnes, reprinted in the former's *Discussions in Economics and Statistics,* I, 283.

[9] See Jevons's reference to Cairnes in the former's *Investigations,* 124 f.; and cf. Jevons's own remarks on the effect, on the price structure, of the fact that the production of certain commodities may be "incapable of any but slow extension" (or "incapable of great increase"), in his *Investigations,* 25, 43.

[10] For Robertson's translation of Cairnes's argument in the terms indicated, see the former's *Study of Industrial Fluctuation,* 232 f. For Robertson's own argument with respect to the "dysenteric trades," see pp. 201 f. of the same work.

with charging that "economists" have failed to make use of
the concept of "elasticity of supply" in their versions of
the Theory of Money and Prices.   On the contrary, he went
on to present a further argument designed to demonstrate
that supply schedules of the Marshallian type are subject
to limitations which deprive them of usefulness in account-
ing for the functioning of the economic process in the world
we know.   Mr. Keynes, that is to say, simultaneously (1)
charges "economists" with having failed to make use, in
their theories of Money and Prices, of the "homely but in-
telligible concept" of "elasticity of supply"; and (2) warns
them that such a use is either impossible or inadvisable.[11]
Quite apart, however, from the confusion which was bound
to be engendered by this apparent inner contradiction in the
argument of the *General Theory*, it can be shown that Mr.
Keynes's reasons for urging an avoidance of formal use of
particular supply schedules of the Marshallian type are as
irrelevant in the case of particular supply schedules as his

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[11] A resolution of this paradox is, of course, possible if we assume that,
by the charge indicated under (1), Mr. Keynes meant that "economists"
have not made the *particular use* of the concept of "elasticity of supply"
(namely, its mechanical extension to "output as a whole") which he spon-
sors; just as, on the demand side, he may be presumed to mean that
"economists" have not used the *particular concept* of "elasticity of demand"
which he himself uses (namely, the concept of an "elasticity of effective
demand," in his sense of the term).   It must then be further supposed that
Mr. Keynes's statement that the "homely but intelligible concepts" of
"elasticity of supply and demand" "nowhere appear" in received versions
of the Theory of Money and Prices was intended to mean that the *par-
ticular concepts* of "elasticity of demand" and "elasticity of supply" (or
the *particular applications* made of one of these "elasticities") that do
"appear" in these versions, might just as well have *not* "appeared" there,
since these concepts or applications are useless in attempting to account for
movements in the level of Output as a Whole.   It should hardly be neces-
sary to comment at length upon the degree of strain which such an inter-
pretation (the only one which would save Mr. Keynes from the charge of
self-contradiction) puts upon the plain meaning of words.   It *is* necessary
to point out, however, that, with respect to the concept of "elasticity of
demand," even as *Mr. Keynes* uses the term, he is substantively wrong on
his facts (see Chapter Thirteen, below); and it is necessary to point out
also that in no case would it be possible to credit Mr. Keynes with having
accomplished what he blames "economists" for not having accomplished:
namely, the application, to the Theory of Money and Prices and of Output
as a Whole, of the "homely but intelligible concepts" of "elasticity of de-
mand and supply" in the sense in which these concepts appear *within the
"general" Theory of Value*,

reasons were shown, in Chapter Four of this volume, to be in the case of particular demand schedules.

For Mr. Keynes's principal reasons for wishing to avoid the formal use of devices of the kind typified by "the ordinary supply curve for a particular commodity" are simply (1) that this "ordinary supply curve is drawn on some assumption as to the output of industry as a whole and is liable to change if the aggregate output of industry is changed"; and (2) that this, in turn, means that when "we are examining the response of individual industries to changes in *aggregate* employment, we are necessarily concerned, not with . . . a single supply curve [for each industry], but with . . . [a "family"] of such curves [for each industry] corresponding to different assumptions as to the aggregate employment." [12]   This statement, of course, is merely another way of stating what would be regarded as a virtual axiom by all those who accept the Walrasian conception of the general interdependence of economic variables: the "axiom," in this case, being that the level of *costs* for any given industry, for example, will be greatly affected by the prices offered for factors or materials of production common to other industries—the level of these price-offers, in turn (and therefore the cost-curves for any one firm or industry), being necessarily affected by the level of output at which other firms and industries are operating.[13]   And this amounts merely to saying that the data upon the basis

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[12] *General Theory,* 281.   Cf. also p. 43 n., of the same work, where it is pointed out that the "shape" of "particular supply curves" will "depend on the demand for suitable labor in other directions."

[13] It should hardly be necessary to stress at length the bearing of this fact upon (1) both the novelty and the justification of Mr. Keynes's proposed "dichotomy" between "the Theory of the Individual Industry or Firm and of the rewards and the distribution between different uses of a *given* quantity of resources on the one hand, and the Theory of Output and Employment *as a whole* on the other hand" (*General Theory,* 293; italics in the original); and (2) the justification of his further implications (i) that "the study of the individual industry or firm" is necessarily associated with "the assumption that the aggregate quantity of employed resources is constant, and, provisionally, that the conditions of other industries or firms are unchanged," and (ii) that the assumptions in question are necessarily coextensive in their implications with a lack of concern, designed or unwitting, "with the significant characteristics of *money*" (*ibid.;* italics mine).

of which a cost curve for any given firm or industry, for example, is constructed are *subject to change* as the result of changes elsewhere in the price structure.

In Chapter Four of this volume, however, it was shown that the use of particular supply schedules in the explanation of changes in the structure of money prices is not made impossible either by the Walrasian warning that these schedules *are* subject to change as the result of changes elsewhere in the price structure, or by the *fact* that these particular supply schedules may in reality change in either position or conformation as between any two instances of realized purchase and sale.[14]   The only judgment, therefore, that can be passed upon Mr. Keynes's treatment of the rôle to be assigned to particular supply schedules (and therefore to the Marshallian "elasticity of supply") in the determination of money prices is one analogous to the judgment passed upon his treatment of the rôle to be assigned to particular demand schedules (and therefore to Marshallian "elasticity of demand") in this determination: namely, that his own use of the otherwise perfectly familiar proposition that "the ordinary supply curve for a particular commodity is drawn on some assumption as to the output of industry as a whole and is liable to change if the aggregate output of industry is changed" represents an attempt to prove too much; and that, *if taken at its face value,* this attempt to prove too much would amount to a premature renunciation of a set of analytical devices, developed originally within the "general" Theory of Value, which must represent an indispensable part of any apparatus designed to account for the forces actually determining money prices.

3. In one significant respect, to be sure, the *General Theory's* treatment of the rôle to be assigned to particular supply curves in the determination of money prices differs from its treatment of the rôle to be assigned to particular demand curves.   As we saw in Chapter Four of this volume, Mr. Keynes made no serious attempt to provide an analytical substitute for the latter: that is, he made no serious attempt to provide a device, or set of devices, which would

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[14] See above, pp. 166 ff.

perform the analytical tasks properly to be assigned to particular demand schedules (and therefore to Marshallian elasticity of demand) in the explanation of the determination of money prices.[15]   In the case of supply curves, however, he has attempted to provide such an analytical substitute, in the form of an "aggregate supply function for *a given firm or industry*."[16]   It can be shown, however, that

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[15] See above, pp. 154 f., 160 ff.; also the following note.

[16] See the *General Theory*, 44 f.; 55, n. 2; 115 f.; 246.   It is of first importance to note the words italicized; for it is precisely these words which summarize the difference between Mr. Keynes's treatment of the equivalent of particular "supply" curves, on the one hand, and particular *demand* curves, on the other, to which attention is called in the text.   There is no reason, of course, why Mr. Keynes *might* not have introduced the concept of an "aggregate demand function for the product of a *particular firm or industry*," which would bear the same relation to an Auspitz and Lieben *demand* schedule as that which his "aggregate supply function for a given firm or industry" bears to an Auspitz and Lieben supply schedule.   Indeed, at one point in the *General Theory*—namely, in its discussion of "The Employment Function" (280 ff.)—Mr. Keynes did come close to presenting such a concept; for in this context he made use of the expression D_{wr} to represent "the amount of effective demand, measured in wage units, directed to a firm or industry" r.   But he made use of this concept only in the way in which it is used in Auspitz and Lieben *supply* curves—namely, as measuring the ordinates of these *supply* curves, which in turn represent the amount of *commodity* that suppliers stand ready to *supply* at different levels of money "demand"; he did not use the concept D_{wr} to derive the equivalent of an Auspitz and Lieben *demand* curve—that is, a curve representing the amount of money which *demanders stand ready to offer* for various amounts of a given commodity.   It will be observed, moreover, that there is nothing in Mr. Keynes's algebraic treatment of the "aggregate *demand* function" (see, for example, the *General Theory*, 25) corresponding to that part of his treatment of the "aggregate supply function" in which (as on pp. 44 f. of the *General Theory*) the subscript r is used to distinguish the "aggregate supply function for a given industry" r from "the aggregate supply function . . . for industry as a whole."   Similarly, there is, in the *General Theory*, no discussion of the relation of an "aggregate demand function" for the product of a particular firm or industry to the "ordinary demand curve" of the "general" Theory of Value, of the kind that is presented with respect to the alleged relation between an "aggregate supply function for a particular firm or industry" and "the ordinary supply curve" (*General Theory*, 44).   See, on the contrary, what is said above, pp. 203 ff., concerning the *General Theory's* treatment of the "ordinary demand curve."   Particular attention is to be called, finally, to the passage on pp. 294 f. of Chap. 21 of the *General Theory* ("The Theory of Prices"), where a sharp distinction is drawn between the treatment of *supply*, on the one hand, and the treatment of *demand*, on the other: it being held that "there is no reason to modify" significantly the type of analytical device involved in the problem of *supply* "in a single industry," "when we pass to industry as a whole," whereas "when we are dealing with *demand* as a

this substitute, when divested of certain of its external trappings, amounts essentially to an *"Auspitz and Lieben"* *supply curve.* These curves are *subject to all the limitations* which, according to Mr. Keynes himself (though *not* according to the best of received versions of the Theory of Money and Prices and of the Theory of Output as a Whole), greatly impair, if they do not destroy altogether, the usefulness of the "ordinary supply curve" (and therefore the Marshallian concept of "elasticity of supply") in dealing with the processes realized in the world we know.[17]   And the

whole and no longer with the demand for a single product," "we have to introduce quite new ideas."

[17] That supply curves of the Auspitz and Lieben type are subject to all of the limitations indicated was repeatedly emphasized, and with the utmost explicitness, by Auspitz and Lieben themselves. See, for example, their *Untersuchungen über die Theorie des Preises,* pp. xiv, xxiii f., 4, 20 f., 157, 168 ff., 232 ff., 243 ff., 475 f. I am not sure whether, in the passage quoted from the *General Theory* above, p. 526, n. 12, the "particular supply curves" whose "shape," according to Mr. Keynes, "will depend on the demand for suitable labor in other directions," are thought of as "ordinary" supply curves (of the type used in Marshall's *Principles*) or as "aggregate supply functions" for "particular firms or industries" (that is, as "Auspitz and Lieben" supply curves). In any event, the limitations which apply in the two cases are precisely the same; and this is true also of Mr. Keynes's "employment function," which, as Mr. Keynes points out (*General Theory,* 280) is, in effect, the inverse of his "supply function." It is interesting to observe, therefore, that although Mr. Keynes admits (*General Theory,* 43 n.) that it would "be unrealistic" to "ignore" the "interesting complications" which arise as a result of the fact that the "shape" of "particular supply curves . . . will depend on the demand for suitable labor in other directions," he is himself content to dismiss these "interesting complications" with the brave proposition that "we need not consider them when we are dealing with employment as a whole, *provided we assume that a given volume of effective demand has a particular distribution of this demand between different products uniquely associated with it"* (italics mine; cf. also the comparable proposition with respect to the "employment function" on p. 280 of the *General Theory*). It is hardly necessary to comment at length upon the heroic quality of such an assumption; nor is it necessary to dwell at length upon the necessity for further assumptions of the type indicated by Mr. Keynes himself in the following sentence of his note on p. 43. What it is necessary to emphasize is that when Mr. Keynes admits, in this same note, that "all this . . . belongs to the detailed analysis of the general ideas . . . set forth" in his *General Theory,* he admits that "particular supply curves," whether they are of the type used in Marshall's *Principles* or are of the Auspitz and Lieben type, *are* needed for "detailed analysis" of the nature of the forces affecting the level of Output as a Whole, and that they *can* be used for such a purpose *despite* the fact that they are subject to change as the result of changes elsewhere in the price structure, such as might be associated with changes in the level of Output as a Whole.

very fact that Mr. Keynes himself regards particular supply curves of this ("Auspitz and Lieben") type as not without significance in accounting for events within the world we know, may be taken as proof of the failure of his attack upon *Marshallian* supply curves as analytical devices useful in accounting for the determination of money prices.[18]

The relation of Mr. Keynes's "aggregate supply function for a particular firm or industry" to an Auspitz and Lieben supply curve, on the one hand, and to "the ordinary supply curve" (that is, the type of "supply curve" popularized by Marshall's *Principles*) is best seen if we consider (1) the relation of Mr. Keynes's *"elasticity of production* for a particular firm or industry" to the Marshallian "elasticity of supply"; and (2) the implications of the expression which Mr. Keynes himself presents by way of relating the Z_r of his "aggregate supply function for a particular firm or industry" to "the ordinary supply curve."

i. Mr. Keynes's "elasticity of production for a particular firm or industry" is defined by the formula $e_{or} = \dfrac{dO}{dD_{wr}} \cdot \dfrac{D_{wr}}{O_r}$, in which e_{or} represents the "elasticity of production," or of "output" (O_r), of the firm or industry r, and D_{wr} represents the "effective demand" for O_r, when this "effective demand," though conceived of as an "expected" *stream of money payments,* is measured in terms of "wage units." [19] We may assume, for the present, that the "wage unit" is introduced into the formulation only as a *numéraire.*[20]   For the purposes of the present argument (though *only* for those purposes), we may assume also that no significant problems are raised by the distinction between "supply," on the one hand, and *"production"* or *"output,"* on the other.[21]   If both assumptions are made, it follows that the difference between Mr. Keynes's formula for the "elasticity of production" of a particular firm or industry, on the one hand, and Marshallian "elasticity of supply," on the other, is that the term corresponding to the y of the general "elasticity" formula $\dfrac{dy}{dx} \cdot \dfrac{x}{y}$ is a given amount of "money *demand*" in the case of Mr. Keynes's "elasticity of production," whereas, in the case of the Marshallian "elasticity of supply," it is a given *price per unit* of the article "supplied."   And this, of course, is precisely the difference between a graphic representation of "supply" and "demand"

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[18] See the last sentence of n. 17, immediately preceding.

[19] See the *General Theory,* 283.   On the meaning of the subscript r, see pp. 280, 285, of the same work.

[20] On this matter, see what is said below, p. 597, n. 101.

[21] On the reasons for insisting that "significant problems" *are* raised by the distinction in question, see what is said below, pp. 553 ff., under our Proposition XXIX.

of the type employed by Auspitz and Lieben, on the one hand, and the graphic representation of supply and demand popularized by Marshall's *Principles*, on the other.[22]

ii. This conclusion is confirmed by Mr. Keynes's own argument as to the relation between the Z_r of his "aggregate supply function for a particular firm or industry," on the one hand, and "the ordinary supply curve" (that is, a supply curve of the type popularized by Marshall's *Principles*), on the other. Mr. Keynes's argument is summarized by his identification of "the ordinary supply curve" with the expression $p = Z_r/O_r$, in which p represents the price per unit of output (O_r) of the firm or industry r, and Z_r is "the return the expectation of which will induce . . . an output O_r."[23] An Auspitz and Lieben supply schedule would of course be represented by a curve showing the different amounts of O_r that the producers would be willing to offer for different amounts of "money demand" (that is, for different amounts of Z_r).[24] The value for the p corresponding to any

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[22] It is of the utmost importance to observe, on the other hand, that neither Marshall nor Auspitz and Lieben proceeded, as does Mr. Keynes, to apply the formula for "elasticity of supply" to industry *"as a whole."* See below, pp. 538 ff., under (4). It is also of some importance to observe that Auspitz and Lieben themselves were quite aware that their supply curves, like their demand curves, *implied* the concept of a given response of supply to prospective price *per unit*. See the reference to Auspitz and Lieben given above, p. 263, n. 93; and cf. also what is said below in the following paragraphs of the text, with respect to Mr. Keynes's discussion of the relation between his "aggregate supply function for a particular firm or industry" and the "ordinary supply curve" of the type popularized by Marshall's *Principles*.

[23] *General Theory*, 44. Actually, Mr. Keynes's formal definition of Z_r, in this context, is "the return the expectation of which will induce a level of *employment* N_r." On the implications of this usage, see the following paragraphs of the text. That no violence is done to Mr. Keynes's argument, however, by the definition of Z_r given in the text, is clear from the fact that he himself undertakes to relate "output" to "employment" by the expression $O_r = \Psi_r (N_r)$, and writes the expression $p = Z_r/O_r$ as given in the text above.

[24] The essential point, for our present purpose, is that what is common to the formulation of Auspitz and Lieben, on the one hand, and the formulation of the *General Theory*, on the other, is the substitution of the "supply price" of a *plurality* of units, considered as a group (Mr. Keynes's "aggregate supply price," or Z_r), for a "supply price" *per unit*. No significance, for the purposes of the present discussion, attaches to the fact that, at one point in the *General Theory* (p. 24 n.), Mr. Keynes defines his "aggregate supply price" (Z) as *"net* of user cost"; whereas Auspitz and Lieben did not. For, as Mr. Keynes himself insists elsewhere in the same work, "the exclusion of user cost from supply price . . . is inappropriate to the problems of the supply price of a unit of output for *an individual firm*," since such a procedure "divorces the 'supply price' of an article from any ordinary sense of its 'price'" (*General Theory*, 55, n. 1, 67; italics mine). On the other hand, the discrepancy thus indicated as between

given amount of O_r (that is, the *supply price per unit* of this amount of O_r) would be represented by the tangent of the angle obtained by drawing a line from the origin to the point on this Auspitz and Lieben supply curve corresponding to the amount of O_r for which it is desired to find the supply price.[25] This tangent is of course equal to the ratio of the ordinate at this point (a given value of Z_r, which is measured along the y axis) to the abscissa at this point (a given amount of O_r, measured along the x axis): that is, it is given by the expression $p = Z_r/O_r$, which Mr. Keynes presents as "the ordinary supply curve." As far as the argument has gone, therefore, it is clear that Mr. Keynes's "aggregate supply function for a particular firm or industry" is the essential equivalent of an Auspitz and Lieben supply curve. And as far as the argument has gone, there is nothing to show why such supply curves are not subject to all the limitations which, by the testimony of all previous users of these curves (including Auspitz and Lieben themselves), these curves share with particular supply curves of the type popularized by Marshall's *Principles*.

It is proper to ask, therefore, whether any significance, for this particular problem, attaches to Mr. Keynes's further argument with respect to his "aggregate supply function for a particular firm or industry." The nature of this argument is indicated by Mr. Keynes's formal definition of "the aggregate supply function for a given firm or industry r" as being given by the expression $Z_r = \Phi_r(N_r)$, in which Z_r "is the return [that is, the amount of sales proceeds] the expectation of which will induce a level of employment N_r." [26] With respect to this expression, the following comments are in order:

a. The formal difference between the Keynesian $Z_r = \Phi_r(N_r)$ and an Auspitz and Lieben supply curve as applied to "output" is that in

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Mr. Keynes's treatment of the supply price of "output as a whole" and his treatment of the supply price of the output of a particular firm is significant for a judgment of the procedure whereby Mr. Keynes passes from the "supply price" for the output of a particular firm or industry to the "supply price" for output as a whole (see below, pp. 539 ff.). For the effect of the procedure in question is to remove the magnitudes involved in the concept of an "aggregate supply function for industry as a whole" still further from those involved in the calculations of the *individual* entrepreneurs whose actions make realized market events (including realized changes in "output as a whole") what they are. Actually, of course, Mr. Keynes's difficulties with respect to the relation between "user cost" and "supply price" derive chiefly from his failure to distinguish adequately between the problems of *production,* on the one hand, and the problems of "supply," on the other; and they disappear as soon as use is made of adequate techniques (*including* techniques taken over from the Theory of Money and Prices) for dealing with phenomena such as "the degree of integration of industry and . . . the extent to which entrepreneurs buy from one another" (*General Theory,* 24 n.). On this matter, see what is said below, pp. 554 ff.

   [25] See above, p. 263, n. 93.
   [26] Cf. above, p. 531, n. 23.

the latter the entrepreneurial responses to changes in Z_r ("money demand" for the commodity in question) are expressed in terms of the amount of this *commodity* that the entrepreneurs would be willing to produce, whereas in the former these responses are expressed in terms of the amount of *employment* which an individual entrepreneur will be induced to give as a result of a change in Z_r. It will hardly be denied, however, that, to the entrepreneur himself, the decision to employ a given number of men will itself be *incidental to* a decision to produce a given amount of output; for, in a "business" economy, an entrepreneur will be interested in employing men only insofar as such employment is necessary for the production of the amount of the commodity he hopes to sell.[27] The actual *number* of men (or of "labor units") that he will employ in order to produce this amount of commodity will in turn depend upon the form of the production function involved.[28]   From

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[27] This much is virtually admitted by Mr. Keynes when (1) he speaks of employment as being "inelastic in response to an increase in the effective demand for *its output*" (*General Theory*, 26); when (2) he describes his "elasticity of employment for a given industry" as measuring "the response of the number of labor-units employed in the industry to changes in the number of wage-units [that is, the sums of money, expressed in terms of the "wage-unit" as a *numéraire*] which are expected to be spent on *purchasing its output*" (p. 282); and when (3) he describes his "employment function" in terms suggesting that what is "compared" by entrepreneurs with a given "amount of effective demand" is the "supply price of the *output*" to which the "employment" is intended to lead (p. 280). Contrast, however, the *General Theory*, 77, where the exposition is such as to suggest that the entrepreneur really "fixes" the amount of employment he proposes to give, and that the amount of output is a mere "consequence" of this initial decision. In the light of the simple institutional fact that entrepreneurs, under our system, are interested first in selling commodities, and in employing labor only insofar as such employment is necessary in order to sell commodities, it is, moreover, hardly relevant to suggest that in order "to predict how entrepreneurs possessing a given equipment will respond to a shift in the aggregate demand function [sc.: as reflected in the demand function for the products of their own industries], it is not necessary to know how the quantity of the resulting output . . . would compare with what [it was] . . . at a different date or in another country" (*General Theory*, 44). The suggestion here is merely that it is with the "quantity of the resulting output" that the "responses" of individual entrepreneurs to changes in demand functions are directly concerned; and that the loss in realism which is suffered when such responses are described in terms of "the number of men employed" without direct reference to their responses in terms of "output," should be compensated for by analytical or expository gains in other directions. It is here contended that no such compensatory gains are provided by the Keynesian argument.

[28] Cf. J. Schumpeter, in the *Journal of the American Statistical Association*, XXXI (1936), 793 (and see also the same author's *Business Cycles*, 510). As far as I have been able to discover, the nearest that Mr. Keynes himself came to stating such a proposition was when he undertook to relate the comparative movements in "employment" and "output" to the cases of "constant returns" and "decreasing returns" (*General Theory*, 284, 305 f.)

the standpoint, therefore, of a realistic description of the nature of entrepreneurial calculations with respect to "supply," there can be little doubt that the Auspitz and Lieben "supply curve" is superior to

—the comment with respect to "physical supply functions" on p. 173 of the *General Theory* having to do, not with the relation of movements in *output* to movements in *employment,* but with the shape of supply curves generally. This fact might have been of no great significance in itself if it did not call attention to an aspect of the *General Theory* which is strictly relevant for an evaluation of its claim to have effected a new and fruitful type of synthesis between the "general" Theory of Value, on the one hand, and the Theory of Money and Prices and of Output as a Whole, on the other. For, as was pointed out above, p. 125, one of the Lessons of Doctrinal History with respect to earlier attempts at "synthesis" of this kind is precisely that, not infrequently, the reason why a given "synthesis" has actually left the subject in a state *inferior* to that prevailing before the "synthesis," is that "the particular author concerned made use of a 'general' Theory of Value which was itself retrograde, when judged from the standpoint of the developments within that field already available at the time the synthesis was undertaken." The reader himself must be left to judge the relative merits of a treatment of the problem of the relation between the "output" and "employment" of a particular firm which would rest upon an explicit acceptance of all that has been done within the "general" Theory of Value on the theory of production functions, on the one hand, and the oblique and generally unsatisfactory treatment which Mr. Keynes accords to problems with which the theory of production functions is intended to deal. For there can be little doubt that Professor Schumpeter is correct in his statements *(Journal of the American Statistical Association, loc. cit.):* (1) that although "Mr. Keynes is as careful to point out that the number of workmen employed is not proportional to output as Ricardo was to point out that value cannot be proportional to quantity of labor," nevertheless (2) "exactly as Ricardo reasoned as if it were, so Mr. Keynes assumes that employment of labor is an 'adequate' index of the output resulting from it." For examples of passages in the *General Theory* which would support the type of statement indicated under (1), see pp. 42, 296, 299 f., 305 f., of that work; and for examples of passages that would support the type of statement indicated under (2), see pp. 41, 209, 213 f., 230 f., 294, 306. And there can be just as little doubt that an antiquated "general" Theory of Value is not made less antiquated by the substitution, for a reasoned argument capable of refuting the considerations which have led later economists to reject such propositions, of an apodictic expression of "sympathy" with "the pre-classical doctrine that everything is *produced* by *labor,*" and with the general idea that "it is preferable to regard labor . . . as the sole factor of production" (*General Theory,* 213 f.; italics in the original). Such expressions of "sympathy" may be enough to convince those readers who have looked into the *General Theory* for what they regard as long-overdue confirmation of the soundness of specific Marxian doctrines (see, for example, A. L. Rowse, *Mr. Keynes and the Labour Movement* [1936], 10 f.). They can hardly be expected to convince those who have observed with satisfaction the abandonment of the concept of "labor . . . as the sole factor of production" even by professed admirers of Marx (see, for example, O. Lange, "On the Economic Theory of Socialism," *Review of Economic Studies,* IV [1937], 138, 142). On further

Mr. Keynes's "aggregate supply function for a given firm or industry."

b. Mr. Keynes's reason for preferring expressions of the form $Z_r = \Phi_r(N_r)$ to expressions of the form $Z_r = \Psi_r(O_r)$, which more closely approximates an Auspitz and Lieben "supply curve," is that the former usage is supposed to make it possible to pass directly from a series of "aggregate supply functions for particular firms or industries" to an "aggregate supply function for industry as a whole." [29]   The really important difficulties with Mr. Keynes's treatment of the relation between these two types of "aggregate supply function" are discussed below under (4).[30]   Here we are concerned only with that part of his argument which alleges that, for this purpose, functions of the form $Z_r = \Phi_r(N_r)$ are preferable to functions of the form $Z_r = \Psi(O_r)$, because "we can then aggregate the N_r's in a way in which we cannot aggregate the O_r's, since ΣO_r is not a numerical quantity." [31]   To this contention, the following reply is to be made:

The difficulties arising from the lack of *homogeneity* in the units included in a given "aggregate," such as ΣO_r, are *as applicable in the case of "labor units" as they are in the case of units of output*.[32]   So

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aspects of Mr. Keynes's argument which represent the utilization of a retrograde "general" Theory of Value as one of the elements in his "synthesis," see what is said below in note 32, and also pp. 539 ff. It should be pointed out also that by failing to introduce explicitly the concept of production functions, in the modern sense, Mr. Keynes was guilty of retrogression also as compared with the practice established by the abler writers on the Theory of Output as a Whole, as well as by writers on the "general" Theory of Value. In this connection, cf. the comment of Schumpeter, *Journal of the American Statistical Association, loc. cit.*; and on Schumpeter's own usage with respect to changing production functions, see what is said above, pp. 431 ff.

[29] See the *General Theory,* 40 ff.

[30] See below, pp. 539 ff.

[31] *General Theory,* 45.

[32] On the fact that "the community's output of goods and services is a non-homogeneous complex" as an alleged obstacle to the use of the concept of *"output* as a whole," see the *General Theory,* 38 f. The fact that units of "labor" are likewise "non-homogeneous" has of course been a cardinal point in the arguments advanced for generations against "labor theories of value" of the type with which Mr. Keynes, by his own avowal, "sympathizes" (above, p. 534, n. 28). See, for example, Samuel Bailey, *A Critical Dissertation on the Nature, Measures, and Causes of Value* (1825), 209 ff.; and Jevons, *Theory of Political Economy,* 308. It is hardly surprising, therefore, that the meaning of Mr. Keynes's "labor unit" should itself have been called into question. See, for example, T. Greidanus, in *De Economist,* LXXXV (1936), 736; F. H. Knight, "Unemployment: And Mr. Keynes's Revolution in Economic Theory," *loc. cit.,* 115; Lauchlin Currie, "Some Theoretical and Practical Implications of J. M. Keynes's *General Theory," loc. cit.,* 16; W. Leontief, "Implicit Theorizing: A Methodological Criticism of the Neo-Cambridge School," *Quarterly Journal of Economics,* LI (1937), 347; Saulnier, *Contemporary Monetary Theory,* 314 ff. It is worth observing also that the particular device by which, according to the *General Theory* (p. 41) "quantities of employment . . . can be made"

far as this particular problem is concerned, therefore, the analytical difficulties involved in the representation of ΣO_r as a "numerical quantity" are not greater than those involved in the representation of ΣN_r as a "numerical quantity."

In fact, of course, there are standard techniques for circumventing these difficulties sufficiently to obtain a significant measure of variations of ΣO_r, conceived of as a "numerical quantity." [33] The mere fact

"homogeneous"—namely, by "taking an hour's employment of ordinary labor as our unit and weighting an hour's employment of special labor in proportion to its remuneration; i.e., an hour of special labor remunerated at double ordinary rates will count as two units"—is essentially that used by the earlier "classical" economists. See Ricardo's *Principles,* Chap. I, sec. II (pp. 15 ff. of the Gonner edition), and the quotations from Adam Smith there given (*Wealth of Nations,* Book I, Chaps. V and X [pp. 31 and 143]); also James Mill, *Elements of Political Economy,* Chap. III, section II (pp. 95 f. of the third [1826] edition). That the procedure indicated amounts to a complete surrender of certain aspects of the "labor theory of value" was at once pointed out by the early critics of the "classicals" (see, for example, Samuel Bailey, *A Critical Dissertation, etc.,* 210 ff.). It should be clear that it also amounts to a complete surrender on the part of Mr. Keynes with respect to the supposed advantage of the "labor unit" as opposed to a unit of "output" for purposes of summation into "aggregates." Cf., for example, what is said in the following note.

[33] It may be observed that one of these methods—namely, that of multiplying the "quantities" involved "by an assumed price which remained as a constant multiplier for every year" (Fisher, *Purchasing Power of Money,* 480)—is essentially that proposed by Mr. Keynes as a means whereby units of labor which are not in fact "homogeneous" "can be made so" (cf. the preceding note). The limitation which is set upon the use of this method, as applied to the "aggregation" of heterogeneous units of "output," by the fact that there may be changes in the *price structure* over the period in question, applies equally, of course, to Mr. Keynes's proposed method with respect to labor—as he himself admits when he points out that his procedure is strictly valid only "in so far as different grades and kinds of labor and salaried assistance enjoy a more or less fixed relative remuneration" (*General Theory,* 41; cf. also p. 43). For the rest, of course, an answer to Mr. Keynes's insistence upon the impossibility of providing significant measures of variations in ΣO_r is given by the work that has been done on the construction of an "Index of Production" (cf. the comment of Schumpeter in the *Journal of the American Statistical Association,* XXXI [1936], 793, which should be read in conjunction with the discussion presented in Chap. IX [pp. 483 ff.] of the same author's *Business Cycles*). Whether such an index does more than "satisfy historical or social curiosity, a purpose for which perfect precision—such as our causal analysis requires . . . —is neither usual nor necessary"; and whether the very concept implied by such an index is so "vague" that we must leave its use "to the occasions when we are attempting some historical comparison which is within certain (perhaps fairly wide) limits avowedly unprecise and approximate" (*General Theory,* 40, 43)—these are questions each reader must answer for himself. The point made here is merely that measures of changes in the level of "employment as a whole" are subject to precisely the same charges of "vagueness" and lack of "precision" as are measures

that the use of these techniques is beset with pitfalls for the unwary is no more of an argument against an intelligent use of the *concept* of the "output of industry as a whole" than the fact that the construction of measures of the "general price level" is beset with pitfalls is an argument against an intelligent use of the *concept* of a "general price level." [34] That this is so is tacitly admitted by Mr. Keynes, since he continues to make use of the concept of the "output of industry as a whole" just as he continues to make use of the concept of a "general price level" despite his professed distaste for the latter concept.[35]   The point which is important for our present purpose, however, is that the concept of "employment as a whole" is as subject to all the difficulties arising from the lack of homogeneity of "labor units" and the possibility of *divergent* movements in the *amounts of employment experienced by different types of "labor units"* as the concept of "the output of industry as a whole" is subject to the difficulties arising from the lack of homogeneity of units of "output" and the possibility of changes in the *structure* of "output," in the sense of changes in the kinds and qualities of commodities and services produced.[36]

iii. The conception of an entrepreneurial response to changes in money "demand," in terms of the amount of *employment* a given firm

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of changes in *"output* as a whole," and for precisely the same reasons.   And the point made below (pp. 546 ff.) is that the real difficulties attaching to the use of measures of "output as a whole" in the face of the fact of heterogeneity as between different types of output, have to do, not with the process of "aggregation" as such, but with the danger that the desire to provide measures of output *(or employment)* "as a whole" may prevent an adequate recognition of the importance of studying changes in the *structure* of output and employment which may be of the greatest importance for the explanation of movements in output "as a whole" or employment "as a whole" themselves.

[34] See above, pp. 333 ff.

[35] See above, pp. 155 f., nn. 27–29.   The fact that Mr. Keynes does continue to make explicit and repeated use of the concept of "Output as a Whole" has been pointed out by the same critics who have pointed out that he continues to make use of the concept of a "general price level."   See, for example, the references given above, p. 156, n. 28.

[36] Since this is so, it is relevant to point out that the real differences between Mr. Keynes's treatment of concepts such as "employment as a whole" and "output as a whole" and the treatment accorded to such concepts in the best of received doctrine on the subject are (1) that the best of the alternative versions have concerned themselves with the causes and consequences of changes in the *structure* of "output" and "employment," *as well as* with the problem of changes in the *level* of "output and employment as a whole," and with the *interrelation between* the two problems, whereas Mr. Keynes has in effect *ignored* the problems of *structure* (see below, pp. 547 f.) and their possible relations with the problem of changes in the level of output and employment as a whole; and (2) that the alternative versions, unlike the *General Theory*, have *not* been guilty of a mechanical extension, to "output as a whole," of categories that are properly applicable only to particular firms or industries (see below, pp. 539 ff.).

will be willing to give for each level of "expected" demand, *is subject to all the limitations* attaching to the conception of individual entrepreneurs' responses to changes in "money demand" in terms of the amount of *production* these entrepreneurs will be willing to undertake at each level of "demand." This follows, first, from the fact emphasized above under 3, ii, a: namely, that entrepreneurial decisions to employ a given number of men will themselves be *incidental to* a decision to produce a given amount of output; and, secondly, from what was characterized above under (2) as a virtual axiom: namely, that these decisions are subject to change as the result of such changes in data as might be associated with changes in "the output of industry as a whole" ("the aggregate output of industry") and in "aggregate employment." [37] For what these two propositions mean, when taken in combination, is that entrepreneurial decisions to *employ a given number of men* in response to given changes in "money demand" *are likewise subject to change for the reasons indicated.* And from this it follows, in turn, that Mr. Keynes's "aggregate supply function for a particular firm or industry" $Z_r = \Phi_r(N_r)$ is subject to precisely the same limitations, in the face of possible changes in the level of "output and employment as a whole," as are either Marshallian ("ordinary") supply curves or Auspitz and Lieben supply curves of the general form $Z_r = \Psi_r(O_r)$.

4. In Chapter Four of the present volume, it was argued that the *General Theory's* treatment of the demand side of the problem suffered from a fatal defect: namely, that Mr. Keynes's interest in the concept of a "general" or "aggregate" money demand for the products of "industry as a whole" had led him to reject particular demand schedules, of the Marshallian type, and therefore the Marshallian "elasticity of demand for the products of particular industries," as useless in dealing with the nature of the forces affecting the level of output as a whole; and it was further argued that the relation between the two types of concept is in fact of a mutually complementary character. [38] If, on the other hand, the argument advanced above under (3) were considered without reference to the *General Theory's* further treatment of the relation between the "aggregate supply functions for given firms or industries" and the "output of industry as a whole," it could be regarded as evidence that Mr. Keynes's treatment of the supply side of the problem is superior to his treatment of the demand side. For it

[37] See above, pp. 526 ff., 533 ff.
[38] See above, pp. 204 ff.

would mean that by leaving room, in the study of the forces determining the level of output as a whole, for the use of concepts of the kind typified by his "aggregate supply functions for *particular firms or industries*," Mr. Keynes was in effect leaving room for analytical analogues to these concepts, such as Marshallian supply schedules for the products of particular firms or industries.

Unfortunately, however, this virtue of the *General Theory's* treatment of the supply side of the problem is cancelled by a further, and crucial, vice, from which his treatment of the demand side of the problem is entirely free. Specifically: in his treatment of demand, Mr. Keynes rightly rejected that *type* of attempt to apply a concept such as the Marshallian "elasticity of demand for the products of particular industries" to the problem of the forces affecting the demand for, and therefore the level of, output as a whole, which would be represented by a mechanical extension of the Marshallian "elasticity of demand" to the demand for the products of "industry as a whole." [39]   Yet it is just such a mechanical extension of a concept, properly applicable only to the product of a particular industry, to the products of industry as a whole, which is represented by the *General Theory's* method for passing from the "aggregate supply functions for particular firms or industries" to an "aggregate supply function for industry as a whole." [40]

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[39] See above, p. 161, and the reference to the *General Theory* given in n. 37 thereto; also p. 201, n. 124. It will be recalled, however, that Mr. Keynes's practice in this respect has unfortunately not always been followed by certain of his disciples. Cf. above, p. 163, n. 41, and the references there given.

[40] Cf. the *General Theory*, 25, where the "Aggregate Supply Function" for industry as a whole is written $Z = \Phi(N)$, and p. 44 of the same work, where it is said not only that "the aggregate supply function for a given firm" is given by the expression $Z_r = \Phi_r(N_r)$, but also that such an "aggregate supply function" may be written *"similarly . . . for industry as a whole"* (italics mine). Cf. the reference given at the end of n. 16 to p. 528, above, with respect to the *General Theory's* method for passing from the problem of supply "in a single industry" to supply in "industry as a whole"; also the *General Theory*, 282, where the same type of mechanical summation of the *"employment* functions" for each separate industry is used in order to obtain an "aggregate" "employment function." It may be noted here that this type of illicit extension, to "output as a whole," of a type of concept properly applicable only to a particular firm, or at best (and then only under severe limitations) to a particular industry, had

This is a usage for which no important precedent can be found in "traditional" writings on the subject.[41]   This, how-

been made by some of Mr. Keynes's disciples even before the *General Theory* was published. See, for example, the use of the concept of a "supply curve of consumption-goods in general" by R. F. Kahn, "The Relation of Home Investment to Unemployment," *loc. cit.*, 177 f.; also J. Robinson, "The Theory of Money and the Analysis of Output," *loc. cit.*, where it was asked (p. 24) why we should not "try what progress can be made by thinking in terms of the . . . cost of production [of "output as a whole"], *just as we have been taught* to think of the . . . cost of a *single commodity*" (p. 23; italics mine), and it was suggested that the use of the concept of an elasticity of supply of "output as a whole" presents "an interesting analogy with the traditional Theory of Value" (p. 25). Indeed, Mr. Keynes himself has given us reason to believe that he was led to the usage in question by the article of Mr. Kahn, in particular. See the comment, in Mr. Keynes's later article, "Relative Movements of Real Wages and Output," *Economic Journal*, XLIX (1939), 39 n., on Mr. Kahn as the writer "who first attacked" the problem with which Mr. Keynes was there concerned "in the same way as that in which . . . [the problem] of *particular* prices has always been handled" (italics mine).

    [41] This very fact makes an absurdity of Mr. Keynes's characterization of "the assumption of equality between the demand price of output as a whole and its supply price" as "the classical theory's 'axiom of parallels'" (*General Theory*, 21), as well as of his translation of "Say's Law" into the proposition that "the aggregate demand price of output as a whole is equal to its aggregate supply price" (*ibid.*, 26). Apart, indeed, from Mr. Robertson's use of the "old" Cambridge, "portmanteau" concept of an "elasticity of the effort demand for commodities in general" (*A Study in Industrial Fluctuation*, 200 n.), the only case known to me in which an attempt has been made by a writer other than Mr. Keynes to translate some of the issues associated with "Say's Law" in terms of "supply" and "demand" *schedules* is that of J. Viner, *Studies in the Theory of International Trade*, 198. On Mr. Robertson's usage, however, nothing need be added to what was said above, p. 143, n. 6, and p. 522, n. 2, with respect to the relation of the concepts involved to the problem of the *structure* of prices and of output. And with respect to Professor Viner's usage, it is of the first importance to observe (1) that Professor Viner himself *criticizes* the sponsors of something called "Say's Law" for *not* having stated their argument in terms of demand and supply *schedules;* and (2) that his own use of such schedules, instead of involving the concept of "demand prices" and "supply prices" for *output as a whole,* runs explicitly in terms of a *plurality* of demand schedules and supply schedules. For a further example of the way in which, in writings prior to the appearance of the *General Theory*, use was made of the concepts of "supply price" and "supply curves" in discussions of the theory of the forces affecting Output as a Whole, without the concepts *themselves* being applied directly to "Output as a Whole," see Pigou, *Industrial Fluctuations*, 167 f., 170, 173. In the light of these facts, and of the argument that follows in the text, it should hardly be necessary to comment at length on the criticism of earlier writers, by supporters of the *General Theory*, for having been unaware of "the danger of taking propositions that have been established as true when applied to sections of the economy and illegitimately apply-

ever, is entirely to their credit; for to have established such
a precedent would have been to fly in the face of objections
that can be raised at once upon the basis of principles ac-
cepted as fundamental in the best of the received versions
of *both* the "general" Theory of Value, on the one hand,
and the Theory of Money and Prices, on the other.   Spe-
cifically:

i. It is a fundamental methodological proposition of
"modern" versions of the *"general" Theory of Value* that
all categories with respect to "supply" and "demand" must
be unequivocally related to categories which present them-
selves to the minds of those "economizing" *individuals* (or
individual business *firms*) whose calculations, and actions
based upon these calculations, make the "supplies" and "de-
mands" realized in the market what they are.[42]   It is not
denied that it is permissible, in some cases, to make use of
categories which represent a summation of certain elements
involved in the calculations and "plans" of separate indi-
viduals.   It is insisted, however, that if such categories are
to help us to understand why realized market events are
what they are, they must in all cases refer to elements which
are significant to those whose decisions and actions bring
about these realized market events.

Thus, a "collective demand schedule" for a particular
commodity is significant in the calculations of entrepreneurs

ing them to the economy as a whole," and of the fact that "what is true
of a firm or a particular industry need not be true of the economy as a
whole" (so Lerner, "The Relation of Wage Policies and Price Policies,"
loc. cit., 158).

[42] See, for example, the discussion of the concept of a "Social Organism"
in Davenport, *The Economics of Enterprise,* 387 ff. (especially the satirical
comments [p. 391] on "the idiotic judgment of the aggregate social idiot,"
and the need for "a social insane asylum in which to confine the social
organism"). Cf. also Hayek, *Prices and Production,* 4: "It is on the as-
sumption of a knowledge of the decisions of *individuals* that the main
propositions of . . . economic theory are based" (italics mine). In this
connection, see Volume I, 417 f., 440 f., on the "cash-balance approach" as an
example of the application of the principles of "modern" value theory to
the problems of *monetary* theory. This, however, is by no means the only
instance in monetary theory in which emphasis has been placed upon the
necessity relating market actions to the type of calculation by individuals
which is the subject matter of so large a part of the "general" Theory of
Value. See, for example, what is said in Volume I, 449 f., and also what is
said above, pp. 465 ff.

who must consider the probable market response to a given change in the price of their product. Such a "collective demand schedule" is significant also in the explanation of the *demand* side of the market because the elements involved in it—namely, the possible prices of a given commodity and the amounts that individuals are prepared to take at those prices—are precisely the same as those involved in the demand schedules of the *individuals* whose decisions to purchase or not to purchase that particular commodity make the realized price and the realized sales of that commodity what they are. Similarly, the concept of a "collective" supply schedule for a particular commodity, as applied to a given *industry* producing that commodity, has significance *insofar as* the elements involved in such a "collective" supply schedule—namely, the possible prices of a single homogeneous commodity and the amounts of this commodity that will be supplied by individual firms at those prices—are the same as those involved in the supply schedules of the individual firms which constitute the industry.[43]

This, however, is precisely what cannot be said of Mr. Keynes's "aggregate supply schedule for the products of industry *as a whole*." [44] In a "business economy" such as

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[43] It is, indeed, a striking feature of recent developments within the "general" Theory of Value that they should have led to an increasing dissatisfaction with the concept of a supply schedule for an *industry* (as opposed to that of a supply schedule for a *firm*) precisely on the ground that the conditions required in order to give meaning to the concept are not usually present in the world we know, and that we must therefore build up our categories "from the elementary maximizing units (*firms* and households)," rather than on the basis of "vague, ill-defined, 'industries.'" In this connection, see especially R. Triffin, *Monopolistic Competition and General Equilibrium Theory*, 7 ff., 12 f., 49 f., 67 f., 78 ff., 88 f., 93, 141, 145 ff., 148, 170 f., 188; and cf. also the comment of Schumpeter, *Business Cycles*, 66 n. But if the application of a single supply schedule to a given *industry* is to be regarded as retrograde from the standpoint of the better versions of the "general" Theory of Value, what shall be said of its application to "industry as a whole"? Cf. also the following note.

[44] Cf. the comment of Professor Schumpeter on this aspect of the argument of the *General Theory*, in the *Journal of the American Statistical Association*, XXXI, (1936), 792 f., and particularly his comment on its retrograde character as compared with the position of "those writers who in the sixties of the past century criticized some of the tenets of what *to them* was 'classical' doctrine." "These," Professor Schumpeter points out, "knew perfectly that the old supply and demand apparatus renders its very limited service only if applied to individual commodities . . .

ours, it is the decisions of *individual entrepreneurs* (firms) with respect to supply which make "aggregate supply" what it is. If an "aggregate supply schedule" is to be related directly to these decisions, it must, like the "collective" supply schedule for a particular commodity, refer to the same elements as those contained in the supply schedules of particular firms (or "entrepreneurs"). In the latter case, the common elements are: assumed unit selling-prices for identical products (or assumed amounts of sales proceeds for given quantities of these identical products) and the amount of these identical products that the sellers thereof will be willing to sell at these assumed prices (or for the assumed amounts of sales-proceeds). In the latter case, also, it is not implied that the decisions which determine market action are concerned directly with the amount of inducement required to elicit an "aggregate supply" which would include products other than those which the makers of these decisions themselves supply: it is not implied, that is to say, that the process of "supply" of the heterogeneous products included in "output as a whole" is a process which presents itself to the mind of the decision-making entrepreneur for action by such an entrepreneur.[45] But this is precisely what is implied by Mr. Keynes's easy passage from the "aggregate supply function for a particular firm or industry" to an "aggregate supply function for industry as a whole." [46]

There is nothing in this argument, it should be observed,

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and that it either loses or changes its meaning if applied to comprehensive social aggregates."

[45] The reader will observe the relation of this argument to that presented above, pp. 449 ff., with respect to the differences in the logical basis for assuming a "tendency toward equilibrium" in the case of the particular *firm*, on the one hand, and a "tendency toward equilibrium" of the *system*, on the other.

[46] See, for example, the *General Theory*, 25, where it is suggested that whether "there will be an incentive to entrepreneurs to increase employment" will depend upon whether "the expected proceeds are greater than the aggregate supply price, i.e., if D is greater than Z"; here, indeed, the very "substance of the [Keynesian] General Theory of Employment" is regarded as capable of summary by the proposition that "the volume of employment [in "industry as a whole"] is given by the point of intersection between the aggregate demand function and the aggregate supply function." Cf. also p. 29, where the "essence of the General Theory of Employment" is stated (under point 5) in terms involving the "aggregate supply function."

which would justify the contention that in reiterating these elementary methodological principles underlying the "modern" Theory of Value, we are neglecting what may be held to be the essential element in Mr. Keynes's position—namely, an insistence upon the necessity for doing justice to the requirements of "*macro*economic," as well as "*micro*economic" analysis.[47]   Our argument is directed, not against Mr. Keynes's concern with macroeconomic analysis, but against his illicit extension, to the latter, of categories proper only to certain types of microeconomic analysis.   It is a proper task of economic analysis to show how processes representing movements of the system as a whole ("macroeconomic" processes) may be expected to affect the particular data upon the basis of which the decisions of individual entrepreneurs ("microeconomic" in character) are made. It is also a proper task of economic analysis to show how these microeconomic decisions, and the microeconomic actions to which they lead, in turn affect the macroeconomic processes which take the form of movements of the system "as a whole."   But the type of problem raised by the necessity for establishing a relation between these "microeconomic" decisions and these "macroeconomic" processes is not solved by the arbitrary introduction of an "aggregate supply function" and an "aggregate demand function" for industry *as a whole,* in defiance of the fact that neither of these "functions" deals with elements which enter directly into the calculations of the individual entrepreneurs whose "microeconomic" decisions and actions make "macroeconomic" processes what they are.   On the contrary, it must be said, of such an attempt at "solution," that it misconceives entirely the true nature of the relation between microeconomic analysis and macroeconomic analysis; and that it represents the use of a type of "value theory" so retrograde, from the methodological standpoint, as to destroy a large part of the basis for any claim that the *General Theory* represents a significant and fruitful "synthesis" between that part of the "general" Theory of Value which is represented by the Theory of the Firm or Industry, on the one hand,

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[47] On the relative rôles actually assigned to the two types of analysis in the analytical system presented in this work, see what is said above, pp. 498 ff.

and the Theory of Money and Prices, and of Output as a Whole, on the other.

ii. It is equally important to observe, moreover, that the procedure recommended in that part of the *General Theory* which is here under discussion is just as retrograde when judged from the standpoint of an adequate *Theory of Money and Prices* or of *Output as a Whole,* as it is when judged from the standpoint of an adequate "general" Theory of Value.

That this is true will become clear if we ask what light is thrown upon the theory of the way in which monetary expansion and contraction affect the level of output as a whole, by the use of the concept of an "aggregate supply function" for industry as a whole, or an "elasticity of production" for industry as a whole. What these concepts tell us is that aggregate output and employment may be expected to vary in different degrees in response to the amount of money proceeds expected by entrepreneurs. But the mere statement that increased money-spending, and entrepreneurial expectation of further money-spending, has often had a stimulating effect on business has been a commonplace in writings on Money ever since the days of the mercantilists; and if an advance has been registered over these early crudities with respect to the effect of increased money-spending (and the expectation of increased money-spending) upon the level of output as a whole, it has been represented by an insistence upon the development of analytical categories which help us to understand *why* the effects commonly attributed to increased expenditure (or the expectation of increased expenditure) are likely to come about.

In our own day, to be sure, certain admirers of the *General Theory* have been content to regard the alleged connection between the total of money-spending and the level of business activity, particularly insofar as the former may result in "rising *prices,*" as merely a "brute fact," the "causal" explanation of which need not seriously engage our attention.[48] Fortunately, however, for the present state of the Theory of Money and Prices and of Output as a Whole,

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[48] See, for example, Harrod, *The Trade Cycle,* 39 ff.; and cf. the comment of Hawtrey, *Capital and Employment,* 327. See also the reference to R. F. Kahn in the following note.

non-Keynesians as a group have neither imitated this confession of surrender, nor adopted the analytical nihilism it implies. On the contrary, they have insisted on attempting to explain why an increased volume of money-spending has in some cases led to an increase in output, and in other cases not; why a rise in prices has in some cases had a stimulating effect upon output, and why in other cases it has not.[49] And the conclusion, in every case, has been that the explanation is to be found in the effect of monetary expansion and contraction upon the *structure* of money prices, and particularly upon the structural relations between *selling prices* and *costs,* and therefore upon the relations between selling prices and costs "expected" by individual entrepreneurs in different parts of the economic system.[50]

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[49] It is of considerable importance to observe that the attempts at explanation made by the best of earlier writers have really been attempts at *explanation,* and not statements which would merely translate the problem to be solved in terms of "the slope of the supply curve." Contrast, in this connection, Kahn, "The Relation of Home Investment to Unemployment," *loc. cit.,* 178 ff. I have been unable, moreover, to find in the earlier literature on the effect of monetary expansion. upon the level of output as a whole, any statement as crude as that of Mr. Kahn, to the effect that "it is impossible to maintain at the same time that prices will rise and that there will be no increase in output" (*op. cit.,* 179). It will be observed that, quite apart from the confusion of a *supply* curve with an *output* curve (see below, pp. 553 ff.), on which such a statement necessarily rests, the possibility that prices may rise *without* an increase in "output" is one that is frankly recognized by Mr. Keynes (see, for example, the *General Theory,* 295 f.). Indeed, the possibility is tacitly admitted by Mr. Kahn himself when he evidences a willingness to consider at least the possibility of either a "completely inelastic" supply of "consumption-goods" or "fixed" "*total* employment" (*op. cit.,* 181).

[50] Contrast, in this connection, the implication, by Mr. Kahn, that "a rise in prices" is necessarily associated with "an increase in *profits*" ("The Relation of Home Investment to Unemployment," *loc. cit.,* 187). A full discussion of the possible effects of monetary expansion and contraction on the structure of prices, and especially on the structural relation between costs and selling prices (and therefore on "profits" and their distribution), must obviously be left for my later publication on *Money and Production.* I need add here only that an emphasis upon the importance of a study of the structural relation between costs and selling prices in no sense implies a lack of interest in the causes and consequences of changes in the level of "money *demand*" in the face of an *unchanged* relation between costs and selling prices. On the contrary, the two types of emphasis are indissolubly connected (see below, p. 562, n. 23, and p. 624). It need only be pointed out, for example, that the very posing of the problem of the

This, of course, is merely another way of saying that a cardinal element in the best versions of the Theory of Money and Prices and of Output as a Whole is their insistence that a concern with aggregates such as "general" money demand, or the "general" price level, or "output as a whole" should not be allowed to blind us to the importance of studying the nature of the forces determining the *structure* of "demand," of prices, and of output, and the mutual relations between these problems of *structure,* on the one hand, and changes in "aggregates" of the type thus indicated, on the other.[51] It would be unfair to the *General Theory* to fail to record the fact that it contains references (albeit of an extremely oblique character) to these problems of *structure.*[52] It is not

effects of an increased money demand in the face of an "unchanged" relation between costs and selling prices itself implies that the consequences of a change in "money demand," when there *are* changes in the structure of costs and selling prices, may be very different from the consequences that might follow if there were *no* changes in the structure of selling prices and costs. For this proposition is all that is needed to justify the statement in the text.   Contrast Lerner, "Some Swedish Stepping Stones in Economic Theory," *loc. cit.,* 585, where a transference of emphasis (such as is found in Myrdal's *Monetary Equilibrium*) from movements in the absolute *"scale"* of money prices to movements in the *structure* of money prices is characterized as "an emancipation of monetary equilibrium from *prices"* altogether, without any indication of a realization of the significance of changes in the *structure* of prices (and specifically in the structural relation of *costs* to *selling prices*) for any analytical system alleged to rest upon the propositions that "the number of people who find employment in a modern capitalistic society depends upon the *profitability to businessmen* of providing employment for them," and that "the profitability depends on the money demand for the goods and services that the people seeking employment are able to produce" (Lerner, *op. cit.,* 575; italics mine).

[51] An adequate history of the development of an awareness of the importance of these problems of *structure* for the theory of Output as a Whole would of course be of monographic dimensions. I shall deal with the matter at some length in my later *Money and Production.* It is sufficient here to point to the type of emphasis to be found in Haberler, *Prosperity and Depression,* 359 f.

[52] For examples of a recognition, explicit or implicit, of the possible importance of studying what might be called the structure of *demand,* see, in addition to the quotation from the *General Theory* given above, p. 317, n. 207, the remarks on pp. 29 and 43 n. of the *General Theory,* together with the comment in the later article, "The General Theory of Employment," *loc. cit.,* 220 (cf. the comment on the latter passage by J. W. Angell, in *The Lessons of Monetary Experience: Essays in Honor of Irving Fisher* [1937], 84, n. 41). For examples of a recognition, explicit or implicit, of the possible importance of studying the structure of *output,*

unfair, however, to point out that an unquestioning accept-
ance of Mr. Keynes's formal method for dealing with the
relation between "aggregate supply functions for particular
firms or industries," on the one hand, and output as a whole,
on the other—namely, the method of mechanical aggrega-
tion of *particular* "aggregate supply functions" into a single
"aggregate supply function" for industry "as a whole"—
would effectively bar the way to a study of these problems
of *structure* and the true nature of the mutual interrelations
of these problems of structure with the problem of the de-
termination of the magnitude of significant economic aggre-
gates.   And it is not unfair to point out also that in failing
to take advantage of what was offered by the best available
versions of the Theory of Money and Prices and of Output
as a Whole with respect to the causes and consequences of
changes in the *structure* of money prices generally, and of
the relations between costs and selling prices in particular,
the argument of the *General Theory* with respect to "elas-
ticity of supply" is as retrograde on the side of monetary
theory as it is on the side of that "general" Theory of Value
with which the findings of monetary theory were to be
"synthesized." [53]

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or *employment,* see the *General Theory,* 43 n., 115 f., 286, 288, 296 (under
point 3), 300, 321, 379.   Over against such examples of recognition of the
possible importance of these problems of structure, on the other hand,
must be set a series of examples of an easy dismissal, explicit or implicit,
of the problems thus indicated. See, for example, the *General Theory,*
43 n., 45, 55, n. 2, 116, 245 f., 280 ff., 287; and for a general criticism of this
aspect of the argument of the *General Theory,* see Saulnier, *Contemporary
Monetary Theory,* 315 f., 355, 357, 379 f.

[53] See also what is said on this matter below, pp. 562 ff.

CHAPTER ELEVEN

Particular Supply Curves, Stream Equations, and the Determination of Money Prices

IF THE ARGUMENT of Chapter Ten is sound, it follows that (1) "particular" (Marshallian) supply schedules for particular commodities must continue to play an important rôle in any adequate "synthesis" of the Theory of Money and Prices with the "general" Theory of Value; but that (2) the rôle to be assigned to these "particular" supply schedules, or their analytical analogues, is not satisfactorily indicated in Keynes's *General Theory*. The failure of the *General Theory* in this respect, like its failure with respect to the treatment to be accorded to particular *demand* schedules in the theory of the determination of money prices, is the more regrettable in view of the fact that very serious gaps existed in the received treatment of the problem.

As in the case of particular demand schedules, moreover, one of the most important of these gaps was represented by a failure, all too frequently, to establish an unequivocal relation between these particular supply schedules and the *"stream equations" of monetary theory;* or, if one prefers, to those sectors of the Theory of Money and Prices and of Output as a Whole which are concerned with the nature of the forces determining the amount and orientation of the streams of objects (including objects "produced," or "Output") sold against those streams of monetary expenditure whose magnitude and direction, in combination with the magnitude and direction of the streams of objects sold for money, make realized money prices what they are. The 22 propositions presented in Chapters Five to Seven of the present volume represent, it is hoped, a contribution toward the filling of this gap; and a further contribution may be

represented by the following propositions, which have par-
ticular reference to the *supply* side of the problems.

XXIII. According to our Proposition II (p. 240), a real-
ized price, in a fully developed money economy, represents
the passage of money for an article sold for money.   Accord-
ing to Proposition II, also, the "passage of money for articles
sold for money" is precisely what constitutes the subject
matter of those aspects of the Theory of Money and Prices
(as summarized, for example, in the "stream" equations of
monetary theory) which undertake to explain why the
dimensions of the stream of money which "passes" for a
given commodity or group of commodities are relatively
large at one time and relatively small at another.   But the
abler users of these "stream equations," or their analytical
equivalents, have also made it clear from the very begin-
ning that, if we are to understand the nature of the forces
establishing the scale and structure of money prices, any
analysis of the forces which make the stream of *money ex-*
penditure as large as it is must be supplemented by a body
of analysis designed to account for the size and the orienta-
tion of the stream of *objects sold against this money ex-*
penditure.[1]   This, of course, is merely another way of say-
ing that the theory of the forces determining the amount
and the nature of the *objects sold for money* is, and must
always be, an integral part of the subject matter of the
Theory of Money and Prices.[2]

XXIV. From Proposition III (p. 240), however, we
know also that the problem of explaining why realized
prices are what they are also constitutes the subject matter
of that part of the "general" Theory of Value which is
built upon the proposition that a given realized price is what
it is as the result of the conformation and position of the

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[1] See Volume I, 93 ff., of the present work.

[2] Contrast the statement of Kaldor, quoted above, p. 344, n. 67; and
see also what is said concerning "The Rôle of Output in the Theory of
Prices" in Volume I, 599, of the present work.   The reader is reminded
that one of the principal reasons for postponing to the later volume of
Money and Production the material there announced for the present
volume (cf. also p. xi of the Preface to Volume I) is the desire to devote
an amount of attention to this problem commensurate with its importance.
See the Preface to the present volume.

market demand curve and the market supply curve prevailing at the moment the price is realized. The fact, therefore, that the determination of a realized price involves the presence of a market *supply* curve as well as of a market *demand* curve means that the sectors of the "general" Theory of Value which undertake to explain why these market *supply* curves are what they are, are as relevant to the determination of money prices as are the sectors of the "general" Theory of Value which undertake to explain why market *demand* curves are what they are.[3]

XXV. By Proposition IV (p. 263), it was shown that formal consistency is established between Proposition II (with respect to the representation of the realization of money prices by the use of stream equations) and Proposition III (with respect to the representation of the realization of a money price as resulting from the intersection of market demand and supply curves) by the use of expressions of the general form $D = pq$, in which D represents the *amount of money spent* by the "demander" of a given quantity of commodity q in the purchase of q, and p represents the money price per unit of that commodity. The relevance of this proposition to the supply side of the problem is established by the fact that to say that the "quantity of commodity q" is *purchased* is to say simultaneously that it is *sold*—that is, *supplied*.[4]

XXVI. By Proposition V (p. 274), it was established that whether we regard the expression $D = pq$ as referring to Proposition II or to Proposition III, the D in this expression is to be taken only as a chapter heading for analysis

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[3] See, for example, what is said above, pp. 255 ff., with respect to "the alleged absence of a market supply curve"; and cf. also what is said with respect to the supply side of the problem in (1) our discussion of "period analysis" (above, pp. 397 ff.); in (2) our discussion of the use of the analytical system here outlined to represent either a system functioning in "equilibrium" or one subjected to disruptions of "equilibrium" (above, pp. 429 ff.); and in (3) our description of the three-dimensional model which may be taken as a graphic representation of the analytical system here described (pp. 480 ff.).

[4] On the suggestion that the statement that the quantity purchased is always equal to the quantity supplied is not only a "truism," but is necessarily a *meaningless* "truism," see what is said above, p. 244, nn. 45 and 47. Cf. also the following note.

of an extremely elaborate character. Precisely the same thing must be said with respect to the q of this expression: this q, by our Proposition XXV, being taken to represent quantity actually *supplied* as well as actually "demanded." And just as the very great differences of subject matter covered by Propositions II and III, respectively, make it necessary to use *both* bodies of analysis if we are to explain why D is as large as it is at one time and as small as it is at another, so these differences make it necessary to use both bodies of analysis in order to explain why quantity *supplied* (q) is as large as it is at one time and as small as it is at another.

XXVII. Specifically, the concept of "supply" which bulks largest in most versions of the "modern" theory of value is the concept of "supply" as a *function of price;* that is, $q = \Phi(p)$, which is the general expression for the supply schedule for a particular commodity.[5] From our Proposition XXIV (p. 550), it follows that the conditions of supply which are summarized by the expression $q = \Phi(p)$ constitute one of the elements which will determine the magnitude of the q of our expression $D = pq$, whenever the latter is regarded as applying to the determination of the price of a particular commodity. And, since all recorded money prices are the prices of "particular" commodities, this, in turn, amounts to saying that *the whole of that part of the "general" Theory of Value which undertakes to establish*

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[5] The relation between $D_p = F(p)$ and $q = \Phi(p)$, on the one hand, and expressions of the general type $D = pD_p = pq$, on the other, is easily established. The separate use of F and Φ, respectively, is intended to indicate that there is no reason to suppose that the demand curve and the supply curve will coincide throughout, or even over a portion of, their respective lengths. (On the possibility of such coincidence, and the method of dealing with the relevant cases, see what is said above, pp. 247 ff.) What *will* "coincide" will be the particular *values* of $D_p = F(p)$ and $q = \Phi(p)$ involved in *realized* purchases and sales. Since our expression $D = pq$ always refers to *realized* transactions, it follows that in all cases it will be possible to substitute the value of D_p *in the expression* $D = pD_p$ for the q *in such an expression,* and *vice versa.* There is, therefore, no inconsistency in referring to the q of such an expression simultaneously as the quantity *demanded,* as when D_p is substituted for q in the expression $D = pq$, and as quantity *supplied,* as in the text above. Cf. Fisher, *The Purchasing Power of Money,* 180: "The Q's are the quantities finally sold by those who supply, and bought by those who demand."

the nature of the forces determining the form of the function $q = \Phi(p)$ must constitute an essential part of the theory of the determination of money prices.

XXVIII. It must be remembered, however, that Proposition XXVII is to be interpreted in the light of our Proposition I (p. 222), according to which the ultimate goal of any Theory of Prices is to explain why *realized* prices are what they are. To say this is to say simultaneously that our ultimate goal must be to explain why realized *sales* are what they are; and that categories such as "output" (or "production"), "costs" of production, particular elements in cost of production (such as "wages"), and even *"supply prices"* themselves, are to be introduced into the argument only insofar as they help to explain why prices actually realized on the market, and the amounts of sales of goods and services actually realized at these prices, are what they are.

XXIX. The corollaries to which we are led by an acceptance of Proposition XXVIII are corollaries the truth of which has long been recognized within the best versions of the "general" Theory of Value.[6] The truth of these corollaries has long been recognized also in the best versions of the Theory of Money and Prices, which may thus be said to have provided a continuing *control* over the results obtained within the "general" Theory of Value. The corollaries in question are:

1. The amount *supplied* (either in the sense of the amount *offered for sale,* or in the sense of the amount *sold*) in any given time period is not necessarily the same thing as the amount *produced* in that time period. Within the "general" Theory of Value, this has been recognized by the

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[6] The evidence for this statement is provided in connection with the discussion, which follows, of each of the corollaries in question. It need be pointed out here only that the failure of the Keynesian apparatus to do justice to these recognized achievements within the "general" Theory of Value, and the reversion of the Keynesian apparatus, at several points, to a type of reasoning characteristic of that "labor theory of value" with which Mr. Keynes avows his sympathy (see above, p. 534, n. 28, and p. 535, n. 32, and below, pp. 566 ff.), itself suggests that the Keynesian "synthesis" of the "general" Theory of Value with the Theory of Money and Prices may be taken as providing a further illustration of what was presented in Chapter Three of this volume (p. 125) as the third of our "Lessons of Doctrinal History."

standard "Marshallian" distinction between "short-" and
"long-period" supply, on the one hand, and, on the other,
by the general concept of "reserve prices," with all that the
latter involves with respect to the rôle to be assigned to
the element of "expectation." [7] Within the Theory of

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[7] The passages in Marshall dealing with the distinction between "short-"
and "long-period supply," and the distinction between "supply" and
"production" which follows from the first distinction indicated, are, of
course, thrice-familiar (see, for example, Marshall's *Principles,* 330 ff.,
372 ff., 455 ff., 660). It need be pointed out here, therefore, only (1) that
Marshall himself traced his distinction between supply in the "short-
period" and supply in the "long-period" to the "classical" economists,
and to Adam Smith in particular (see above, p. 27, n. 70); and (2) that,
despite Mr. Keynes's criticism of "economists" for having failed to use, in
their theories of Money and Prices, the concept of an "elasticity of *short-
period* supply," which, as he correctly asserts (*General Theory,* 292), has
"played a prominent part" in the "general" Theory of Value, his own
references, in the *General Theory,* to the problems of "short-period sup-
ply" have to do with the "short-period supply price" of additional *output,*
and not to the kind of "supply price" in a "dealers' market," which Marshall
emphasized particularly in his account of the forces determining "short-
period supply." See, for example, the *General Theory,* 67 f., 328; and
contrast Marshall's *Principles,* 456. That the concept of "reserve prices,"
and its association with the element of "expectation," should have bulked
large, particularly in theories of "subjective" value, is hardly surprising,
in view of the greater emphasis placed by such theories upon the short-run
aspects of the pricing process. See, for example, (1) Fleeming Jenkin,
The Graphic Illustration of the Laws of Supply and Demand, 96, and the
further references to Jenkin given above, p. 184, nn. 84 and 85; (2) the
references to Wicksteed given above, p. 256, n. 76, and p. 188, n. 95; and (3)
Auspitz and Lieben, *Untersuchungen über die Theorie des Preises,* 267 ff.,
304 ff., 452, and the further references given above, p. 189, n. 97. It would
be a serious mistake, however, to suppose that a recognition of the im-
portance of "reserve prices," with all that such recognition implies with
respect to the element of "expectation," on the one hand, and the distinc-
tion between "supply" and "production," on the other, is to be found only
in the writings of "modern" sponsors of an outspoken theory of "subjective"
value. See, for example, (1) the comment in the next to the last paragraph
of the chapter "Of Market Prices" in Cantillon's *Essai,* 159 (p. 121 of
Higgs's translation); (2) Ricardo's illustration, in Chap. XXI of his *Prin-
ciples,* of the proposition that prices "may vary also, as we have already
shown, from the alteration in the proportion of supply to demand, although
there should not be [that is, even if there is not] either greater facility
or difficulty of *production,*" by reference to the case of the manufacturer
who, confronted by a fall in prices owing to monetary or non-monetary
causes, "naturally accumulates an unusual quantity of finished goods,
being unwilling to sell them at very depressed prices"—the final result
depending on whether or not "the manufacturer's expectations were well
grounded" (p. 282 of Gonner's edition of Ricardo's *Principles*); (3) the
detailed discussion in Tooke's *History of Prices,* V, 166 ff., of the influence

Money and Prices, it has been recognized by (1) the distinction between "output transactions" and "non-output transactions"; (2) the distinction between "output" and "transactions in output"; (3) the distinction between "goods produced" and "goods intended for sale"; (4) the explicit introduction of the concept sometimes called "the velocity of circulation of goods," which includes (a) the "number of middlemen's sales"—that is, the phenomenon of a repeated offering for sale (repeated "supply") of goods already "produced"; and (b) the "rate of sale," which represents the analogue, within monetary and business-cycle theory, of a good part of the range of phenomena with which, within the "general" Theory of Value, the concept of "reserve prices" is designed to deal.[8] On the other hand,

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of traders' practices with respect to the holding of stocks, in accordance with their expectations as to the future course of prices (see, in addition, the references to Tooke given above, p. 181, n. 74). Cf. also F. W. Newman, *Lectures on Political Economy* (1851), 65, 70; and Sidgwick, *The Principles of Political Economy*, 193 ff., with its "Hawtreyan" emphasis upon the effect of interest charges upon the holding of inventories (see also the references to Sidgwick given above, p. 188, n. 94). The concept of "reserve prices" and a full recognition of their relation to the element of "expectation" is of course implicit in Marshall's treatment of his "dealers' market" (see the references given at the beginning of this note).

[8] See Volume I, 538 ff., of the present work. References to earlier writers on the topics indicated will be found in my own earlier articles referred to in the pages indicated. The treatment accorded in Keynes's *Treatise* to the issues thus raised by an analysis of the components of the Fisherine *T* is discussed on pp. 525 ff., 564 ff., and 595 ff., of Volume I. Unhappily, the difficulties there raised against Mr. Keynes's treatment have not been resolved by the argument of the *General Theory*: on the contrary, Mr. Keynes's professed desire to allow "technical monetary detail" to fall "into the background" (see again the *General Theory*, p. vii) has resulted in a less satisfactory presentation than was to be found in the *Treatise*. This, it may be added, is particularly striking in view of the fact that it is precisely at this point that the Theory of Money and Prices can contribute heavily to the kind of "synthesis" with the "general" Theory of Value which Keynes's *General Theory* was supposed to represent: first, by providing the kind of analytical "control" to which reference was made above; and, secondly, by adding details with respect to institutional practices with which the "general" Theory of Value has not usually been concerned. The reader interested in a comparison of the treatment accorded to these issues in the *Treatise* and the *General Theory*, respectively, should compare (1) the discussion of "Keynes's *Treatise* on Transactions in Securities and the Price Level of Output," in Volume I, 595 ff., of the present work, on the one hand, and, on the other, the discussion of the *General Theory's* treatment of "the relationship between the marginal efficiency of capital and stock-market speculation"

it is precisely this distinction between the amount *"sup-plied"* in any given time period and the amount *"produced"* within that period, to which justice is *not* done by the easy substitution of an "elasticity of production" for the broader "elasticity of supply," when our problem is that of account-ing for prices *realized* and the amount of *sales realized* within a given historical ("clock-time") period.[9]

by P. T. Ellsworth, "Mr. Keynes on the Rate of Interest and the Marginal Efficiency of Capital," *Journal of Political Economy*, XLIV (1936), 778 ff.; (2) the discussion, in my Volume I, 509, 545 ff., of the *Treatise's* manner of dealing with the relations between the prices of "old" assets and of "newly produced" assets ("current output"), respectively, on the one hand, and, on the other, the treatment accorded to the problem in the *General Theory* (see, for example, pp. 223 ff.), particularly when the latter treatment is judged from the standpoint of the degree of articulateness with which it is related to the argument of the *General Theory* with respect to the process by which the price of current "output" is determined; (3) the evaluation, in Volume I, 565 f., of the *Treatise's* discussion of that component of the "velocity of circulation of goods" which is represented by the concept of a "number of middlemen's sales," on the one hand, and, on the other, the attempt of the *General Theory* to deal with some of the phenomena involved under the head of the concept of "user cost" (cf. above, p. 531, n. 24); and (4) the evaluation, in Volume I, 566 f., of the *Treatise's* discussion of the component of the "velocity of circulation of goods" represented by the concept of a "rate of sale" of goods, on the one hand, and, on the other, the treatment of the concept of "surplus stocks" in the *General Theory* (51 n., 70 f., 226, 288, 318 f., 331 f.). That a concept such as the "rate of sale" of the Theory of Money and Prices is definitely related to the "reserve prices" of the "general" Theory of Value was pointed out in my earlier articles (see, for example, the *Journal of Political Economy*, XL [1932], 486, n. 19 [cf. also the reference there given to A. A. Young]); and the same thing is true of the relation, to the concept of "reserve prices," of the treatment of the problem of "old assets" in the Theory of Money and Prices (see, for example, *Economica* for November, 1932, pp. 436 f.). For an example of a formal treatment of the problem of "old assets" in terms of the categories of general value theory, and with particular reference to the distinction between "supply" and "production," see Sidgwick, *Principles of Political Economy*, 207 f.

⁹ See the comment above, p. 554, n. 7, on the *General Theory's* treat-ment of "elasticity of short-period supply" as identical with the elasticity of *output* (or "production") over the "short" period; and cf. also the quotation from R. F. Kahn given above, p. 546, n. 49, with respect to the alleged inevitability of a rise in *output* as the result of a rise in realized prices, despite the fact that the only thing that is inevitably associated with a rise in realized prices is a rise in *market supply price*. It will be observed, moreover, that not all of the elements of "supply" included in certain of the components of the Fisherine *T* are concerned exclusively with the "supply" of current *output*. Again, therefore, it should hardly be necessary to discuss at length the falsity of the antithesis involved in the proposition that analysis in terms of the Fisherine *T* and its components

2. Even with respect to that part of the "supply" offered for sale within a given time period which is derived from the current "production" of that period, it is not necessarily true that the market *supply* prices involved in the realized sales of this current "production" are identical with the *"cost"* prices for this part of the supply.[10] That the "cost prices" *may* be identical with the supply prices included in the market supply curves that are involved in the determination of realized prices, is not denied; nor would anyone deny that costs (and particularly *expected* costs) can be assumed to *affect* the supply prices included in these market supply curves.[11] But that the better versions of the "general" Theory of Value have always recognized the possibility of a divergence of market supply price from cost price will be clear if one considers the implications of (1) the "classical" distinction between "market" and "normal" value, and (2) the whole theory of monopoly value.[12]

is made unnecessary as long as we make use of the concept of "elasticity of supply" in the sense of an "elasticity" of *output*. Cf. the references to B. P. Whale given above, p. 465, n. 5, and p. 475, n. 28.

[10] No one would deny that it is possible to *define* "cost prices" in such a way as to make them equal under all circumstances to the market supply prices involved in realized sale-transactions. What is affirmed is that such a usage, for all its apparent sophistication, is such as to deprive the concepts of "market supply price" and "cost price" of most of their instructiveness in analysis of the type *with which we are here concerned*. Cf. Robertson, "A Survey of Modern Monetary Controversy," *loc. cit.,* 6 f. (*Essays in Monetary Theory,* 139). Of a careless identification of cost curves with supply curves, the examples are, of course, legion. For some general objections to the practice, see J. Viner, "Cost Curves and Supply Curves," *Zeitschrift für Nationalökonomie,* III, (1931), 23.

[11] The emphasis upon the fact that the "costs" involved are *"expected"* costs has of course been a cardinal element in theories of "subjective" value (including those which were advanced in explicit opposition to the value theory of the "classicals"), and therefore in the textbook formulations of these "subjective" theories of value. For examples of the latter, see Fisher, *Elementary Principles of Economics,* 310 f., and Davenport, *Economics of Enterprise,* 69 ff. This circumstance in itself provides a striking commentary upon the type of treatment of the relation between realized "costs" and selling prices (realized "profits"), on the one hand, and the further realization of prices and incomes, on the other, which is found in Keynes's *Treatise* (cf. Volume I, 127, n. 62, of the present work), as well as in certain parts of the argument of the *General Theory.* On the latter point, see what is said below, pp. 613 f.

[12] I have preferred to cite the two "classical" instances indicated, in order to avoid controversy as to whether the abler among the "classical" economists ever did hold the particular views with respect to the "govern-

Within the Theory of Money and Prices, on the other hand, the causes and consequences of *divergences* between the realized selling price of a given commodity (and therefore the market *supply price* for that commodity), on the one hand, and the "cost" of producing that commodity, on the other, has been regarded as a central element in that part of monetary theory which is concerned with the effects of monetary expansion and contraction upon the level and structure of output as a whole.[13]   The suggestion, therefore,

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ing" of "prices" by "costs" that some of the "revolutionaries" of the 1870's, and the "rebels" of earlier date, attributed to them. The accusations levelled against the classical writers, in this connection, by writers from Malthus and Macleod to Jevons and Wicksteed, are familiar; and so are the standard defenses offered on behalf of the "classicals" by writers ranging from Tooke (*History of Prices*, V, 176 ff.) to Marshall ("Mr. Mill's Theory of Value" [*Memorials*, 127 ff.], and *Principles*, 819 ff.), and even to a writer as generally sympathetic to theories of "subjective" value as Professor Robbins (*An Essay on the Nature and Significance of Economic Science*, 78, n. 1). The really interesting aspect of these defenses, however, is that all of them amount to an insistence that the "classical" writers were quite aware of the possibility of a divergence of market supply price from cost price. And even if these defenses of the "classical" writers should not be regarded as convincing, it would still be true that the very fact that the authors (including Alfred Marshall) who undertook this defense showed, by that very fact, that *they* did not regard prices as "governed" by "costs" in the sense in which the "classical" writers have been criticized for having regarded them as being "governed." In any case, I cannot see how it is possible to dispute the contention that in the two cases indicated in the text (namely, the case of "market" value, as opposed to "normal" value, and the whole theory of monopoly value), the "classical" writers made it quite clear that a divergence of market price from "cost" price was possible. Contrast the position of the *General Theory*, as described even by *supporters* of its "general tenor" (see the quotation from Harrod given below, pp. 561 f., and n. 19 thereto).

   [13] The very fact that the point in question *has* been a central element in the theory of the effect of monetary expansion and contraction upon the level and structure of output as a whole, and the further fact that the literature upon the latter problem is of enormous dimensions means, of course, that an extended list of citations is out of the question here. Since, however, so large a part of the argument of the *General Theory* is directed against the "classical" theory of "old" Cambridge, attention may be called to the discussion in Pigou, *Industrial Fluctuations*, 167 ff., of some of the reasons for, and the consequences of, the fact that market supply prices may differ from the supply prices that would correspond to "the short-period norm" represented by "marginal prime expenses of successive quantities of output." From the point of view of the history of the application of the concepts of the "general" Theory of Value to the theory of Output as a Whole, the passage is striking, also, by reason of (1) its use of the concepts of "supply price" and "supply curves" in a discussion of the forces affecting the level of Output as a Whole, without directly

that "economists" should have applied, to monetary theory
and the theory of output as a whole, the proposition that
realized prices are immediately and exclusively "*governed
by marginal prime cost*," or any other kind of "cost," repre-
sents at once a complete misreading of what we have been
"taught to believe" by "the general tenor of the classical
theory" with respect to the rôle played by "costs" in the
determination of realized prices, and a serious retrogression
as compared with the best of the "classical" versions of both
the "general" Theory of Value and the Theory of Money
and Prices.[14]

applying these concepts to "Output as a Whole" (see above, p. 540, n. 41);
(2) its explicit emphasis upon the element of *expectation* as a factor affect-
ing the level of the market supply curve, and therefore its position relative
to the relevant *cost* curve (Pigou, *op. cit.*, 170 f.); and (3) its explicit in-
troduction of the "institution" of *monopoly* as a factor affecting the posi-
tion of the market supply curve (Pigou, p. 171; cf. above, pp. 257 ff.).

[14] The statements quoted as to what "the general tenor of the classical
theory . . . has taught us to believe" with respect to the "governing" of
"prices" by "marginal prime cost" are from the *General Theory,* 12 (cf.
also, however, what is said below, p. 584, n. 73, with respect to Mr. Keynes's
acknowledgment of the influence of Mr. Kahn on the-point in question).
On p. 292 of the *General Theory,* to be sure, it is stated merely that
"changes in marginal cost . . . have *played a prominent part*" in the doc-
trine (as expounded within the "general" Theory of Value) that "prices are
governed by the *conditions of supply and demand.*" It is clear, however,
that the statement first quoted represents *Mr. Keynes's* understanding of
what the "part" played by "changes in marginal cost" in the determination
of realized prices has been in the best of received versions of the "general"
Theory of Value; and it is equally clear that his followers have adopted his
own application of this perversion of "classical" doctrine to the problem of
the determination of realized money prices. See, for example, J. E. Meade,
"A Simplified Model of Mr. Keynes' System," *Review of Economic Studies,*
IV (1937), where, although the condition that the "prices" of goods will
be equal to their "marginal prime cost" is first laid down only as a condi-
tion determining "the position of short-period *equilibrium*" (p. 99), the
argument is later (p. 103) applied to "the effect on employment" of changes
in certain variables, and it is then assumed that a reduction of "marginal
prime cost" by 10 per cent would lead to a fall in "the price of all com-
modities . . . by 10 per cent"; and see also H. W. Singer, "Price Dispersion
in Periods of Change," *Economic Journal,* XLVIII (1938), 659, where it is
proposed that "we conceive of prices as determined by the scale of output
and the *rate of remuneration of the different factors of production which
enter into marginal cost,*" and where a reference is given to Chap. 21 of the
General Theory ("The Theory of Prices") in support of this "conception"
of the process of price determination. It is passages such as these which,
when taken together with passages such as those quoted below, p. 584, n. 74,
and p. 592, n. 91, provide support for the interpretation of "Keynes and
his followers" as arguing that "prices are established without direct refer-

The episode represented by the *General Theory's* position on the issues under discussion, is, indeed, another one of those episodes which make us wonder what could have happened to the celebrated "oral tradition" at old Cambridge, if Mr. Keynes could actually bring himself to think that he was "taught to believe" propositions of the kind which he thus proposes to foist upon "the classical theory." The record of the *written* "tradition," at any rate, is unequivocal. For it was Alfred Marshall who, in the preface to his *Principles,* advanced, as a "universal rule," the proposition that "marginal costs do *not* govern price." [15]   And it was Alfred Marshall who, in the same preface, laid down the further "universal rule" that while it is true that "it is only at the margin that the action of those forces which do govern price can be made to stand out in clear light," it is also true that "the margin which must be studied in reference to *long* periods and enduring results differs in character as well as in extent from that which must be studied in reference to short periods and to passing fluctuations." [16]   Surely it is a fair deduction from the latter proposition, in particular, that Marshall intended to remind his readers of his insistence, elsewhere in his *Principles,* that the rôle played by "costs" in the determination of price might be expected to be greatly different in "long" periods from what it is in those "short" periods within which alone is taken the market action which leads to the actual *realization* of market prices.[17]   It is certain, at any rate, that others who were subjected to the influence of the "oral," as well as the written, tradition of "old" Cambridge, did not come out with the extraordinary conclusions, with respect to what they had been "taught to believe" on the point under discussion, with which Mr. Keynes seems to have emerged. Mr. Robertson, for ex-

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ence to the effective quantity of money (*MV*) by the cost functions of individual goods," and that indeed "the general doctrine of his [Keynes's] book seems to be that the price-level has no determinants separate from costs to a single industry" (so H. S. Ellis, "Some Fundamentals in the Theory of Velocity," *Quarterly Journal of Economics,* LII [1938], 431, 468).

[15] See p. xvi of the eighth edition of Marshall's *Principles* (italics mine). In the light of this passage, and of the general discussion which follows, the reader must be left to provide his own comment upon the statement by Mr. Keynes, in his later paper on "Relative Movements of Real Wages and Output," *loc. cit.,* 46, that "it is rare for *anyone but an economist* to suppose that price is predominantly governed by marginal cost" (italics mine).

[16] Marshall, *loc. cit.* (italics mine).

[17] On the possibility of *defining* "costs" in such a way that every realized price will always be equal to marginal "cost," see what is said above, p. 557, n. 10. For our present purpose, it is necessary to point out only (1) that Keynes's distinction between "short-period" supply price and "long-period" supply price turns, not upon the relative rôle played in each by *costs,* but upon the *elements* of "cost" that may be expected to be included in the two types of "supply price," respectively (see the *General Theory,* 270 f., and cf. also above, p. 554, n. 7); and (2) that the "costs" thus included in Keynes's "short-period supply price" are *not* such as to make every realized price equal to "marginal cost" *by definition.*

ample, has insisted, in a manner completely consistent with the written Marshallian tradition, that "to say that price is determined by marginal cost is always bad theory."[18] And it is clear that even some of those, among Mr. Keynes's followers, who avow no explicit allegiance to "classical" theory in any of its variants, have been troubled both by Mr. Keynes's reading of the "general tenor" of that theory and his application of this "general tenor" to the theory of the determination of realized money prices. This much is evident from the treatment by Mr. Harrod, for example, of the proposition that "the price of each commodity is determined by the marginal money cost of production," and the further proposition, "still more crude and common," that "prices . . . vary not merely in proportion to changes in the number of units of factors required per unit of output, as output varies, but also in proportion to changes in rates of reward to the factors"—both propositions, as Mr. Harrod points out, representing "precisely what Mr. Keynes supposes actually to happen."[19] According to Mr. Harrod, on the other hand, the propositions in question correspond, not to "the true classical theory of cost of production," but to "the crude way that

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[18] Robertson, "A Survey of Modern Monetary Controversy," *loc. cit.*, 6 (*Essays in Monetary Theory*, 139).

[19] Harrod, "Mr. Keynes and Traditional Theory," *loc. cit.*, 81. Mr. Harrod notes, to be sure, that Mr. Keynes's proposition is "subject to the qualification that the equality [between marginal cost and price] may be disturbed, in accordance with certain principles, if competition and markets are imperfect." The reference given by Mr. Harrod is to p. 5 of the *General Theory*. In that passage, however, Mr. Keynes was explicitly discussing, not the relation between "marginal cost and price," but the relation between "the wage of an employed person" and "the marginal product of labor"—or "the value which would be lost if employment were to be reduced by one unit (after deducting any other costs which this reduction of output would avoid)." The passages in the *General Theory* which are to be regarded as qualifications of the proposition with respect to the "equality between marginal cost and price" on the ground of "imperfections" in "competition" and "markets" are those with respect to "administered" and "monopoly" prices (*General Theory*, 268, 270), to which reference was made above, p. 468, n. 14; cf. also Mr. Keynes's later paper, "Relative Movements of Real Wages and Output," *loc. cit.*, 46 ff.). That a serious blow is delivered to the thesis with respect to the "equality between marginal cost and price" by this particular concession will be admitted even by those who are not prepared to accept the type of overenthusiastic generalization with respect to the omnipresence of "monopoly" that has characterized some of the discussions of "general" value theory in recent years. It should be clear, however, that this blow is as nothing when compared with the blow that is administered by the simplest considerations with respect to the "classical" distinction between "market" and "normal" value, even under conditions of "competition," and all that this distinction can be made to imply in the light of the findings of *monetary* theory with respect to the divergence between realized prices and "cost" prices during periods of monetary expansion and contraction. See the following paragraphs of the text.

a tiro might describe" the matter, "erroneously supposing himself to be explaining the true classical theory." [20]

But if the statement that "the classical theory has taught us to believe that prices are governed by marginal prime cost in terms of money" is a statement one would expect only from a "tiro" within the *"general"* *Theory of Value*, and particularly from one who had not thoroughly understood the rôles to be assigned to "costs" in the short and in the long runs, respectively, what shall be said of such a statement when it comes from one who might be presumed to be familiar with the teachings of *monetary* theory? Indeed, what shall be said of it when it comes from the author of the *Treatise on Money*, with its central emphasis upon "profit inflations" and "profit disequilibria"—that is, upon *discrepancies* between costs and selling prices—as the "mainspring of change in the existing economic system"? [21]   No one could assert that the *details* of the *Treatise's* argument with respect to the nature, the causes, and the consequences of such discrepancies between costs and selling prices were in all respects satisfactory.[22]   Nor could anyone assert that the *Treatise* itself succeeded in establishing (or, indeed, even attempted to establish) a satisfactory analytical relation between its own emphasis upon discrepancies between "costs" and "selling prices," on the one hand, and, on the other, that emphasis upon the causes and consequences of changes in the level of "aggregate" (or "general") money "demand" which is found in the *General Theory*.[23]   Yet one has only

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[20] Harrod, *loc. cit.* From the general tone of Mr. Harrod's argument, the reader is led to suppose that Mr. Harrod believes that Mr. Keynes's own use of the curious perversion of "classical theory" thus indicated is somehow free from all the faults which would attach to it in the hands of a "tiro." This, however, is a matter which each reader must settle for himself on the basis of the discussion which follows. In this connection, see especially what is said below, pp. 566 ff.

[21] See the *Treatise*, I, 140; and on "profit inflation and deflation," or a "profit disequilibrium," as equivalent to the difference between costs and selling prices, see the *Treatise*, I, 138, 151 ff., 207 f., and II, 90, as well as the references in the index to that work (II, 416) under "Inflation, Profit."

[22] See, for example, what is said in Volume I, 277 ff. of the present work (as well as the references to other writers given in nn. 26 and 27 thereto), with respect to (1) the *Treatise's* emphasis·upon *aggregate* profits, at the expense of an emphasis both upon the *distribution* of profits and upon the concept of "marginal returns (in respect of a given outlay)"; (2) the *Treatise's* failure to distinguish adequately between *expected* "profits," on the one hand, and *realized* "profits," on the other (cf., in this connection, the comment of Mr. Keynes himself, in the *General Theory*, 77 n.); and especially (3) the unhappy attempt, in the *Treatise*, to combine in a single algebraic expression a "stream" formulation with a "cost-profit" formulation. On the latter point, in particular, see what is said above, pp. 439 ff.

[23] A full discussion of this analytical relation must be left for another occasion. That an analytical relation between the two *must* and *can* be established, however, should be evident from these simple facts: (1) that the amount of "aggregate" money demand will itself be affected by decisions to "invest" or to refrain from investing, with all that this means with

to compare the treatment of the problem of the causes and consequences of discrepancies between costs and selling prices ("profits") which one found in the *Treatise,* despite all its inadequacies, with the treatment of the same problem which one finds in the *General Theory,* to be convinced that in this respect, as in others, the argument of the *General Theory* represents a retrogression as compared with the monetary theory not only of non-Keynesian writers, but also as compared with the earlier Keynes (of the *Treatise*) himself.[24]

Virtually all, in fact, that the *General Theory* has to say with respect to the issues raised (however inadequately they were *resolved*) in the *Treatise* with respect to the causes and consequences of a discrepancy between costs and selling prices is to be found in two brief passages in the former work.[25] In the first of these passages, Mr. Keynes undertook to relate the emphasis of the *Treatise* upon "changes of profit" to the emphasis of the *General Theory* upon "aggregate demand," by suggesting that there must be *some* relation between entrepreneurial "profit," on the one hand, and the "expected *proceeds*" of the *General Theory's* "aggregate demand function."[26] It is not necessary to comment here at length upon the difficulties raised by the fact that Mr. Keynes's "aggregate demand function" is not the type of "demand function" with which individual entrepreneurs are directly concerned.[27] Nor is

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respect to the amount of bank borrowing and the utilization or non-utilization of existing cash balances (the M' and the V of our general formulation); (2) that these decisions to "invest" or to refrain from investing will themselves be affected by the relation, actual and "expected," between *costs* and *selling prices;* (3) that the *effect* of a given change in "general [money] demand" upon output and employment will depend upon the height of the prices asked by sellers; and (4) that whether a given price will continue to be asked by prospective sellers over an extended period will depend largely upon the relation of such a price to other prices, and particularly to the structure of "costs" in relation to selling prices.

[24] In connection with the argument that follows in the text, compare what is said in Volume I, 139 f., of the present work.

[25] A third passage—namely, that on pp. 289 f. of the *General Theory*—is interesting chiefly by reason of its contrast with the type of reasoning in the second passage discussed below (namely, that on p. 283 of the *General Theory*), in which an attempt is made to dispose of the problem on the assumption that "the price is equal to the marginal prime cost." In the passage on pp. 289 f., it is pointed out that although "the conditions of strict *equilibrium* require . . . that wages and prices, and consequently profits also, should *all* rise in the same proportion as expenditure," there are "certain practical qualifications to this conclusion which must be borne in mind in applying it *to an actual case*" (italics mine). See also the references to "windfall losses and gains," as the terms are used in the *General Theory,* which are given below, p. 564, n. 29.

[26] *General Theory,* 77 f.

[27] A partial escape from these difficulties could of course be found if the argument were translated into terms of the D_{wr}'s of Mr. Keynes's "employment functions" for particular firms or industries (see above, p. 528, n. 16). The statement quoted in the following sentence of the text,

it necessary to comment here at length upon the fact that the individual entrepreneur is hardly likely to be interested in the relation between the amount of the "proceeds" he "expects," on the one hand, and the "various hypotheses," on the other hand, on the basis of which the amount of these proceeds can be supposed to result "from consumption and investment, respectively." [28] What it is necessary to point out is that the real problem is that of *relating* "proceeds," as defined in the *General Theory,* to "profits," in some sense of the latter term that would make them relevant, as they were supposed to be in the *Treatise,* to entrepreneurial decisions with respect to the expansion or contraction of output.[29] In the passage in question, however, we are told merely that an increase in the "profits" of the *Treatise* (or, in the language of the *Treatise* itself, an "increase of investment relatively to saving") may be taken as "a *criterion* of an increase in effective demand"—that is, as a "criterion" of the amount of "proceeds" expected by entrepreneurs.[30] This can hardly be regarded as a very precise statement with respect to the relation between expected "proceeds," on the one hand, and expected "profits," on the other. Indeed, if the

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however, is itself proof of Mr. Keynes's refusal to accept what were characterized above (pp. 541 ff.) as the methodological presuppositions of the "general" Theory of Value, as well as of his failure to provide an adequate bridge between the "microeconomic" and the "macroeconomic" aspects of his analysis (see above, p. 544). The same thing must be said of the context of the very passage (*General Theory,* 280) in which the expression D_{wr} was introduced. For in that passage, Mr. Keynes was prepared to dismiss all problems associated with the *structure* of "demand" with the statement that "if we are entitled to *assume* that D_{wr} is a *unique function of the total effective demand* D_w, the employment function is given by $N_r = F_r(D_w)$"—"that is to say, N_r men will be employed in industry r when effective demand is D_w."

[28] Cf. the *General Theory,* 77.

[29] Cf. the *General Theory,* 77 f., where Mr. Keynes, having pointed out that, in his *Treatise,* "the concept of *changes* in the excess of investment over saving, as there defined, was a way of handling changes in profit," pointed out also that he had "there argued that change in the excess of investment over saving was the motive force governing changes in the volume of output"; and, he went on to affirm, "thus the new argument, though (as I now think) much more accurate and instructive, is essentially a development of the old." Whether in fact "the new argument" is "much more accurate and instructive" with respect to the causes and consequences of changes in the relation between *costs and selling prices* ("profits") than the "old" one, with all its inadequacies, is precisely the question which the reader is here asked to decide. It need be added only that the establishment of a clear relation between the argument of the *Treatise,* on the one hand, and the *General Theory,* on the other, can hardly be said to have been greatly helped by the divergences (both with respect to definition and analysis) associated with terms such as "windfall losses" and "windfall gains," as they are used in the two works. Cf., for example, the usage and argument in the *General Theory,* 57 f., 92 f., 288, with that in the *Treatise,* I, 125.

[30] *General Theory,* 78.

statement quoted is meant to suggest that we may take changes in "profits" as an accurate *measure* of changes in "proceeds," and *vice versa*, it is not even formally correct under all circumstances. For it would rest entirely upon a very special set of assumptions with respect to the internal structure of costs and selling prices during periods of monetary expansion and contraction (and therefore during periods evidencing changes in the amount of money "proceeds," actual or "expected")—assumptions which might or might not conform to the reality in all concrete cases.[31]

Unfortunately, moreover, still less can be said for the discussion presented in the second of the passages in the *General Theory* indicated, despite the fact that this passage includes an algebraic expression which undertakes precisely to relate a given increment of "expected profit" to a given increment of "effective demand." [32] For this expression is made utterly useless by the fact that its derivation is based upon precisely that assumption the validity and usefulness of which the whole of the argument presented above was designed to call into question—namely, the unvarying assumption that "price is equal to the marginal prime cost." [33] There are other reasons, moreover, for refusing to assign general validity either to Mr. Keynes's algebraic expression or to the specific conclusion he draws from it: namely, that if the output of a given industry is "perfectly inelastic," "the whole of the increased effective demand . . . is expected to accrue to the entrepreneur as profit." [34] In short: the chief effect of the *General Theory's*

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[31] To say this is of course to say that it asks us to accept a crucial part of the position of the monetary "expansionists" without providing the kind of support, logical or empirical, on the basis of which alone that position (or indeed any position with respect to the effects of monetary expansion and contraction upon the level of output and employment) could be expected to carry conviction. A detailed discussion of the issues involved must, however, be left for my later *Money and Production*.

[32] *General Theory*, 283. One might have thought that Mr. Keynes would have been anxious to prove his contention that his "new argument . . . is essentially a development of the old" (*General Theory*, 78), by calling particular attention to this passage, which undertakes precisely to relate the "effective demand" of the *"new"* argument to the "profits" of the "old" argument. In fact, however, the passage is included in a section of the book which, Mr. Keynes suggested, could be "omitted," with little loss, by "those who (rightly) dislike algebra" (*General Theory*, 280 n.); and this despite the fact that the *argument* to which the algebra leads is not repeated explicitly, as far as I can discover, elsewhere in the book.

[33] See the *General Theory*, 283. It will be observed that "marginal prime cost" is substituted for "the expected price of a unit of output" (p) in the algebraic expression constituting the seventh line of note 1 on p. 283, where Mr. Keynes presents his algebraic derivation of the expression to which reference is made in the text.

[34] The particular expression on which this conclusion is based is $\Delta P = \Delta D -$ (marginal prime cost) ΔO, in which ΔP represents the increment of expected profit; ΔD represents the increment of (expected) "effective demand"; and ΔO represents the increment of "output." But it should be

treatment of the relation between "profits," on the one hand, and the "governing" of prices by "costs," on the other, is to leave the issues in a state of far greater confusion than it had been left in his own *Treatise*.[35]

The full irony of the situation is revealed, however, only when it is remembered that Mr. Keynes's utterances with respect to the "governing" of "prices" by "marginal costs" themselves arose out of a desire to effect a "synthesis" between the "general" Theory of Value, on the one hand, and the Theory of Money and Prices, on the other. For it can be shown that the blunder with respect to the rôle of "costs" in the determination of realized money prices of which the *General Theory* stands convicted is a blunder which could not have been committed if there had been adequate recognition of a proposition which is capable of easy demonstration: namely, that certain of the methodological pre-

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clear (1) that the increment of "profits" (ΔP) attributable to a given increment of effective demand (ΔD) will be determined by the resulting relation between price and *average* cost, rather than between price and *marginal* cost; (2) that the condition that "price is equal to marginal prime cost" is consistent with many divergent possibilities with respect to what is happening to *average* cost; and (3) that from the condition that the *output* of a given industry is taken to be "perfectly inelastic," it does not necessarily follow that there will be no rise in average *costs,* in such wise as to guarantee that "the whole of the increased effective demand is expected to accrue to the entrepreneur as profit."

[35] It is instructive, on the other hand, to compare the *General Theory's* discussion of the relation between "costs" and selling prices, as just summarized, with the discussion that one finds in Mr. Keynes's recent *How to Pay for the War* (1940). For in the latter, unhampered by an academic zeal to "bridge" an alleged "gap" between monetary theory and the "general" Theory of Value, Mr. Keynes has succeeded in presenting an argument which is very much more respectable, from the standpoint both of an adequate monetary theory and of an adequate "general" value theory, than the argument of the *General Theory* itself. He continues, to be sure, to emphasize the rôle played by rising costs in the process of price rise (see, for example, p. v of *How to Pay for the War*); but the context in which this emphasis appears is such as to make it worlds removed from the type of argument with respect to the "governing" of selling prices by "costs" which one finds in those parts of the *General Theory* discussed above. When, for example, Mr. Keynes speaks of "manufacturers and retailers" as "reluctant to charge higher prices except in response to an actual rise in cost," and when he contrasts the "psychology" underlying this "reluctance" with previous experiences during periods of monetary expansion (p. v), he admits tacitly that these "manufacturers" *may* raise their supply prices, and in past instances *have* raised their supply prices, by an amount that *need bear no close relation* to the *"actual rise in cost."* Cf. also p. 64 of *How to Pay for the War,* where the emphasis upon "the profiteers" whose "profits" result from "the initial rise in prices" of "goods which were produced at the lower pre-war price level," is the kind of emphasis upon "windfalls" that one found in the *Treatise* (see, for example, I, 125 of that work), and not the kind of emphasis upon the "governing" of "prices" by "costs" that one finds in the *General Theory.* See also what is said on this matter below, p. 620, n. 134.

suppositions underlying the *type* of "general" Value Theory which reveals most clearly the fallacies with respect to the relation between "costs" and "prices" of which the *General Theory* is guilty, are precisely the methodological presuppositions underlying the particular devices of *monetary* theory which are rejected in the *General Theory* on the ground that they have no analogues within the "general" Theory of Value.

In the case of the "general" Theory of Value, the particular presuppositions involved go back, as we have seen, at least as far as Galiani, whose position with respect to the effect of cost of production upon the value of "things" in general was summarized by his proposition that "the fact that this beauty of glass and crystal is the product of art rather than of nature *does not alter the price, except by* altering the scarcity." [36] This, after all, was the substance of Jevons's proposition, advanced more than a century later, that although "labor is found often to determine [that is, to affect the determination of] value," it does so "only in an indirect manner by varying the degree of utility of the commodity through *an increase or limitation of the supply.*" [37] Moreover, despite the perennial misrepresentations of the argument of so-called "cost of production theorists" such as Ricardo and Mill, it was also their position (as Marshall and others pointed out) that "cost of production could have no effect upon exchange value if it could have none upon the amount which producers brought forward for sale." [38] When Jevons went on to draw the conclusion summarized by his famous "catena"—the third link in which was paraphrased by Marshall as arguing that "value" will be "determined" by "the price which consumers are only just willing to pay" ("marginal demand price") —he enunciated a proposition which required, to be sure, the corrective context in which it was later placed by Marshall; but which, when accompanied by this context, amounts to a proposition unrefuted and irrefutable.[39] For this proposition is simply that in an economy in which purchasers are free to decide upon the amount and the nature

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[36] See above, p. 23, n. 58.

[37] Jevons, *Theory of Political Economy*, 2 (italics mine).

[38] See Marshall's *Principles,* Appendix I ("Ricardo's Theory of Value"), 817, and the quotations from Ricardo and Mill given on pp. 819 f.; also the reference to Professor Robbins given above, p. 558, n. 12.

[39] Cf. Marshall's *Principles,* 817 f. Jevons was, of course, not the only protagonist of the "revolution" of the 1870's whose salutary emphasis upon the rôle of *demand* in the realization of market prices required the provision of a "corrective context" of the type provided by Marshall. See, for example, the comments on Menger in G. J. Stigler, "The Economics of Carl Menger," *loc. cit.,* 242 f. (*Production and Distribution Theories,* 148 f.). In view, however, of the position taken in the *General Theory* with respect to the "governing" of "prices" by "costs," it is particularly striking that, in the very year that the *General Theory* was published, Mr. Keynes himself quoted Jevons's famous "catena" without any indication of disapproval. See the *Journal of the Royal Statistical Society,* XCIX (1936), 534.

of their purchases, the ability of the *suppliers* to "determine" price is limited to their ability to effect adjustments with respect to the *amount offered* for sale, and the terms on which they are *prepared* to sell this amount; and that thereafter the determination of the *realized* price and the amount of *realized* sales is entirely a matter of the conditions of *demand* for the commodity in question.[40]   And it is not the least paradoxical aspect of the argument of the *General Theory* that after having announced its determination to give a place of honor to that emphasis upon the importance of Demand which, according to Mr. Keynes, had been rejected by the "classical" economists, and had been forced to "live on furtively, below the surface, in the underworlds of Karl Marx, Silvio Gesell, and Major Douglas," it proceeded to present a Theory of Prices in which the conquests of generations with respect to the rôle of Demand in the determination of realized prices were lightly thrown into the ash heap in favor of a treatment of the rôle of "costs" in "governing" prices which was never advocated even by the most extreme among the abler partisans of a so-called "cost of production" Theory of Value.[41]

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[40] The complete acceptance of *this* aspect of Jevons's "catena" by Marshall is indicated by the latter's insistence that "the difficulty of producing a thing [as measured by "Expenses of Production"] determines *supply* in the first instance, and *value* [only] in the second [instance]." See Marshall, *Economics of Industry*, 80; and cf. also Marshall's paper on "Mr. Mill's Theory of Value" (*Memorials of Alfred Marshall*, 128, n. 2), on the "serious mischief" that would be done "by diverting attention from the forces which govern supply in the first instance and value in the second." In the light of Marshall's comment on *both* the "classicals" and Jevons with respect to the advisability of introducing explicitly the concept of "supply *price*" (*Principles*, 818), it is clear that Marshall himself would have included, in the "determination" of "supply," the "determination" of the *price* at which a given amount is *offered* for sale (cf. also below, p. 571, n. 47). The very fact, however, that Marshall insisted that "value" (that is, *realized* prices) would be "determined" only "in the *second* instance" (that is, as a *second* step, *after* adjustments had been made with respect to supply and supply price) shows that he did not regard even the setting of *supply* price (which is not necessarily the same thing as *cost* price) as equivalent to the determination of *realized* prices. For examples of a failure to make this elementary point clear, even in recent writings on the "general" Theory of Value, see above, pp. 244 f., nn. 47 and 48, and pp. 259 f., nn. 83 and 84. On the implications of the proviso that we are dealing with "an economy in which purchasers are free to decide upon the amount and the nature of their purchases," see what is said below, p. 582.

[41] See, for example, the citations given below, p. 584, n. 74. Cf. also Joan Robinson, *Introduction to the Theory of Employment*, 56 ff., where all three of the "causes" of "Changes in Prices" listed have to do with changes in the level of "costs per unit of output"—even an expansion of "demand" being held to affect prices only insofar as the increased "demand" leads to conditions of production involving higher "costs per unit of output." To my citation of the statement in the *General Theory* (p. 32) with respect to the rôle played by "Demand" in economic theory, it may

For our present purpose, however, it is of more importance to call

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be objected (i) that I have removed the statement from its context, which had to do, not with the rôle of Demand in the determination of *prices*, but with its rôle in the theory of Employment and Output; and (ii) that the argument advanced in the text above does not disprove the argument, in the *General Theory*, that *prices* are "governed" by *"costs,"* and that what Demand "determines" is the *amount sold* at these prices (and therefore the level of output and employment). A full treatment of both propositions must be left for my later *Money and Production*. With respect to (i), however, I may say here (1) that when Malthus "wrestled" with "the great puzzle of Effective Demand" *(General Theory, loc. cit.)*, he "wrestled" with it as much in connection with the problem of the relative rôle of "costs" and of Demand, respectively, in the determination of *prices* ("Value") as he did in connection with the problem of Employment and Output; and (2) that although it is impossible to defend everything said by the "classical" economists with respect to the rôle of Demand in the theory of Output, it is equally impossible to maintain either (a) that the problem was "not mentioned even once" by the earlier "classicals" or in "the whole works of those" cited by Mr. Keynes as having given "the classical theory . . . its most mature embodiment"; or (b) that Mr. Keynes is accurate in his description of the rôle assigned to Demand in the "underworld" of a writer such as Karl Marx *(General Theory,* 32). (It may be observed, for example, that it was James Mill, one of the originators of the Law of Markets [described, on p. 18 of the *General Theory,* as having alleged that "supply creates its own demand"], who insisted that "Demand creates, and the loss of demand annihilates, supply" [*Elements of Political Economy,* 91]; that it was Ricardo, cited in the *General Theory* [p. 18] along with Say in connection with the proposition that "supply creates its own demand," who insisted that if "the demand would not increase, neither would the supply, for a commodity is not supplied merely because it can be produced, but because there is demand for it" [*Principles,* Chap. XXX, p. 376 of the Gonner edition]; and that, among those critics of an alleged overemphasis upon Demand who have argued that "it is purely a tautology to say that crises are caused by the scarcity of solvent consumers, or of a paying consumption," was Karl Marx [*Capital,* Vol. II, 475, of the Untermann translation].) With respect to (ii), on the other hand, I may point out (1) that the proposed defense of the *General Theory* does not dispose of the consequences of its confusion of *cost* curves with *supply* curves; and (2) that since the determination of *how much* will be "sold" cannot be separated from the question of the *price* at which any given amount will be actually sold, or indeed from the question whether *any* amount will be sold at a given supply price, the admission that Demand will affect the "amount sold" means an admission that the conditions of Demand are also the immediate arbiter of realized *prices,* in the sense indicated in the preceding sentence of the text above. Contrast R. F. Kahn, "Public Works and Inflation," *Journal of the American Statistical Association,* XXVIII (1933), Supplement, 169, where it is alleged that "the rise in prices is determined by the *supply curve and nothing else"*; and where, from the further context in which the statement appears, we are left to assume that the "supply curve" in question will be identical with a *cost* curve. Cf. also the comment above, p. 556, n. 9, on another statement by Mr. Kahn.

attention to the second part of the proposition advanced above: namely, that the methodological propositions underlying the type of "general" Value Theory which has just been described, and which is the very antithesis of the crude propositions advanced by Mr. Keynes with respect to the "governing" of realized prices by "costs," are inherent also in the particular devices of *monetary* theory which Mr. Keynes has rejected on the ground that they bear no clear relation to the "homely but intelligible concepts" of the "general" Theory of Value. For if anything is clear, it is that the devices thus rejected by Mr. Keynes (namely, those "stream equations" which rest upon the conception of the realization of prices as "governed by the quantity of money, by its income-velocity, by the velocity of circulation relatively to the volume of transactions . . . *et hoc genus omne*") rest upon methodological assumptions which are, in all important respects, the assumptions underlying the type of value theory which, as we have seen, was characteristic, not of the "underworlds of Karl Marx, Silvio Gesell, and Major Douglas," but of the ablest writers in the "general" Theory of Value from the days of Galiani to our own day.[42] It is a corollary of this conception of "stream equations" that any factor which is alleged to affect realized prices and realized sales must be shown to operate through a change in the dimensions of either the flow of money or the flow of goods. We know that the problem of the relation of the "flow of money" (as represented by the first member of our "stream" equations) to the "Demand" side of the process of price-formation, was recognized, even if it can hardly be said to have been solved, as early as the eighteenth century, when the first known variant of a "stream"

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[42] Since we are here discussing the relation between the "general" Theory of Value and the Theory of Money and Prices, it may be pointed out that the numerous writers who have sought, in recent years, to establish similarities between the argument of the *General Theory* and that of Marx, would have done better to observe the extent to which Mr. Keynes's expressed sympathy for "the labor theory of value" (see above, p. 553, n. 6) and his perversion of the supposedly "classical" doctrine that "prices" are "governed" by "costs" have led him to a type of conclusion within the Theory of *Money and Prices* which bears a strong resemblance to the conclusions reached by Marx within that field. On the other hand, it is doubtful whether those who have remained unimpressed by Mr. Keynes's expression of sympathy with the "labor theory of value," as long as Mr. Keynes has not undertaken to refute the long-standing objections to such a theory of value, will be more impressed when the latter is combined with a type of *monetary* theory open to objections of long standing, as long as equally little effort is made to remove *these* objections. On the objections themselves, which apply directly to the argument of the *General Theory* even if the latter does not stress particularly the "cost of production" of the money *metal,* see, for example, Wicksell, *Interest and Prices,* 35 ff., and *Lectures,* II, 150 f., and also what is said below, pp. 574 ff., with respect to the "passiveness" of the velocity of circulation of money.

equation was presented in algebraic form.[43] We know that it was recognized by John Stuart Mill, who himself made use of a non-algebraic "stream equation," and that it was greatly emphasized by Simon Newcomb, whose own "stream equation" led to that of Fisher, the very model of the type of formulation rejected by the *General Theory* as implying a breach with accepted formulations of the "general" Theory of Value.[44] And we know also that it was just this type of formulation that was categorically rejected by sponsors of a "cost of production" Theory of Money and Prices precisely on the ground that the rôle assigned by it to *Demand* in the theory of price formation was either misleading or meaningless.[45]

We know, at the same time, that it is nothing less than a libel upon these "stream" formulations to suggest that their sponsors have concerned themselves *only* with the Demand side of the problem of price determination, at the expense of the side of supply.[46] We know, on the contrary, that an emphasis upon the nature of the forces affecting the flow of *"goods"* is implicit in the very concept of a *"mutual"* impact of two flows. What is to be observed here is that, by the very statement of the problem of Supply in terms of a *flow* of "goods," rather than directly in terms of the "cost of production" of those goods, we establish a link with those variants of the "general" Theory of Value according to which (in Marshall's words) "cost of production could have no effect upon exchange values if it could have none upon the amount which producers brought forward for sale." [47] And it is to be observed, also, that the concern of later sponsors of these "stream" equations with the nature

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[43] See above, p. 20, n. 49; p. 154, n. 24; p. 264, n. 94; p. 270, n. 107; and p. 271, n. 109.

[44] On Mill, see above, pp. 46 f., n. 123, and p. 104, n. 36; and on Newcomb, see p. 106, n. 37; p. 265, n. 97; and pp. 272 f., nn. 112 and 113.

[45] See above, p. 104, n. 36.

[46] See our Proposition XXIII (above, p. 550).

[47] See above, p. 567, n. 38. Cf. Fisher, *The Purchasing Power of Money*, 179: "Improvements in production will affect price levels simply as they affect the volume of business transacted. Any rational study of the influence of improvements in methods of production upon the level of prices should, therefore, fix attention, first, on the resulting volume of trade, and should aim to discover whether this, in turn, carries prices upwards or downwards. . . . From what has been said, it must be evident that, other things remaining equal, trusts cannot affect the general level of prices through manipulating special commodities except as they change the amounts sold. . . . If trade unions seek to raise prices of labor while trusts raise prices of commodities, the general level of everything may rise or fall; but it can rise only by a general decrease in the quantities of commodities, labor, etc., sold, or by an increase of currency, or by an increase in velocities of circulation." For Fisher's understanding of the relation of this type of argument to movements in the "supply *curve or schedule*," in the face of different possibilities with respect to the conformation and position of "demand *curves or schedules*," see pp. 382 ff., of the same work.

of the forces determining the *"rate* of flow" of "goods" has had the result of establishing more firmly than ever the central proposition here advanced: namely, that the rôle played by the analysis lying behind the "goods side" of our stream equations is precisely that played by those parts of the "general" Theory of Value which have been concerned with the relation of "costs," of "reserve prices," of "production," and of "expectations" to the phenomena of *market* value, and therefore to the determination of prices and sales *realized* in the "market." [48]

It is easy to show that this conclusion is not the mere result of a grim, unreasoning determination to find analogies between an adequate version of the "general" Theory of Value, on the one hand, and the "stream" equations of monetary theory, on the other, by means of tortuous constructions which cannot be shown to have occurred to writers who have made use of these "stream" equations.[49] The contrary, indeed, is evidenced by the fact that the argument, as just stated, is in all essentials that which is implied in Robertson's proposition that "we cannot . . . in the analysis of a dynamic process, do without the . . . Marshallian concept of *market* equilibrium, *with price emerging from the impact, at each moment, of the existing flows of money and of goods"*; and in Robertson's further proposition that *"this is the concept of the quantity theory."* [50] It is of even greater interest, however, to observe

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[48] See our Proposition XXVIII (above, p. 533), and Proposition XXIX, 1 (p. 553); and particularly the references given above, p. 555, n. 8, to my earlier articles on the "rate of sale" of goods, and related concepts. To be noted, also, are the references to the contrasting treatment of the "goods side" of the problem in both Keynes's *Treatise* and his *General Theory*, with its easy substitution of "output" for both "goods intended ['offered'] for sale" and "goods sold."

[49] In addition to the quotation from Mr. Robertson given in the following sentence of the text, see also his essay of 1927, "Income Tax and the Price Level" (*Economic Fragments*, 35) where, in discussing the rôle of costs of production in the determination of realized prices, in the face of considerations raised by "the quantity theory of money" (cf. n. 50, immediately following), Mr. Robertson characterized, as "the obvious answer," the proposition that "the rise of prices would occur through a *shrinkage of supply of goods."* Cf. also the quotation from Fisher given above, p. 571, n. 47.

[50] Robertson, "A Survey of Modern Monetary Controversy," *loc. cit.*, 7 (*Essays in Monetary Theory*, 139; except for the italicizing of the word "market," in *"market* equilibrium," the italics are mine). On the characterization of the type of situation in question as one of "market *equilibrium,"* see above, p. 233, n. 27. In the light of the amount of confusion engendered in the past by the use of the expression "the quantity theory" when "quantity *equations"* are meant, it should be clear also that Mr. Robertson's statement that "this is the concept of the quantity theory" (cf. also the passage quoted in the preceding note) is not entirely happy. It should be added, on the other hand, that the damage done by Mr. Robertson's usage in this particular instance is not serious, since he is not *attacking* the quantity equations on grounds that would be relevant only in an attack upon the cruder forms of "the quantity *theory"* (cf. Vol-

that the same thing is evidenced by a further fact: namely, that both Mr. Keynes and his followers (such as Mr. Harrod) have recognized that the proposition that realized prices are exclusively and immediately "governed by marginal prime cost" runs into conflict with monetary analysis of the "stream" type—or, as Mr. Keynes puts it, with the "preoccupation" of "the classical school" with "the idea that prices depend on the quantity of money." [51]

The *difference*, of course, between Mr. Robertson and Mr. Keynes is that Mr. Robertson was aware, as Mr. Keynes was not, of the nature of what Mr. Harrod has called "the true classical theory" with respect to the "governing" of prices by "costs," and that therefore he understood, as Mr. Keynes did not, why the alleged "preoccupation with the idea that prices depend on the quantity of money" neither (1) led "the classical school" to results in conflict with what "one would have expected" it to argue with respect to the effect upon realized prices of changes in "marginal prime costs"; nor (2) led to the establishment of an unbridged "hiatus" between the "general" Theory of Value, on the one

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ume I, 22, of the present work; and see also the following note). It is also true that Mr. Robertson's proposition, which explicitly involves the concept of price as "emerging from the mutual impact, at each moment, of the existing flows of money and of goods," implies an interpretation of the Quantity Equations which would attach to them connotations they have not always had in the minds of self-styled "*anti*-quantity-theorists" (see, for example, the references given in Volume I, p. 21, n. 37); and this would justify some designation of the concept he has in mind other than merely "Quantity *Equations*." In such cases, I myself have followed the practice of referring to "the Quantity Equations in their *stream* aspect," or simply (as in the text above) to "stream equations." But it is clear that these terminological matters have nothing to do with the *substance* of Mr. Robertson's proposition, as quoted in the text. For it is the *substance* of this proposition that provides the answer to those who would suggest that "starting with the quantity theory [identified with a "stream equation" of the general form $MV = PT$], it is easy to conclude that a reduction in wage rates means a reduction in P," and that, in general, the use of stream equations of this type makes it difficult to choose "assumptions which . . . have an economic significance which is . . . easily perceived" (cf. R. M. Bissell, "Price and Wage Policies and the Theory of Employment," *loc. cit.*, 203 f.).

[51] See the *General Theory*, 12. In what follows, I shall assume that by the particular "preoccupation" indicated, Mr. Keynes meant the type of "preoccupation" represented by the concept (in Mr. Robertson's words) of price as "emerging from the mutual impact . . . of the existing flows of money and of goods." (Cf., however, what is said on this matter in Volume I, 34, of the present work, in connection with the confusion of "the quantity theory" with "quantity *equations*," in the *General Theory* as well as in the *Treatise*.) In the same way, I shall assume that Mr. Harrod's comment on "the Quantity Theory of Money," in this connection (Harrod, "Mr. Keynes and Traditional Theory," *loc. cit.*, 82 ff.), is intended to refer to "the quantity *equations*" (see, for example Mr. Harrod's reference to "the monetary equation" in this context). Cf. also Harrod's *The Trade Cycle*, 125 ff.

hand, and the Theory of Money and Prices, on the other. And the difference between Mr. Robertson and those followers of Mr. Keynes (such as Mr. Harrod) who have been disturbed by the "tiro" character of Mr. Keynes's interpretation of the "true classical theory" with respect to the "governing" of realized prices by "marginal prime cost," is that Mr. Robertson's understanding of the earlier conquests of *monetary* theory would not permit him to seek the kind of escape from an entirely factitious dilemma which was chosen by Mr. Harrod, and which amounts to a combination of an innocence of any knowledge of these conquests with an implication that a "tiro's" general Theory of Value loses its "tiro" character when it is married to an equally "tiro" Theory of Money and Prices.

It was not unreasonable to expect, for example, that anyone undertaking to state the relation of the *General Theory's* argument to certain devices of monetary theory that had enjoyed wide acceptance, would evidence (1) an awareness that these devices had themselves been developed as a result of dissatisfaction with alternative approaches to the problem of the determination of money prices; and (2) an understanding of the substance of these later devices themselves. Yet Mr. Harrod's argument evidences neither. There is no evidence, for example, of an awareness that the substance of the argument imputed to Mr. Keynes by Mr. Harrod was the substance of the "cost of production" Theory of Prices of Karl Marx.[52] There is no evidence, in particular, of an awareness that Marx's argument, like that imputed to Mr. Keynes by Mr. Harrod, should have led, and in the case of Marx did lead, to the specific conclusion that monetary "velocity," for example, is a purely passive factor which somehow adjusts itself to the circulation requirements established by a previously cost-determined level of "prices" and a previously determined quantity of commodities to be sold at this cost-determined price-level.[53] Nor is there evidence of an awareness of the objections to the Marxian argument raised by such a writer as Wicksell, whose position amounted essentially to an argument *against* the type of explanation (or lack of it) thus implied with respect to

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[52] See above, p. 570, n. 42. Cf. also H. S. Ellis, "Some Fundamentals in the Theory of Velocity," *loc. cit.*, 466, and the references to Marx's *Capital* there given (the passages indicated are to be found on pp. 135 ff. of the Kerr edition).

[53] As Wicksell remarked in his discussion of this part of Marx's argument (*Interest and Prices*, 37), the "conception" involved "is not peculiar to him [Marx], but is to be found in the works of very many other writers on monetary questions." It represented one of the aspects, for example, of the celebrated Fullarton doctrine with respect to "hoards." It is not surprising, therefore, that Marx should have quoted Fullarton with approval (*Capital*, I, 161). Nor is it surprising that the treatment of the problem of "velocity" which was implicit in certain of Fullarton's propositions with respect to the functioning of "hoards" should have been attacked by later protagonists of the "cash balance approach" on precisely the grounds on which Marx's argument was attacked by Wicksell. See, for example, Mises, *The Theory of Money and Credit*, 146 ff.

what Marshall referred to as "the *causes that govern* the rapidity of circulation of the currency," and an argument in favor of the *cash balance approach* as a weapon for *discovering* the "causes that govern the rapidity of circulation of the currency." [54]

It was, of course, open to Mr. Harrod to provide reasons for rejecting the argument for the cash-balance approach as a weapon for explaining the movements in monetary velocity. It was hardly open to him, however, to refer to "the unsatisfactory theory of the 'traditional' theory of velocity of circulation," in terms which indicate a complete unawareness of what the cash-balance approach has had to say on the subject.[55]

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[54] See Volume I, 418 f., of the present work, and the reference to Marshall given on p. 419, n. 12. For Wicksell's criticism of Marx on the point in question, see the former's *Lectures*, II, 150, where Wicksell based his objection to Marx's argument on the absurdity of supposing that "merchants and bankers" would take no *decisions* with respect to the administration of the cash balances assumed, by the Marxian argument, to increase or diminish (relative to outlay); and see especially Wicksell's *Interest and Prices*, where, having pointed out (p. 37) that Marx's argument rested upon the unstated assumption that the administrators of cash balances would refrain from modifying the situation imposed upon them by the changes in "velocity" assumed in the Marxian argument, he called attention to "the more detailed analysis" of the following chapter, which was concerned precisely with the consequences of the fact that "everyone, and particularly every business man, has to keep by him a certain amount of money" (p. 39)—that is, with the consequences of the methodological considerations underlying the cash balance approach. In this connection, see also the passage from Mises's *Theory of Money and Credit* cited in the preceding note.

[55] See Harrod, *The Trade Cycle*, 125 f., where, after remarking that "in the developed theory there has always been a weak spot, namely, the velocity of circulation," Mr. Harrod goes on to state that "the *causes which govern it* are less easy to distinguish," without any indication that what Mr. Robertson has called "the powerful weapon forged originally by Petty and sharpened by Marshall and Pigou" ("The Monetary Doctrines of Messrs. Foster and Catchings," *Quarterly Journal of Economics*, XLIII [1929], 498) has anything to contribute to our understanding of "the causes which govern" velocity. In this connection, see the references to Volume I given in n. 54, immediately preceding; and contrast also Bissell, "Price and Wage Policies and the Theory of Employment," *loc. cit.*, 203, 237, where it is suggested that, if we "start with the quantity theory as a general proposition" (by which, in dealing with the problem indicated by the context, we should understand the quantity *equations* and the analytical techniques which must be held to lie behind each of their terms), "it is easy to conclude that a reduction in wage rates means a reduction of P and *also, probably, in V*," and that in general this type of approach makes it less easy to perceive the "economic significance" of the assumptions we employ, in particular because "stating the matter in this fashion . . . does not cast much light upon the *motives and decisions* which underlie the phenomenon" (italics mine; cf. also the comment made above, p. 573, n. 50). Similarly, in Mr. Harrod's article, "Mr. Keynes and Traditional Theory," *loc. cit.*, 85, "the unsatisfactory character of the theory of velocity

Nor, in a generation which has known Mr. Hawtrey's analysis (itself possessed of a long and distinguished ancestry) with respect to the generation and utilization of money income, and its clear statement of the relation of this analysis to the concept of "income velocity," was it open to Mr. Harrod to suggest that while "the old theory pre-supposed that income velocity was somehow determined," "precisely how was something of a mystery." [56] And to anyone who should have been aware of the true relations between the concept of a "demand for money" inherent in the cash-balance approach and in "monetary equations" of either the "Cambridge" or the "Fisher" type, it was hardly open to suggest that Mr. Keynes's introduction of the element of "liquidity preference," with its explicit reference to the concept of a "demand for money" for "holding" purposes, somehow succeeds in preventing "the monetary equation" from exerting any "direct influence in the general field," on the ground that the "power residing in the monetary equation has already been used up in Mr. Keynes' system." [57] For what this

of circulation" is regarded as being evidenced, "in other words," by "the failure of monetary theory to explain how the total stock of money is divided between liquid reserves and active circulation." On the manner in which those sectors of "monetary theory" represented by "cash balance analysis" have in fact dealt with the latter problem, see Volume I, 459 ff., and the references there given (including those given on p. 463, n. 10). Cf. also Ellis, "Some Fundamentals in the Theory of Velocity," loc. cit., 463 ff. Readers of Volume I of the present work (see especially pp. 436 ff.) will be aware that I myself would prefer to state the argument without bringing in the concept of "real balances," as Professor Ellis does. On this matter, however, cf. Volume I, 449, n. 96, on there being, "from the standpoint of heuristic value, little to choose" as "between a cash-balance approach running in 'monetary' terms" and the best of the "real balance" variants of the "cash-balance approach," such as that sponsored by Professor Ellis (see Volume I, 455, n. 112, and cf. "Some Fundamentals in the Theory of Velocity," loc. cit., 451 ff.). The present instance, on the contrary, seems to me to provide an excellent example of the wisdom of a proposition that I ventured to advance a decade ago, in a desire to avoid, as far as possible, internecine disputes among a group to be characterized generally as "cash balance theorists": namely, "that all forms of the 'cash-balance approach' belong to one family, and that differences within the family are less important than the good name of the family as a whole" ("Léon Walras and the 'Cash-Balance Approach' to the Problem of the Value of Money," loc. cit., 599, n. 74; cf. also Volume I, 416, n. 4, of the present work).

[56] Harrod, "Mr. Keynes and Traditional Theory," loc. cit., 85. On Hawtrey's treatment, in particular, of the relation of the concept of "income velocity" to his own analysis with respect to the generation and utilization of money income, see Volume I, 408, of the present work.

[57] Harrod, "Mr. Keynes and Traditional Theory," loc. cit., 82. For Mr. Keynes's "explicit reference" to the relation between the concept of "liquidity preference" and the "demand for money," see the General Theory, 194: "The subject [namely, the analysis of the motives to liquidity-preference] is substantially the same as that which has been sometimes discussed under the heading of the Demand for Money." Cf. also p.

amounts to saying is that "Mr. Keynes' system" *does* have a "monetary equation." [58] In this "monetary equation," however the fact may have been obscured by Mr. Keynes's own exposition, room is left for a device which, properly handled, is capable of providing an *explanation* of movements in "velocity," of a type which is the very antithesis of the neo-Marxian assumption of passive adjustability of "velocity" that, if we are to believe Mr. Harrod, follows from that part of the *General Theory's* argument which has to do with the "governing" of "prices" by "costs." [59] And what this means, in turn, is simply that Mr. Harrod's

305 of the same work on "the liquidity factors which determine the demand for money in each situation," and what is said on this matter below, pp. 583, n. 70; 604, n. 112; 616, n. 132; 653, n. 58; 673, n. 111; 724, n. 117; and 729 ff., nn. 125 and 126. Those readers who would still insist that although the concept of a "demand for money" thus implied is consistent with "monetary equations" of the "Cambridge" type, it is not consistent with "monetary equations" of the "Fisher" type, are urged to read again not only the general discussion of the significance of the expression $K = 1/V$ presented in Volume I, 417 ff., but also the statement of Professor Pigou himself with respect to the Fisher equation as "an equation of [the] demand [for money]," cited in Volume I, 540, n. 35.

[58] It is worthy of more than passing notice that Mr. Harrod, like virtually all of the avowed defenders of the *General Theory*, continues to call attention to those aspects of its argument which seem "to gut the monetary equation" ("Mr. Keynes and Traditional Theory," *loc. cit.*, 82), without being aware of a further fact, which itself provides a bitter commentary upon the war that Mr. Keynes has been waging upon the more familiar "monetary equations" ever since 1930. This further fact is that Mr. Keynes's own formal framework for the study of "the Theory of Prices—that is to say, the analysis of the relation between changes in the quantity of money and changes in the price-level with a view to determining the elasticity of prices in response to changes in the quantity of money" (*General Theory*, 296)—is itself nothing more than a bedizened variant of the familiar Quantity Equations. If it is not the purpose of this "monetary equation" to show how the "price level" is "determined," in the only sense in which the same thing can be said of the "monetary equations" of "the traditional theory" (Harrod, "Mr. Keynes and Traditional Theory," *loc. cit.*, 82 f.), it would be interesting to know how we are to interpret Mr. Keynes's own statement that the "purpose" of his formulation "is to exhibit the extreme complexity of the relationship between prices and the quantity of money, when we attempt to express it in a formal manner," and that his "*e* without suffix . . . measures the response of money-prices to changes in the quantity of money" (*General Theory*, 305). See Chapter Fourteen, below.

[59] On the relation of the Keynesian argument, as interpreted by Harrod, to that of Marx, see what is said above, p. 570, n. 42, and p. 574, nn. 52–54. On the "passive adjustability" of velocity, in Mr. Harrod's version, see his *The Trade Cycle*, 126: "Those forces which have been enumerated govern the volume of output and the level of prices; these in turn cause the velocity of circulation to be what it is." The elements in Mr. Keynes's exposition, referred to in the text, which have tended to obscure the fact indicated are (1) his occasional refusal to admit that the phenomena

elaborate attempt to reconcile the "tiro's" account of the "governing" of "prices" by "costs," which he himself has found in the *General Theory*, with the requirements of an adequate *monetary* theory, has succeeded merely in showing that the retrograde "general" Theory of Value thus indicated is consistent only with a type of monetary theory so retrograde that it is implicitly rejected elsewhere by Mr. Keynes himself—with results, for the logical consistency of "Mr. Keynes' system," as interpreted by his leading disciples, that the reader must judge for himself.[60]

associated with "liquidity preference" are capable of translation in terms of "velocity"; and (2) his insistence, in certain of the cases in which he did admit such a connection, on relating it directly to *"income* velocity," rather than to "Fisherine" velocity. For an example of the former practice, apart from his general rejection of "concepts such as . . . the velocity of circulation [of money] . . . *et hoc genus omne"* as concepts useful in accounting for how "prices are governed" (*General Theory,* 292), see the article "The General Theory of Employment," *loc. cit.,* 211, where Mr. Keynes rejected Professor Viner's suggestion that "in modern monetary theory the propensity to hoard is generally dealt with, with results which in kind are substantially identical with Keynes', as a factor operating to reduce the 'velocity' of money," with the brusque comment that "on the contrary, I am convinced that the monetary theorists who try to deal with it in this way are altogether on the wrong track"; although Mr. Keynes developed no further argument in support of this comment, beyond the *non-sequitur* that a belief that "changes in the propensity to hoard, or in the state of liquidity preference as I have called it" may affect "the *rate* *of interest"* should demand a belief that the effects of changes in liquidity preference upon *commodity prices,* by way of their effects upon velocity, are either non-existent, or unimportant, or are produced only by "repercussion as an ultimate *consequence* of a change in the rate of interest" (*op. cit.,* 216; contrast Ellis, "Some Fundamentals in the Theory of Velocity," *loc. cit.,* 471, and the reference to D. H. Robertson there given). For an example of an admission of the kind indicated under (2) with respect to the relation between liquidity and *income* velocity, see the *General Theory,* 309, and cf. also p. 299 of the same work. In both cases, of course, Mr. Keynes was merely restating positions which had already been taken by earlier writers, and with the same degree of confusion as a result. See above, p. 87, n. 89, and cf. also Volume I, 366 ff.

[60] Mr. Keynes's "implicit rejection" of the type of monetary theory in question, it is here argued, follows from (1) his insistence upon the element of *volition* in the determination of the degree of liquidity preference; (2) his relation of "liquidity preference" to the *demand for money* (see above, p. 576, n. 57); (3) his relation of the "demand for money" to the amount of *"effective demand"* (*General Theory,* 305); and (4) his inclusion of changes in "effective demand" as an element affecting "the response of money prices to changes in the quantity of money," and therefore his inclusion of such changes in the expression for his "*e* without suffix" (*ibid.;* see above, p. 577, n. 58). These concessions are in obvious conflict with the unqualified statement that Mr. Keynes "regards the general price level as . . . determined . . . without reference to the quantity of money" (Harrod, "Mr. Keynes and Traditional Theory," *loc. cit.,* 80). The reason why Mr. Keynes could make these concessions, and still hold to his

The extent of Mr. Harrod's unawareness of what were referred to above as "the earlier conquests of monetary theory" will be further evidenced as soon as it is observed that his interpretation of the aspect of the argument of the *General Theory* which is here under discussion has been translated in terms of an argument with respect to the "direction of causation" as between the two members of the familiar Quantity Equations; and, in particular, in terms of an argument according to which "causation runs from right to left," rather than from left to right, "in the Fisher type of equation." [61] That this is a fair interpretation of the Keynesian argument is further evidenced by the fact that the problem has been put in just these terms by other members of the Keynesian group.[62] What should have been evident also, however, is that the very statement of the problem in these terms brings us back to a range of controversies in monetary theory which reached their zenith in the days of Tooke and his celebrated twelfth thesis with respect to the relation between money and prices: namely, "that the prices of commodities do not depend upon the . . . amount of the circulating medium; . . . on the contrary, the amount of the circulating medium is the consequence of prices." [63]

It might have been expected, therefore, that any attempt to revive

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doctrine that *prices* are to be conceived of as "governed" by "costs," is, of course, that (unlike some of his disciples) he was prepared to admit, implicitly, that the level of these *"costs"* would themselves be greatly affected by the *flow of money payments*. See the discussion presented below, pp. 590 ff., in connection with our Propositions XXX and XXXI.

[61] See especially Ellis, "Some Fundamentals in the Theory of Velocity," *loc. cit.*, 431 ff., 465 ff.; and cf. the references given in Volume I, 210, n. 12, to Professor Ellis's comments, in his earlier *German Monetary Theory*, on certain aspects of the argument of Keynes's *Treatise*. From what is said in the text, it should be clear that Professor Ellis was entirely justified in entering a courteous protest against my reference to his treatment as involving a raising of "the ghosts of ancient controversy," as long as I did not take pains to remind the readers of my first volume of the fact that these "ghosts" had been given a new lease of life by the argument of the *General Theory*. See the *Journal of Political Economy*, XLVI (1938), 876.

[62] See, for example, R. F. Kahn, "Public Works and Inflation," *loc. cit.*, 170, on "the quantity of money" as "an effect, not a cause." Cf. the same author's "Dr. Neisser on Secondary Employment: A Note," *Review of Economic Statistics*, XVIII (1936), 145: "The size of the active circulation (which is the only portion of the stock of money that can be directly related to prices and output) is determined *by* the level of prices and of output and cannot be regarded as their determinant"; also Mr. Kahn's review-article, "The League of Nations Enquiry into the Trade Cycle," *Economic Journal*, XLVII (1937), 673, and his Rejoinder to Professor Haberler, *ibid.*, XLVIII (1938), 334; Joan Robinson, "The Theory of Money and the Analysis of Output," *loc. cit.*, 23 f.; the same author's *Introduction to the Theory of Employment*, 95; and Harrod, *The Trade Cycle*, 126.

[63] See the references to Tooke given above, p. 150, n. 19.

the substance of Tooke's thesis would have evidenced (1) an awareness of the grounds on which it had come to be rejected by a majority of able monetary theorists as an unilluminating and generally inadequate way of dealing with the issues involved; and (2) an equal awareness of the bearing, upon the problem indicated, of analytical devices and types of emphasis which have come to the fore in recent years particularly.[64] Again, unfortunately, there is evidence of neither in the argument of Mr. Harrod and other defenders of the relevant aspects of the argument of the *General Theory*. There is no evidence, for example, of a clear recognition of the importance of distinguishing, in this connection, between (1) *"expected"* and *"realized"* changes in *"prices"* as alleged *"causes"* of changes in the quantity of money and its *"velocity"*; and between (2) the clock-time periods which witness the occurrence of changes in the quantity of money and its velocity, on the one hand, and, on the other, the clock-time periods which witness changes in the *realized*

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[64] It is of considerable interest to observe that the "rejection" referred to under (1) antedated by many years the development of the particular "analytical devices and types of emphasis" indicated under (2)—that is, those "devices and types of emphasis" associated with an articulate distinction between *"expected"* and *"realized"* prices, on the one hand, and those associated with a clear treatment of the time "periods" involved, on the other. See, for example, the comments on Tooke's twelfth "conclusion" in Wicksell, *Interest and Prices*, 44; also the same author's *Lectures*, II, 153 f., on the view—"nowadays advanced even by writers who claim to be rigorously scientific," but in fact "still less scientific, if possible, than the Marxist and kindred theories"—according to which "money is . . . regarded as a kind of amorphous, infinitely elastic, or plastic mass which adapts itself without any pressure to any price level and is therefore entirely passive in relation to the pricing mechanism, whilst the latter is regulated only by circumstances concerning the commodities themselves." Cf. also the sharp comments by Schumpeter, "Das Sozialprodukt und die Rechenpfennige," *loc. cit.*, 681 (especially n. 41 thereto), on the proposition that the *PT* of the quantity equations is to be regarded as "the cause of changes in the quantity of money, in the sense, say, that a rise [in *PT*] 'makes necessary' an increase in [the quantity of] money." It is to be observed that what are rejected here are statements of the type indicated on the grounds that they represent "an unilluminating and generally inadequate way of dealing with the issues involved." Whether the statements themselves are to be regarded as substantively false depends entirely upon the context in which they appear. It should be observed, for example, that Wicksell rejected Tooke's twelfth thesis, not because he was prepared to deny that there may be "much truth" in it, but because "it provides no clue to the causes that determine the value of money," and "simply leaves the question an open one." Similarly, attention should be called to Schumpeter's comment on the difficulty of deciding how far a given writer is to be held guilty of the "layman's view" that "price changes *cause* changes in the quantity of money," because of the "lack of clarity" that is bound to characterize "the representation of monetary reasoning" as long as the problem is posed in these terms ("Das Sozialprodukt, etc.," 681, n. 41).

"prices" which are held to "cause" these changes in the quantity of money and its velocity. An adequate use of the distinction indicated under (1), for example, would have made it clear that no adequately instructed believer in what has been called "quantity-theory causation" has ever denied that *expected* changes in prices may affect movements in the quantity of money and its velocity in such a way as to lead the latter changes to "cause" *realized* changes in prices.[65] On the contrary, the substance of *this* particular thesis with respect to the "determination" of the quantity of money and its velocity by "prices" has been a commonplace for generations in a monetary theory which has nevertheless refused to admit that, in all of the cases indicated, the quantity of money and velocity have been "passive" factors in the determination of *realized* prices.[66] And the same thing must be said of the suggestion that the *realized* prices of one clock-time period "determine" the quantity of money and its velocity in a *subsequent* period. For, since *realized* prices are, simultaneously, realized money receipts, this amounts simply to saying that the amount of money receipts of one "period" may be expected to affect the level of money outlay (and therefore the level of the quantity of money and its velocity) in the following period.[67]

Surely there should be no difficulty in observing the difference between (1) this type of analysis, which is capable of accounting for all observed empirical instances in which a change in the quantity of money or of "velocity" has "followed" a change in prices; and (2) the type of "anti-quantity-theory causation" which is covered by the bald statement that "prices" are first "determined" by "costs," in complete independence of such factors as the quantity of money and its

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[65] In this connection, see Ellis, "Some Fundamentals in the Theory of Velocity," *loc. cit.,* 433, on the relation between "quantity-theory causation" and "a subjective calculus not involving *given* prices but *resulting in prices*" (italics mine). Unhappily, Professor Ellis's use of the expression "quantity-theory causation" in this context is only too well justified by the precedents provided by earlier discussions of the range of problems involved. I myself should argue, on the other hand, for a statement of the issues without once introducing the *term* "the quantity theory" into the exposition, on the ground that the history of discussion of the issues involved in terms of "the quantity theory" shows all the vices specified in Volume I of this work in connection with our discussion of "The Residuum of the Quantity Theory Controversy" (p. 22).

[66] The refusal to "admit" such a conclusion has, of course, been based chiefly upon an insistence that "expectations" *must themselves be accounted for,* and must, above all, themselves be related to *realized* processes —including, in this case, realized changes in the quantity of money and its velocity, and therefore in realized prices, in the *preceding* stages of, say, an inflationary process. On the general methodological point involved, see above, p. 229, and the references given in n. 19 thereto.

[67] This proposition is of course easily translatable into simple algebra, with the help of the notation suggested in Volume I, 382 ff., of the present work, and especially nn. 85 and 88 thereto. Cf. also what is said in the latter note with respect to the mutual "governing" of "prices" by "incomes."

velocity, and that these changed prices then force an adjustment in the quantity of money and its velocity. For in the one case we are dealing with a monetary theory which is completely consistent with both (a) the specific *findings* of an adequate "general" Theory of Value with respect to the rôle of "costs" and even of "supply price" in the determination of realized market prices; and with (b) the methodological *presuppositions* of such a theory with respect to the necessity for referring changes in market facts (including changes in the quantity of money and its "velocity") to the decisions of "economizing" individuals.[68] In the other case, on the contrary, we are dealing with an attempt to combine a retrograde "general" Theory of Value either with (a) a Theory of Money and Prices sufficiently advanced to be irreconcilable with this retrograde "general" Theory of Value; or with (b) a Theory of Money and Prices which itself is completely retrograde, and according to which changes in the quantity of money and its velocity are mere "passive" adjustments, of a kind that would correspond to reality only in a community whose members were reduced to automata, deprived of any choice with respect to the amount and direction of their money-spending by governmental decrees fixing all selling prices at "cost" levels, all amounts to be "purchased" at these "cost" levels, and therefore the amounts of money, relative to outlay, to be kept in the form of cash balances.[69] There is no suggestion, in the *General Theory,* that this is

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[68] On (a), see above, pp. 566 ff. On (b), see what is said above, pp. 465 ff. To be contrasted with what is said in the latter passages with respect to the nature of the forces affecting the quantity of money, in particular, is a statement such as that on p. 307 of the *General Theory,* where, after having argued that a rise in the "wage-unit" may be expected to have "a corresponding effect on prices," Mr. Keynes goes on to suggest that, after the wage-unit has risen, "when money is relatively scarce, some means is found to increase the effective quantity of money." In this connection, cf. the statement quoted above, p. 580, n. 64, from Wicksell, with respect to the practice of regarding money "as a kind of amorphous, infinitely elastic, or plastic mass which adapts itself without any pressure to any price level." That governments *might* act in such a way as to bring about the result desired, no one would deny. What is denied is (1) that an adequate account of the process of price-determination can be constructed upon the assumption that they will do so in all cases; and (2) that a concern with the ways in which acts of *government* may affect the quantity of money justifies a lack of concern with the rôle played by the private profit calculations of prospective borrowers from commercial banks.

[69] It is of considerable importance to observe that it would be necessary to control *all* these elements in the situation. If, for example, only *some* prices were fixed, and the further utilization of money-spending power were not controlled, we have no reason to suppose that *other* prices might not rise, under the impact of the demand exerted out of the money-spending power still at the disposal of the individuals constituting the community. The fundamental point involved, although it is really a corollary of our Proposition VIII (above, p. 285), with respect to the "money equation" as a summary of the forces "determining the magnitude of the *sum of realized money demands,*" was applied several times in the course of the

the kind of world it envisages.[70]   Yet it is the only kind of world which would permit the perversion of the perfectly justifiable proposition, long recognized in adequate versions of both the "general" Theory of Value and the Theory of Money and Prices, that *costs* may *affect* realized prices by affecting entrepreneurial adjustments upon the side of supply and the position and conformation of market supply schedules, into a proposition which is in conflict alike with a type of "general" value theory and a type of monetary theory adequate to account for events in the world we know: namely, the perverted proposition that *realized* prices are exclusively and immediately "governed" by "marginal prime cost," *and not* by a "mutual impact of relevant flows of money and of goods," and therefore by changes in the magnitude of the variables which, under prevailing institutions and types of economic calculation, make these flows as large as they are.[71]

3. It follows, *a fortiori,* that any particular *element* in cost, such as wage-costs, can affect the determination of realized prices, *upon the side of supply,* only insofar as wage costs can be shown to affect market *supply* price, and only insofar as a given market supply price *becomes an actually realized price.*[72]   For (1) "wages" are only one of the ele-

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argument presented in Volume I of the present work. See, for example, Volume I, 48 f., 249 ff., 258 ff., 518 ff., 581 f. Indeed, the point may be regarded as a corollary of a proposition to which Mr. Keynes himself acknowledged formal allegiance at the time he wrote his *Treatise,* but of which, unhappily, there is little trace in the argument of the *General Theory,* with its comparative indifference to the problems of *structure:* namely, that "a movement in the price of one commodity necessarily influences the movement in the prices of other commodities," and that "the magnitudes of these compensatory movements depend on the magnitude of the change in expenditure on the first commodity as compared with the importance of the expenditure on the commodities secondarily affected"; so that, "instead of 'independence', . . . there is what some writers on Probability have called 'connexity' " between the movements of different prices (*Treatise,* I, 86). See also Mr. Keynes's *How to Pay for the War,* 34, 52; and cf. what is said above, p. 566, n. 35, and below, p. 620, n. 134, on the relation of the Theory of Prices implicit in this most recent publication to the Theory of Prices presented in the *General Theory.*

   [70] Sufficient proof to the contrary is provided by the *General Theory's* treatment, for example, of the nature of the forces affecting the *administration* of *cash balances*—or, if one prefers, of the nature of the forces affecting the degree of "liquidity preference." There can be little doubt, surely, as to the substantial accuracy of Professor Ellis's statement ("Some Fundamentals in the Theory of Velocity," *loc. cit.,* 470) that "Keynes represents people as *deciding* what balances they desire to keep."

   [71] See, for example, the quotation from Kaldor given below, p. 592, n. 91.

   [72] The italicized phrase, "upon the side of supply," is designed to call attention to the fact that we are deferring for later consideration the effect upon prices of cost items, including wage "costs," which may also be elements in *income.* See our Proposition XXXVIII (below, p. 604).

ments involved in "cost"; (2) cost prices are not necessarily identical with *supply* prices; and (3) not all "supply prices" are necessarily *realized* in the market. If, therefore, "the classical school" had actually argued that, when the workers demand a change in the wage rates, *realized prices* "would change in almost the same proportion," on the double ground (1) "that prices are governed by marginal prime cost in terms of money"; and (2) "that money wages largely govern marginal prime cost," it would have been guilty not only of the crudest of analytical blunders, but also of a flagrant disregard of its own teachings within *both* the "general" Theory of Value and the Theory of Money and Prices.[73]

This proposition deserves particular emphasis, precisely in view of the fact that Mr. Keynes not only has stated that the argument in question would, to his thinking, "contain . . . a large element of truth," but has himself made use of the argument in his own positive analysis.[74] He has admitted, to be sure, that "the complete results of a change in money-wages are more complex" than they are represented as being by the argument under consideration; and, in particular, he has on more than one occasion underscored the word "largely," in his proposition that "money-wages *largely* govern marginal prime cost," and the word "almost" in the statement that "prices would change in *almost* the same proportion" as wage rates, by pointing to some of the consequences that follow from the fact that "wages" are, after all, not the *only* element in "marginal prime cost."[75] Whether these concessions are sufficient to

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[73] The quotations are from the *General Theory*, 12. Cf., however, the acknowledgment by Mr. Keynes, in his later article, "Relative Movements of Real Wages and Output," *loc. cit.*, 39 f., to the influence of Mr. R. F. Kahn in convincing him that it was proper to apply, to the problem of "the relation of the general level of prices to wages," propositions such as "that in competitive conditions prices are governed by marginal cost," and "that for a closed system as a whole marginal cost in the short period is substantially the same thing as marginal wage cost," "*rather than*" to regard the result as one "to be derived from monetary factors."

[74] See the *General Theory*, 12 n.; and cf. pp. 251, 253, 262 f., 270, 307, of the same work. In this respect, as in so many others, the practice of Mr. Keynes has been faithfully followed by a number of his disciples. See, for example, A. P. Lerner, "Mr. Keynes' 'General Theory of Employment, Interest, and Money,'" *International Labour Review*, XXXIV (1936), 441 f.; Meade, "A Simplified Model of Mr. Keynes' System," *loc. cit.*, 98, 103; Joan Robinson, *Introduction to the Theory of Employment*, 57, 96 f.; H. W. Singer, "Price Dispersion in Periods of Change," *loc. cit.*, 658, 662 (though see also p. 673 of the same article).

[75] See the *General Theory*, 12 n.; and cf. (1) p. 173 of the same work, where the degree to which "prices will rise" is held to be "partly governed

take the sting from those passages in the *General Theory* which, in stressing the close relation between *wage*-costs and prices, reflect more strongly Mr. Keynes's avowed sympathy with the crudest versions of "the labor theory of value," according to which labor is the *only* factor that need be considered in dealing with costs; whether, in other words, Mr. Keynes has been guilty, in the degree in which he has been charged, of following the alleged "Ricardian" practice of first pointing out that labor is *not* the only factor of production, and then reasoning as if it were: these are matters which need not concern us here.[76] For, given (1) Mr. Keynes's formal recognition of the fact that wages are not the only element in cost, and need not necessarily vary in the same proportion as other elements in cost; and (2) his occasional willingness to deal with movements in a "cost-unit," instead of identifying movements in the "wage-unit" with the movements in such a "cost-unit," he cannot be charged with a formal error in analysis in this part of his argument.[77] The formal error enters, not in the *General Theory's* treatment of the relation between the wage unit and the "cost-unit," but in its treatment of the *rôle played by movements in the "cost-unit" in the*

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by the shapes of the physical supply functions," *as well as* by "the liability of the wage-unit to rise in terms of money"; (2) pp. 270 f., where it is argued that "the price-level will only change in the short period in re- sponse to the extent that changes in the volume of employment affect *marginal prime costs*"; (3) p. 294, where it is argued that "the general price level depends partly on the rate of remuneration of the *factors of produc- tion which enter into marginal cost* and partly on the scale of output as a whole"; (4) pp. 299 f., on the possibility of having "increasing marginal prime costs over and above any increase due to increasing labor costs," so that "increasing output will be associated with rising prices, apart from any change in the wage-unit"; (5) p. 302, on the difficulties in the way of "assuming that the remunerations of the various factors entering into marginal cost all change in the same proportion" as the "wage-unit," and therefore on whether it might not "be better, perhaps, to take a weighted average of the rewards of the factors entering into marginal prime-cost, and call this the *cost-unit*"; and (6) p. 309, where the argument with re- spect to "the long-run stability or instability of prices" is regarded as being "more precisely" stated in terms of "the cost-unit" than in terms of the "wage-unit." Cf. also Mr. Keynes's later article, "Relative Movements in Real Wages and Output," *loc. cit.*, 44.

[76] See the quotation from Professor Schumpeter's review of the *General Theory* given above, p. 534, n. 28. On this aspect of the argument of the *General Theory*, particularly as it has been interpreted by some of Mr. Keynes's disciples, see also Haberler, *Prosperity and Depression*, 239 f., and the reference to A. P. Lerner given on p. 240, n. 1 of that work (together with the references to Mr. Lerner and Mrs. Robinson given below, p. 590, n. 87); and cf. R. M. Bissell, "Price and Wage Policies and the Theory of Employment," *loc. cit.*, 213 f., 225 f.

[77] For examples of a differentiation, in the *General Theory*, between changes in the "wage-unit" and the "cost-unit," respectively, see the last two references given under (5) and (6) in n. 75, above.

determination of realized money prices; and on this point nothing need be added to what was said above under (2).

In view, however, of Mr. Keynes's suggestion that his proposition with respect to the "governing" of prices by movements in the wage-unit is a proposition that one would have "expected the classical school" to have advanced, it *is* worth adding that the leading members of the "classical" school, from Ricardo to Marshall, not only did not advance this argument, but explicitly repudiated it.[78]   It was Ricardo, for example, who insisted that "the rise of wages will not raise the prices of commodities." [79]   It was John Stuart Mill who insisted that "a rise of money wages does not raise prices; that high wages are not a cause of high prices": indeed, that to argue that they are is to attempt to maintain a proposition "equally contrary to reason and to fact." [80]   It was the "orthodox" Cairnes who insisted that the proposition that "high wages . . . do not make high prices" is "indisputably sound and quite fundamental." [81]   It was Wicksell (so persistent, according to Mr. Keynes, in *"trying* to be 'classical' ") who gave his explicit approval to the proposition of "Ricardo, and later John Stuart Mill . . . that a general rise in wages cannot possibly increase the price of goods produced by the same labor." [82]   And it was Marshall (a "classical" economist, according to Mr. Keynes, by virtue of his position among those "who adopted and perfected the theory of the Ricardian economics") who insisted that "movements in wages almost always follow, and scarcely every occasion, movement in prices"; that it would be very much closer to the truth to say that in most cases "each rise in wages

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[78] It is also worth adding that the only cases known to me in which a proposition similar to that advanced by Mr. Keynes *was* advanced by earlier writers are cases in which the writers concerned refused, unfortunately, to be "diverted from this line of thought" (in the words of Mr. Keynes) "by preoccupation with the idea that prices depend on the quantity of money" (*General Theory*, 12). See, for example, J. L. Shadwell, *A System of Political Economy* (1877), 334, where a preliminary assault on "the proposition that the value of money varies inversely as its quantity" was followed by the proposition that "the price of a commodity depends on the quantity of labor employed in producing it, and on the rate at which that labor is remunerated, and if the price rises, it must either be because more labor has been expended, or because the laborers have received higher wages."

[79] *Principles,* Chapter V; cf. also Chapter VII (pp. 81 f., 113 of the Gonner edition).

[80] *Principles,* Book III, Chap. XXVI, sec. 3; cf. also Book III, Chap. IV, secs. 2 and 3, and Book III, Chap. XXV, sec. 4 (pp. 692, 459 ff., 684 f. of the Ashley edition).

[81] *Some Leading Principles of Political Economy Newly Expounded,* 200 ff.; cf. also the same author's *Essays in Political Economy,* 60 n. On Cairnes as an "orthodox" economist, see above, p. 313, n. 194.

[82] *Lectures,* II, 156. For Mr. Keynes's comment on Wicksell, see above, p. 7, n. 11.

is caused directly by a rise in price"; and that "the fall of wages . . .
is occasioned by and is not the cause of the fall in prices." [83]

No one could assert that everything that was said by these "classical"
writers with respect to the relation between wages and prices can stand
the most exacting scrutiny on either the analytical or the empirical
side.[84] What one can assert, however, is that what they had to say on
the subject does show an awareness of the necessity for making use of
the elements which, it is here argued, must be involved in any attempt
to provide a satisfactory solution of the problem. The first of these ele-
ments is an adequate version of the "general" Theory of Value, in which
justice would be done, in particular, to the element of *demand* in the de-
termination of realized money prices (including the determination of
realized money wage-rates).[85] The second of these elements is an ade-

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[83] Marshall, *Economics of Industry,* 165 ff.

[84] It is of considerable interest to observe that this was also the position
of an "orthodox" writer such as Cairnes, who, as we have seen, neverthe-
less accepted as "indisputably sound and quite fundamental," and, indeed,
as "incontrovertible," the proposition that "high wages . . . do not make
high prices" (cf. the reference given above, p. 586, n. 81). For, without
relaxing for a moment his adherence to this general principle, and without
hesitating for a moment to characterize "the popular inductions" to the
contrary as "erroneous and even absurd," Cairnes was prepared to argue
that the received theory on the subject of the relation between wages and
prices was "at best . . . incomplete," and indeed was sufficiently "defec-
tive" in certain respects to warrant the generalization that "the recognition
given [by earlier writers] to the connection between prices and wages is
quite inadequate" (*Some Leading Principles of Political Economy Newly
Expounded,* 201 f.). The *difference* between Cairnes and the type of crit-
icism levelled against "the classical school" by the *General Theory* and its
adherents is that while Cairnes was certainly prepared to say that "the
actual state of this portion of economic theory . . . is plainly inadequate;
failing as it does to elucidate many familiar phenomena of wages and
prices," he nevertheless continued to insist that the relevant "portion of
economic theory" was *"irrefragable so far as it goes,"* and that what ought
to be done was not to *overthrow* "this portion of economic theory," but
"to supplement, as far as seems needful, existing deficiencies" in received
doctrine on the point in question (*Some Leading Principles,* 203). It is to
be observed, for example, that instead of emphasizing cases in which it
could be said that "wages are the cause . . . and prices the effect," Cairnes
objected to the failure of earlier writers to emphasize sufficiently those
cases in which *"prices* may affect *wages"* (*ibid.,* 200, 202; italics mine). And
it is to be observed that virtually all of Cairnes's own improvements can
be regarded as further illustrations of the two elements indicated in the
following sentences of the text. Cf. also nn. 85 and 86, immediately fol-
lowing.

[85] In this connection, cf. Ricardo's proposition that "when wages rise,
it is generally because" of "a new *demand for labor"* (*Principles,* 81; italics
mine), and Mill's objection to the suggestion that an entrepreneur will in
all cases be able to "get rid" of the difficulties caused by a "rise of wages"
by "raising his price" on the commodities whose production would thus

quate appreciation of the rôle played in the problem by the substance of the Theory of Money and Prices.[86]   In both respects, the respective

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require added expense (Mill, *Principles*, 692).   The first quotation, in particular, itself provides a partial correction of the suggestion, by Wicksell, that "the classical economists" perhaps overlooked" the relation between "a rise in wages" and "an increased (money) *demand for labor*" (*Lectures*, II, 157; italics mine).   If, moreover, Cairnes is included among "the classical economists" who wrote prior to Marshall, it is he who provides the clearest case of an insistence upon the rôle played by *demand* in establishing the relation between wages and prices, by affecting the realized price of the *commodity*, in the first instance, and the wages of the workers engaged in the supply of the commodity in the second instance (see, for example, Cairnes's *Some Leading Principles*, 202, 204, 206, 208, 211); and it is Cairnes who provides the clearest case of an insistence upon the fact that rises in wage rates would affect prices on the side of *supply* by inducing entrepreneurs to effect supply-adjustments as the result of changes in the relation between costs and selling prices, actual and prospective (see, for example, *Some Leading Principles*, 205 ff.).   Cairnes's understanding, indeed, of the relation of the principles of the "general" Theory of Value to the problem in hand is sufficiently indicated by the summary comment which follows his proposition that "there is thus a real and fundamental connection between money wages and prices."   "I conceive," he wrote, "it would be incorrect to describe either phenomenon as the cause or the effect of the other: they are rather co-ordinate results of a common cause —that cause being the influence, whatever it may happen to be, which *determines the products of a particular industry to exchange for those of others on more or less favorable terms than had previously obtained*" (*Some Leading Principles*, 210; italics mine).   Nor can there be any doubt as to *Marshall's* awareness of the relation of his argument to the particular variant of the "general" Theory of Value which he sponsored.   For the discussion indicated above (cf. the reference to Marshall given above, p. 587, n. 83) appears in that Book (Book III) of his *Economics of Industry* which is entitled "Market Value," and in the particular chapter of that Book entitled "Market Fluctuations"; and the chapter in question ends its examination of the relation between wages and prices on a note which can leave no doubt as to Marshall's awareness of the bearing of his "general" Theory of Value upon the specific problem in hand.   "Thus," he wrote, "we see how the Law that Normal value is determined by Normal Expenses of production is consistent with the fact that market fluctuations of values are the cause and not the consequence of market fluctuations of Expenses of production.   If Ricardo and Mill had taken more pains to make clear the distinctions between the theory of Normal value and that of Market value, there could not have been as much controversy as there has been on the question whether value is governed by Expenses of production, or Expenses of production by value" (*Economics of Industry*, 167).   Cf. our Proposition XXIX, 1 (above, pp. 553 f.).

[86] See Ricardo's *Principles*, 82, where Ricardo undertook to support his proposition "that the rise of wages will not raise the prices of commodities" by adducing the further proposition that "all commodities cannot rise at the same time without an addition to the quantity of money" (or, as we should now say, without an increase in the aggregate money demand, in the determination of whose magnitude changes in the quantity of money

positions of these "classical" writers, and the analytical results to which these positions led, are to be contrasted not only with the analysis underlying the bizarre propositions with respect to the control of realized prices by the control of *wage rates* which can be found in the writings of unmitigated cranks in our own day, but also with the propositions of the *General Theory* with respect to the relation between "stability" in wage rates and "stability" in the "price level." [87] And in both

will be an important factor). Cf. also Mill's *Principles,* 459 f., where, in characterizing the proposition "that high wages make high prices" as "a popular and widely-spread opinion," Mill remarked that "the whole amount of error involved in this proposition can only be seen thoroughly when we come to the theory of money." It was Cairnes, however, who provided both the *improvements* in "the theory of money" which were required if the received treatment of the relation between wages and prices was to be made less "inadequate," and the *application* of these improvements to the problem in hand. See his *Some Leading Principles,* 207 ff., and his *Essays,* 58 ff. It should be added that there are aspects of Cairnes's discussion which bear directly upon Mr. Keynes's suggestion that one of the things, in addition to "preoccupation with the idea that prices depend on the quantity of money," that "diverted" members of "the classical school" from advancing the proposition that "prices would change in almost the same proportion" as changes in the wage unit, was "the settled conviction that labor is in a position to determine its own real wage" (*General Theory,* 12). See, for example, the remarks on the relation of "real wages" to "money wages" in Cairnes's *Some Leading Principles,* 203 f. (including the footnote), 205 f., 211 ff.; and contrast the comment in the *General Theory,* 14, on "the workers" as being "instinctively more reasonable economists than the classical school," in that their policy rests, "though unconsciously," upon a clearer recognition of the fact that movements of "money-wages . . . are seldom or never of an all-round character." Much the same thing must be said with respect to the contention which Mr. Keynes not only attributes to "the classical economists," but regards as "indefeasible": namely, that "with a given organization, equipment and technique, . . . an increase in employment can only occur to the accompaniment of a decline in the rate of real wages" (*General Theory,* 17). As Professor Viner has pointed out, such a proposition fails to allow, among other things, for "the possibility that the prices of wage-goods and of other goods may have divergent movements" ("Mr. Keynes on the Causes of Unemployment," *Quarterly Journal of Economics,* LI [1936], 149 f.). It is worth noting, therefore, that precisely this possibility was taken into account by Cairnes (*Some Leading Principles,* 211 f.). If, therefore, Cairnes is to be regarded as an exponent of "classical" doctrine (and on this matter see the remarks of Cairnes himself in the Preface to *Some Leading Principles,* p. 1), it is certainly open to question whether it is "the *classical* doctrine" which Mr. Keynes has followed "too closely" in advancing the proposition indicated. Cf. Viner, *loc. cit.;* and on the general point involved, see the further references given in Haberler, *Prosperity and Depression,* 239, n. 1.

[87] For an example of "the writings of unmitigated cranks," to which reference is made in the text, see A. G. McGregor, *The Correct Economy for the Machine Age* (1935), 17 ff., 35, *et passim,* and the same author's *Right Wages and Abundance* (1938), 17 ff., 34 ff., *et passim.* For examples,

respects they provided a further illustration of the extent to which the most widely accepted versions of the "general" Theory of Value and of the Theory of Money and Prices, instead of being in flagrant contradiction with each other, have in fact provided a continuing analytical *control* over each other in any attempt to account for the determination of realized money prices.

XXX. To argue, as our Proposition XXIX argues, that certain sectors of the Theory of Money and Prices may be regarded as providing a kind of analytical *"control"* over the implications assigned to the "supply curve" $q = \Phi(p)$ of the "general" Theory of Value when this "supply curve" is used in accounting for the determination of realized money prices, is not to argue ,that this is the *only* way in

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from the *General Theory,* of propositions of the type to which the text refers, with respect to the relation between stability in wage rates (or "money wages") and stability in the "price level" (or "money prices"), see pp. 239, 251, 253, 270 of that work. It is, indeed, propositions such as these which are probably the origin of the unqualified propositions, advanced by supporters of the *General Theory,* that if we assume "that all prices other than wages are perfectly flexible and that the monetary supply is infinitely elastic," then we may conclude that "the level of wages determines all prices"; that therefore any "decision as to the level of wages" is also a decision as to the level of "prices"; that "a rigidity of the money wage is necessary to give . . . stability to prices"; that if we assume that not only wages, but also rents, are inflexible, then "prices depend upon both wages and rents"; that if "the prices of both labor and land . . . are raised or lowered in the same proportion, this will . . . change all prices in the same proportion," and "at lower wages and rents . . . all prices will be lower" (Lerner, "The Relation of Wage Policies and Price Policies," *loc. cit.,* 163 ff.). It must be left to the reader to judge, on the basis of the argument developed above, the validity of Mr. Lerner's claim that "the simple tools" involved in arguments of this type "enable us to see the effects and the mechanism of the effects of absolute and relative changes in factor prices" (*op. cit.,* 167). To the reader, also, must be left a judgment of Mr. Lerner's implication that the only type of case in which "the level of wages" will *not* "determine all prices" is that in which such prices are "kept up by rigidities" (*op. cit.,* 168); as well as of Joan Robinson's brave proposition that "without rising money wages, inflation cannot occur, and whatever starts a violent rise in money wages starts inflation," since "each rise in wages raises prices" (*Economic Journal,* XLVIII [1938], 510 f.). And the reader must be left to decide, finally, whether a full appreciation of what is at stake, for the future of both the Theory of Money and Prices and the "general" Theory of Value, in the Keynesian treatment of the relation between wages and prices, is conveyed by the simple statement that he assumes all prices to be "malleable" (or "flexible"). In this connection, cf. Harrod, "Mr. Keynes and Traditional Theory," *loc. cit.,* 80, 82, and Bissell, "Price and Wage Policies and the Theory of Employment," *loc. cit.,* 213, 235.

which the Theory of Money and Prices may be said to supplement that part of the "general" Theory of Value which is summed up by the expression for the "supply curve" $q = \Phi(p)$. For, as in the case of the demand side of the problem, if we are to describe the particular market supply curve which is involved in the determination of a given realized price, it is not sufficient merely to establish the *general form* of the function $q = \Phi(p)$. It is necessary, as in the case of the demand side, to establish, among other things, the *position* of the particular supply schedule, of the general form $q = \Phi(p)$, in the system of co-ordinates of which the price axis represents *absolute* money prices.[88] Again it should be pointed out that there is nothing in the "general" Theory of Value, as ordinarily expounded, which provides an answer to this question; and again it should be pointed out that, in order to provide such an answer, we need a special "money equation," such as is represented by the Fisherine equation $MV = PT$.[89]

XXXI. From this follows a corollary which is strictly relevant to the problem of the determination of money prices even if we interpret the proposition that these prices are "determined" by "the scale of money remunerations of the factors of production" as meaning that the prices of commodities are *partially* "determined" by the level of their *supply prices* (that is, by the position of market supply curves in the system of co-ordinates of which the price axis represents *absolute* money prices).[90] This corollary is that to contend that acceptance of the proposition indicated

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[88] Cf. above, p. 280 (Proposition VII), and also n. 126 thereto.

[89] In this connection, cf. the references to Fisher given above, p. 283, n. 132. Fisher, of course, applied the argument as directly to the particular "*supply* curves" of the "general" Theory of Value as he did to particular "*demand* curves." See the references given above, pp. 106 f., nn. 38, 40, and 41; and see especially *The Purchasing Power of Money*, 176: "If we attempt to explain the *money* price of a finished product in terms of the *money* prices of its raw materials and other *money* costs . . . of production, it is clear that we merely shift the problem. We have *still* to explain these antecedent prices" (italics Fisher's).

[90] In the light of the discussion presented above, pp. 553 ff., in connection with our Proposition XXIX, it should be clear that very generous canons of interpretation are required in order to justify this particular interpretation of the proposition quoted. For the proposition itself, see, for example, N. Kaldor in the *Economic Journal*, XLIX (1939), 497.

involves a *denial* that "prices" are "determined" by the "flow of money payments," as represented by "stream" equations of the general form $MV = PT$, is to set up an *antithesis which is entirely false.*[91]   For the magnitude of *the flow of money payments, in combination with the magnitude of the stream of objects offered for sale against such payments, is precisely one of the things that explain why the absolute levels of the realized "money remunerations of the factors" (and therefore the absolute height of the market supply curves of these factors and of the market supply curves which are derived from the latter) are as high or as low as they are.*[92]   And it is precisely one of the major pur-

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[91] Cf. Kaldor, *loc. cit.*: "The level of prices is determined by the scale of money remunerations of the factors of production, *and not* by the flow of money payments" (italics mine). A similar false antithesis is, of course, implicit in most of the writings by members of the Keynesian group on the determination of commodity prices. Again, however, attention should be called to the testimony of Mr. Keynes which would suggest that the real source of the "Keynesian" revival of this false antithesis was Mr. R. F. Kahn. See again the quotation from Mr. Keynes's "Relative Movements of Real Wages and Output" given above, p. 584, n. 73, with respect to Mr. Kahn as the writer "who first attacked the relation of the general level of prices to wages in the same way as that in which that of particular prices has always been handled, namely as a problem of demand and supply in the short period *rather than* as a result to be derived from monetary factors" (italics mine). By "first," of course, Mr. Keynes can mean only "first among the Keynesian group"; for the antithesis in question is one of the most ancient of false antitheses in the history of our subject; and the demonstration of the falsity of the antithesis is likewise very ancient. See, for example, (1) the comment made above, p. 69, and in n. 40 thereto, on this aspect of Menger's general theoretical position, and his reference to Bodin as one of his predecessors; (2) what is said above, p. 150, n. 19, and p. 152, n. 21, with respect to Tooke's final position that prices are determined by "demand" and "supply," *and not* by monetary factors; (3) the comments on Lubbock, Newcomb, and others, made above, p. 153, n. 24; p. 270, n. 107; p. 271, n. 109; p. 273, n. 113; and p. 273, n. 114; and (4) the comment made above, p. 271, n. 108, on Cairnes's criticism of Newmarch.

[92] The inevitability of the conclusion indicated could not be better demonstrated than it is by the fact that it is a conclusion to which, despite all superficial appearances to the contrary, Mr. Keynes himself was driven in his *General Theory*. It has been recognized, even by some of Mr. Keynes's followers (however obscurely they may have stated the point), that much of the *General Theory's* argument with respect to the establishment of the level of *absolute* (or "actual") prices is prevented from making the "course" of "actual" money prices "quite indeterminate" only because we are presumed to "know the course of the money-price of some one single or composite valuable (e.g., labor)" (so, for example, H.

poses of a "money equation" of the general form $MV = PT$ to establish the nature of the forces which make the magnitude of these "flows" as large, in absolute terms, as it is.

XXXII. According to our Proposition VIII (p. 285), one of the fundamental purposes of a "money equation" of the general form $MV = PT$ is to establish the nature of the forces determining the magnitude of the *sum of realized money demands* (ΣD). By that proposition it was established, further, that only if we know the absolute level of the total amount of money-spending power which is available for realizing money demands, as well as the degree to which this money-spending power is actually utilized in the realization of money demands (that is, only if we know the magnitude of $MV = \Sigma D = \Sigma pq$), can we determine what will be the absolute level of any *one* of these individual demands (the individual D's) and therefore the absolute level of the various individual p's involved in individual expressions of the form $D = pq$.

But it should be clear also (1) that the proportions in which aggregate money demand will be distributed in the realization of individual demands will *depend as much upon the conditions of supply* for individual commodities as it will upon the conditions of demand for such commodities;

Townshend, "Liquidity-Premium and the Theory of Value," *Economic Journal*, XLVII [1937], 164). What most of Mr. Keynes's followers do not seem to have recognized, however, is that Mr. Keynes's account of what *does* determine the absolute height of the money price of "labor" (the "wage-unit") actually runs in terms of an "impact of mutual flows" of money and of objects (in this case, labor) sold for money. The account in question is, of course, not to be found in statements such as that the wage unit is "determined by the bargains reached between employers and employed" (*General Theory*, 247); since such statements tell us little more than is told us by the statement that "prices" are "determined by supply and demand." It is summarized, rather, by the expression $e_w \left(= \dfrac{DdW}{WdD} \right)$ which represents "the elasticity of money-wages in *response to* changes in effective demand [D] in terms of money" (*General Theory*, 285; cf. also pp. 295, 298 ff., 304 ff. of the same work; italics mine). Indeed, it is not at all surprising that Mr. Keynes himself regards the equation to which he is led by the use of this expression for e_w as "a first step toward a generalized Quantity Theory of Money" (*General Theory*, 285)—which in turn can be shown to be nothing more than a disguised Quantity Equation of the general Fisherine form. On the latter point, see Chapter Fourteen below.

since, by Propositions III (p. 240) and XXIV (p. 550), individual realized prices, and therefore individual realized demands, are what they are quite as much as a result of the conformation and position of market *supply* curves as of the conformation and position of particular demand curves. And it should be clear (2) that knowledge of the conditions of supply for particular commodities is necessary if we are to know how much of a given realized money demand (D) will be reflected in the rise of the price (p) of a given commodity, and how much will be reflected in an increase in the realized supply (q) at that price. It follows, therefore, (3) that a proper use of a "money equation" of the general form $MV = \Sigma D = \Sigma pq$ for the purpose of understanding why individual realized *demands* and individual *prices* are what they are, requires a simultaneous interpretation of the *right*-hand side of the "money equation" $MV = PT$ in such a way as to do full justice to the implications of the expression $q = \Phi(p)$—the expression for a "particular supply curve"—which were pointed out by our Propositions XXV (p. 551) and XXVI (p. 551).[93]

A close parallellism obviously exists between the corollaries that may be drawn from our Proposition XXXI with respect to *supply* and the corollaries that were drawn from the corresponding Proposition VIII with respect to *demand:*

1. In our discussion of Proposition VIII, attention was called to the essential identity of the argument of that proposition with the argument underlying the devices, familiar within the "general" Theory of Value, for dealing with the phenomena of "joint demand." [94] Here, therefore, I need only point out that, according to the theory of joint demand,

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[93] It should be clear that this conclusion follows even if we make use of *"Auspitz and Lieben"* supply curves, which are of the general form $q = \Psi(D)$, rather than of the form $q = \Phi(p)$. For in all cases it is possible to derive a representation of supply as a function of *price* from a representation of supply as a function of the amount of money demand, by representing prices as tangents of the angles made by lines from the origin to any point on an Auspitz and Lieben supply curve (cf. above, p. 263, n. 93). And in all cases such a procedure is *required* if we are to put ourselves in a position to understand *why* suppliers will be prepared to supply given quantities of commodity in response to given amounts of money demand; for these responses will be what they are as the result of entrepreneurial calculations with respect to the relation between a given proposed selling *price* and other *prices*, actual and expected (including "cost" prices, actual and expected).

[94] See above, p. 286.

the establishment of the amount of "demand" that will be directed
against any one commodity which is "jointly" demanded with others
requires knowledge not only of the demand price for the whole com-
bination, but also of the *supply* prices of the commodities demanded
"jointly" with the commodity the demand for which it is desired to
determine—that is, it requires knowledge of the form of the individual
supply curves $q = \Phi(p)$.[95]

2. It should be clear, in the second place, that the argument de-
veloped in Chapter Five with respect to the relation of our Propo-
sition VIII to the case for a "total transactions equation" is reënforced
by the argument of our Proposition XXXII.[96] The contention, for ex-
ample, that a *"complete* picture of the economic process" involves an
application of the argument of Proposition VIII (with respect to the
relation between *"aggregate"* and *"particular"* realized demands) to the
realized "demands" of *entrepreneurs* takes on particular cogency in
view of the fact that the amount and direction of these realized entre-
preneurial "demands" represent a key station in the analysis of the
forces which make the level and structure of *output* (included in the q's
of a total transactions equation) what they are. And the contention
that a full accounting for the dimensions of any one type of money-ex-
penditure stream requires knowledge of the facts with respect to the
amount of money expenditure (1) available in the aggregate, and (2)
devoted to uses other than that represented by the type of monetary
expenditure taken for examination, is obviously crucial for an under-
standing of the supply responses (as represented by individual supply
curves of the general form $q = \Phi(p)$) of any one group of entre-
preneurs to the exertion of realized demands by others, whether these
"others" are entrepreneurs or non-entrepreneurs; for these supply re-
sponses will depend in very large part upon the *structure* of realized
money demands. It should be observed, finally, that the supply re-
sponses in the different sectors of the economic process may themselves
be closely interconnected, quite apart from the effect, upon these supply
responses, of changes in the structure of realized money *demands*. This
will be true, for example, whenever the "real" resources required to
effect a supply response are capable of *alternative uses;* for this means
that any attempt to satisfy money demand in one sector of the eco-
nomic process will be bound to affect the conditions of supply (and
therefore the form of individual supply functions of the general form
$q = \Phi(p)$) in another sector. These are propositions, of course, which
are thrice-familiar to those acquainted with the type of apparatus as-
sociated with the concept of general economic interdependence. They
are repeated here only by way of emphasizing that the type of ap-
paratus recommended represents a direct application to *monetary*
theory of propositions accepted as axiomatic within the "general" Theory

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[95] See Marshall's *Principles,* 383, 852 f., 855.

[96] For the earlier argument to which reference is here made, see above,
pp. 287 ff.

of Value; and that an adequate understanding of the implications of a "total transactions" equation would extend these applications to the facts of realized "*supply*," as summed up in the second member (the right-hand side) of such an equation, as well as to the facts of realized "demand," as summed up by the first member.

3. The bearing of our Proposition XXXII upon the essentially factitious issues raised in discussions of "the alleged 'Law of Compensatory Price Change'" should be clear as soon as it is remembered that the "refutation" of this "alleged 'Law'" is based entirely upon possibilities with respect to changes in the q's of our formulation (and therefore the T of Fisher's formulation).[97] The purpose of Proposition XXXII is to remind the reader that the magnitude of the q's (amounts "sold") will depend not only upon the conditions of *demand* (including the Marshallian "elasticity" of demand, so greatly stressed in "refutations" of "the alleged 'Law'"), but also upon the conditions of *supply*. The bearing of this conclusion upon the validity of "the alleged 'Law'" is so obvious that the real question is again whether any important group of monetary theorists ever did support "the alleged 'Law'" in the face of such cogent reasons against its acceptance. On this matter, the reader is referred to the discussion presented above.[98]

XXXIII. From our Proposition XXXII (p. 593), it is clear that the apparatus described here cannot be charged with a lack of interest in "aggregative" concepts such as "aggregate" demand and "aggregate" supply. But it is of the first importance to observe *how* these "aggregative" concepts are defined, and how they are related to the "particular" demand and supply schedules of the "general" Theory of Value. What are "aggregated" are the individual *realized* "demands" ($\Sigma D = MV$) and the individual *realized* "supplies" (Σq).[99] Demand and supply *schedules* are retained as an essential part of the apparatus; but they are retained in a form—namely, the "Marshallian" form of demand and supply schedules for *particular* commodities— which is consistent with the requirements (1) that they be capable of direct relation to the "plans" and decisions of the *individuals* and the individual *firms* whose market actions make realized money prices what they are; (2) that

[97] See above, p. 291.

[98] See above, pp. 291 ff.

[99] The "aggregation" of individual realized "demands" obviously gives no trouble, since these individual realized demands are all realized *sums of money*. On the techniques used in the aggregation of individual realized "supplies," see what is said above, p. 536, n. 33.

they take full account of the fact that any given demand or supply schedule is subject to change as the result of changes elsewhere in the structure of prices and output; and (3) that changes in supply, in particular, are to be regarded as dependent upon the *structure* of prices—specifically, the structural relation, realized and expected, between those prices, on the one hand, which are *costs* to particular entrepreneurs, and those prices, on the other hand, which are selling prices to those entrepreneurs.[100]

XXXIV. The fact that problems of price-*structure* are involved has sometimes led, for reasons of exposition, to the use of (1) "real" demand and supply functions; or (2) the use of a *numéraire* in terms of which both the "aggregative" and "particular" magnitudes involved in the pricing process are then translated.[101] In a completely developed money

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[100] See above, pp. 541 ff. On the suggestion that an interest in problems of *structure* (and particularly the structural relation between "costs" and selling prices) necessarily involves a blindness to the importance of studying the causes and consequences of changes in "aggregates," see what is said above, pp. 543 ff.

[101] The first method is of course that which has been most widely used by representatives of "old" Cambridge, though the fact that what was involved was an attempt (however cumbersome) to take account of changes in the *price structure* has often not been recognized by critics of "old" Cambridge. See above, pp. 141 ff., and especially p. 143, n. 6. Representatives of "old" Cambridge—including Marshall and Pigou—have also made use, however, of the device of a *numéraire,* of which "wheat" is an example. See my "Léon Walras and the Cash-Balance Approach," *loc. cit.,* 579, n. 24, and 582, n. 32. In his *General Theory,* therefore, Mr. Keynes has simply followed an "old Cambridge" practice in making use of his "wage-unit" as a *numéraire.* (For a characterization of Mr. Keynes's use of his "wage-unit" in these terms, see, for example, Lange, "The Rate of Interest and the Optimum Propensity to Consume," *loc. cit.,* 12 f., 19, and especially the reference on p. 19, n. 2, to the parallellism with the use of "wheat as a *numéraire*" by "Marshall and Professor Pigou"). It is not without interest, consequently, to observe that a kind of Nemesis has overtaken Mr. Keynes, in this instance as in others (see, for example, Volume I, 102 ff., of the present work). For just as, in his *Treatise,* he had charged that Professor Pigou's use of "wheat" as a *numéraire* rested upon the assumption that "relative prices are unchangeable, all individual prices and therefore all price levels being *fixed* in terms of wheat" (see my "Léon Walras and the Cash-Balance Approach," *loc. cit.,* 583, n. 32), so his own habit of *expressing* magnitudes in terms of "wage-units" has been regarded as equivalent to assuming that the magnitudes in question will "move with money wages" (see, for example, Hicks, *Value and Capital,* 256). Actually, of course, as we have seen (above, pp. 583 ff.), Mr. Keynes's "assumption" that prices "move with money wages" is de-

economy, however, a figure for any "real" magnitude (or for a magnitude stated in terms of a given amount of the *numéraire* commodity) can be obtained only from information with respect to (*a*) the absolute height of the money magnitude which is to be translated into "real" terms (or in terms of the "numéraire" commodity); and (*b*) the absolute height of the money prices of the particular commodities included in these "real" magnitudes (or the absolute height of the price of the *numéraire* commodity). And since, by our Propositions IX (p. 296) and XXX (p. 590), information with respect to the nature of the forces determining the absolute level of money prices and the other monetary magnitudes involved can be provided only by the Theory of Money and Prices, the latter again becomes a

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rived, not from his use of the "wage-unit" as a *numéraire*, but from a separate series of propositions with respect to the "governing" of prices by "marginal prime costs" and the relation of wages to "marginal prime cost." It may be said, indeed, that the use of the wage-unit as a *numéraire* "raises quite preposterous difficulties unless prices are assumed to be highly flexible" (so that they may be assumed to vary, for example, with changes in the wage-unit: see Bissell, "Price and Wage Policies and the Theory of Employment," *loc. cit.*, 209, and cf. above, p. 589, n. 86); and it may very well be that Mr. Keynes would not have proposed the use of his "wage-unit" as a *numéraire* if he had not regarded as reasonable on other grounds the particular assumption with respect to the structural relation between movements in the "wage-unit" and prices, respectively, which was examined above, pp. 583 ff. The real point to be made, however, is that the mere use of a *numéraire* as an *expository* device does not excuse us from the necessity for studying the causes and consequences of changes in the relation of the *numéraire* commodity to other elements in the price structure. There can, of course, be no objection to substituting, for example, a concept such as "real income" for "income in wage units" (so Bissell, *loc. cit.*), if the purpose of the alternative usage is to take advantage of the fact that the concept of "real income" does (or should) immediately suggest the problems of change in price *structure* which constitute the essence of the "index number problem" (see above, p. 298, n. 164, and p. 301, n. 170), whereas the concept of a *numéraire*, when correctly understood, does not *formally* involve the index number problem at all (cf. Lange, "The Rate of Interest and the Optimum Propensity to Consume", *loc. cit.*, 13), and therefore could hardly be expected to suggest the problems of structure associated with the "index number problem." The chief purpose, in any event, of our Proposition XXXIV is to re-emphasize, in connection with problems on the side of *supply,* the substance of our Proposition X (p. 297) with respect to concepts, such as "real income," which have been held to be particularly relevant to the *demand* side of the problem, but which, as sometimes used, are certainly relevant also to problems on the side of *supply.* Cf. the following note.

necessary part of the *supply* sectors, as well as of the *demand* sectors, of any analytical equipment designed to account for the causes and consequences of changes in "real" magnitudes (or magnitudes expressed in terms of a "numéraire") in the world we know.[102]

XXXV. But the relation of the Theory of Money and Prices to the explanation of the structure of money prices is in no sense confined to its use in accounting for the *absolute* level of the individual prices bound together in the price "structure." On the contrary, one may say of the supply side of the problem what was said above, in Proposition XI (p. 304), with respect to the demand side: namely, that although the whole of that part of the "general" Theory of Value which is summarized by the concept of a "particular supply curve" of the general form $q = \Phi(p)$ is relevant to the problem of the nature of the forces determining the *structure* of money prices, a part of at least equal importance in the solution of the problem is provided by *monetary theory, and by monetary theory alone.*

This conclusion, it should be observed, would in any case follow from the argument of our Proposition XI itself, according to which use must be made of those parts of the Theory of Money and Prices (and of Output as a Whole) which undertake to account directly for the particular sequence in which, in the course of monetary expansion and contraction (or of expansion or contraction of Output as a Whole), the money-spending power of particular sectors of the community is successively raised or lowered. For the

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[102] In this connection, see what is said above, p. 302, and nn. 171 and 172 thereto, on the use of the concept of an "income effect" in dealing with the effect of price changes upon the "incomes" of *sellers.* Relevant also to the supply side of the problem, of course, is the concept of "real *wages,*" when they are looked at (as they should be in dealing with the effect of changes in "wages" upon the level of output as a whole) from the standpoint of their effect upon entrepreneurial *costs.* It is, indeed, a striking feature of recent discussion of the movements of "real wages" in relation to the theory of output as a whole, that there should have been increasing recognition of the necessity for examining more closely the facts with respect to, and the analytical issues involved in the choice of, the "basic variables," in their original *money* form. See, for example, R. Ruggles, "The Relative Movements of Real and Money Wage Rates," *Quarterly Journal of Economics,* LV (1940), 138, 146 ff., and the references to the recent literature there given.

changes in the position of market demand schedules which are likely to be associated with these successive changes in the distribution of money-spending power are bound to be reflected in changes in the structure of *realized money prices* during the course of monetary expansion or contraction, and in entrepreneurial expectations with respect to changes in prices; and both these realized changes in prices and expectations with respect to such changes are, in turn, bound to affect the terms upon which entrepreneurs will be willing to *supply* the objects they sell.

It must be remembered also, however, (1) that *entrepreneurial* decisions with respect to spending represent key stations in the *subsequent* generation of changes in the level and distribution of money-spending power; (2) that since these entrepreneurial decisions with respect to spending will themselves be indissolubly connected with entreprenurial decisions with respect to *supply,* the conditions of supply, as affected by changes (realized and "expected") in the structure of selling prices and costs, are themselves strictly relevant for an explanation of the subsequent generation of changes in the level and distribution of money-spending power; and therefore (3) that those parts of the Theory of Money and Prices which contribute to an explanation of why this structure of money-selling prices and money costs is what it is, provide an indispensable supplement to the "general" Theory of Value in the explanation of the determination of the structure, as well as of the level, of realized money prices generally.[103]

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[103] For examples of a recognition of the importance of studying the causes and consequences of changes in the magnitude and direction of *entrepreneurial* spending (and therefore the conditions of *supply* as affecting, and as affected by, such spending), see above, pp. 313 ff., and the references given in nn. 197–201 thereto. Since, moreover, Auspitz and Lieben are cited above, p. 316, n. 203, as having translated their argument into terms of successive shifts of demand curves, it should be pointed out that they explicitly included, in their account of the effects of monetary expansion upon the structure and level of money prices, not only shifts in *entrepreneurs' demand* in response to an improved "situation," but also the resulting shifts in the *"supply* curves of the various articles" (*Untersuchungen über die Theorie des Preises,* 65). It should hardly be necessary, finally, to stress the relevance, for the point under discussion, of Professor Schumpeter's argument with respect to the effect of the impact of "innovation" on production functions, and therefore upon the conformation and

XXXVI. By our Proposition XII (p. 319), it was established that any rejection of expressions of the general form $MV = PT$ on the ground that such expressions can be concerned only with the determination of the "general" price level, or the "value of money," was itself to be rejected in view of the possibilities inherent in the use of "partial" equations of the *general form* $MV = PT$ as a method of dealing with a "plurality of price levels." By Proposition XIII (p. 320), this argument was applied directly to the problem of obtaining expressions for individual realized *demands,* including the realized demands for *individual commodities.* An expression for a realized money *demand* for a particular commodity is, however, simultaneously an expression for the realized *supply* of that commodity, multiplied by the realized price for that commodity.[104] Moreover, the meaning of any "price level," among a "plurality" of "price levels," is itself established only by the specification of the particular *commodities* (the particular q's) whose prices are included in the "price level" in question.[105] The argument, therefore, for the use of a plurality of "stream equations," as a means for dealing with the processes by which the *"system"* of realized money demands and of realized money prices is determined, applies as directly to the forces on the side of *supply* (and therefore to the forces affecting the structure of *output,* which is a component of "supply"), as it does to the forces on the side of *demand.*[106]

XXXVII. By precisely the same type of argument, we are able to apply to the "supply" side of the problem the substance of our Proposition XIV (p. 323), with respect to the need for supplementing the use of "partial" money equations (that is, equations representing the realized

position of entrepreneurial cost curves and supply curves, and the subsequent reactions of entrepreneurs as disbursers of money funds. See what is said on this matter above, pp. 433 ff.; and cf. Schumpeter's *Business Cycles,* 88 ff., 130 ff., 524, 527 f. (on this particular passage, cf. what is said above, p. 197, n. 116), 536.

[104] See above, pp. 551 ff., and nn. 4 and 5 thereto; also the references to Schumpeter given above, pp. 118, n. 67, and p. 342, on the relation of the concept of a "sum of realized money demands" to the concept of a "product sum" of "prices *times quantities sold.*"

[105] Cf. Volume I, 65, n. 67, of the present work.

[106] See above, pp. 343 ff.

money demands and supplies for particular commodities, or for particular groups of commodities) by the use of a "total transactions equation" (that is, an equation representing the sum of realized demands and supplies and of all other money payments and of countervailing "transactions" effected within a given period of time). This is clearly the case, for example, with respect to that argument for the supplementary use of a "total transactions equation" which is connoted by the concept of a "competition" of individual sectors of the price structure for monetary "purchasing power." [107]　For it must be clear that the extent of this "competition" will depend not only upon the height of the *prices* in these individual sectors, but also upon the amount of *objects sold* at these prices.[108]　It must be remembered, moreover, that the Theory of Money and Prices has concerned itself in much greater degree than has the "general" Theory of Value with the nature of the forces determining the amount of "objects," sold within any given time period, *other than* "commodities," and in particular with the amount of "objects" sold other than commodities *"produced"* in the time period in question.[109]　This fact in itself demonstrates simultaneously (1) the need for supplementing the findings of the "general" Theory of Value by the findings of those sectors of the Theory of Money and Prices which have been concerned with the analysis of the components of the T of a "total transactions equation"; and (2) the falsity of any argument designed to show that the need for analysis in terms of changes in the Fisherine T

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[107] See above, p. 323, and the references to Volume I given in n. 13 thereto.

[108] Cf. Volume I, 209. Since what is involved is an aspect of what has been called in this work the "composite demand for cash balances" (see Volume I, 521 ff., 584, 598); and since this "composite demand" is an *"absolute"* demand for cash balances (see Volume I, 209, 444 ff.), it follows that each of the segments of this composite demand may be represented by expressions of the general form $M_1 = (P_1 T_1)/V_1$, in which the numerical subscripts would refer to a particular segment of the composite demand. That is, the absolute volume of cash demanded in any one sector of the system will depend *not only* upon "the height of the prices" and the "amount of the objects sold" in that sector, but also upon the degree of economy in the administration of cash balances effected within that sector, as registered in the magnitude of $K_1 = 1/V_1$.

[109] See above, p. 555, n. 8, and the references there given.

(and therefore in terms of changes in the *components* of the Fisherine *T*) is removed by a statement of the analysis in terms of an "elasticity of supply," or, even worse, an "elasticity of production," of *commodities*.[110]   And since the list of "economic variables" involved in "any theoretical apparatus which would do justice to the phenomenon of the general interdependence of economic variables" (Proposition XIV, p. 323) includes the variables on the side of *supply*, it follows that a concern with the side of supply reënforces the further argument of our Proposition XIV: namely, that there is a clear relation between the case for the use of a "total transactions equation," on the one hand, and the case, on the other hand, for a theoretical apparatus concerned, as constructions of the Walrasian type within the "general" Theory of Value are concerned, with the phenomenon of "general economic interdependence." [111] For again it must be insisted that the theoretical apparatus here outlined not only *includes* the simpler type of "Walrasian" construction, but also involves a much broader range of phenomena than have *those formulations of* the Walrasian "system" which have not explicitly undertaken to provide "a summary of the whole system of money flows and 'commodity' flows which, in a fully developed economy, must constitute the very subject matter of a 'general' theory of pricing." [112]

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[110] Contrast the argument of B. P. Whale to which reference is made above, p. 556, n. 9; and cf. also what is said on this matter above, p. 554, n. 7.

[111] See above, p. 323, and the reference to Volume I given in n. 14 thereto; also what is said above, pp. 526 f., with respect to the relation of the general Walrasian conception to the study of the effect, upon particular cost and supply curves, of changes in the level of output at which other firms and industries are operating.

[112] See above, pp. 324 ff. and 364 ff.   I have already commented upon the relation of this conclusion to the interpretation of the Walrasian system as being based upon "barter assumptions" (see above, p. 328, and the references given in n. 27 thereto); and I have commented also upon the suggestion that "Walras tells us nothing concerning the monetary circulation in its relation to enterprise" (see above, p. 363, and n. 34 thereto). It is not out of place here, however, to point out that an adequate appreciation of the facts with respect to the treatment accorded by Walras to the rôle of money in the functioning of the economic process provides its own commentary upon Mr. Keynes's implication that a concern with "the theory of stationary equilibrium" (of the "system" as a whole) necessarily implies a lack of recognition of the proposition that one set of considerations

XXXVIII. One must say, of "Marshallian" particular *supply* schedules, what was established in our Propositions XVI (p. 347) and XVII (p. 349) with respect to particular *demand* schedules: namely, (1) that such schedules, and the body of analysis which they are designed to summarize, are intended to deal only with *discrete situations,* since there is nothing in such schedules in themselves which tells us how we *pass from one discrete situation to another;* and (2) that the bridging of this gap in our understanding of the pricing process as it unfolds itself in time is provided by the fact that the prices realized in discrete situations are part and parcel of the process of receiving and *expending money in time.*[113] From Proposition XVIII (p. 350), on the other hand, it should be clear that a major element in the establishment of this "bridge" is to be sought on the *supply* side of discrete pricing situations, which are the only type of situation to which "Marshallian" demand and supply schedules are directly applicable. For the purpose of Proposition XVIII was to remind the reader of the ancient

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leading to an emphasis upon "the importance of money" flows from a recognition that money serves as "a link between the present and the future" (*General Theory,* 293). For, from the context, with its reference to the element of "expectations," what Mr. Keynes seems to have in mind is the range of considerations associated with the concept of "liquidity preference," which in turn is included within the range of phenomena with which the "cash-balance approach" was intended to deal; and the "cash-balance approach" is precisely one of the elements which Walras himself included within his general "system." In this connection, see the comments of Rist, *Histoire des Doctrines relatives au Crédit et à la Monnaie,* 335 f., on the relation of Walras's "encaisse désirée" to the conception of the rôle of money as that of serving as a "bridge between present and future." Since, moreover, Mr. Keynes makes much of the "line of division between the theory of stationary equilibrium and the theory of shifting equilibrium—meaning by the latter the theory of a system in which changing views about the future are capable of influencing the present situation," and which, therefore, is capable of application to "the problems of the real world, in which our previous expectations are liable to disappointment and expectations concerning the future affect what we do to-day"—it may not be out of place to call attention to what was said above, p. 356, n. 18, with respect to the relation of the Walrasian conception of a "circular flow" to the construction of Francesco Ferrara, with its emphasis both upon the rôle of *money* in the "circular flow" and upon the "re-tying of the present to the future" through the element of "expectations," as well as its discussion of the consequences of the fact that these expectations may be "disappointed" (see above, p. 362, n. 31).

[113] See above, pp. 349 f.

propositions of "general" economic theory with respect to the relation between the *prices received* from the *sale* of commodities or services, on the one hand, and the *"incomes"* of the sellers of these commodities and services, on the other.[114] And since, by our Propositions XXIV (p. 550) and XXVII (p. 552), the explanation of "prices received" ("realized") by sellers involves the introduction of particular supply schedules of the general form $q = \Phi(p)$, this amounts to saying that a key station in the solution of the problem of the relation between the concepts directly applicable only to discrete pricing situations, on the one hand, and the "flow" analysis which is necessarily associated with the concept of money *income,* on the other, must be the establishment of a relation between these "discrete" *supply* schedules and the *generation of money income in time.*[115]

XXXIX. From Proposition XXVIII (p. 553), however, we know that not all of the "prices" involved in market supply *schedules* are necessarily the *realized* supply prices which alone are directly related to the realization of money receipts, and therefore to the realization of money "incomes." And from Proposition XXIX (p. 553), we know that this statement applies *a fortiori* to prices, such as "cost" prices, which affect realized prices, on the side of supply, only insofar as they help to make the supply prices included in market supply *schedules* what they are, and only insofar

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[114] See above, pp. 350 ff.

[115] Since these market "supply schedules" may be regarded as equally applicable in the explanation of the market supply actions of *"dealers,"* it follows that their use does not demand acceptance of the assumption that the total money receipts involved in a given market transaction based upon a particular supply schedule are all directly resolvable into payments which represent "income" to the particular "suppliers" involved. It must be remembered, however, that one of the central elements in an adequate theory of "the generation of money income in time" is the distinction between those money payments which do "enter into income" and those which do not. The point here is merely that an adequate theory of the generation of money income in time necessarily involves an adequate theory of the realization of money *prices* in time; and the further points are (1) that an adequate theory of the realization of money prices involves the full use of the relevant sectors of monetary theory, as well as of the "general" Theory of Value, and (2) that the *relation* of the theory of realized money prices *to* the theory of the generation and utilization of money income likewise requires the full use of those sectors of monetary theory which are concerned with precisely this problem.

as these supply prices are then actually *realized*.   It follows that we must reject any apparatus for dealing with the relation between discrete pricing situations and the generation of money income which rests upon an arbitrary identification of *"costs"* with *incomes,* and would argue that all reductions in "costs" must necessarily result in a reduction of "incomes."   The argument obviously applies still more clearly to any apparatus which rests upon assumptions such as (1) that all *incomes* are "costs"; or (2) that "wages" are the *only* type of "cost" that need be considered in discussing the nature of the forces affecting the generation of "incomes."

The reader will be aware that the propositions indicated are among those associated with what has recently been designated as "Keynes' Law." [116]   He will be aware also, however, that recognition of the existence of a *relation* between "costs" and "incomes" (and therefore between wages as "costs" and wages as an element in *demand*), instead of deserving to be regarded as a discovery of Mr. Keynes, goes back very far in economic literature.[117]   We have seen, for example, that, within the "general" Theory of Value, recognition of the existence of such a relation was implicit in the "classical" propositions (1) that the "natural" price of a given commodity sold is resolvable into *income*-"shares," which are then available for subsequent expenditure; and (2) that the "natural" price of a given commodity will be equal to its "cost" price, and is therefore resolvable into elements of "cost." [118]   Within *monetary*

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[116] See above, p. 443, n. 88, p. 444, n. 91, p. 445, n. 93, and the references to A. P. Lerner there given.

[117] Contrast Lerner, "The Relation of Wage Policies and Price Policies," *loc. cit.,* 159 f., where the proposition that "total demand is not independent of total cost" (one of the formulations given by Mr. Lerner to what he designates as "Keynes' Law") is said to have been "pointed out" by Mr. Keynes. On the *other* formulations of "Keynes' Law" presented by Mr. Lerner in the same paper, see what is said below, p. 618, n. 133.

[118] On the first proposition, in particular, see what is said above, p. 350 f., and in nn. 9 and 10 thereto. With respect to the second proposition, it is to be observed particularly that, according to "classical" doctrine, it was only the *"natural"* price of a commodity which is completely resolvable into elements of "cost" (see below, p. 609, n. 123). It should be observed also that, if nothing more is involved in "Keynes' Law" than the proposition that "total demand is *not independent of* total cost" (cf. the preceding note), then one would have to list, as earlier sponsors of the substance of this "Law," the earlier sponsors of one of the versions of "Say's Law" presented by Mr. Keynes: namely, that "the whole of the *costs of production* must necessarily be *spent* in the aggregate, directly or indirectly, in *purchasing the product*" (*General Theory*, 18; italics mine). And the same thing would have to be said of the alternative version of "Say's Law"

theory, on the other hand, such a recognition must be regarded as having been implicit in the case of those early writers who recognized both (1) the relation of the realization of money *prices* to the realization of money *incomes;* and (2) the rôle played by "costs" in the determination of realized prices.[119] Moreover, as we have seen, it is by no means

(though it is not presented as such by Mr. Keynes) which Mr. Keynes himself characterizes as a "proposition which is indubitable": namely, "that the income derived in the aggregate by all the elements in the community concerned in a productive activity necessarily has a value exactly equal to the *value* of the output" (*General Theory,* 20; italics in the original). Actually, of course, whether the latter proposition is to be regarded as "indubitable" depends entirely upon (1) how it is interpreted, particularly with respect to the *timing* of the *realization* of "income" and the *realization* of the "value" of the "output" (cf., for example, Volume I, 132, and the reference given in n. 72 thereto); and also upon (2) the particular context in which the proposition is applied. On the latter point, see above, p. 95, n. 15, where it is pointed out that "Say's Law" has had not one version, but several versions, and that the contexts in which these several versions were applied were often different, even in the writings of the "classicals" themselves. The fact that these different versions are of differing degrees of validity in themselves, and the further fact that a proposition which is both formally and substantively valid when applied in one context may be substantively invalid when applied in another context, ought, one would have supposed, to have argued against the setting up of formal antitheses between "Say's Law," on the one hand, and a "Keynes' Law" which is itself given several formulations within the limits of a single article (see again below, p. 618, n. 133). Given, indeed, the heterogeneity characterizing the numerous versions of "Say's Law" in the past, and the heterogeneity of the contexts in which these different versions were applied, it would be just as easy to argue that *"the"* substance of "Say's Law" is precisely "the" substance of certain of the fundamental propositions of Keynes's avowedly heterodox *General Theory.* Something regarded as "Say's Law," for example, has been used to establish propositions such as that (1) movements in the productive process tend to become cumulative, because employment and the income "produced" by such employment breeds further employment and income (the "multiplier effect"); and that (2) *"investment"* itself generates "income," and therefore provides "purchasing power" which may be used in the purchase of the products yielded by the process of "investment" (cf. the Keynesian "investment multiplier"; and see above, p. 355, n. 17). The point made here is simply that the mere recognition of a *relation between* "total demand" and "total cost" (in Mr. Lerner's words, an absence of "independence" between "total demand" and "total cost") is very ancient indeed; and that what really matters is whether the *particular* relation alleged by a particular writer in a particular context to exist between "costs," "incomes," and "demand" can be said to exist under all circumstances.

[119] On the first point, in particular, see the references to Senior's *Three Lectures on the Value of Money* and to Simon Newcomb's *Principles* given above, p. 351, n. 9, as well as the account given above, pp. 352 ff., of the development of the concept of a "circular flow" up to and including the work of Walras. In view, moreover, of the fact that what is here involved

necessary to rely merely upon these cases of *implicit* recognition of such a relation. On the contrary, the instances of Wicksell and Hawtrey—to go no further—show that the existence of a relation between "costs" and "incomes" was not only recognized, but was insisted upon with all possible articulateness in the years preceding the publication of Keynes's *Treatise*.[120] It could fairly be said, therefore, that by the time the *Treatise* was published, what was needed primarily was not a reaffirmation of the principle that there is *some relation between* "costs," "incomes," and "demand," but a critical examination of earlier formulations with respect to the *nature* of this relation, and particularly of those formulations that were so phrased as to encourage a belief in the universality of a *type* of relation between "costs," "incomes," and "demand" which would in fact be found to exist only under a highly special set of circumstances.[121]

We know that it was precisely such a critical examination which was *not* to be found in Keynes's *Treatise*. We know, on the contrary, that the Fundamental Equations of the *Treatise* themselves rested upon a

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is the construction of an adequate apparatus for dealing with the relation between (1) "costs"; (2) realized prices; (3) incomes; (4) "demand"; and (5) the *flow of money payments,* attention should be called to the treatment accorded to all these elements by Cairnes, in his "Essays Towards a Solution of the Gold Question" (*Essays in Political Economy,* 6 ff., 18, 24, 28, 58 ff., 148 ff.). Cf. also the discussion in Marshall's *Economics of Industry,* 152 ff., of the relation between prices, "wages," and "the demand for all kinds of commodities" over the course of the "established cycle," as described in "the famous words of Lord Overstone"; and see also the comment of Wicksell (*Lectures,* II, 157), on the relation between a rise in wage-*incomes,* induced by an "increased (money) demand for labor," and the "prices of the goods *already on the market*" (italics in the original).

[120] See Volume I, 130 ff., of the present work, and the references given in nn. 66–71 thereto. There, of course, I was referring, as I am referring here, to discussions of the relation between costs and "incomes" which can be said to have been both directly relevant to the issues with which "Keynes' Law" is alleged to be concerned, and generally available to students of monetary problems. It would be possible, of course, to provide a much longer list of instances, from writings of the years preceding the publication of Keynes's *Treatise,* of discussion, in various contexts, of the relation between "costs," "incomes," and "profits and losses." See, for example, E. Lindahl, *Penningpolitikens Medel* (1930), 24, n. 1, and the references there given to earlier writings of Myrdal and of Lindahl himself.

[121] In this connection, see, for example, the comments on Wicksell in Volume I, 130 f., and in n. 67 to p. 131; and on Hawtrey, *ibid.,* 131 f., and in nn. 68–70. It is only fair to Mr. Hawtrey, however, to point out again that any judgment as to the adequacy of his treatment of the relations between (1) realized prices; (2) "costs," including wage costs; (3) incomes; (4) "demand"; and (5) the flow of money payments, would have to go beyond the simple aphorisms discussed in Volume I to the detailed treatment one finds throughout his writings. See, for example, Hawtrey's *Monetary Reconstruction,* 43, and the passages from Hawtrey's writings cited by Saulnier, *Contemporary Monetary Theory,* 63 ff.

series of assumptions with respect to the relation between "costs" and "incomes" which would conform to reality only under highly "stationary" conditions.[122] Yet it must be said, on behalf of the *Treatise*, that, apart from the Fundamental Equations themselves, its argument was not associated with further dangerous propositions which in turn have since come to be associated with some versions of "Keynes' Law." The *Treatise* did not explicitly argue, for example, that realized prices (the only prices that are directly resolvable into realized money receipts, including receipts representing "income" to the recipient) are necessarily equal to the *cost* price of the commodities whose realized selling price is resolvable into realized money "incomes." On the contrary, the *Treatise's* emphasis upon *profits* (positive or negative) as a component of realized selling prices at least suggested the possibility of a recognition that, under a definition of the relation between "profits" and "incomes" different from that employed in the *Treatise*, not even a reduction of *realized* "costs" need necessarily mean a reduction in *all* "incomes." [123] Nor (despite possible, though superficial, appearances

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[122] See Volume I, 132 ff., of the present work; and cf. also above, pp. 439 ff.

[123] It will be remembered that, according to "classical" doctrine, it was only the *"natural* price" (the competitive long-run equilibrium price) which was entirely resolvable into elements of "cost" (see above, p. 606, n. 118). Cf. the proposition of Keynes's *Treatise* that "prices" (Π) would equal "costs" (W_1) only in "equilibrium"—that is, when *profits* ($Q = I\text{-}S$) are equal to zero; and contrast the version of "Keynes' Law" represented by the proposition that "a general reduction of wages would constitute a reduction in costs, *in incomes,* and *in demand*" (Lerner, "The Relation of Wage Policies and Price Policies," *loc. cit.,* 159). The reason, of course, why it would be necessary to provide a definition of the relation of "income" to "profits" different from that provided in the *Treatise* is that "Income" was there defined in such a way as to exclude "profits" greater than "normal." On the reason for this practice, in relation to the Fundamental Equations of the *Treatise*, see Volume I, 126, of the present work, and n. 61 thereto. In the *General Theory*, of course, Mr. Keynes formally abandoned the practice in question (see, for example, pp. 60 f. of that work)—quite naturally, since he no longer wished to retain the Fundamental Equations, which rested upon a definition of "Incomes" as equivalent to "costs," and therefore excluded anything more than "normal" profits. The analytical gain thus represented, however, was almost entirely lost, as we have seen, by Mr. Keynes's relegation of "profits" to a position far below that of the "mainspring of change in the existing economic system" which it had occupied in the *Treatise* (see above, pp. 562 ff.). It must be observed, moreover, that an insistence upon *taking account of* movements in "profits" in tracing the effect upon "incomes" of changes in *costs,* is not equivalent to an insistence upon assuming that reductions in *costs* will keep total incomes unchanged, and that what the wage earners will lose, profit receivers will gain. The point is merely that it is impossible even to *raise* the question of what happens to *total* incomes (which *include* "profit"-incomes) as a result of movements in costs, as long as "incomes" are identified outright with *wage* incomes. It will be observed, finally, that the version of "Keynes's Law" cited above is not formally

to the contrary) did the *Treatise* argue explicitly that realized prices are "governed" by costs, in such wise that one could argue that a reduction in costs would necessarily lead to a reduction in realized prices, and therefore in the prices which alone are directly resolvable into money receipts, including receipts representing "income" to the recipient.[124] In short, the argument of the *Treatise* with respect to the

correct even apart from the fact that a reduction in realized *costs* need not necessarily involve a reduction of total realized *incomes;* for movements in items of realized "cost," other than movements in *wage* costs, might counteract the movements in the latter, and leave the total cost bill unchanged. Mr. Lerner's usage in this instance, that is to say, provides a further example of the unfortunate influence of those aspects of the *General Theory's* exposition which reflect Mr. Keynes's "sympathy" with those variants of "the labor theory of value" according to which *labor* is "the sole factor of production" (see above, p. 534, n. 28, and p. 585, n. 76). It is true that the exposition of the *Treatise* was also sometimes such as to imply that wages *were* the only element in cost (cf. Volume I, 111, of the present work, and the references given in n. 26 thereto, and also p. 270 of the same volume); but it is likewise true that Mr. Keynes specifically included other costs than wage costs in the E of his Fundamental Equations (cf. Volume I, 112, and the reference given in n. 30 thereto).

[124] Contrast the quotations from A. P. Lerner given above, p. 590, n. 87. The possibility of interpreting the *Treatise* as having argued, in the manner of the *General Theory*, that the level of costs "governs" the level of prices can be based solely upon a use of the Fundamental Equations in their *cost* aspect (that is, when they are written in the form $\Pi = W_1 + [I\text{-}S]$), the argument then being that prices (Π) are held to be "governed" by changes in the level of the rate of "efficiency-earnings" (W_1). It will be observed, however, that the very presence, in this formulation, of the terms $I\text{-}S$ makes it impossible to argue that every reduction in "costs" will necessarily "result in" a fall in selling prices. And indeed it is a striking commentary on the difference between the *Treatise* and the *General Theory* in this respect that the disciples of the former were likely to insist as emphatically that "there is no guarantee in falling costs that prices, even if left to themselves, would follow suit," since "the price mechanism is not so plastic as that" (so B. P. Adarkar, *The Theory of Monetary Policy* [1935], 56), as the disciples of the *General Theory* are to insist that when "costs" fall, prices will ·fall in proportion (cf. the references to Lerner given above, and the comment on this aspect of Lerner's argument by Haberler, *Prosperity and Depression*, 244). It must be remembered, in the second place, that, by the terms of the *Treatise's* argument, the "effects" upon the "price level" which could be imputed to "spontaneous" changes in the cost-item W_1 were deduced only upon the assumption that "the banking system" would not inaugurate price changes by changing the magnitude of $I\text{-}S$. See, for example, the *Treatise*, I, 153, 167. And it must be remembered, finally, that the *Treatise* explicitly identified $W_1 = E/O$ with the expression $\dfrac{M_1 V_1}{O}$; that is, with a "stream" formulation, according to which "prices" are supposed to result from a "mutual impact of relevant flows" (cf. the references to Keynes's *Treatise* given in Volume I, 134, n. 79, of the present work). It should hardly be necessary

relation between "costs" and "incomes," while it certainly suffered from very serious faults, was not such as to make it unreasonable to hope that a revised formulation of it might contribute to a satisfactory solution of the problem to which, for all its admitted shortcomings, the *Treatise* must be regarded as having directed attention: namely, that of establishing the precise *nature* of the admitted relation between "costs," on the one hand, and "incomes," on the other.

Unhappily, however, as we know, the argument of the *General Theory* can certainly not be regarded as having brought such a solution nearer. It did, to be sure, formally abandon the Fundamental Equations of the *Treatise*, with their arbitrary implications with respect to the conditions under which alone an outright identification of "costs" with "incomes" is permissible.[125] Moreover, the *General Theory* did not itself lay down the type of formal, categorical proposition with respect to the relation between "costs" and "incomes" which has since been presented as one version of what has been designated as "Keynes' Law."[126] Against these advantages of the argument of the *General*

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to emphasize again that this identification is permissible only under a highly special set of assumptions (see, for example, what is said on this matter in Volume I, 127 ff., and 379, n. 76, of the present work). It should be pointed out, however, that the conception of the "governing" of prices by the level of "earnings," when the "earnings" involved are conceived of as *streams of money payments directed against the goods whose prices are thus said to be "determined"* is a conception different in vital respects from a conception of the "governing" of prices by "costs" which rests upon an inadequate appreciation of the relation of "costs" to *supply* prices, on the one hand, and to *realized* prices and money receipts, on the other.

[125] It must again be pointed out, however, that the difficulty indicated is *not* among those mentioned by Mr. Keynes as having led to his abandonment of the Fundamental Equations of the *Treatise*. See Volume I, 138 ff., of the present work. This fact, in combination with the further fact that the argument of the *General Theory* has itself been taken by some of its supporters as the basis for the type of identification of "costs" with "incomes" which is involved in some versions of "Keynes' Law," itself provides a further commentary upon the suggestion that the *General Theory's* formal "abandonment" of certain positions adopted in the *Treatise* makes "otiose" any detailed consideration of the argument of the latter. Cf. above, p. 157, n. 31, and p. 464, n. 4.

[126] The only version of "Keynes' Law," indeed, for which I have been able to find unequivocal justification in the exposition of the *General Theory* is the least exceptionable form of that "Law": namely, that "total demand is not independent of total cost" (cf. above, p. 606, n. 117). See, for example, the argument on pp. 258 ff. of the *General Theory* with respect to the lack of justification for assuming, in all cases, that "the reduction in money-wages will leave demand unaffected." No one could disagree with Mr. Keynes's contention that "whilst no one would wish to deny the proposition that a reduction in money-wages *accompanied by the same aggregate effective demand as before* will be [likely to be?] associated with an increase in employment, the precise question at issue is whether the reduction in money-wages will or will not be accompanied by the same

Theory over that of the *Treatise,* however, further disadvantages must be set. The *General Theory's* "Theory of Prices," for example, with its

aggregate effective demand as before measured in money, or, at any rate, by an aggregate effective demand which is not reduced in full proportion to the reduction in money-wages" (*General Theory,* 259 f.; italics in the original). Nor would anyone disagree that it was necessary to utter the kind of warning contained in Mr. Keynes's rejection of the type of analysis indicated. What one must disagree with, in the first place, is the suggestion that a generalization of this type of warning into a "Law" which tells us merely that "total demand is not independent of total cost" is either a proposition which runs counter to anything that deserves to be characterized as "classical" doctrine, or is a proposition which itself represents a positive solution of the problem of the relation of movements in "wages," viewed as elements in "cost," to "incomes" and "demand." What one must disagree with, in the second place, is the further suggestion that such a positive solution has been provided either by the apparatus of Keynes's *Treatise* or by the versions of "Keynes' Law," avowedly based on the argument of the *General Theory,* which go *beyond* the negative proposition that "total demand is not independent of total cost" to positive (but by no means self-evident) propositions such as that "a general reduction of wages would constitute a reduction in costs, *incomes, and in demand.*" And what one must disagree with most profoundly is the *General Theory's* insistence that "if classical theory is not allowed to extend by analogy its conclusions in respect of a particular industry to industry as a whole, it is wholly unable to answer the question what effect on employment a reduction in money-wages will have" (*General Theory,* 260). On the contrary, the argument of the present work is, and the argument of my later *Money and Production* will be, that the way in which "classical theory" would "answer the question" indicated, is by tracing the effects of a "reduction in money-wages" (in the sense of a reduction in money wage-*rates*) on (1) the *structure* of money *prices,* "expected" and realized, by way of its effect upon *ex ante* market supply curves; and on (2) the *flow of money payments* ("realized money demands") as affected by the decisions of individuals and individual firms with respect to borrowing, "investing," and "spending," in the face of a given price structure and a given set of institutional and conjunctural conditions, as well as in the face of a given type of action pursued by governmental and quasi-governmental agencies. It should hardly be necessary to remind careful readers of the *General Theory* that there are aspects of the latter's argument which are perfectly capable of translation into terms of the propositions just advanced. See, in particular, the inclusion of possible "repercussions" of "a reduction in money-wages . . . *on the schedule of marginal efficiencies of capital*" in the *General Theory,* 262 ff., with all that this can be made to mean with respect to movements in the quantity of bank money and its rate of utilization (the two elements being related to changes in "investment"); and contrast the *General Theory,* 278, where, having taken (p. 260) "Professor Pigou's *Theory of Unemployment*" as an example of "all that can be got out of" "the Classical Theory," so far as the problem under discussion is concerned, Mr. Keynes goes on to suggest that the "Classical Theory" (identified with the "theory" of Professor Pigou) has failed to "include" in its "formal scheme" "the relation between the schedule of marginal efficiency of capital and the rate of interest." Just how much

altogether inadequate treatment of the relation between "cost" prices and *"supply* prices," and its equally inadequate treatment of the relation between *ex ante* "supply" prices, on the one hand, and *realized* prices and realized money receipts, on the other, made it incapable of dealing with the effects of such reductions in *ex ante* "cost-" or "supply-prices" as might be shown to increase the magnitude of the *sum* of *realized* selling prices—the only prices, again, which are directly resolvable into money receipts, including those receipts which represent income to the recipients.[127] This amounts, of course, to saying again that,

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of a caricature the latter statement is of anything deserving to be called "the Classical Theory" can be judged by those who (1) will not be misled by the statements made with respect to the implications of the concept of a "natural rate of interest" on pp. 183 and 243 f. of the *General Theory* into a failure to recognize the identity of the rôles assigned to Mr. Keynes's "marginal efficiency of capital" and the "natural rate" in the best interpretations of the latter concept; and (2) will inform themselves as to the rôle actually played by the concept of a "natural rate" (or its equivalent), as so interpreted, in the development of "Classical Theory." (See, for example, Volume I, 191 ff., of the present work, including the references to Marshall and Pigou there given. The reader must provide his own commentary on an attitude of mind which starts from propositions such as that in the last ten years "economic theory has moved forward so rapidly—much more rapidly than in any previous period of comparable length—that even the revisions of five or six years ago are very clearly dated," and then proceeds to suggest that *earlier* discussion with respect to the relation between the "rate of interest," "investment," "income," and "employment," "has nothing to add to the simplified modern [read: Keynesian]" propositions on this head; instead of considering whether these "simplified modern propositions" have themselves anything to "add" to the earlier discussion. Cf. Lerner, "Some Swedish Stepping Stones in Economic Theory," *loc. cit.,* 574, 584 f.) It should hardly be necessary to emphasize that nothing but the most ludicrous misrepresentation is involved, likewise, in Mr. Keynes's further suggestion (*General Theory,* 279) that "Classical Theory" (for which, again, Professor Pigou's account is taken as the standard) has "omitted," from its "analysis," "the unstable factor, namely fluctuations in the scale of investment, which is most often at the bottom of the phenomenon of fluctuations in employment" (*General Theory,* 279).

[127] Again it must be said that Mr. Keynes's own treatment of the relation of "wages" to "prices" is not as objectionable as that of some of his followers (cf., for example, the quotations given above, p. 590, n. 87). Yet this does not mean that Mr. Keynes's treatment is not itself exposed to the general criticism offered in the text above. In addition to the statements with respect to the relation between "wages" and "prices" quoted from the *General Theory* above, p. 589, n. 87, see the loose conclusions presented on p. 262 of the *General Theory* with respect to the effect of "a reduction of money-wages" on "prices," in effecting an alleged "redistribution of real income," and on the "propensity to consume." See, likewise, the equally loose conclusion presented on p. 263 of the same work with respect to the relation between a "reduction in the wages-bill," on the one hand, and a "reduction in prices and in money-incomes," on the other.

despite the claims to the contrary advanced by Mr. Keynes and by defenders of certain forms of "Keynes' Law," the Theory of Prices advanced in the *General Theory* underestimates the importance of *Demand* in the determination of realized *prices*.[128] And since realized money *incomes*, in the sense of a stream of "income" receipts in the form of money available for subsequent expenditure, are generated primarily (apart from the "income"-generating effects of a dole) as part of the process of the realization of money *prices*, the effect of this part of the *General Theory's* argument was to leave almost completely untouched what must be regarded from many points of view as the very heart of the problem.[129]

Nor is this all. Any adequate account of the rôle played by Demand in the realization of money prices would have to do justice to the range of considerations associated, in *monetary* theory, with the concept of a "moneyed demand." No one familiar with the argument of the *General Theory* could assert that it makes no attempt to deal with the forces determining, and the consequences of changes in, the level of aggregate ("general") money demand. But neither could anyone familiar with the treatment accorded to the element of "general" money demand by Mr. Hawtrey, in particular, have been satisfied with the treatment accorded in the *Treatise* to the problem of the relation between *income*, on the one hand, and, on the other, *outlay* from that income (realized

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(It is not clear from Mr. Keynes's exposition, for example, whether the reduction in the latter two magnitudes is a condition *assumed* for the *purpose of argument*, or is alleged to follow necessarily from a "reduction in the wages-bill," which, it should be observed, is in turn assumed to result from "a reduction of money-*wages*" in the sense of a reduction in the wage-*rate*. The latter type of assumption was, of course, one of the aspects of the apparatus presented in the *Treatise* for dealing with the relations between "costs" and "incomes," which was most open to objection. See Volume I, 127 f., 271 f., of the present work.)

[128] On this matter, cf. what is said above, p. 568, and especially in n. 41 thereto.

[129] In terms of the notation utilized in the present work, this proposition can be stated in the form of a charge that the apparatus of the *General Theory* fails to do justice to the possibilities opened by the use of expressions such as $(PT)_I$, with all the further development of which they are capable with respect to the treatment of the *timing* of expenditure out of income receipts (M_iV_i) and the *direction* of such expenditure. It should be observed, incidentally, that the amount of payments into income $(MV)_I = (PT)_I$ will be as large as it is not only because of the magnitude of the P's, but also because of the magnitude of the T's. This fact provides a further commentary on the suggestion that a concern with the effect of changes in moneyed demand upon the further generation of money incomes by way of their effect on realized *"prices"* implies a lack of interest in the effect of changes in moneyed demand upon the *quantities sold* at these prices. See above, p. 569, n. 41, and the comment on Cournot and Newcomb above, p. 352, n. 10.

"demand").[130] The relevant aspects of the argument of the *General*

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[130] Cf. above, p. 445, n. 94, and the references to Volume I there given. On the distinction between Income and Outlay from Income, see Volume I, 354 ff., 364, 379 ff., 404, 431, n. 50. It should hardly be necessary to labor the point that the expression "outlay from income" is only a shorthand expression for "outlay from the cash balances held by income recipients which are replenished by the receipt of income." This was implicit in the whole of the argument of Volume I on the point in question. In addition, for example, to what is said on p. 404 of that volume, it should be observed that the fact that outlay is always made in the first instance from *cash balances* is indicated by our representation of changes in the level of "outlay from income" as changes in $M_i V_i$ (see Volume I, 369, and 382, n. 85). Nor should it be necessary to labor the point that the importance of distinguishing, and establishing the relation between, *income* and *outlay from income* ("demand"), respectively, is merely hidden, instead of being diminished, by a conceptual usage which would dispose of the whole problem by having regard only to the way in which a given *outlay* would result in "income" to *some one else,* and would solve even this part of the problem of the generation of money income by easy reference to platitudes such as that "income cannot be received by anybody unless someone else is paying it out," and that "total payments and total receipts are merely different names for the same transactions, distinguishing merely whether they are viewed from the paying or the receiving end" (so, for example, Lerner, "Some Swedish Stepping Stones in Economic Theory," *loc. cit.,* 575). It is not necessary to stress here the absurdity of the suggestion that only a "slight amplification" of these platitudes is represented, in the case of their application to the problem of the generation of money income, by recognition of the fact that *outlay* is not necessarily outlay *into income*. (Cf. Lerner, *op. cit.,* 575, n. 1. On the method for dealing with this element in the problem, see Volume I, 382 ff., of the present work. As for Mr. Lerner's suggestion [p. 575 n.] that the problem is disposed of by saying that "ultimately" all "proceeds from the sale of goods and services . . . must finish up as *somebody's* income," it is sufficient to point out [1] that this will not be the case whenever the proceeds are paid to some agency which either "destroys" the "proceeds" in question— as when a commercial bank does not reissue funds used to repay to it loans previously made—or keeps them as "idle" cash balances; [2] that since the statement that a certain amount of "income" is generated by a given act of spending has meaning only if one specifies the *time-period* over which the "income" was generated, the *degree of roundaboutness* involved in the generation of income is vital in the determination of the magnitude of the income-generating effect of a given act of outlay; and [3] that it is precisely the question of the degree of roundaboutness, along with a host of other questions, which is cavalierly disposed of by the suggestion that all outlay "must finish up as *somebody's* income" "ultimately.") The major point to be made here is rather that the platitudes cited above tell us nothing with respect to the nature of the forces determining the relation between a given amount of income received, on the one hand, and the outlay (realized "demand") *out of that income,* on the other. See also nn. 131 and 132, immediately following, and the forward references there given.

Theory can hardly be said to have cleared up these relations satisfactorily.[131]   Mr. Keynes must therefore be held partly responsible for those versions of a so-called "Keynes' Law" in which variations in "costs" are regarded as necessarily resulting not only in corresponding changes in "incomes," but also in "demand." [132]

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[131] On this matter, see what is said below, pp. 694 ff., in connection with the concept of an "elasticity of effective demand," as used in the *General Theory*.   In *Economica* for February, 1940 (p. 91), Mr. B. P. Whale undertakes to dismiss the argument for distinguishing sharply between "payments into income" and "payments out of income" ("realized money *demand*," in one of its senses) by saying that "[1] apart from that tiresome question of definition which has caused so much dispute, [2] this difficulty does not arise for long-period theory."   With regard to what I have designated as proposition [1], I need only say that the issues raised by the "tiresome question" thus indicated cannot be avoided simply by *assuming* that no importance attaches to the distinction between payments into and payments out of income; on the contrary, such a procedure amounts to the familiar device of "solving" difficulties by pretending that they do not exist.   With respect to Mr. Whale's second point—namely, that the "difficulty does not arise for long-period theory"—I might point out that this is a strange comment indeed from one who "questions" the usefulness of the apparatus outlined in the present work, when "certain dynamic problems," involving the study of "processes of change," are under discussion (Whale, *op. cit.*, 91).

[132] Cf. again the version of "Keynes' Law" presented by Mr. Lerner in the form of the proposition: "A general reduction of wages would constitute a reduction in costs, in incomes, and in *demand*" ("The Relation of Wage Policies and Price Policies," *loc. cit.*, 159).   Mr. Lerner follows this statement with another to the effect that "a general scheme" for dealing with the relation between "costs, incomes, and demand" would have to be "built on" certain "fundamental, independent determinants," of which *"liquidity preferences"* would be one; yet it is characteristic that he indicates no awareness that the existence of "liquidity preferences" (*alias* the factors determining the administration of cash balances) represents one of the principal reasons for preferring to use an analytical apparatus which would refuse to regard *income* as necessarily identical in magnitude with *outlay* from income ("demand," in one of its manifestations), or to *define* "income" in such a way as to make it equivalent to "demand" by definition (cf. Volume I, 379 f., and the references there given; also below, p. 698). The clarification of the relation of the distinction between income and outlay from income, on the one hand, to the administration of cash balances (or, if one prefers, to the element of "liquidity preference"), on the other hand, is due, in our own day, chiefly to Mr. Hawtrey (see Volume I, 354, n. 21).   It was also *implicit*, however, in the argument of those defenders of the more satisfactory versions of the Law of Markets who nevertheless refused to use the "Law" as a club with which to belabor writers who insisted upon the possibility of the occurrence, and the serious consequences, of a contraction of "moneyed demand."   Cf., for example, the proposition of Marshall: "Though men [may] have the power to purchase [read: income available for expenditure] they may not choose to use it [read: they may not choose to use it to exert an effective money *demand*]" (*Economics of Industry,* 154; *Principles,* 710; cf. above, p. 349,

In sum, the issues raised by Mr. Keynes's rediscovery of the fact that there is *some* kind of relation between "costs, incomes and demand" are

n. 6). Mr. Keynes, confronted by this Marshallian proposition, is nevertheless determined to support at all costs his contentions (1) that the proposition that "the whole of a man's income is expended in the purchase of services and of commodities" "still underlies the whole classical theory, which would collapse without it"; (2) that "the conviction . . . that the theory of production and employment can be worked out . . . as being based on 'real' exchanges with money introduced perfunctorily in a later chapter is the modern version of the classical tradition"; and (3) that "contemporary thought is still deeply steeped in the notion that if people do not spend their money in one way they will spend it in another" (*General Theory*, 19 f.). His defense of this procedure, in the face of the quotation from Marshall given above, is based on the judgment of Mr. J. A. Hobson that Marshall had failed to "grasp the critical importance of this fact" (namely, that "though men have the power of purchase, they may not choose to use it") "and appears to limit its action to periods of 'crisis'" (*General Theory*, 19 n.). Whether (1) Marshall did in fact fail to "appreciate the importance of this fact" for the theory of the forces affecting the *level of output and employment as a whole;* whether (2) the passage quoted is consistent with an interpretation of Marshall, a representative of "classical" tradition, as having argued that such a theory "can be worked out . . . as being based on 'real' exchanges with money introduced perfunctorily in a later chapter," and as having argued that it is safe to say in all cases that "if people do not spend their money in one way they will spend it in another"; (3) what is meant by "crisis," and particularly what is the relation of "crisis" to the inauguration and prolongation of "depression," with all that the latter means for "underemployment"—these are questions which each student of Marshall must decide for himself, particularly in the light of (1) what is said above, p. 75, n. 59, with respect to Marshall's announced program for the analysis of "Employment," including the effect on "Employment" of "Money" and the "complex actions and reactions of *Credit*"; and also in the light of (2) "old" Cambridge applications, to the problem in hand, of the Marshallian version of the "cash-balance approach." See, for example, Mr. Robertson's comment, in a review written in 1925, to the effect that "in Mr. Keynes's doctrine [as presented in his *Monetary Reform*] of the increase of 'real balances' during depression the modern Socialist can legitimately find support for his old conviction that the fact that those who have got money do not always want to spend it, even on the instruments of production, is a potent cause of unemployment" (Robertson, *Economic Fragments*, 186). The full irony of this comment can be appreciated only if it is remembered (*a*) that, in the *Monetary Reform* itself, Mr. Keynes presented his "doctrine of . . . 'real balances'" as following "the general lines of Professor Pigou and of Dr. Marshall" (*Monetary Reform*, 85 n.), and as an aspect of "the Quantity Theory of Money" [read: the type of analysis associated with the use of Quantity Equations], which was characterized as a "Theory" which is "fundamental," and whose "correspondence with fact is not open to question," though "it is often misstated and misrepresented" (*Monetary Reform*, 81); and (*b*) that Quantity Equations in general, and the "cash-balance" variants thereof, in particular, were rejected by Mr. Keynes in his *Treatise* on the ground that their use "only causes confusion" (see

not satisfactorily resolved by the apparatus of the *General Theory,* any more than they were satisfactorily resolved by the apparatus of the *Treatise.* On the contrary, if we are to judge by the treatment accorded to these issues by avowed defenders of the argument of the *General Theory,* they have been left in greater confusion than ever before. Surely there is evidence of confusion, for example, when "Keynes' Law" is given no less than four formulations in a single article designed explicitly to demonstrate the fitness of the "Law" for dealing with the relation between "costs, incomes, and demand"—particularly since the four formulations are by no means necessarily identical in substance.[133]

Volume I, 414 f., of the present work). What is really difficult to understand is that it should be thought proper, in the face of passages such as that cited from Marshall's *Principles,* to berate Marshall, as a "classical" economist, for having insisted that "if people do not spend their money in one way, they will spend it in another," at the same time that one insists upon retaining a type of analytical apparatus which encourages one's disciples to insist that "demand" may be substituted without qualification for "income."

[133] The four formulations in Mr. Lerner's paper on "The Relation of Wage Policies to Price Policies" are: (1) "Total demand is not independent of total cost," or "demand is not independent of cost" (pp. 159, 160); (2) "Under certain circumstances a change in wages and so in total costs including normal profits will bring about an equal change in demand" (presented [p. 160] as "a more complete formulation of Keynes' Law"); (3) "A general reduction of wages would constitute a reduction in costs, in incomes, and in demand" (p. 159); and (4) "The costs incurred in the production of any commodity constitute the incomes out of which comes the demand for all the other commodities" (p. 159). On the essentially innocuous version (or versions) indicated under (1), see what is said above, p. 606, nn. 117 and 118, and p. 611, n. 126. Whether the "more complete formulation of Keynes's Law" is or is not to be regarded as innocuous depends entirely, of course, upon what the "circumstances" are under which the proposition indicated under (2) is supposed to hold. (One would have to include, for example, provisos to the effect [a] that there is no change in elements of cost *other* than wage costs; [b] that the change in "wages" is a change in wage *incomes,* and not merely a change in wage *rates,* since changes in the latter would be consistent with a very large number of possibilities with respect to the movements in wage *incomes;* [c] that the change in "wages" did *not* increase incomes which are not also costs (such as profits greater than "normal"); and [d] that no change was made in the administration of cash balances relative to outlay $(K = 1/V)$, or any change leading to an increase in non-income receipts (such as borrowing from commercial banks) to make the change in "demand" greater or less than the change in "incomes".) With respect to the anything but innocuous version indicated under (3), see what is said above, pp. 444 f., nn. 91 and 93; p. 609, n. 123; p. 611, n. 126; and p. 616, n. 132. With respect to the version indicated under (4), see Volume I, 128 ff., in connection with the corresponding aspect of the argument of the *Treatise;* also what is said above, p. 609, n. 123, with respect to the argument of the *Treatise* when this argument is stated in terms of a different definition of the relation of "profits" to "income." It should

Surely evidence of confusion appears also in statements with respect to
the rôle of wage-increases in bringing about inflation, when these state-
ments fail to distinguish sharply between the type of rise in wage rates,
on the one hand, which is directly relevant for *ex ante* cost curves and
supply curves, and a rise in "wages," on the other hand, which takes the
form of a rise in realized wage *incomes*.[134]

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be observed, in addition, that the statement that "the costs incurred
in the production of any commodity constitute *the* incomes out of which
comes the demand for all the other commodities" is much more difficult
to defend than it would be if the article *"the"* were omitted. See above,
p. 444, n. 90, and the reference to Volume I there given; also below,
pp. 702 f.

[134] See again, for example, the argument of Joan Robinson in the *Eco-
nomic Journal* for September, 1938, cited above, p. 590, n. 87. Having in-
sisted, in the course of a discussion of the "great German inflation," that
"neither the budget deficit nor the increase in quantity and velocity of
circulation of money can produce the effects attributed to them," Mrs.
Robinson insists, further, that "in each explanation some essential item
is missing," and that "it is the *rise in money wages*" which represents
"the missing item," since "each rise in wages raises prices." "Without rising
money wages," writes Mrs. Robinson, "inflation cannot occur, and what-
ever starts a violent rise in money wages starts inflation." Cf. also
Lerner, "Some Swedish Stepping Stones in Economic Theory," *loc. cit.,*
578, 585, where it is argued that it is the rise in "wages" that leads "to a
cumulative rise in prices at an increasing rate," and that it is possible to
"avoid inflation only by an undemocratic holding down of wages." It
is significant that both Mrs. Robinson (*op. cit.,* 510) and Mr. Lerner (*op. cit.,*
578) reason as if a rise in "wages," even when viewed as a factor affecting
costs, necessarily means a rise in *"money incomes."* The following ob-
servations are therefore in order: (1) If one includes under "inflation,"
as one must, the cases of "inflation" induced by private borrowing from
commercial banks, a "violent" rise in money wage *rates,* instead of being
the factor which makes inflation "occur," may be precisely the factor
which will prevent the continuance of the inflationary process, by destroy-
ing a previously favorable relation between expected costs and expected
selling prices, and therefore making borrowing unprofitable at existing
rates of interest, or indeed at any positive rate of interest. In this case,
clearly, realized wage *incomes,* and indeed *all* realized "money incomes,"
may be expected to fall, rather than to rise. (2) Even in cases of a
governmental "inflation," the same argument will hold with respect
to that *part* of borrowing which is *commercial* borrowing, and therefore
subject to calculations of profit and loss, in all those cases in which the
conditions of demand reflected in governmental spending and the
"secondary" spending generated thereby, have not been such as to drive up
particular selling prices high enough to offset the "violent rise" in the
corresponding particular wage rates. For in such a case the supply prices
which, presumably, are supposed to evidence a "violent rise" in order
to cover the assumed "violent rise" in expected costs, will not be *realized,*
with the result that no *receipts* will be realized, and therefore no wage
incomes or any other kind of incomes will be generated. (3) Even if,
by "money wages," we mean "money wage *incomes*," to say that "without

.To say this is not to deny to Mr. Keynes historical credit for having
reminded us, even if by the very excesses and deficiencies in his own
exposition, that no argument with respect to the effect of changes in
"wages" upon prices and output can be accepted if it fails to do justice
to both the *income* aspect and the "cost" aspect of changes in wage
rates.[135]   The point made here is merely that something more than the

rising money wages, inflation cannot occur" is to assume (I) that the
governmental expenditure in itself, and the spending of incomes *other*
than wage incomes, will have no effect in driving up prices on the side
of *demand;* and (II) that entrepreneurs will not raise their market sup-
ply prices unless their wage costs rise.   There is, of course, no reason
whatever for regarding either assumption as a necessary assumption.
See, for example, what is said above, p. 566, n. 35, with respect to the
nature of the argument as to the relation between costs and market supply
prices which one finds in Mr. Keynes's own *How to Pay for the War.*
Contrast, also, with Mrs. Robinson's suggestion that "without rising
money wages, inflation cannot occur," and with Mr. Kaldor's statement
that "the level of prices is determined by the scale of money remunera-
tions of the factors of production *and not* by the flow of money payments"
(see above, p. 592, n. 91), the emphasis one finds in this later work by
Mr. Keynes on (1) the rôle of the "pressure of spending power" in
bringing on "the tide of inflation" (p. vi); on (2) the relation of an
"increased quantity of money" to "prices" (pp. 8 ff.); on (3) the relation
of "the weight of purchasing power available in the hands of consumers"
to attempts at "price fixation" (pp. 33 f.); on (4) the "vicious process"
as being "started by prices being forced up at the *demand* end" (p. 56
[italics mine]); and on (5) the relation between the *lag* of "wages and
other costs" *behind* "prices" and the income of the "profiteers" (pp. 66 ff.).

   [135] Cf. above, p. 611, n. 126; and see also the comments of Haberler,
Prosperity and Depression, 241 f., 395 ff.   It will be observed that I
have preferred to speak of "the income *aspect*" rather than the "income
effect," as applied to the problem of the consequences of changes in
"wages."   Contrast Bissell, "Price and Wage Policies and the Theory of
Employment," *loc. cit.,* 214 f., 230 f.   A first reason for hesitating to adopt
the latter type of usage is that there are grounds for believing that the
term "income effect" has already been used to cover a range of phenomena
too wide to permit adequate attention to the heterogeneous elements
involved (see above, pp. 298 ff.).   In the second place, Professor Bissell's
usage, with its contrasting of "income effects" with the effects of "relative
price change," would suggest that the consequences of changes in wage
rates are to be dealt with, under the dichotomy of "income effects *versus*
substitution effects," on the assumption that the changes in wage rates
involved are *realized* changes in wage rates, and that the associated
"price" changes are changes in *realized* prices; whereas one of the major
reasons for distinguishing between the effects of changes in wage *rates*
as an element in *cost,* on the one hand, and realized changes in wage
incomes, on the other, is that the latter distinction permits us to begin
with the effect of a change in the wage rate *asked by workers* upon *ex ante*
cost curves and supply curves, and to consider in each case whether the
change is likely to result in *realized* wage rates and therefore in *realized*
prices and *realized* wage incomes.   And it should be observed, in the third
place, that the use of the term "substitution effect" as the other element

granting of historical "credit" is involved in a judgment as to the relative merits of rival sets of apparatus for tracing the relations between "costs, incomes, demand"—and, one must add, realized *prices*. It is for the reader to decide as between the merits, for this purpose, of either the apparatus presented in Keynes's *Treatise* or that presented in the *General Theory,* on the one hand, and, on the other, an apparatus based upon the fundamental propositions defended in this work: namely, (1) that the problem of establishing a relation between "costs" and *realized* "incomes" (and therefore the "demand" which is represented by realized expenditure out of such incomes) must be attacked from the standpoint of the relation of "costs" to *realized* money *prices,* the only kind of "prices" resolvable into realized money receipts, including receipts which represent income to the recipient; (2) that if changes in "costs" are to be related to the realization of money prices, they must first be related to changes in *ex ante* market supply schedules; (3) that if the latter changes are to be held to result in changes in *realized* incomes, they must themselves be shown to result in changes in realized money prices or changes in the quantity of objects actually sold at these prices, or both; (4) that no conclusions can be reached with respect to the relation between changes in market supply schedules and changes in realized money prices unless full use is made of all that is offered by both the "general" Theory of Value and the Theory of Money and Prices with respect to the rôle of *Demand* in the realization of money prices and realized sales of commodities and services; and (5) that this is merely another way of saying that an adequate solution of the problem as to the relations between "costs, incomes, and demand" requires the construction and use of a system of *stream equations,* which would represent the flow of money payments (realized *Demand*) and of objects sold against these money payments (realized *Supply*), in all sectors of the economic process, and which would do full justice to all of the material available within both "monetary theory," in the narrower sense of the term, and the "general" Theory of Value, for helping us to understand why the components of realized Demand and of realized Supply are of the magnitude, in the aggregate and in detail, that they are.[136]

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of the dichotomy, "income effects *versus* substitution effects," hardly does justice to the range of problems which is opened up by a consideration of the effect of changes in the wage rates asked by workers upon the structure, realized and expected, of "relative prices" (cf. Bissell, *op. cit.,* 215). For the type of "relative price change," actual and expected, which is most crucial for a study of the relations between changes in wage *rates* and changes in wage *incomes* is that which affects the relation of "ex ante" *costs* to "ex ante" *selling prices* confronting individual entrepreneurs, from whom proceed the decisions with respect to *money outlay* which are so fundamental for the process of the generation of money income.

[136] It should hardly be necessary to emphasize that an understanding of why "realized Supply" is as large as it is necessarily includes an understanding of why realized *Employment,* considered both in the aggregate

XL. By Proposition XIX (p. 351), it was established that the part of the "general" Theory of Value which is directly relevant to the problem of the generation and utilization of income is that part which is summed up by the Walrasian concept of a "circuit flow," in which the money proceeds realized from the sale of goods are regarded as being returned, through entrepreneurial purchases of the services of the factors of production, as incomes to these factors. Since the very concept of a reward to the "factors of production" involves the concept of *costs*, it is clear that constructions of the Walrasian type (and their historical precedents) do establish a permissible type of connection between "costs" and "incomes." [137] From our discussion of Proposition XIX, also, we know that an adequate understanding of the implications of the Walrasian system must lead to a rejection of statements commonly made with respect to (1) the supposedly "timeless," non-"process" character of "Walrasian" analysis; (2) its alleged failure to do justice to the rôle of money in that "circular flow of economic life" of which it was intended to be a picture; and (3) its alleged unfitness as a *starting point* for "dynamic" analysis, including an analysis of processes involving

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and in terms of structure, is as large as it is. Quite apart from other considerations, this conclusion follows from the fact that the supply of "labor" is one of the elements included in our "Supply." The *difference* between the treatment of the "realized supply of labor" ("employment") in the analytical system here outlined and the treatment found in Keynes's *General Theory*, for example, is that the *relation* of the amount of "employment" to the amount of "output" is frankly recognized as a problem of production functions, instead of being skirted by loose expressions of "sympathy" with the "labor theory of value," with its proposition that "labor is the sole source of value," and so on. See above, p. 534, and n. 28 thereto.

[137] It should be observed that the interpretation of the Walrasian system as a representation of the "circular *flow*" of economic life means (1) that the "costs" involved are *realized* "costs"; and (2) that these realized "costs" are associated with cost *payments* in the form of money. At the same time, the Walrasian emphasis upon the *ex ante* "dispositions" of "demanders" and "suppliers" (see above, p. 185, n. 88) leaves full room for the type of economic *calculation* with respect to "costs" which makes realized cost *payments* and the associated realized income *payments* what they are. It is true, of course, that Walras's own representation of the "circular flow" was primarily a picture of a *"stationary"* circular flow (see above, p. 113). But cf. the references given in the following note.

changes in (*a*) the *level* of money incomes and other money receipts; and (*b*) the *rate of use,* and the *direction of use,* of money received as income or as some other form of money receipts; as well as (*c*) changes in the level and structure of output as a whole.[138]

But it was the purpose of Propositions XX (p. 364) and XXI (p. 365) to show that a *full exploitation* of the possibilities inherent in constructions of the Walrasian "circular flow" type is possible only if (1) our whole picture of the Walrasian process is translated into a series of "stream" equations of the general form $MV = PT$ (Proposition XX); and only if (2) these equations are themselves subjected to the further elaboration and development of which they can be shown to be capable (Proposition XXI). In Chapter Eight of the present volume, this conclusion was applied to problems such as that of the relation between "costs" and "incomes" in both a stationary and a non-stationary world.[139] In Chapter Nine, it was illustrated by the use of a three-dimensional mechanical "model" designed to bring out the ways in which the structure outlined in this work proposes to make simultaneous use of (1) the particular *supply* schedules, as well as the particular demand schedules, of that part of the "general" Theory of Value which is intended to deal with "discrete" pricing situations ("particular" equilibrium analysis); (2) the substance of the Walrasian "general interdependence" analysis, in both its simultaneous and its "time" aspects, including, in the latter, its "flow" aspects on the side of *supply* as well as of demand; (3) the whole of those sectors of *monetary* theory which are concerned precisely with the forces determining the level and direction of money flows (including the flows involved in the generation and utilization of money incomes), and with the effects of these money flows upon the structure of prices and of "supply" (including that part of "supply" which is derived from current output); and (4) a set of analytical devices which make it possible for us (*a*) to date all realized events (including events on the side of supply)

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[138] See above, pp. 358 ff., and especially the comments on the related aspects of the analysis of Schumpeter, on pp. 111 ff.

[139] See above, pp. 431 ff.

in terms of the *clock* (historic) time in which alone economic processes unfold themselves in the world we know; and at the same time (*b*) to refrain from sacrificing any of the heuristic values attaching to the use of significantly defined "analytical" time periods.[140]

In all this, in other words, room has been left for a treatment of problems upon the side of *supply*, as well as on the side of demand, which will do full justice to the substance of those parts of *both* the "general" Theory of Value and the Theory of Money and Prices which can be shown to have abiding heuristic value for the explanation of the economic processes realized in the world we know. That not all of the argument with respect to the Theory of Supply has been presented in this work should be clear from our announced intention to deal in detail, in a later publication, with the range of problems connoted by the topic *Money and Production*.[141] But the instructed reader should have little difficulty in envisaging the way in which it is proposed to use the apparatus here outlined in dealing, in this later work, with the two cardinal pillars of the theory of the effects of monetary expansion and contraction upon the level and structure of Output as a Whole: namely, the Theory of Money Demand, on the one hand, and the Theory of the Structure of Money Prices, on the other. Again it must be insisted that the two are indissolubly connected.[142] They are as indissolubly connected, indeed, as the "general" Theory of Value and the Theory of Money and Prices must be in any adequate attempt to account for the determination of money prices in the world we know.

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[140] See above, pp. 384 ff., 471 ff.

[141] See the Preface to the present volume.

[142] Cf. above, p. 546, n. 50, and p. 562, n. 23.

PART FOUR
HIGHROAD AND BLIND ALLEY
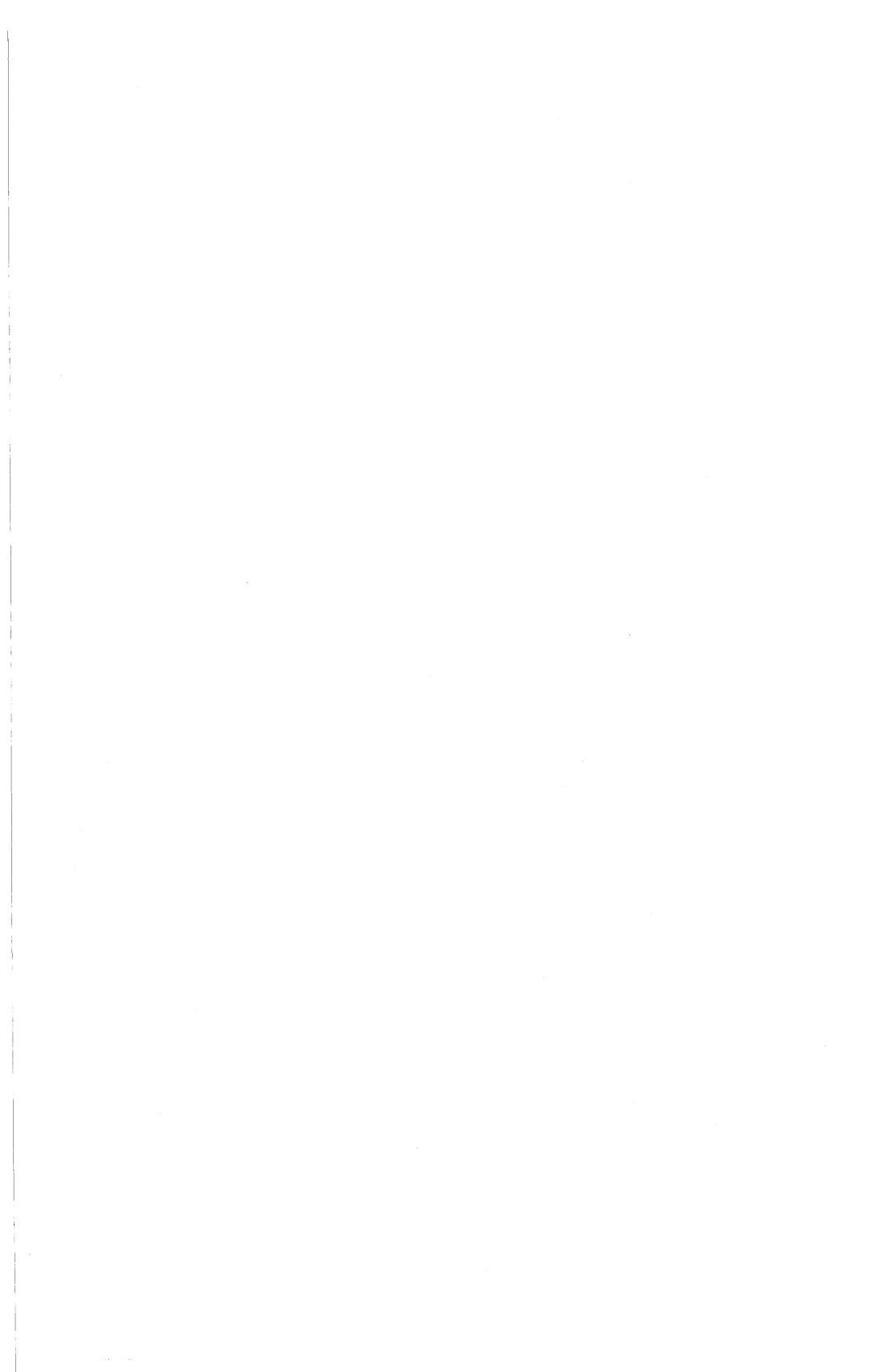
CHAPTER TWELVE

On Certain Elasticities of Supply and Demand in Monetary Theory

THE WHOLE of Parts Two and Three of the present volume may be taken as a positive answer to the challenge to economists, implicit in Keynes's *General Theory*, to demonstrate the untruth of the charge that when these economists have passed to the Theory of Money and Prices we hear no more of "notions," such as "the elasticities of supply and demand," which have played such a large part in the general Theory of Value. No survey of this part of our subject would be complete, however, if it failed to evaluate Mr. Keynes's challenge in the light of certain "Lessons of Doctrinal History" which bear directly on a number of other applications of the "notions" of "elasticity of supply" and "elasticity of demand" which have been made, and are currently being made, to the Theory of Money and Prices.

Specifically, attention must be called to one of the very first of the Lessons of Doctrinal History presented in Chapter Three of this volume: namely, that each successive attempt to apply, to the Theory of Money and Prices, concepts developed originally within the general Theory of Value, has not always led to specific substantive results which have left the subject in a more advanced state than it was in before such an application was attempted; that, on the contrary, the results obtained were often actually inferior to those already available.[1] Attention must be called, secondly, to the nature of the *reasons* for the disappointing nature of the results thus obtained: namely, the fact that the applications in question in some cases either resulted only in the posing of problems that are purely factitious, or in the statement of

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[1] See above, p. 125, and the references given in nn. 83 and 84 thereto.

real problems in such a way as to result in an actual obscuring of the nature of the subtantive issues in dispute; while in other cases these applications represented only an excessive formalism which confused a gain in *substance* with either a mere restatement in unfamiliar terms of substantive results already perfectly familiar within the Theory of Money and Prices, or with a mere restatement in other terms of the problem to be solved.[2]

These, one may suggest, are the blind alleys which have drawn too many writers anxious to effect a "synthesis" between the Theory of Money and Prices and the "general" Theory of Value away from the highroad leading toward a genuinely fruitful synthesis, of the kind which I hope is represented by the argument presented in Parts Two and Three of the present volume. It is essential to progress on this highroad that as little effort as possible be wasted in struggles through the mire of what not only *can* be shown to be blind alleys, but *have* been shown to be blind alleys.[3] In the present chapter it is proposed to illustrate these propositions by considering certain applications of the concepts of "elasticity of supply and demand" to the Theory of Money and Prices which, it is here argued, have led to little or no *substantive* gains for the Theory of Prices; and, by way of emphasizing the fact that recognition of the futility of most of these applications is still a *desideratum* in our own day, it is proposed to illustrate the issues involved, wherever possible, by a consideration of usages sponsored in Keynes's *General Theory*.

I

The "Elasticity of Supply of Money"

The formal application of the concept of an "elasticity of supply" to the supply of *money* was not unknown in the years preceding the publication of the *General Theory*.[4]

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[2] See above, pp. 126 ff., and the references given in nn. 86–90 thereto.

[3] Cf. Lambert, *La Théorie quantitative de la Monnaie,* 260: "Il importe tout autant de connaître les chemins embourbés que les routes royales."

[4] See, for example, Pigou, "The Value of Money," *loc. cit.,* 55 ff. (*Essays in Applied Economics,* 189 ff.). The "formal application of the concept of an 'elasticity of supply' "—in the *Marshallian* sense of "elasticity

In view, however, of the degree of influence exerted by the latter work, and in view also of its claims to have effected a particularly fruitful synthesis between the "general" Theory of Value, on the one hand, and the Theory of Money and Prices, on the other, it cannot be a matter of indifference that it should have given its support to this type of application of the concept of "elasticity of supply" to the Theory of Money and Prices—particularly since Mr. Keynes's substitution of his concept of "elasticity of *production*" for the concept of "elasticity of *supply*" in this context, can be shown to have introduced a special series of difficulties on its own account.[5]   By way, therefore, of indicating the rea-

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of supply"—"to the supply of money" is, of course, to be sharply distinguished from loose statements with respect to the "elasticity of supply of money" which, instead of relating changes in the "supply" of a particular commodity (in this case, "money") to changes in the price (or "value") of that commodity, have reference only to the range of problems traditionally discussed under the head of the consequences of an "elastic" or "inelastic" currency. For examples of such a use of the term "elasticity of the money supply," see Lavington, *The English Capital Market,* 41, 154 ff.; Edie, *Money, Bank Credit and Prices,* 33 f., 142; Haberler, *Prosperity and Depression,* 33, 101, 146, 182, 356, 387; and Lambert, *La Théorie quantitative de la Monnaie,* 231, 241, 251, 260. Unhappily, the usage of the followers of the *General Theory* is often such as to make it impossible to determine whether "the elasticity of supply of money" is thought of in terms of Mr. Keynes's formal use of the concept, or in loose terms such as those just indicated. See, for example, the generalizations with respect to "the elasticity of supply of money" in M. Kalecki, "A Theory of the Business Cycle," *Review of Economic Studies,* IV (1937), 87.

  [5] For examples of an application, in the *General Theory,* of the concept of an "elasticity of production" to *money,* see pp. 230 f., 234–236, and 238 of that work. That Mr. Keynes regarded his "elasticity of *production*" as being capable here, as elsewhere, of application to all problems with which the concept of "elasticity of *supply*" was intended to deal, is evident from the fact that the Index to the *General Theory* contains (p. 400), under the entry "Supply, elasticity of," only a cross-reference to "Production, elasticity of." The substantive difficulties, to which reference is made in the text, arising from a substitution of the concept of an "elasticity of *production* of money" for an "elasticity of *supply* of money" are discussed below, pp. 633 ff., 637 ff. Here it is necessary to observe only that the particular definition of "elasticity of production" given by Mr. Keynes in his discussion of the "elasticity of production of *money*" is closer to the "ordinary" (Marshallian) definition of "elasticity of supply" than is his *formal* definition of "elasticity of production" in general. For, as we have seen, this formal definition of "elasticity of production" would relate the responsiveness of "supply" (in this case, of *production,* or "output"), not to changes in the *price per unit* of the commodity "supplied," but to changes in the amount of *"effective demand . . .* directed

sons for refusing to greet the type of application indicated with unqualified enthusiasm, the following propositions are submitted for consideration:

1. The concept of "elasticity," as Mr. Hawtrey, in commenting on the expression "the elasticity of supply of money," has reminded us, is in itself "merely a device for describing the behavior of anything in relation to its price in terms of the differential calculus." [6]   If, despite this fact, the concept of "elasticity of supply" has proved fruitful within the "general" Theory of Value, it is only because it

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towards" the particular commodity in question. (See the *General Theory,* 282. On the relation between the two types of "elasticity," see what is said above, pp. 530 ff. It may again be observed that this relation can hardly be said to have been discussed with sufficient articulateness by Mr. Keynes in his *General Theory.* On the contrary, the definition of the "elasticity of production" of *money* which is cited below is characterized, in the Index to the *General Theory* [p. 398, under "Production, elasticity of"], merely as a "preliminary definition" of "elasticity of production" in general, to the discussion of which, in Chap. 20 of the *General Theory,* a forward reference is given on p. 230, n. 1. Cf. also p. 302 of the *General Theory,* where the "elasticities of supply" of "different factors" of production are discussed in terms of "response to changes in the money-rewards offered," without any indication of whether the "rewards" in question are "rewards" per *unit* of factor, or "rewards" per *block* of any one factor.) The "elasticity of production" of *money,* on the other hand, is defined by Mr. Keynes as relating the responsiveness of the "production" of "money" to changes in the value of the *monetary unit*—or, in the Keynesian terminology, to "the price [of money] . . . in terms of the wage-unit" (*General Theory,* 230). The "wage-unit" is presumably to be understood here, as elsewhere, as a kind of *numéraire* (cf. above, p. 597, n. 101). It therefore corresponds to "old" Cambridge's "in terms of wheat," which was used by Pigou precisely in connection with the concept of an "elasticity of supply" of *money.* (See Pigou's "The Value of Money," *loc. cit.,* 55 f. [*Essays in Applied Economics,* 189 ff.]; and cf. what is said above, p. 597, n. 101, concerning Mr. Keynes's earlier misrepresentation of the meaning of this practice.) With respect, on the other hand, to Mr. Keynes's alternative definition (*General Theory,* 230) of the "elasticity of production" of money as measuring "the response of the *quantity of labor applied* to producing" money (rather than as measuring the extent to which entrepreneurs are willing to *"produce money"*), no general comments need be added to what was said above, p. 534, n. 28, concerning (1) the *General Theory's* treatment of the problems traditionally discussed under the head of production functions and the combination of the agents of production; and (2) its relation to Mr. Keynes's avowed sympathy "with the pre-classical doctrine that everything is *produced* by *labor,*" and that "it is preferable to regard labor . . . as the sole factor of production." On the special consequences, however, of Mr. Keynes's usage in this particular instance, see what is said below, p. 636, n. 20.

[6] See *Economica* for February, 1938, p. 97.

has been possible to relate it to the actions of economizing individuals, for whose "plans" and decisions the relation of price to quantity supplied can be shown to have economic significance. It follows that if the application of the concept of "elasticity of supply" to *money* is to be equally fruitful, it must be demonstrated that the relation of price (or "value") to quantity supplied has the same kind of significance for the economic calculations and decisions of the "suppliers" of money that it has in the case of the "suppliers" of commodities *other* than money.

2. Actually, however, the extent to which this can be demonstrated varies greatly in the case of different *types* of "money." Specifically:

i. The one instance in which there might be said to be a *prima facie* case for the application of the concept of "elasticity of supply" to the supply of *money* is the instance of "commodity money," as that term was defined by Mr. Keynes in his *Treatise on Money*: namely, money "composed of actual units of a particular freely-obtainable . . . commodity which happens to have been chosen for the familiar purposes of money." [7]   In this case, as Mr. Keynes himself argued in the *Treatise* (a work, it should be recalled, which made no pretense of having discovered a new type of synthesis between monetary theory and the "general" Theory of Value), the supply of the money commodity will

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[7] *Treatise*, I, 7. For purposes of the present discussion, it is not necessary to specify also, as Mr. Keynes did in his *Treatise (loc. cit.)* that the "commodity" in question must be not only "freely-obtainable," but also "non-monopolised." All that need be specified is that the production of the money commodity be not "monopolised" *by an agency, such as an agency of government,* which is prepared to pay no attention to the economic *profitability* of producing the money commodity: as in cases, for example, in which production has been undertaken (1) to satisfy the whim of a capricious sovereign or a conqueror able to direct forced labor into such production; or (2) by governmental agencies not subject to the condition that costs must be recovered from a *given type* of enterprise (the case of Russia, according to some accounts—impossible to confirm or contradict—of her methods of gold mining). All other "monopolists" *can* be presumed to be sensitive to conditions of profitability; and these conditions will be affected not only by the selling price of the money commodity, but also its cost of production, even if the *rôle played by* "cost of production" in the case of monopoly in affecting supply is not the same in all respects as in the case of competition.

be "governed—like that of any other commodity—by scarcity and cost of production."[8]   In so arguing, Mr. Keynes was of course merely restating a proposition which had been advanced at least as early as the days of Petty (that is to say, long before the relations of "supply" to "scarcity" and "cost of production" came to be subsumed under the *expression* "elasticity of supply"), and which has been modified since the days of Petty only to the extent that the theory of "cost of production" and its relation to supply and realized price has itself been subjected to improvement.[9]   And in so arguing, Mr. Keynes was stating a proposition that has been accepted without question even by writers who have protested with some vigor against the sweeping application to "money" of categories originally developed within the "general" Theory of Value.[10]

ii. On the other hand, the application of the concept of

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[8] *Treatise*, I, 7. It is particularly worthy of note that, in the passage indicated, Mr. Keynes spoke of the supply of the *money commodity*, and not of the supply of *commodity money*, as being "governed—like that of any other commodity—by scarcity and cost of production." On the rôle of "cost of production" in "governing" supply in the case of a "monopolised" commodity, see the preceding note.

[9] On Petty, see above, pp. 15 f., and especially the references given in n. 32 thereto. Cf. also the references to Galiani, Cantillon, Smith, Say, Senior, and Jevons given above, p. 23, n. 58; p. 24, n. 62; p. 28, n. 72; p. 28, n. 74; p. 31, n. 84; p. 40, n. 112; p. 56, n. 14. On the application, in our own day, of advances within the "general" Theory of Value with respect to the theory of "costs," to the special problem of the supply of the money commodity, see Volume I, 155, of the present work, and the reference there given; and contrast what is said at the end of n. 5 to p. 630, above, with respect to the *General Theory's* application, to the problem in hand, of the results of Mr. Keynes's avowed "sympathy" with "preclassical doctrine" within the "general" Theory of Value.

[10] See, for example, the references to Wicksell and Schumpeter given above, p. 14, n. 25, and p. 97, n. 17. See also the entirely sympathetic discussion of the rôle played by cost of production in the supply of the money metal in Fisher, *The Purchasing Power of Money*, 99 ff. (cf. the same author's *Elementary Principles of Economics*, 311 f.); and see, finally, Hawtrey, *Currency and Credit*, p. 171 of the first edition (p. 199 of the third edition), where the case of "metallic money" is specifically listed as an *"exception"* to Mr. Hawtrey's main proposition: namely, that "the theory that money behaves like a commodity" (and is therefore best treated under the head of the analytical categories developed within the "general" Theory of Value for dealing with the value of "commodities"), although it "has the attractiveness of a paradox which completes a generalization," nevertheless "melts away under analysis."

"cost of production," as affecting what came later to be
called the degree of "elasticity of supply," to "money" *other*
than "commodity money" was specifically *rejected* by "econ-
omists" at least as early as the days of Ricardo.[11]   In Chap-
ter One of this volume it was contended that Ricardo, in
so arguing, showed a much greater sense of realism, and a
much more nearly adequate appreciation of the issues of
substance involved, than did those "synthesizers" of the
Theory of Money and Prices with the "general" Theory of
Value who found a fundamental "inconsistency" in Ri-
cardo's refusal to apply specific categories of the "general"
Theory of Value to instances in which they are clearly not
applicable.[12]   Mr. Keynes, therefore, must be charged with
having been guilty of serious retrogression when, in his
General Theory, he applied his concept of an "elasticity of
production" to "money" generally, without clear specifica-
tion that the concept could have even a *prima facie* appli-
cability only in the case of the "production" of what, in his
Treatise, he had called "commodity money." [13]   For the

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[11] See above, pp. 33 ff.

[12] See above, pp. 32 ff.

[13] The only specification I have been able to find in the *General Theory*
is that in which Mr. Keynes limits generalizations with respect to the
"elasticity of production" of "money" by the clause "so far as the power
of private enterprise is concerned, as distinct from the monetary authority"
(*General Theory,* 230). Apart from the question whether commercial
banks "creating" currency without interference by the "monetary authority"
are or are not to be regarded as an example of "private enterprise" en-
gaged in the "production of money," it should be observed that the terms
of Mr. Keynes's proposition with respect to "money" having "both in the
long and in the short period, a zero, or at any rate a very small elasticity
of production" make no direct specification whatever to the effect that
the "money" involved is "commodity money." On the contrary, the
limitation with respect to the "power of private enterprise" is a limitation
upon the proposition that "money" has "a zero, or at any rate a very small
elasticity of production," and not upon the applicability of the concept
of an "elasticity of production" to "money" in general—including money
"produced" under the guidance of the "monetary authority." That Mr.
Keynes does intend to apply the concept of "elasticity of production"
to non-commodity money is evidenced, in fact, by his statement (*General
Theory,* 230) that his proposition that "money has . . . a zero, or at any
rate, a very small, elasticity of production" (or, alternatively, that "labor
cannot be turned on at will by entrepreneurs to produce money in increas-
ing quantities as its price rises in terms of the wage-unit") represents a
condition which is "strictly satisfied" "in the case of an *inconvertible
managed currency*" (italics mine). Otherwise, to be sure, from Mr. Keynes's

only effect of such a usage could be to obscure the substantive issues, with respect to both analysis and policy, which are necessarily involved in categorical statements with respect to the degree of "elasticity" that we may expect the "production" of "money" to evidence in the world we know.

From Mr. Keynes's account of the matter, the problems of monetary policy which are associated with the "elasticity of supply" of metallic money derive from the alleged fact that even "in the case of a gold-standard currency," it is at least "approximately" true that "money has, both in the long and in the short period, a zero, or at any rate a very small, elasticity of production"; so that "there is no remedy" but to rely upon currency management for such changes in the quantity of money as may be felt to be necessary.[14] It is not entirely easy to evaluate the first part of this proposition, because of Mr. Keynes's tendency to speak as if his proposition that "the output of money . . . is . . . perfectly inelastic" were merely a "hypothesis" introduced for the sake of argument.[15] The use of "hypothesis" in this context, however, would surely be justified only in the case of the "output" of money under "an inconvertible managed currency," or the "output" of bank money as affected by the rate of discount and other relevant institutional and conjunctural factors. The concept of "elasticity of supply" as applied to the supply of the money *metal* is, on the contrary, one which is capable of treatment by methods much more concrete than those represented by the use of a series of "hypotheses" with

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reference to "a green cheese factory (i.e., a central bank) under public control," and the need "to persuade the public that green cheese is practically the same thing" as "money" (*General Theory*, 235), one might have concluded that in these passages Mr. Keynes regards nothing as money but *"commodity* money." But apart from the remarkable fact that so drastic a departure from the usage of the *Treatise* (see I, 9 f., 31 ff., of that work) is presented without comment on the significance of the terminological departure itself, the effect of the change in usage, if accepted as a deliberate change, would be to destroy the meaning of the whole context, which is concerned as much with the "elasticity of *substitution"* of money as with its "elasticity of production," and in which this "elasticity of substitution" is certainly discussed without any implication that what is involved is the "elasticity of substitution" of "commodity money" *alone.* The very opposite, indeed, is implied by the fact that Mr. Keynes actually bases his proposition that "money . . . has an elasticity of substitution equal, or nearly equal to zero" on the alleged "peculiarity of money that its utility is solely derived from its exchange-value," and insists that this is why the "tendency to substitute some other factor for it," in the case in which "the money-commodity is also used in manufacture or the arts," can operate only "to some trifling extent" (*General Theory*, 231).

[14] See the *General Theory*, 230, 235, 238.

[15] Cf. the *General Theory*, 234 f. On p. 235, for example, "inelasticity of supply" is spoken of as having been *"postulated* as a normal characteristic of money" (italics mine).

respect to what the "monetary authority" chooses arbitrarily to do
In all strictness, therefore, what is called for is an investigation, on
both the analytical and the empirical sides, of the elements actually
entering into the "elasticity of production" of a specific money metal.

It is quite clear, however, that such an investigation does not underlie
Mr. Keynes's proposition that "money," even "in the case of a gold-
standard currency" has, "both in the long and the short period, a zero,
or at any rate a very small elasticity of production, so far as the power
of private enterprise is concerned." [16] With respect to the "short pe-
riod," for example, much depends on what is meant by "short." If
the period taken is very "short" indeed, the supply of the money metal
could be regarded as quite "inelastic." [17] It is certainly true, on the
other hand, that if, by the "short period," we mean periods up to, say,
five years, there have been "short periods" during which the "elasticity
of supply" evidenced by the money metal has been very great indeed—
witness, for example, the periods of the Australian and Californian gold
discoveries, or the developments within the last few years.

It might be retorted, to be sure, that these were periods so abnormal
as not to invalidate seriously Mr. Keynes's generalization.[18]   There is,
however, no such escape with respect to Mr. Keynes's extension of his
proposition that the elasticity of the supply of the money metal has a
"zero, or at any rate a very small, elasticity of production."   The basis

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[16] See again the *General Theory*, 230.

[17] The practice of regarding the "supply" of the money metal as com-
pletely inelastic in the "short period" is a not uncommon one. It was
followed, for example, by Pigou (*Essays in Applied Economics*, 190) and
Marshall (*Money, Credit, and Commerce*, 283 f.). It is worth noting,
however, that in neither case was the problem put in terms of the elasticity
of *production* of metallic money. On the contrary, it was put in terms
of changes in "the aggregate *stock*" of metallic money, the argument
being that the fact that this stock "is very large relatively to the total
annual output" would justify the practice of regarding the "supply" of
metallic money as being, "for periods of moderate length, . . . practically
constant" (so Pigou, *loc. cit.*). Whatever may be said of this type of
statement, it has at least greater plausibility than a statement, such as
that of Mr. Keynes, to the effect that the "elasticity of *production*" of
gold is, *even in the long period,* "zero, or at any rate . . . very small."
Professor Pigou, on the contrary (after pointing out that "the supply
schedule relevant to immediate effects will not be the same as that relevant
to later effects," and that "to the question how the value of money will
be affected" by responses in the supply of the money metal to a change
in the unit value of metallic money, "no intelligent answer can be given
without reference to the time that is supposed to have elapsed since the
change occurred"), concluded that "the supply schedule . . . displays
greater and greater elasticity the longer the period over which the effect
of . . . [a] changed demand [upon the value of the money metal] is being
calculated" (*Essays in Applied Economics*, 192 f.).

[18] In this connection, cf. the quotation from the *General Theory* given
above, p. 634, n. 15, with respect to inelasticity of supply as a *"normal*
characteristic of money" (italics mine).

for Mr. Keynes's generalization is certainly not empirical, as, in the last analysis, it should be; nor does it seem to be based upon a reference to the separate elements affecting elasticity of supply which are revealed by a study of the peculiarities of the gold mining industry. Mr. Keynes's sole supporting argument, on the contrary, is that "the maximum proportional addition to the quantity of labor which can be . . . employed [in the gold-mining industry] is very small, except indeed in a country of which gold-mining is the major industry." [19] As applied to the concrete problem of gold-mining, the chief result of this argument is merely to display again the weaknesses inherent in Mr. Keynes's insistence upon thinking of the elasticity of the supply of the money metal as involving the response (to changes in its unit value), not of its production, in the literal sense of the term, but of the "quantity of labor applied to producing it." [20] It may well be, indeed—though it is anything but clear that the facts would provide unequivocal support for such a generalization—that the supply of gold has been so inelastic at critical periods in history (even when one remembers that its adequacy is to be judged in the light of the fact that the gold thus supplied is intended to serve as the base for a superstructure of bank money) as to justify

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[19] *General Theory*, 230 f. On the relevance, for the purpose in hand, of the reference to countries in which "gold-mining is the major industry," see the following note.

[20] Cf. above, p. 630, n. 5. So far as the problem under discussion is concerned, it should be clear, of course, that what really matters, for the "elasticity of production" of the money metal, in any significant sense of the term, is not merely the *quantity*, but also the *effectiveness* of the labor applied to the work of producing the money metal. The "elasticity of production" of gold, for example, will certainly be affected not only by the amount of labor devoted to mining it, but also by the possibility of using more efficient methods of mining; and there cannot be the slightest doubt that at certain periods in history the changes in these methods (and in the richness of the veins available to the mining industry) have counted for vastly more, in determining the output of gold, than a change in the "quantity of labor" devoted to the business of mining. It may be observed, in addition, that it is difficult to see the relevance, in this connection, of Mr. Keynes's suggestion that the only case in which we may expect more than a "very small" "maximum proportional addition to the quantity of labor . . . employed" in the "production" of "money" is the case of "a country of which gold-mining is the major industry"; and that "for the world as a whole the maximum diversion [of labor] in this way is almost negligible." It is of no importance, for the "elasticity of production" of the money metal, in any meaning of the term which would make it significant for, say, Mr. Keynes's argument with respect to the inadequacy of the gold standard (cf. the following note), whether a given amount of "labor" is "diverted" to the business of gold production within a *particular country*. What matters is the proportionate increase in annual output in the world as a whole; and it is a very familiar fact that the periods of great increase in gold output, and of a resulting rise in prices, have been periods in which the sources of increased gold supply, as often as not, were geographically **very few.**

the abandonment of a metallic basis for currency altogether, in favor of the adoption of an "inconvertible managed currency" which would make possible "a deliberate increase in . . . [the] supply [of money] by the monetary authority." [21] Such a conclusion would have to be based, however, upon a realistic investigation of the factors affecting the elasticity of supply of the money metal, of the kind that earlier writers, with a minimum of claims to the effect that they had succeeded in constructing a "bridge" between the Theory of Money and Prices and the general Theory of Value, had made of the factors affecting the "marginal cost" of gold output in relation to its value.[22] It should not be allowed to rest upon an uncertain application of an inadequately phrased definition of "elasticity of supply" supposedly drawn from the "general" Theory of Value.[23]

3. Even as applied to the problem of the nature of the forces affecting the "supply" of *"commodity* money," the substitution of the concept of an "elasticity of *production"* of "money" for the concept of an "elasticity of *supply"* of money, in such a way as to imply that they are interchangeable, is to be regarded as unfortunate.[24]   For it is elemen-

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[21] In this connection, cf. the *General Theory,* 230, and especially the remarks, on p. 235 (already referred to above, p. 634, n. 13), concerning the advisability of having "a green cheese factory (i.e., a central bank) under public control."

[22] For our present purpose, it is not necessary to go beyond the presentations which are to be found in current textbooks. See, for example, Edie, *Money, Bank-Credit, and Prices,* 239 ff., and the citations of other writers there given. One need add only that analysis of the type indicated goes far back in economic literature. See, for example, the references to Senior given above, p. 41, n. 113, under (2).

[23] It is interesting, indeed, to compare Mr. Keynes's more recent utterances with respect to the suitability of gold for use as the "standard of value" (cf. the *General Theory,* 230) with his utterances on the same subject before he took as seriously as he now does his mission of bringing "the theory of prices as a whole back to close contact with the theory of value." See, for example, *Monetary Reform,* 14, 178 f.; and the *Treatise,* II, 293 ff. Defenders of the gold standard will hardly be satisfied with all that appeared in these earlier discussions; it is difficult, however, to believe otherwise than that, as contributions to an answer to the question whether gold has or has not evidenced "both in the long and the short period, a zero, or at any rate a very small elasticity of production," these earlier discussions will be found more satisfying than the particular method by which, supposedly with the aid of concepts of the general Theory of Value, Mr. Keynes has reached his latest conclusions with respect to the fitness of gold to serve as a "standard of value."

[24] For evidence that Mr. Keynes did intend that the terms "elasticity of supply" and "elasticity of production" should be used interchangeably, see the reference to the Index of the *General Theory* given above, p. 629, n. 5. In one passage of the *General Theory,* to be sure, Mr. Keynes did

tary that the "supply" of such money in the world as a
whole is a function not only of the amount of the money
metal *produced,* but also of the amount of this metal
diverted to the "arts" uses; and the "supply" of such money
in any given country is a function not only of the amount
"produced" and the amount diverted to the arts in the world
as a whole, but also of its *distribution* among the nations
of the world.[25]   The substitution of the concept of an "elas-
ticity of production" for an "elasticity of supply" of *money*

seem to be on the verge of drawing a distinction between a "rigidly fixed"
production of money, and a "rigidly fixed" "effective *supply*" of money (p.
232). From the context, however, it is clear that Mr. Keynes understood,
by an increase in "effective supply," not an increase in the "quantity of
money," but a *redistribution* of this "quantity of money" as between its
different *monetary* uses, as the result of a possible decline in the demand
represented by certain of these monetary uses; it is clear, in other words,
that his introduction of the concept of an increase in the "effective supply"
of money was not intended to draw a distinction between a change in
the production of the money metal, on the one hand, and a change in
the amount of *money* available for *all* monetary uses, on the other. From
the argument which follows in the text, therefore, it should be clear that
in this respect the bad consequences of Mr. Keynes's identification of
"supply" with "production" are analogous to the consequences of such
an identification when the latter is applied to the case of commodities other
than "money" (see above, pp. 553 ff.), despite the other respects, commented
upon above, p. 629, n. 5, in which the definition of "elasticity of production"
used by Mr. Keynes in connection with "money" differs from the definition
of this "elasticity" which he uses in connection with commodities other
than money.

[25] This was clearly recognized by those writers, such as Professor Pigou,
who, in applying the Marshallian concept of "elasticity of supply" to the
supply of "money," made explicit use of the term "elasticity of *supply,*"
and not "elasticity of *production.*" See, for example, Pigou's *Essays in
Applied Economics,* 190, 192, 198, where the elasticity of supply of money
is discussed in terms designed to take account of both the arts demand
and the international distribution of the precious metals. The only refer-
ence, on the other hand, which I have been able to find in Keynes's
General Theory, to the *arts demand* is in connection with his discussion
(p. 231) of the elasticity of substitution for the money metal; and even
here he made no attempt to relate this "elasticity of substitution" of the
money metal (dismissed, in any case, as a factor having only "trifling"
consequences) to a possible distinction between an "elasticity of *produc-
tion*" of the *money metal,* on the one hand, and an "elasticity of *supply*
of *money,*" on the other (on the latter point, see also the following note).
Certainly no recognition is shown in the *General Theory,* in any case, of
the fact that the problems associated with the international distribution
of the money metals provide further grounds for distinguishing between
an "elasticity of *production*" of "money" and an "elasticity of *supply* of
money." On this matter, see above, p. 636, n. 20.

is therefore a retrogression even in the one case—namely, the supply of "commodity money"—in which even a *prima facie* case can be made for the application of the concept of "elasticity of supply" to the supply of "money."

4. Unfortunately, moreover, the mere fact that a *prima facie* case can be made for applying the concept of "elasticity of supply" to the supply of "commodity money" does not mean that even this case is convincing upon closer examination. On the contrary, it follows, from the argument presented above under (3), that the provision of a case for applying the concept of "elasticity of supply" (or "elasticity of production") to the *money commodity* does not necessarily amount to providing a case for applying the concept of "elasticity of supply" to *commodity money*.[26]

Admittedly, the need for taking account of the arts demand as a factor affecting the supply of "commodity money" need not present insuperable difficulties in this connection; though it would be much more reasonable to apply the concept of elasticity of supply to the *money commodity* (in the sense that this supply is held to be what it is as the result of entrepreneurial calculations with respect to the profitability of producing the money commodity in the face of given conditions with respect to mint price), and then to represent the division of this supply between an arts "supply" and a monetary "supply" as resulting from the play of a composite demand for the money commodity.[27] It is

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[26] Again it should be observed that this was implicitly recognized by those writers who, in making use of the Marshallian concept of "elasticity of supply" *in connection with* the "supply of money," actually *applied* the concept only to the production of the *money metal*. See, for example, A. Cabiati, *Fisiologia e Patologia Economica negli scambi della ricchezza fra gli Stati* (1937), 150; and cf. also D. H. Robertson, "Notes on Mr. Keynes' General Theory of Employment," *loc. cit.*, 191 n.

[27] The problem was stated in these terms by Marshall himself. Cf. his *Money, Credit, and Commerce*, 284. The only question, indeed, that can be raised with respect to Marshall's formulation is whether it is necessary or even helpful to describe the *"currency* demand" component of the "composite demand" for the money metal in terms of an "elasticity of demand" in general, and of *unitary* elasticity of demand, in particular. On the factitious issues which have arisen in connection with the concept of an "elasticity of demand for money," see below, pp. 650 ff. Similar reservations, of course, do *not* apply to the use of the concept of an elasticity of the *arts* demand for the money commodity. For the applicability of

quite another thing, however, to pretend that the concept
of an "elasticity of supply" of *"commodity money"* repre-
sents a convenient weapon for dealing with the nature of
the forces affecting the "supply" of such money *in any
given country*.

For the latter problem must envisage not only the type
of calculation concerning the profitability of producing the
money commodity under given conditions with respect to
the relations between cost of production and mint price, but
also the whole theory of the *international movement of
specie*.[28]   And to insist upon subsuming the whole of the
theory of the latter subject under the heading of an "elas-
ticity of supply of money," solely on the ground that there is
a significant relation between changes in the quantity of
"commodity money" in a given country and a change in the
purchasing power of this "commodity money" in the coun-
try in question relative to its value elsewhere is (1) to sub-
sume under a single analytical category phenomena differing
so widely in their nature as to raise the question whether
the substantive analytical issues are not obscured, rather
than illumined, by such a usage; (2) to forget that the prob-
lem of accounting for the international movements of specie
is one which received an elaborate treatment long before
it was proposed to translate the results obtained into terms
suggested by the concept of elasticity of supply of the money
metal within a single country; and therefore (3) to raise
the serious question whether such a translation, instead of
representing a substantive addition to our understanding of
the relation of the theory of international movements of
specie to the money supply in a given country, would not
at best represent a restatement in other terms of results
already familiar within the Theory of Money and Prices, and

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the categories of the "general" Theory of Value to the *arts* demand has
never been seriously questioned, even by writers severely critical of their
application to the *monetary* demand.   See, for example, Fisher's *Purchasing
Power of Money,* 103 f.

[28] For an example of the subsuming of international specie movements
under the heading of the concept of an "elasticity of supply," in the sense
of the responsiveness of supply of metallic money within any one country
to "a given wheat price per unit" of the metallic money of that country,
see Pigou, *Essays in Applied Economics,* 189 f., 192, 193.

at worst the confusion of a substantive solution of the prob-
lem with a mere restatement in other terms of the problem
to be solved.[29]

5. It is, however, when we come to the "supply" of *non-*
"commodity" money that the case against applying the
concept of "elasticity of supply" (and *a fortiori* the concept
of an "elasticity *of production*") of "money," becomes clear-
est.   In the case, for example, in which the supply of non-
commodity money is regulated by the arbitrary fiat of *gov-*
ernment, it might, to be sure, be going too far to suggest
that "analysis is not capable of dealing" with the nature of
the forces affecting the supply of "money." [30]   It would not
be going too far, however, to suggest that to attempt to sub-
sume such "analysis" under the head of the concept of a

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[29] In justice to Professor Pigou, whose treatment of the "elasticity of
supply of money" shows him to be perfectly aware that the matter of
international specie movements is part of the problem, it must be said
that a critic is disarmed both by (1) Professor Pigou's modesty in refrain-
ing from making extreme claims to having effected a significant substantive
advance in the theory of the subject as a result of his application of the
concept of "elasticity of supply" to the supply of "money"; and by (2)
his refusal to set up the false antitheses which have characterized much
of the later discussion of the application, to "money," of the concept of
an "elasticity of *demand*." The propositions advanced in the text may
therefore be regarded primarily (i) as a warning against a repetition, in
the case of the concept of an "elasticity of supply of money," of the
less fortunate results following from an emphasis upon the alleged im-
portance of the concept of an "elasticity of *demand* for money"; and
(ii) as a demonstration of the fact that even if Mr. Keynes's "elasticity
of production of money" is transformed into an "elasticity of *supply* of
money," it would still be anything but clear that the path thus indicated
would be a path toward further substantive achievement within the Theory
of Money and Prices.

[30] Cf. Bowley, *Mathematical Groundwork of Economics,* 52.   Professor
Bowley does specify, to be sure, that the "political interference with cur-
rency" with which, he insists, "analysis is not capable of dealing," must
be an "undefined" political interference; so that any disagreement with
his proposition would turn upon what is meant by *"undefined* interference."
There is, for example, ample room for "analysis" of the consequences, for
the money supply, of "political interference with the currency" which is
"defined" only in the sense that it involves an unbalanced budget. But
there can hardly be doubt as to the warning, which may be regarded as
implicit in Professor Bowley's comment, against attempting to force our
analysis into the type of strait-jacket represented by an insistence upon
applying the type of "analysis" one finds within *the "general" Theory of*
Value to the problem of accounting for such changes in the "supply of
money" as might be associated with "political interference with currency."

functional relation between the "supply" of money and its "value" (as in the case of an "elasticity of supply of money") would be to force the power of analogy to the breaking point, without any visible gain in substance or clarity of exposition.[31]

The same thing must be said with respect to that part of the supply of "non-commodity" money which results from business borrowing from commercial banks. For although elements of economic calculation and "volition" are certainly involved in this case, both on the side of the sup- pliers (the commercial banks) and on the side of the de- manders (business borrowers), these elements neither bear

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[31] Cf. what is said above, p. 633, n. 13, with respect to Mr. Keynes's application of the concept of an "elasticity of production of money" to "the case of an inconvertible managed currency." Cf. also Pigou, *Essays in Applied Economics,* 189 (though see what is said on this matter above, p. 641, n. 29). It should be clear that I have no intention of denying the formal correctness of a statement alleging that, in the periods between changes in the supply of money by governmental edict, under a system in which the actual supply of "money" would be regulated by the arbitrary fiat of government, the supply has an "elasticity of zero." The contention here is merely that the concept of "elasticity of supply" becomes genuinely useful only when the coefficients for this elasticity have values different from zero; and that it is precisely in these cases that the application of the *Marshallian* "elasticity of supply" to the supply of money, when that supply is regulated by government fiat, becomes almost entirely devoid of meaning. For in such cases all that would be established by the use of coefficients of elasticity of supply, in the Marshallian sense, would be a series of empirical statements with respect to the arithmetic relation of a given change in the value of money to a given change in the supply of money. An *economic* connection between the two instances of variation would be almost totally lacking; and where such a connection might be established—as in a case in which the agencies of government would de- liberately vary the supply of money in accordance with a given change in its value per unit—the connection would bear no genuine resemblance to the responses of supply to changes in value per unit in the case, say, of the elasticity of supply of the money metals "when those metals are freely produced under conditions involving computation of profit and loss, and, being subject to free coinage, are added without limitation to the stock of money of ultimate redemption" (see above, p. 33). For, in the latter case, the responses would be, not the result of a deliberate action by governments for reasons quite dissociated from the natural conditions of supply of the money metal and the unimpeded working of the arts demand for that metal, but the result of precisely the type of calculations of profit and loss in production, and of "utility" as a factor affecting demand, which are found in the case of those "ordinary" commodities for the ex- planation of whose variations in value the concepts of the "general" Theory of Value were originally developed.

a recognizable resemblance to the type of economic calculation involved in the supply of the money *commodity* (in the case of "commodity money"), nor are they capable of really satisfactory statement in terms of a functional relation between the supply of "money" and its "value," of a kind that would provide a proper analogy to the application of the concept of "elasticity of supply" to the supply of the money commodity in the case of "commodity money." [32]   On the contrary, the probability is that such a usage would tend only to obscure, rather than to illumine, the true nature of the forces affecting the quantity of "bank money."   And indeed the general neglect of the latter problem which is characteristic of so much of recent discussion (including that of Keynes's *General Theory*) itself provides a commentary upon the relative heuristic value of the re-

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[32] On the rôle of "human volition" in the determination of "the supply of money," and its relation to certain uses of the concept of an "elasticity of supply of money," see the comments of Mr. Hawtrey in *Economica* for February, 1938, p. 97. Again it should be observed that I have no intention of denying the formal possibility of establishing empirically a relation between changes in the value of the monetary unit and changes in the quantity of bank money. Nor should I argue, as Mr. Hawtrey has argued elsewhere, that the chief reason for rejecting the application, by analogy, of the categories of the "general" Theory of Value ("the law of supply and demand") as developed in connection with the value of "ordinary commodities," to the relation between the supply of bank money and the value of the monetary unit, is that the analogy "completely fails" in all those cases which "in exactly the same degree in which an enlargement of supply [of bank money] lowers price [that is, the value of a unit of bank money], a fall of price stimulates the supply" of bank money (*Currency and Credit*, p. 170 of the first edition; cf. also E. M. Bernstein, *Money and the Economic System* [1935], 194, where it is suggested that this fact means that "the supply schedule for money thus seems to be precisely the reverse of a true supply schedule." After all, in a world in which entrepreneurial action is greatly affected by "expectations" in the face of an uncertain future, such developments are by no means unknown in the case of "ordinary" commodities. The chief reason for objecting to the use of the concept of "elasticity of supply" in the *explanation* of the relation between changes in the "supply" of bank money and its value per unit is that the type of apparatus which must be used for the latter problem *bears virtually no relation to that suggested by the concept of "elasticity of supply" as applied to "ordinary" commodities.* To be convinced of this fact, one has only to contrast the nature of the elements with which "human volition" is concerned in the latter case, with the elements of "human volition," as well as the institutional and conjunctural factors affecting this "volition," which are involved in the problem of the nature of the forces affecting the quantity of bank money, as described in Volume I, Chapters Seven to Nine, of the present work.

sults obtained by two types of approach: on the one hand, a mechanical extension of the "homely but intelligible concept" of an "elasticity of supply" to the "supply of money"; and on the other, that adopted by writers, from Ricardo to Wicksell and Hawtrey, who have either been charged with a failure to "integrate" their monetary theory with the "general" Theory of Value, or who have frankly rejected as inadequate or misleading much that has been done in the name of such "integration." [33]

II

THE ELASTICITY OF DEMAND FOR MONEY

The formal application of the concept of "elasticity of demand" to the "demand for *money*" is not the application of the "homely but intelligible concept" of "elasticity of demand" to the Theory of Money and Prices which appears in Keynes's *General Theory*.[34]   The applications, real or supposed, of the corresponding concepts of the "general" Theory of Value which do appear in the latter work are represented by the Keynesian concepts of (1) an "elasticity of substitution of money"; and (2) an "elasticity of effective demand," in the special sense in which Mr. Keynes defines the latter term.   The first of these applications is discussed in Section III of the present chapter, and the

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[33] On the treatment of the problem of the nature of the forces determining the "quantity of money" (including the quantity of bank money) in the *General Theory,* and in the writings of its supporters, see what is said above, p. 582. Cf. also what is said above, p. 98, n. 21, concerning Professor Hicks's analysis, particularly when the latter is judged in the light of Professor Hicks's own criticisms of the relevant part of the analysis of Wicksell. I am of course not contending that the neglect, in recent discussion, of the problem of the nature of the forces affecting the quantity of bank money is a direct *result* of Mr. Keynes's "mechanical extension of the 'homely but intelligible concept' of an 'elasticity of supply' to the 'supply of money.'" On the contrary, I am contending only that the latter is a *symptom* of what I believe to be one of the major causes of neglect of the problem indicated: namely, an unwillingness to use the type of apparatus best fitted for the solution of the problem, solely on the ground that this type of apparatus does not necessarily represent a direct application to the problems of monetary theory of analytical devices developed originally within the "general" Theory of Value.

[34] On certain usages in the *General Theory* and in Mr. Keynes's later writings which might seem to imply the contrary, see below, p. 676, n. 1.

second is discussed in Chapter Thirteen, immediately follow-
ing.   In both cases, it will be argued that the applications
in question have resulted in virtually no substantive gains for
the Theory of Money and Prices.

The purpose of the present section is to demonstrate that
the disappointing nature of these results might have been
forecast upon the basis, and avoided as a consequence, of a
critical examination of the results (or lack of results) ob-
tained by the attempt to apply the concept of an "elasticity
of demand" to the *"demand for money."*   It will be argued,
in other words, that an adequate appreciation of the lessons
of doctrinal history, including the doctrinal history of very
recent times, would have made unnecessary the disappoint-
ment to which, it is here argued, Mr. Keynes's "elasticity of
effective demand" must be said to have led.   It will be
argued also that the necessity for emphasizing the reasons
for this disappointment is demonstrated by the fact that the
same *kind* of reasoning applies in the case of Mr. Keynes's
second attempt of the type indicated: namely, his intro-
duction of the concept of an "elasticity of substitution of
money."   And, by way of reënforcing the "lessons" thus
provided, I propose to state the relevant conclusions in the
form of a schematic series of propositions all of which will
be related to the Lessons of Doctrinal History adduced in
Chapter Three of the present volume.

1. *The Facts of Doctrinal History.*   If one were to take
literally Mr. Keynes's statement that when "economists"
pass from the "the Theory of Value" to the "Theory of
Money and Prices," "we hear no more" of "homely but in-
telligible concepts" such as "elasticity of demand," one
would suppose that the application of the concept of "elas-
ticity" to the *demand for money* was at best something of a
novelty.[35]   And the same conclusion was suggested by

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[35] Such a supposition would of course be contradicted even by an exam-
ination of Mr. Keynes's own writings prior to the *General Theory*. See
below, p. 647, and the references to Keynes's *Monetary Reform* given in
n. 41 thereto.   Unhappily, however, this is not the only instance in which
Mr. Keynes's later statements with respect to the substance of "traditional"
theory have gone untested in the light of his own earlier statements on the
same subject.   On this matter, see, for example, what is said above, p. 6,
n. 9; p. 58, n. 17; and p. 76, n. 62.

those earlier discussions of the "elasticity of demand for
money" which implied that a "critical examination of the
view that the elasticity of demand for money is always
unity" is a phenomenon only of the "post-war" (that is,
post-1918) period.[36]   It can be shown, however, that the
application of the concept of "elasticity" to the demand for
money has had a very long history.[37]   It follows, therefore,
that we may apply directly to the special problem under
discussion the first of the "Lessons of Doctrinal History"
advanced in connection with the general problem of the
relation between monetary theory, on the one hand, and the
"general" Theory of Value, on the other: namely, that since
the virtue of novelty cannot be assigned to the mere *posing*
of the problem, it must be shown that each new posing of
the problem has led to specific substantive results which
leave the subject in a more advanced state than it was in
before the problem was posed anew.[38]

In one respect, the history of the application of the concept of "elas-
ticity of demand" to the *demand for money* is like that of the history
of the concept of "elasticity of demand" itself.   In the latter case, it
was found necessary to distinguish between the use of the *term* "elas-
ticity of demand," on the one hand—a use which dates from Marshall
—and, on the other hand, the use of the *concept* of "elasticity of de-

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[36] Cf., for example, the comments of T. E. Gregory, in the *Encyclopaedia
of the Social Sciences,* X, 609; also the same author's "Professor Cannan
and Contemporary Monetary Theory," *loc. cit.,* 41.
   [37] See the fine-print section immediately following in the text.   It will
be observed that, as in the case of the "elasticity of *supply* of money" (see
above, p. 628, n. 4), no reference is made here to loose usages with respect
to the "elasticity of demand for money," which refer only, in a very
general way, to the "limits" of the "demand for money," without specifically
claiming to have applied to the Theory of Money and Prices the "elasticity
of demand" of the "general" Theory of Value, or even attempting, in
most cases, to state the argument with respect to "elasticity" in terms of
the kind of relation with which the "elasticity of demand" of the "general"
Theory of Value is concerned: that is, the relation between the degree of
change in the "demand for money," on the one hand, and the degree of
change in the *"Value" of Money,* on the other.   For examples of such
loose (and less pretentious, and therefore much less objectionable) usage,
see Davenport, *Value and Distribution,* 240 f., and *Economics of Enterprise,*
269 ff., 316; Mises, *Theory of Money and Credit,* 148 (though see also be-
low, p. 648, n. 46); and A. G. Hart, "Failure and Fulfillment of Expectations
in Business Fluctuations," *loc. cit.,* 74.
   [38] See above, p. 124.

mand," which is much older.[39] The same thing applies in the case under discussion.

To be sure, it is something of a commentary upon Mr. Keynes's statement, quoted above, with respect to the alleged practice of "economists" when they pass from discussions of the "general" Theory of Value to the Theory of Money and Prices, that Marshall, the inventor of the term "elasticity of demand," himself used that term in discussing the "demand for money." [40]   A further commentary upon Mr. Keynes's statement is provided, moreover, by the fact that the term "elasticity" was used in discussion of the "demand for money," in the years preceding the publication of the General Theory, not only by avowed followers of Marshall, such as Professor Pigou and (curiously enough!) the Keynes of Monetary Reform, but also by non-Marshallians such as Professors Cannan, Lehfeldt, and others.[41] And when it is added that this usage had penetrated into the textbooks on our subject, one can only marvel again at the ease with which we allow to go unchallenged irresponsible statements as to what the practice of "economists" has been in the past.[42]

In fact, however, the use of what amounts to the concept of "elasticity of demand" in discussions of the "demand for money" antedates by many years the use of the term "elasticity of demand" in this context. This can easily be shown by considering instances in which use was made, in discussions of the "demand for money," of constructions involving what amounts to the concept of a unitary elasticity of demand.

Mr. Keynes himself, for example, in one of his earlier incarnations, ventured to characterize John Stuart Mill's discussion of the question whether the value of money is affected "in proportion to the deficiency" in the quantity of money, or in greater or less "proportion," as amounting to a discussion "(in effect)" of the supposedly "unitary elasticity of the demand for money." [43]   But if this is an example of an application, "in effect," of the concept of "elasticity" to the demand for money, such an application was made even earlier by Henry Thornton, who, as we have seen, was himself a figure of importance in the development of the concept of "elasticity of demand" in its general application

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[39] See above, pp. 146 ff.

[40] See Marshall, Money, Credit, and Commerce, 283; and cf. also p. 152 of the same work, where, although the phrase "elasticity of the demand" is applied to "gold and silver," rather than to gold and silver in their monetary uses, the context makes it clear that the "elasticity of demand" for gold and silver in the monetary uses was regarded as being involved in the problem.

[41] See Pigou, Essays in Applied Economics, 191, 196 n.; Keynes, Monetary Reform, 53 f., and the references to Cannan and Lehfeldt there given; K. Maier, Goldwanderungen, 73 ff.

[42] For an example of textbook treatment of the "demand for money" in terms of the "elasticity" of this demand, see Edie, Money, Bank-Credit, and Prices, especially p. 208 n., and the references to Cannan, Lehfeldt, and Keynes's Monetary Reform given on p. 211 n., of Edie's book.

[43] See the Memorials of Alfred Marshall, 45, n. 4.

to commodities other than money.[44]   And one does not have to search
very diligently in the literature of the period between Mill and Marshall
to find other examples which would have claims, equal to those accorded
by Mr. Keynes to Mill, to be regarded as treatments, "in effect," of
"the unitary elasticity of the demand for money," by virtue of the
discussion, by the authors concerned, of the extent to which "variations
in the value of currency" in relation to variations in its "quantity"
would or would not be expected to bear a "ratio to . . . quantity"
different from that to be expected in the case of commodities other than
money.[45]

Precisely the same thing may be said of other constructions which
have been held to amount, "in effect," to a use of the concept of a
"unitary elasticity of the demand for money." [46]   If it be remembered,
for example, that Marshall proposed the name "Constant Outlay curve"
to describe the properties of a curve representing unitary elasticity,
it follows that one could regard, as an example of the use of the concept
of a "unitary elasticity of the demand for money," any instance in
which it was argued that "the value of the entire circulating medium
always remains unaltered"; and indeed it is possible to find statements
of this type at least as early as the first part of the nineteenth century.[47]
It will be remembered also that Marshall himself, in his discussion of
the "elasticity of demand for money," pointed out that his "Constant
Outlay curve" would take the form of a rectangular hyperbola.[48]   It is
proper to point out, therefore, that the use of a rectangular hyperbola
to represent the "demand for money" is to be found also in Walras and
Wicksell, as well as in later writers.[49]   It is thus clear that no genuine

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[44] See Thornton's *Inquiry into the Nature and Effects of the Paper Credit
of Great Britain,* p. 227 of the Philadelphia edition of 1807 (p. 243 of the
reprint of 1939, as edited by Hayek).   It is worth noting that it was pre-
cisely in this connection that Thornton quoted Davenant's statement of the
so-called "Law of Gregory King" (see above, p. 147, n. 15).

[45] See, for example, Cliffe Leslie, *Essays in Political Economy,* 70.

[46] Cf., for example, the discussion by Maier, *Goldwanderungen,* 78, of
rectangular hyperbolas in relation to the concept of an "elasticity of de-
mand for money."   The "demand for money," in relation to "mechanical"
forms of the "Quantity Theory," is also discussed briefly in terms of the
type of "demand curve" involved (though without explicit use of the terms
"elasticity of demand" or "rectangular hyperbola") by Mises, *Theory of
Money and Credit,* 145.

[47] See, for example, H. Vethake, *The Principles of Political Economy*
(1838), 150.   It is to be remembered that Vethake himself regarded his
discussion of the "demand for money" as an application, to the special
case of the Value of Money, of the concepts of the "general" Theory of
Value—or, as he put it, of "the established principles concerning the ex-
changeable values of commodities in general" (see above, p. 41, n. 113).

[48] See *Money, Credit, and Commerce,* 283, 284 n.

[49] On Walras, see my "Léon Walras and the Cash Balance Approach,"
loc. cit., 578 f., and the references to Walras given in n. 24 thereto.   For
Wicksell's use of a rectangular hyperbola in this connection, see his *Lectures,*

novelty attaches to the application, in our own day, of the concept of "elasticity of demand" to the "demand for money"; and it is equally clear that this very fact justifies the application to this case of the test suggested by our first "Lesson of Doctrinal History": namely, that of being able to demonstrate that each "new" application of this type has led to specific substantive results which leave the subject in a more advanced state than it was in before the problem was posed anew.

2. *The Lessons of Doctrinal History Further Applied.* When, however, this test *is* applied to the case of the application of the concept of "elasticity of demand" to the "demand for money," it is found that this case provides only another example of the proposition advanced in Chapter Three of this volume as the second "lesson of doctrinal history"—a lesson which, applied to the present instance, would assert categorically that the results obtained by the application of the concept of "elasticity" to the demand for money has in no case been superior to results obtained from the use of formulations in which no attempt was made to apply the concept of "elasticity" to the "demand for money," and in many cases have actually been inferior to these results. And the reasons for this outcome are precisely those indicated in several of the later "lessons" presented in Chapter Three. Specifically, it can be shown (i) that in a number of cases the application of the concept of "elasticity" to the "demand for money" has resulted only in the posing of problems that are purely factitious, in the sense that even the "solution" of these problems would throw very little light on the issues of substance involved (Lesson Four); (ii) that in fact this particular application has often led to an actual *obscuring* of the nature of these issues of substance, whereas the mode of stating the problem which does allow the substantive issues to appear most clearly is precisely that which has been rejected by some of those who have made much of the application of the concept of "elasticity" to the demand for money (Lesson Five); (iii) that at best the application of the concept of "elasticity" to the "demand for money" has amounted only to a statement in

II, 141 ff. Cf. also Pigou, *Essays in Applied Economics,* 177; J. Marschak, "Die Verkehrsgleichung," *Archiv für Sozialwissenschaft und Sozialpolitik,* LII (1924), 359; and the reference to K. Maier given above, p. 648, n. 46.

unfamiliar terms of substantive results already made perfectly familiar by those formulations of the Theory of Money and Prices which made no use of the concept of "elasticity of demand" in the context indicated (Lesson Six); (iv) that the mere introduction of the concept of "elasticity," in this context, instead of providing a substantive solution of the problems that really matter, has meant only a restatement in other terms of the real problem to be solved (Lesson Seven); and, finally, (v) that in the few instances in which substantive results were *associated with* the type of application in question, exactly the same results were obtained, and obtained more directly, by writers who made no attempt to apply the concept of "elasticity of demand" to the demand for money (Lesson Nine).

These conclusions may be established *seriatim*, under the heading of the respective "lessons" indicated.

i. *Lesson Four: Factitious Issues* **versus** *Issues of Substance.* In order to illustrate this type of "lesson" in the present instance, it is sufficient to call attention to the supposed difference between opinions expressed by writers as eminent as Professor Pigou, on the one hand, and Professor Cannan, on the other, both of whom have sponsored the application of the concept of "elasticity of demand" to the demand for money. Professor Cannan, as is well known, made much of the contention that the demand for currency need not be expected always to have an elasticity equal to unity.[50] Professor Pigou, on the other hand, has stated categorically that "the demand for money always has an elasticity equal to unity."[51] On the surface, this would seem to represent a fundamental difference of opinion as to certain facts of economic life. It can be shown, however, that it represents nothing of the kind.

That this is so should become clear as soon as it is recognized that all that Professor Cannan, for example, meant to say when he insisted that the demand for currency need not be expected always to have an

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[50] Cannan, "The Application of the Theoretical Apparatus of Supply and Demand to Units of Currency," *loc. cit.*, 458, 460. Cf. also the references to T. E. Gregory given above, p. 646, n. 36.

[51] Pigou, *Essays in Applied Economics*, 191; cf. also *ibid.*, 196 n., where Pigou objected to "Professor Cannan's inference that the elasticity of demand for money is less than unity." It is worth noting that neither passage occurred in the original version of Pigou's essay ("The Value of Money") in the *Quarterly Journal of Economics* for 1917. The reader may himself decide as to the degree to which the discussion of the substantive issues involved was furthered by the addition of the two passages indicated—an addition presumably occasioned by the publication of Cannan's article in 1921. Cf. also what is said on this matter below, p. 658, n. 72.

elasticity equal to unity is that price changes need not be expected in all cases to be strictly proportional to changes in the quantity of currency; and that the reason for this is that the expectation of further depreciation of the monetary unit which is characteristic of certain stages of the inflationary process may lead individuals to hold ("demand") smaller amounts of cash, relative to outlay.[52]   Yet if anything is certain, it is that Professor Pigou would not for a moment be prepared to argue either for strict proportionality as between changes in the quantity of money and its purchasing power, or for constancy in the relation between the size of cash holdings and the level of outlay (or "income").[53]   Where, then, was the difference?   Simply in the fact that, whereas Professor Cannan preferred to speak in terms of an

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[52] See, for example, "The Application, etc.," *loc. cit.*, 460: "When it is seen that the value of currency is steadily falling, people see that it is more profitable to hold goods than currency, the demand for currency fails to extend in proportion to the enlargement of the supply, and its value consequently falls more rapidly."

[53] That Professor Pigou was not prepared to argue for a strict proportionality as between changes in the quantity of money and its value per unit is evidenced by the very fact that so much of his paper was concerned precisely with the forces which may be expected to cause changes in the magnitude of the variables *other than* the M of his Quantity Equation. From this Quantity Equation itself, as Professor Pigou pointed out, it follows that changes in P could be expected to be strictly proportional to changes in M (or, as Pigou put it, "the demand schedule . . . represented by the equation $P = (kR)/M$" would take the form of "a rectangular hyperbola") *only* "when k and R are *taken as constant*" ("The Value of Money," *loc. cit.*, 42; *Essays in Applied Economics*, 177; italics mine).   For Pigou's discussion of the forces which would be expected to cause *changes* in R and k (and also in the variables included in the expanded form of his Quantity Equation), see "The Value of Money," 43 ff. (*Essays in Applied Economics*, 180 ff.); and for specific statements to the effect that under certain conditions "prices will tend to rise less than in proportion to the expansion of currency," and under other conditions "prices will tend to rise *more* than in proportion," see *Essays in Applied Economics*, 196.   The essential identity of Professor Pigou's substantive analysis with that of Professor Cannan is attested further by two aspects of the former's argument as to *why* prices may be expected to rise *more* or *less* than proportionally to changes in the quantity of money: namely, (1) that Professor Pigou himself treated his k as a variable rather than a constant (see, for example, the reference, on p. 196 of Pigou's *Essays in Applied Economics*, to "the variable k in the demand formula"); and (2) that he regarded, as an "important circumstance" affecting the magnitude of k, individuals' "expectations" with respect to the course of "general prices" and the "suspicion that a nation will fail to maintain or to restore the full convertibility of a paper currency" (*Essays in Applied Economics*, 183 f.; cf. also p. 196).   Marshall's position was of course in all essentials identical with that of Professor Pigou, despite the fact that, like the latter, he insisted on speaking of the "elasticity of demand for money" as equal to unity, and on representing this demand by a rectangular hyperbola.   See, for example, Marshall's *Money, Credit, and Commerce*, 47 f.

elasticity of demand different from unity, Professor Pigou preferred to speak in terms of a demand curve with an elasticity always equal to unity *shifting* upward or downward in response to a change in the supply curve.[54]

Now, no one would deny that, in many cases, the distinction between a shift in the demand curve and a movement along the curve is of considerable importance. But it is certainly possible to deny that this distinction is of any importance whatever in the present instance; nor should there be any doubt as to the reason why it is entirely without importance. This reason is simply that there was no difference between the disputants as to what may actually happen, in terms either of simple description of events in the real world, or—as we shall see below —in terms of a translation of this description into effects upon the variables of the familiar Quantity Equations.[55] Given this amount of agreement, it is difficult to see anything in the dispute beyond a further confirmation of the proposition embodied in the particular "lesson" here under discussion: namely, that a concern with the formalization of problems within monetary theory in terms of the concepts suggested by the general Theory of Value ceases to be fruitful when it gives rise to controversy on issues that are important (if they are important at all) not for an understanding of economic processes in the world we know, but solely for issues raised by the act of formalization itself.

ii. *Lesson Five: The Obscuring of Issues of Substance.* In a fundamental sense, the very substitution, for issues of substance, of factitious issues of the kind just discussed under (i), may be said to represent an "obscuring" of issues of substance. It can be shown, however, that the application of the concept of "elasticity" to the "demand for money" has obscured the really important issues even in cases in which factitious issues of the kind just discussed were not introduced; and it can be shown further that this obscuring of the issues would *not* have occurred if explicit use had been made of even simple Quantity Equations of the general Fisherine form, which have been rejected as unilluminating by sponsors of the application of the concept of "elasticity" to the "demand for money." [56]

That this is so will become clear as soon as one asks what, precisely, we are told when we are told that the elasticity of demand for money has a value of unity, greater than unity, or less than unity. All that we are

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[54] See Pigou, *Essays,* 194 ff., especially p. 196.

[55] See below, pp. 654 ff., 658 f., 661 f.

[56] It will be recalled that Professor Cannan's rejection of the type of formulation represented by a Fisherine equation (and particularly of the concept indicated by the term for "velocity" in such a formulation), on the ground that there is no analogue, within the "general" Theory of Value, to the concept of Fisherine velocity, antedated by several years Mr. Keynes's rejection of such formulations, with their "velocity of circulation . . . *et hoc genus omne,*" on just such grounds. See the reference to Cannan given in Volume I, 294, n. 12, and cf. also Cannan's *Money,* p. 73 of the fifth (1926) edition.

thus told is that sometimes the changes in prices associated with changes in the quantity of money will be strictly proportional to the changes in the latter (the case of unitary elasticity of demand), and that sometimes the changes in prices will be more or less than proportional to the changes in the quantity of money (the cases of elasticity of demand greater or less than unity).[57] Can it be seriously argued that such a statement of the issues involved in any attempt to establish the relation of changes in the quantity of money to changes in the scale of prices would be regarded as illuminating by students of monetary theory of any reasonable degree of maturity? For such students, surely, the issues of real importance are introduced only when we go *beyond* the statement that changes in the scale of prices may be exactly proportional, or more or less than proportional to changes in prices, and ask *why* we get the precise relation that we do get in concrete cases between changes in the scale of prices, on the one hand, and changes in the quantity of money, on the other.[58]

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[57] It may be objected that statements of the type indicated tell us more than this; specifically, that they tell us (1) that the *reasons* for the degree of proportionality of price change consequent upon a given change in the quantity of money are to be found in the facts with respect to the *demand* for money; and (2) that the "demand for money" is to be conceived of as a demand for *holding* purposes. But it may be replied, in the first place, that, for anyone who is prepared to put the problem of the determination of the "value of money" in terms of "supply" and "demand" altogether, the first of these statements amounts to no more profound a proposition than that if the same change in supply results in different degrees of price change under different circumstances, these differences in the degree of price change must be due to changes on the side of *demand*. On this matter, see what is said in n. 58, immediately following. It may be replied, in the second place, that the proposition indicated under (2) is one that is conveyed, not by a statement with respect to the degree of *elasticity* of the "demand for money," but by a separate proposition with respect to the *nature* of this demand. See below, pp. 662 f., under (v).

[58] From what follows, it should be clear that the answer which is desired is one which would go far beyond the mere statement that the explanation is to be found on the side of the *demand* for money. See particularly the reference given below, p. 655, n. 61, to Professor Pigou's desideration of an *"anatomy"* of demand." The argument which follows is obviously relevant also to the possible suggestion that the answer which is desired is given by the statement that the explanation is to be found in the concept of "liquidity preference." For what is required is an "anatomy" of "liquidity preference" as well; and the argument in the text is precisely that it is the familiar Quantity Equations, with their supposedly "vaguer" concepts of "velocity of circulation, . . . the volume of transactions, . . . *et hoc genus omne*," which provide just such an "anatomy." The same thing must be said with respect to the possible suggestion that the answer is given by the facts with respect to "hoarding": for it is precisely the familiar Quantity Equations which provide the kind of "anatomy" of hoarding that would have prevented the extraordinary statements with respect to "hoarding" that have been made by Mr. Keynes and his followers in the face of Mr.

It is when this second question is asked that we realize how much light is thrown upon precisely the problem indicated by concepts of the type specified by the variables of the familiar Quantity Equations, *and the body of detailed analysis which these concepts are designed to summarize:* by precisely those concepts, in other words, which Mr. Keynes himself has regarded as much "vaguer" than the concepts suggested by an application of the concept of "elasticity of demand" to problems of monetary theory other than that of the explanation of differential price change during periods of monetary expansion and contraction.[59] Nor would it be a valid objection to this conclusion to argue that when the words italicized in the preceding sentence are applied also to the concept of an "elasticity of demand for money," the latter concept may be regarded as being as helpful a heuristic device as the concepts summarized by the "vaguer phrases" which Mr. Keynes and others dislike so heartily. For the point made here is precisely that these "vaguer phrases" are themselves part of the "detailed analysis" which must be held to "lie behind" the concept of an *elasticity of demand for*

Keynes's own inclusion of "hoarding" among the "vaguer" concepts of the Theory of Money and Prices *along with* "the velocity of circulation, . . . the volume of transactions, . . . *et hoc genus omne*" (*General Theory*, 292). An adequate distinction, for example, between the components of the "absolute" demand for cash balances and the "relative" demand for cash balances (see the references to Volume I given above, p. 102, n. 27)—which in turn involves a use of the variables of the familiar Quantity Equations —would have prevented Mr. Keynes's assertion that "it is impossible for the actual amount of hoarding to change as a result of decisions on the part of the public, so long as we mean by 'hoarding' the actual holding of cash," on the ground that "the amount of hoarding must be equal to the quantity of money . . . and the quantity of money is not determined by the public" (*General Theory*, 174). For such a distinction would have revealed the fact that "hoarding" may take the form of reducing the rate of spending from a given (absolute) amount of cash balances: in other words, it may take the form of a decline in Fisherine V. It will be observed that this would be revealed even if recourse is *not* had to what Mr. Keynes calls "the vital difference between the theory of the economic behavior of the aggregate and the theory of the behavior of the individual unit" (*General Theory*, 85). For the equation $V = 1/K$ is one that holds in the case of both the "economic behavior of the aggregate" and the "behavior of the individual unit"; and the distinction between the T (or PT) elements in the "demand for money" and the V (or $1/K$) element is one that applies to the "anatomy" of individual monetary demand as well as to the "anatomy" of "aggregate" monetary demand. See Volume I, 447, of the present work. The consequences, indeed, of a failure to provide an adequate "anatomy" of the demand for money, were to be seen in the statements of the *Treatise*, as well as of the *General Theory*, with respect to what is "determined by the public," and what is determined by the "decisions of the bankers" (see Volume I, 437 ff., of the present work).

[59] See again Keynes's *General Theory*, 292. Our contention, it must be remembered, is that, despite the fact that Mr. Keynes does not himself apply the concept of "elasticity of demand" to the *demand for money*, the *methodological issues* raised by the applications of the concept of "elasticity

money itself.[60] They provide, that is to say (to apply an expression of Professor Pigou) a "true *anatomy*" of this "elasticity of demand for money." [61] To that extent, they are less "vague" than the latter concept—just as the analysis lying behind each of the variables in the familiar Quantity Equations is less "vague" than the variables themselves. After all, what is claimed in this work for these Quantity Equations, in this context, even when they are regarded solely as *summarizing* devices, is that they point with more precision than do alternative summarizing devices to the nature of the forces which must be studied in any attempt to determine the consequences, for the scale and structure of money prices, of changes in the quantity of money.

The sole question here, therefore, is whether the concept of "elasticity of demand" as applied to the demand for money is as illuminating, as a summarizing device, as are the familiar Quantity Equations. The argument here is that it is not, for the simple reason that it is impossible to *explain* the degree of "elasticity" evidenced by the "demand for currency" *without having recourse at once to the variables included in the Quantity Equations*. And the proof of this contention is provided by no more profound a proposition than this: that a statement which establishes a relation between only two variables (namely, M and P) is less helpful than one which establishes a relation not only between these two variables, but also between them and other variables *whose magnitude helps to make the first relation what it actually is*.[62] It is

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of demand" to monetary theory which he does make are the same, particularly when these issues are viewed in the light of his charge that concepts such as "velocity of circulation, . . . the volume of transactions, . . . *et hoc genus omne*" are "*vaguer*," as analytical devices, than these applications. These methodological considerations are, of course, *not* such as to minimize the importance of the type of application of the concept of "elasticity of demand" to the Theory of Money and Prices which is represented by its application "to the explanation of differential price change during periods of monetary expansion and contraction." It must be remembered, however, that this is precisely the type of application that is rejected by Mr. Keynes. See above, pp. 154 ff.

   [60] Precisely the same proposition holds with respect to the relation of these "vaguer phrases" to Mr. Keynes's own concept of an "elasticity of effective demand." See below, pp. 712 ff., 721 f., 729 f.; and cf. n. 59, immediately preceding.

   [61] See Pigou's *Essays in Applied Economics*, 187 f.

   [62] Cf. the comment of Mr. Hawtrey in *Economica* for February, 1938 (p. 97), to the effect that "elasticity . . . is only applicable to a function of one variable"; and see also Pigou, *Essays in Applied Economics*, 194, on the relative difficulties involved in translating an argument involving more than two variables into "demand and supply curves," on the one hand, and into "*algebraic* formulae," on the other (italics mine). It should hardly be necessary to add that the very fact that it has been possible to establish a *modus vivendi* between the simpler "algebraic formulae" for our particular "demand and supply curves" and the more complicated "algebraic formulae" used in the statement of "*general* economic interdependence" shows that the use of the concept of an "elasticity of demand for money" is not neces-

this type of issue which is revealed by the familiar Quantity Equations, particularly when these equations are judged from the standpoint of the historical growth of our understanding of the nature of the forces which make the scale and structure of money prices what they are.[63] And it is precisely this type of issue which is obscured by statements, such as those with respect to possible variations in the "elasticity of demand" for money, which in themselves tell us no more than that the scale of money prices may, under different circumstances, evidence different degrees of response to changes in the quantity of money.

A further example of the relative fitness of formulations in terms of the "elasticity of demand for money," on the one hand, and in terms of the familiar Quantity Equations, on the other, for revealing, rather than obscuring, the nature of the issues of genuine substance in monetary theory, was touched upon in Volume I of this work, in the course of our examination of the differences alleged to exist between the "real balance" and the "money balance" variants of the cash-balance approach. Specifically, attention was there called to the suggestion that the "money balance" variant leads necessarily to the conclusion that the demand for money always has an elasticity equal to unity, whereas the "real balance" variant was supposed to permit the assumption of elasticities different from unity.[64] It was argued, however, first, that this generalization was not supported by the instances represented, on the one hand, by the "real balance" variants of Walras and

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sarily *inconsistent with* a use of the familiar Quantity Equations. The practice, indeed, of Professor Pigou is itself not only a proof to the contrary, but is also an example of the honoring of our eighth Lesson of Doctrinal History, with respect to the dangers of an unreasonable *exclusivism* in the treatment of "rival" analytical devices in monetary theory. The points made here are merely (1) that it is just such exclusivism which *is* represented by the argument of those who have insisted, or implied, that the use of the concept of an "elasticity of demand for money," or analogous concepts, makes unnecessary the use of supposedly "vaguer" concepts such as "velocity of circulation, . . . the volume of transactions, . . . *et hoc genus omne*"; (2) that the effect of the acceptance of such exclusivism, in this particular instance, would be to throw monetary theory back to a stage which is actually *retrograde* in comparison with the stage represented by even the simpler forms of modern "Quantity Equations"; (3) that the reason for this retrograde character is that a geometric statement of the relation between changes in value and changes in a *single* variable (in this case, the quantity of money), even if formally correct, is less illuminating than an algebraic statement of the relation between changes in value and changes in *several* variables (of which the quantity of money is only *one*); and (4) that a statement of the latter type, such as is represented by the Quantity Equations, is not subject to the criticism advanced (often with justice) against statements of the relation between changes in value and changes in *several* variables: namely, that they merely state that changes in value are a function of *several* variables, *without specifying concretely what these variables are*. See also what is said below, pp. 659 ff., under (iv), and also p. 672, n. 107.

[63] See Volume I, 90, 95 ff., of the present work.

[64] See Volume I, 457 f.

Marshall (both of whom had, in their graphic representations of the "demand for money," made use of a rectangular hyperbola), and, on the other hand, by the "money balance" variant of Cannan, who had stressed the proposition that the demand for money could not be expected to be always equal to unity.[65] It was argued, secondly, that, as a matter of strict logic, there is no reason whatever for concluding that the "real balance" and the "money balance" variants ought to lead to different results, if each is correctly stated.[66]

The only point that need be emphasized here, therefore, is that this would have been seen immediately if, instead of being concerned with matters of an essentially formal nature, the whole problem had been put in terms suggested by the simplest of Quantity Equations. A truly satisfactory version of the "real balance" variant, it was argued, would be of the general form $M/P = kT$; but this, it was also argued, is the exact mathematical equivalent of a truly satisfactory version of the "money balance" variant, of the form $M = k(PT)$.[67] If formulations of this type had been used, the alleged differences with respect to the assumption of a unitary elasticity of demand would have disappeared. Indeed, it is difficult to believe that the question would ever have been posed in terms of an elasticity of demand for money altogether. For, as is pointed out below under (iii), the formula for unitary elasticity (that is, for a curve of the form of a rectangular hyperbola) would be $M = P(kT)$, in which (kT) is a constant.[68] And it is difficult to believe that if the whole discussion had been carried on in terms of simple Quantity Equations, without reference to the concept of an elasticity of demand for money, it could ever have been seriously argued that the "monetary" variant of the cash-balance approach involves the assumption of a constancy in the (kT) of this expression, whereas the "real balance" variant does not. Again, therefore, it may be argued (1) that the chief effect, in this case, of the introduction of the concept of an "elasticity of demand for money" has been to obscure issues whose nature would have been revealed at once if the supposedly "vaguer" Quantity Equations had been used throughout; and (2) that this was true even with respect to those substantive issues involved in a choice between "real" and "monetary" variants of the cash-balance approach that still merit discussion.[69]

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[65] See Volume I, 457, and the references to Cannan, Walras, and Marshall given in nn. 117 and 118 thereto.

[66] See Volume I, 457 f.

[67] See especially Volume I, 449, n. 96, and 455, n. 112. (To the references given, in the latter note, to the use, by Robertson and Ellis, of the equivalent of a "Marshallian K" which is expressed as a fraction of turnover, rather than of income, should be added also a reference to the suggestion, by K. Maier, Goldwanderungen, 80, that use be made of "real" "turnover units" in place of—or in addition to—"real" "consumption units.")

[68] See below, pp. 658 f.

[69] An issue of this kind which still "merits discussion" is, for example, that of the relative "realism" of the two types of variant. See what is said on this matter in Volume I, 436, n. 65, 446, n. 88, and 457 f.

iii. *Lesson Six: The Disguising of Ancient Platitudes.* As we have seen, one of the chief results claimed on behalf of the application of the concept of "elasticity" to the "demand for money" is that this application has revealed a truth which is supposed to have been undiscovered by "pre-war" writers on money: namely, that there is no reason to suppose that the elasticity of demand for money is always equal to unity.[70] It is extremely easy to demonstrate, however, that this finding is nothing more than a bedizened restatement of what would have revealed itself as a hoary platitude to anyone not blinded by the appearance of sophistication conferred by discussion of the problem of the relation between money and prices in terms of the elasticity of demand for money.

That this is so will become clear if we consider the equation of the curve representative of unitary elasticity—namely, the rectangular hyperbola. It is of the form $xy = (k)$, in which (k) is a constant. In the graphic representation of the "demand for money" presented by Wicksell, Marshall, and others, the quantity of money was measured along the abscissa, and its value per unit was measured along the ordinate.[71] We may therefore substitute M for x and $1/P$ (representing the value of the monetary unit, or the inverse of the price level P) for y. We then obtain $M(1/P) = (k)$, or $M = P(k)$. The contention is that, since the elasticity of demand for money is not necessarily equal to unity, we are not dealing with a rectangular hyperbola. In other words, (k) cannot be expected to be constant.[72]

In what sense can this be regarded as a significant discovery? If (k) represents an arbitrary constant, the equation $M = P(k)$, when dissociated from the connotations suggested by the concept of a unitary elasticity of demand for money, is nothing more nor less than an algebraic

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[70] See again the references given above, p. 646, n. 36, and p. 650, n. 50.

[71] See the references given above, p. 648, nn. 48 and 49.

[72] It is a striking commentary upon the statement of the problem in terms of an "elasticity of demand for money" that Professor Pigou himself, despite his insistence that this elasticity is "*always* equal to unity" (see above, p. 650, n. 51), was (1) careful to point out that the equation of the curve representing the demand for money would be "a rectangular hyperbola" *only* if his "k and R are taken as constants" (*Essays in Applied Economics,* 177); and was (2) quite explicit in recognizing that k and R, instead of being assumed to remain "constant" in the world we know, were to be treated as true "variables" (*ibid.,* 180 ff.). The explanation, of course, is again that Professor Pigou preferred to represent changes in the "variables" k and R as leading to *shifts* in a curve held to be of unchanging (unitary) elasticity. See above, p. 652, and the references given in n. 54 thereto. Obviously, the very fact that Professor Pigou could insist upon the *variable* character of his k and his R, *despite* his insistence that the elasticity of demand for money is "*always* equal to unity," shows how little substantive gain was brought to the Theory of Money and Prices by an insistence that the "elasticity of demand for money" may *not* be equal to unity, on the ground that account must be taken also of movements in variables *other* than M.

representation of the crudest of "quantity theories." [73]   In Volume I
of this work, on the other hand, we saw that progress in the development
of the Theory of Money and Prices was registered precisely in the
degree that in place of (k), an arbitrary constant without economic
meaning, there was substituted the series of magnitudes of genuine
economic significance that gave us the elaborate "quantity equations"
we have today.[74]   Can it be seriously argued that if the whole discus-
sion had been put, not in terms of what Mr. Keynes characterizes as
the "homely but intelligible concept" of elasticity of demand, but in
terms of what he refers to disparagingly as "vaguer phrases" such as
"the velocity of circulation relatively to the volume of transactions,"
it would ever have been regarded as a major achievement to have
called attention to the fact that the "demand for money" cannot be
expected in all cases to have an elasticity of unity? [75]

iv. *Lesson Seven: The Restatement of the Problem to be Solved,*
versus its Solution. In earlier chapters of the present work, attention
has been called to much-vaunted instances involving "applications"

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[73] See Volume I, 23. This much was virtually admitted by Professor
Cannan when he suggested that the substantive question associated with
the assumption of unitary elasticity is whether "an increase in the supply
[of currency] should [be expected to] cause an exactly reciprocal diminu-
tion in the value of the currency" (cf. Cannan, "The Application, etc.," 459).

[74] See Volume I, 93 ff.

[75] Indeed, the exposition of some of the writers who have made much of
the contention that the demand for money need not evidence a unitary
elasticity in all cases, has been such as to make one wonder whether they
themselves realized just how narrow is the range of cases in which prices
could be expected to change in exact proportion to changes in the quantity
of money. Cannan, for example, believed that there is a *"prima facie*
reason for believing that the elasticity [of the demand for currency] at
bottom is unity"; and he implied that the elasticity of demand would cease
to be unity *only in the later stages of extreme paper-money inflation,* when
the issuers of currency would discover that the distrust of the people would
not permit an "indefinite" continuance of paper-money issue (Cannan,
"The Application, etc.," *loc. cit.,* 459 f.). Lehfeldt, at any rate, interpreted
Cannan's argument as amounting to the contention that it is only *"when a*
currency becomes extremely depreciated" that it becomes "no longer true
that the elasticity of demand is equal to unity" (Lehfeldt, "Statistics of
Extremely Depreciated Currencies," *Economic Journal,* XXXII [1922], 556;
italics mine). It is difficult to believe that either writer could have reached
this oversimplified conclusion if he had approached the question whether
"an increase in the supply should cause an exactly reciprocal diminution in
the value of the currency" (Cannan, "The Application, etc.," 459) with the
help of a simple Quantity Equation, instead of with the help of the con-
cept of an "elasticity of demand" for currency. Cf. also the statement of
J. Marschak (on whose use of a rectangular hyperbola in his discussion
of the relation between changes in the quantity of money and its value, see
above, p. 649, n. 49) that it is only "recently" that the assumption of a con-
stant velocity of circulation of money has been "discarded" ("Money and
the Theory of Assets," *loc. cit.,* 312).

of certain concepts of the "general" Theory of Value to the Theory of
Money and Prices, in which the only effect of such an application was
to encourage a belief that this application itself brought a solution for
substantive problems within the field of the Theory of Money and
Prices, whereas in fact the supposed "solution" amounted merely to
a restatement of the problem in other terms.[76]   A reconsideration of
the argument as developed thus far should be sufficient to convince
the reader that the application of the concept of "elasticity" to the
demand for money represents just such an instance. For we have
seen that the introduction of the concept of an elasticity of demand for
money, instead of aiding in a solution of the problem of *why* we get
the precise relation that we do get in concrete cases between changes
in the scale of prices, on the one hand, and changes in the quantity of
money, on the other, amounts merely to a statement of that problem
in other terms.

I wish here, however, to emphasize a further point: namely, that it
is easy to see why the use of the concept of "elasticity of demand" in
connection with the demand for money *should* have encouraged a
premature complacence with supposed "solutions" of problems of
monetary theory. The chief reason is that, in the case of commodities
other than money, the statement that the demand for a given com-
modity evidences a greater or less degree of "elasticity" than the
demand for another commodity is often as far as we can go by means
of *economic analysis alone*. For, as was pointed out in Volume I,
many of the factors bringing about differences in the conditions of
demand (and therefore in the elasticity of demand) for commodities
other than money, must be sought in the realm of physiology, psy-
chology, and of sociology in the broadest sense of the term.[77]   It is
hardly surprising, therefore, that in the case of commodities *other* than
money, most economists have preferred to stop short before the gates
of these other disciplines, rather than to carry on detailed explorations
within the gates themselves. They have contented themselves, in
some cases, with the establishment of the empirical facts (so far as such
facts can be established) with respect to the different degrees of elas-
ticity evidenced by the demand for specific commodities; in other cases,
they have contented themselves with venturing only a series of ex-
tremely broad generalizations with respect to the nature of the ele-
ments, many of them not amenable to study by the use of the weapons
of economic analysis, which help to account for these differing degrees
of elasticity. In all these instances, such a result does not, to be sure,
represent a "solution" in any real sense of the term; but it does mean
that the problem is solved as far as it can be solved by economic
analysis alone.[78]

[76] See above, p. 128, and the references given in n. 90 thereto.

[77] Cf. Volume I, 480 f.

[78] The difference in the area open to strictly economic investigation in
the case of the "elasticity of demand for *money*," on the one hand, and the
elasticity of demand for "ordinary commodities," on the other, is illustrated

It is precisely this type of result, however, which monetary theory, even in its present stage of development, has left far behind; and the proof that this is so is again provided by the vast body of analysis, parts of which were summarized, in Volume I of this work, as *"lying behind"* the terms M', V, and T of the familiar Quantity Equations.[79] It is perfectly true that some writers who have chosen to state the theory of the relation between money and prices in terms of the elasticity of demand for money, rather than in terms of the variables of the older Quantity Equations, have gone beyond the phrase "elasticity of demand" to the extent of giving some indication as to the nature of the forces affecting this elasticity.[80] Nor can it be denied that a

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by Pigou's treatment of the arts demand for the money metal, as contrasted with his treatment of the monetary demand. It is doubtful whether the economist, as such, could add much of an *analytical* nature to the considerations listed in the sentence in which Pigou describes the forces affecting the arts demand, even if, unlike Pigou in the essay under consideration, his major concern were with this arts demand. "The demand curve for the arts," writes Professor Pigou, "depends for its shape and position, like any ordinary demand curve, upon fashion, taste, the availability of substitutes, such as silver, to fulfil like artistic purposes, and so on" ("The Value of Money," *loc. cit.*, 56; *Essays in Applied Economics*, 190). Yet Professor Pigou had no difficulty in filling several crowded pages with what was obviously intended as a mere sketch of the factors affecting the demand for money. It should again be noted that this discussion of the demand for money was carried on under headings provided by the specific variables included in Pigou's own Quantity Equation.

[79] Again, therefore, we are dealing with an illustration of the eighth of our Lessons of Doctrinal History, with respect to the dangers of an unreasonable *exclusivism* in the treatment of "rival" analytical devices. See above, p. 655, n. 62.

[80] It is anything but clear, on the other hand, that the solutions of this problem presented by the various writers indicated can be said to have shown equal degrees of comprehensiveness and precision. On the contrary, it can be argued that the degree of comprehensiveness and precision characterizing the analysis of these writers has varied directly with their readiness to use a "quantity equation" as the *framework* for their discussion of the forces which make the "elasticity of demand for money" what it is. See, for example, what is said above, p. 655, n. 62, and above, n. 78, with respect to Professor Pigou's analysis, which was distinguished precisely by the fact that he made use of just such a framework; and contrast the comment made above, p. 652, n. 56, on Professor Cannan's statement of the problem, as well as the comment made above, p. 659, n. 75, on the details of Cannan's *analysis* of the problem. A partial explanation of the slight amount of attention devoted by.Cannan to the forces affecting the "elasticity of demand for money" *apart* from periods of extreme monetary depreciation, may lie in the fact that he seems to have regarded most of the forces which may be held to determine the relation of money to prices on the side of demand, as affecting not the degree of *elasticity* of demand, but the *position* of the demand curve. In this connection, see what was said by Cannan with respect to the difference between an "increase of demand" and an "extension of demand" in his "The Application, etc.," *loc. cit.*, 459. The reader must

really satisfactory description of the factors making the elasticity of demand for money what it is would give the same results as a really satisfactory account of the forces that give the variables of the familiar Quantity Equations the values they actually have.   What *can* be denied, however, is that the mere *statement* of the problem in terms of the elasticity of demand for money adds anything to what we already knew.   And what can be affirmed is (1) that a description of the factors *making* the elasticity of demand for currency *what it is* must traverse the paths indicated by the variables of the familiar Quantity Equations, even if this is not explicitly admitted by the users of the concept of an "elasticity of demand for money"; (2) that to the extent that these Quantity Equations indicate the paths to be followed in more detail and with more precision than does the statement of the problem in terms of an elasticity of demand for money, they represent a position closer to the ultimate goal of the Theory of Money and Prices than does the alternative statement in terms of elasticity of demand; and (3) that the very fact that it should have been implied that the latter statement of the problem is superior to the former provides evidence in support of our major proposition: namely, that the statement of the Theory of Money and Prices in terms of the elasticity of demand for money represents the type of formalism which at best may be regarded merely as adding a degree of elegance to the statement of results already familiar, and, in less favorable cases, has encouraged the belief that problems of the greatest importance had been actually solved, whereas in fact the introduction of the concept amounted merely to the statement of a problem still in need of solution.

v. *Lesson Nine: From Alchemy to Chemistry?*   From other incidents of doctrinal history, to be sure, we know that a conscious effort to apply to the problem of the Value of Money certain categories of the "general" Theory of Value has sometimes been associated with the attainment of substantive results that can stand on their own feet as contributions to our understanding of the nature of the forces determining money prices.[81]   As it happens, this may be said to have been so in the case of at least two of the sponsors of the application of the concept of "elasticity" to the "demand for money"—namely, Marshall and Cannan; for in both cases the writers named developed a treatment of the "demand for money," in the form of variants of the "cash-balance approach," whose usefulness is completely independent of the question of the usefulness of the *application of the concept of "elas-*

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judge whether this fact does not in itself provide a commentary upon the amount of light thrown on the problems covered by the Theory of Money and Prices by the application of the concept of "elasticity of demand" to the demand for money.   See again, moreover, what is said above, pp. 651 f., concerning the essential lack of substantive significance in the dispute between Pigou and Cannan as to whether what is involved, in certain cases, is a shift in the demand curve or an elasticity of demand different from unity.

[81] See above, p. 130, and the references in n. 94 thereto.

ticity" to the "demand for money." However, at least two aspects of the history of doctrine on the subject make it difficult to assign more heuristic value to this application than can be assigned historically to alchemy as a precursor of chemistry.

In the first place, it is not true that *all* writers whose analysis has been regarded as having represented an application of the concept of "elasticity of demand" to money have also been sponsors of a "cash-balance approach." [82] In the second place, even in some of the cases (as in the case of Wicksell) in which a given writer may be regarded simultaneously as a sponsor of the application of the concept of "elasticity" to the demand for money and as a sponsor of the cash-balance approach, there was no explicit attempt to associate the two; and this in itself would indicate that a concern with the applicability of the concept of "elasticity" to the demand for money was not a necessary condition for the emergence of the "cash-balance approach." [83] One must, indeed, go further, and point out that, in still other cases, a "cash-balance approach" was presented by writers who attained their results not only independently of any concern with the applicability of the concept of "elasticity" to the demand for money (as in the case of Menger), but also (as in the case of Hawtrey) in the face of an emphatically stated lack of sympathy for an emphasis upon the desirability of applying to the special problem of the Value of Money the categories of the "general" Theory of Value. It is in the light of these clear facts of doctrinal history that one must judge the seriousness of the consequences, for the Theory of Money and Prices, that would have followed from a failure to apply, to the "demand for money," the "homely but intelligible concept" of "elasticity of demand."

III

THE ELASTICITY OF SUBSTITUTION OF MONEY

To my knowledge, the Keynes of the *General Theory* was the first to apply the concept of "elasticity of substitution" *formally* to Money.[84] This may be regarded as an achieve-

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[82] John Stuart Mill and the Walras of the first edition of the *Eléments* are cases in point. See above, p. 647, n. 43, and the references to the first edition of Walras's *Eléments* given in the article cited above, p. 648, n. 49.

[83] It must be remembered, indeed, that, by the testimony of no less articulate a champion of the "cash-balance approach" than Marshall, the origins of this approach go back as far as Petty, who certainly can hardly be said to have reached his conclusions as the result of a conscious application of the concept of "elasticity" to the "demand for money." See Volume I, 418, and the references to Marshall and Petty given in n. 11 thereto.

[84] Cf. the *General Theory*, 231, 234, 236, 238. For purposes of a judgment as to the substantive significance of the application in question, it is important to observe that what is involved is precisely the *formal* application of the concept, rather than the type of discussion with respect to the

ment of some importance by those to whom every formal application to the problem of the Value of Money of a concept developed originally within the "general" Theory of Value necessarily represents a substantive advance in monetary theory.[85]   A much greater degree of skepticism, on the other hand, would be natural to those aware of the paucity of positive achievements and even the obfuscation of important issues that has often been the sole result of applications of this type.   And indeed it can be shown that in all important respects the results obtained by Mr. Keynes from his application to Money of the concept of an "elasticity of substitution of money" parallel those obtained in earlier attempts to apply to the "demand for money" the "homely but intelligible concept" of "elasticity of demand."

Again this conclusion may be established by a schematic application to the case in hand of certain "Lessons of Doctrinal History" which can be shown to bear directly upon the problem.

"substitution" of holdings of "money" for holdings of "other things," and the nature of the factors governing the extent of such substitution, which is commented on below, under "The Facts of Doctrinal History." It is striking, for example, that a formal application of the kind indicated was *not* made by Professor Hicks, one of the writers chiefly responsible for the formal introduction of the concept of "elasticity of substitution" into "general" economic theory (see below, p. 665, nn. 86 and 87). This is *not* to say, of course, that Professor Hicks has refrained from discussing the phenomenon of *"substitution"* as applied to money and "other things." See, for example, his "Suggestion for Simplifying the Theory of Money," *loc. cit.,* 10, and his *Value and Capital,* 170, 239. The point at issue, however, *is* precisely the degree of significance which is to be held to attach to the *formal* application of the concept of "elasticity of substitution," as developed originally within the "general" Theory of Value, to the special case of money; and from this point of view, the fact that Professor Hicks himself did not make such an application itself provides a commentary upon the degree of substantive significance that can be held to attach to his own formal application of the concept of *marginal utility* to the problem of the "demand for money." See above, pp. 84 ff.

[85] To my knowledge, the example set by Mr. Keynes's formal application to money of the concept of "elasticity of substitution" has thus far been followed in print by only one member of the Keynesian group. See Lerner, "Some Swedish Stepping Stones in Economic Theory," *loc. cit.,* 579. The details of the application, however, even in this single case, are such as to provide an illuminating commentary upon the application itself (see below, p. 667, n. 94; p. 669, n. 97; and p. 675, n. 114); and even if they were not, the history of analogous applications (such as that represented by the application of the concept of "elasticity of demand" to the "demand for money") is such as to justify, on grounds of prophylaxis, the discussion which follows in the text.

1. *The Facts of Doctrinal History.* The *term* "elasticity of substitu-tion" is, of course, of much more recent origin than the term "elasticity of demand." [86] It is true also that the formal definition of the *concept* of "elasticity of substitution" is of comparatively recent origin.[87] It is not surprising, therefore, that this concept should have been *formally* applied to money only in our own day. From Section II of this chap-ter, however, we know that what amounts to the *substance* of a given concept may have been used before the concept was given a generally accepted *name,* and even before the concept was formally *defined.*[88] It is easy to demonstrate that the same statements hold true in the case of the application to money of the concept (or concepts) of "elasticity of substitution." [89]

According to Mr. Keynes, the concept of an "elasticity of substitu-tion of money" has to do with the "tendency to substitute some other

[86] See, for example, J. R. Hicks, *The Theory of Wages* (1932), 117, 245; Joan Robinson, *The Economics of Imperfect Competition,* pp. vii, 123, 256 f., 330; P. M. Sweezy, A. P. Lerner, R. F. Kahn, J. R. Hicks, L. Tarshis, and J. E. Meade, "Notes on Elasticity of Substitution," *Review of Economic Studies,* I (1933–1934), 67 ff., 144 ff.; Hicks and Allen, "A Reconsideration of the Theory of Value," *loc. cit.,* 58 f., 199, 205, 209.

[87] As is well known, these "formal definitions" have not always been such as to mean the same thing to different readers. In addition to the "Notes on Elasticity of Substitution" cited in the preceding note, see F. Machlup, "The Commonsense of the Elasticity of Substitution," *Review of Economic Studies,* II (1935), 202 ff., and M. Friedman and others, "Further Notes on the Elasticity of Substitution," *ibid.,* III (1936), 147 ff. I have no means of knowing to what extent Professor Hicks's virtual avoidance of the formal use of the concept of "elasticity of substitution" even in those parts of his *Value and Capital* which are concerned with the "general" Theory of Value (see, for example, the single oblique reference to the concept on p. 96 n., of that work) is due to a dissatisfaction with the con-cept itself. It is hardly unfair to suggest, however, that a commentary upon both Professor Hicks's earlier remarks with respect to the necessity for the formal application of the concept of "marginal utility" to the problem of the demand for money (see above, p. 664, n. 84), and upon Messrs. Keynes's and Lerner's application to money of the concept of "elasticity of substitution" is provided by the facts (1) that Professor Hicks now ap-parently feels no need to apologize for his discussion of the phenomenon of "substitution," even within the "general" Theory of Value, without specifically introducing the formal concept of "elasticity of substitution"; and (2) that Messrs. Keynes and Lerner should not have been deterred from applying, to money, a concept the discussion of which was characterized, less than a year before its application to money in Keynes's *General Theory,* as "conspicuous for its unintelligibility." Cf. Machlup, "The Com-monsense of the Elasticity of Substitution," *loc. cit.,* 202. Cf., however, what is said on the latter point below, p. 666, n. 89.

[88] See above, pp. 647 ff., and the references given in nn. 43–49 thereto.

[89] It should be observed in what follows that I am concerned with the phenomenon of "substitution" in relation to *money,* and not to the *money commodity.* From Mr. Keynes's oblique reference to the use of the "money-commodity . . . in manufacture or the arts" in connection with

factor for it" "as the exchange value of money rises." [90]   Thus, two elements are involved in the concept, as Mr. Keynes here uses it: first, an implied emphasis upon the necessity for studying the forces which will lead economizing individuals to "tend to substitute" non-monetary assets for their holdings of cash under varying conditions; and, second, an implied emphasis upon the importance of studying the strength of this "tendency to substitute" non-monetary assets for holdings of cash under the *particular* "varying conditions" which are represented by changes in the "exchange value of money."

It should require only slight reflection, however, to observe that the extent to which administrators of cash balances do or do not show a "tendency to substitute" non-monetary assets for their holdings of cash under varying conditions is precisely the problem which "cash-balance analysis" has always been intended to solve.[91]   No novelty of substance can be found, therefore, in what was characterized above as

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his discussion of the "elasticity of substitution" of *money* (*General Theory,* 231), one concludes that he would put the case for the use of the concept of "elasticity of substitution" in connection with the arts demand for the money commodity on all fours with the case for its use in connection with the monetary demand.   Again, however, it should be pointed out that no competent monetary theorists have ever denied the desirability, or, indeed, the necessity, of applying the concepts of the "general" Theory of Value to the demand for the money commodity in its *non*-monetary uses—any more than they have denied their applicability to the supply of the money *commodity* when the latter is "produced under conditions involving computation of profit and loss and, being subject to free coinage, is added without limitation to the stock of money of ultimate redemption."   (Cf. above, p. 33, and the references to earlier writers given above, page 632, nn. 9 and 10.   For examples of an unexceptionable application of the concept of "elasticity of demand" to the *arts* demand, in addition to the general comments of Fisher in the passage cited above, p. 640, n. 27, see Pigou, *Essays in Applied Economics,* 190, and J. M. Clark, "Possible Complications of the Compensated Dollar," *American Economic Review,* III [1913], 577, 581, 584 ff.)   If, therefore, we can assume that the obscurity that has sometimes attached to the concept of "elasticity of substitution" within the "general" Theory of Value itself (see above, p. 665, n. 87) has now been cleared up, there is no objection to applying this concept to those phenomena of "substitution" which have been discussed for generations in connection with the arts demand for the money metals.   See, for example, Cairnes, *Essays in Political Economy,* 141; Jevons, *Investigations,* 57 f. (and the reference to his *Theory of Political Economy* there given), 279.

[90] *General Theory,* 231.

[91] An even partially representative list of citations in support of this statement would be so extensive that it cannot be presented here.   It should be sufficient to refer the reader to (1) the passages cited above, p. 663, n. 84, with respect to the rôle of "substitution" in the analysis of the demand for money; and (2) what is said above, pp. 84 ff., with respect to the *substantive* identity of the analysis stated by Professor Hicks in terms of the concept of "marginal utility of money" with the analysis of earlier "cash-balance" theorists.

the first element involved in the use of the concept of an "elasticity of substitution of money."

Only a slight acquaintance with the facts of doctrinal history, more-over, is required to demonstrate that there is just as little novelty in what was characterized above as the second element in the proposed application of this concept to the special case of money. For if any-thing is clear from a study of monetary literature, it is that the special problem of the probable administration of cash balances in the face of a changing "exchange value of money," or of expected changes in that "exchange value," is one that has concerned "cash-balance" theorists at least since the early years of the nineteenth century.[92] It is quite evident, therefore, that in applying to money the concept of "elasticity of substitution," Mr. Keynes has effected the same *type* of formalization of the statement of problems already familiar within the Theory of Money and Prices as that which is represented by the application to the "demand for money" of the concept of elasticity of demand.[93] And the question to be answered is whether this act of formalization has enabled Mr. Keynes to obtain substantive results of greater certainty and generality than were obtained by similar writers who undertook to deal with the same problems without formally intro-ducing the concept of an "elasticity of substitution of money."

2. *Factitious Issues* **versus** *Issues of Substance.* A first reason for doubting that Mr. Keynes's results are of this character is provided by a consideration of the *reason* he gives for his proposition that "money has an elasticity of substitution equal, or nearly equal, to zero."[94]

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[92] Again the very abundance of the instances that could be cited in support of this proposition (despite the irresponsible statements made in recent years with respect to the alleged novelty of an emphasis upon the importance of the factor of "uncertainty" and "expectations" in the determi-nation of the size of cash holdings relative to outlay) makes it impossible to present even a partially representative list of these instances here. It should be sufficient to call attention to the discussion by J. B. Say of the administration of cash balances during the period of the *assignats.* See M. W. Holtrop, "Theories of the Velocity of Circulation of Money in Earlier Economic Literature," *loc. cit.,* 519.

[93] It should be observed that the similarity extends even to the fact that both applications represent an attempt to describe the relation of changes in a given element to a change in a *single* other variable (in this case, changes in the "exchange value of money"), in face of the fact that in both cases techniques already existed for dealing with the relation of changes in a given element to changes in *several* other variables. See above, p. 655, n. 62; and also what is said below, pp. 672 ff.

[94] For Mr. Keynes's statement of this proposition, see the *General Theory,* 231, 234, 236, 238. The substantive validity of the proposition itself, as well as that of the apparently diametrically opposed proposition of Mr. Lerner that "the elasticity of substitution between cash and debts is *very high* over a large range" ("Some Swedish Stepping Stones in Economic Theory," *loc. cit.,* 579), is discussed below, p. 675. Here I am concerned only to demonstrate that the particular *reason* Mr. Keynes gives in support

"This follows," he argues, "from the peculiarity of money that its utility is solely derived from its exchange-value, so that the two rise and fall *pari passu*, with the result that as the exchange value of money rises there is no motive or tendency . . . to substitute some other factor for it." [95]

One has only to read this proposition to recall the enormous amount of confusion that has been engendered as the result of attempts to deduce substantive conclusions with respect to the facts of economic life from the proposition that the "utility of money" is "solely derived from its exchange value." And one has only to recall the details of this earlier discussion to realize that Mr. Keynes's proposition is completely irrelevant to the question whether money does or does not have "an elasticity of substitution equal, or nearly equal, to zero." For we know, from Volume I of this work, that it is not necessary to deny the proposition that the "utility of money" is "solely derived from its exchange value" in order to affirm another series of propositions which are of vastly greater significance for an understanding of the rôle played by the "utility" of money in the determination of the amount of cash which individuals "demand" for holding purposes—or, if one wishes, in the determination of the rate at which individuals undertake to "substitute" other assets for cash. These propositions are (1) that there is such a thing as a *specific* "utility" of a cash balance; (2) that the true problem of cash-balance analysis consists precisely of determining why individuals derive more "utility" from holding cash than they would from holding the non-monetary assets which the cash could be used to purchase; and (3) that in the solution of the latter problem the covering proposition that the "utility of money is solely derived from its exchange value" plays an entirely subordinate rôle.[96] In the light of these propositions, surely, it can be argued that the first effect of Mr. Keynes's use of the concept of an "elasticity of substitution of money" has been to revive a series of factitious issues with respect to the "utility" of money which might have been completely avoided if he had proceeded directly to an examination of those facts of economic life which bear upon the tendency of economizing individuals to "substitute" other assets for cash, instead of starting from the unwarranted assumption that a special virtue attaches to the use, in the discussion of the problem in hand, of the categories of the "general" Theory of Value.

3. *The Obscuring of Issues of Substance.* In our discussion of the application of the concept of elasticity to the "demand for money," it was found that the injection of factitious issues may itself be said to represent an obscuring of the issues of genuine substance. It was

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of his proposition, instead of contributing to a solution of the substantive issues raised by his proposition with respect to the supposed "zero, or nearly zero" "elasticity of substitution of money," succeeds only in raising issues that are entirely factitious in themselves.

[95] *General Theory*, 231.

[96] See Volume I, 451 ff.

observed also, however, that even in cases in which factitious issues were *not* introduced, the attempt to formalize the problem of the Value of Money in terms of the categories of the "general" Theory of Value has often resulted in an actual obscuring of issues which are substantively important. Precisely the same comment applies in the present instance, and for two principal reasons.

i. What is meant, in the first place, by Mr. Keynes's unqualified proposition that money has "an elasticity of substitution equal, or nearly equal, to zero"? [97] If this proposition means anything, it means that there are no circumstances under which economizing individuals are able or willing to change the proportion of their assets which they wish to keep in the form of cash, on the one hand, and in the form of assets other than cash, on the other, in response to changes in the "exchange value of money." It is impossible to believe that if Mr. Keynes had provided this simple translation of his proposition he could ever have advanced it as an accurate description of what happens in the world we know. Nor is it easy to believe that Mr. Keynes would have advanced such a proposition if, instead of hastening to apply the concept of "elasticity of substitution" to what is, after all, only a highly special case, even within the Theory of Money and Prices itself, he had approached the problem from the standpoint of the tested weapons of a truly "general" analytical apparatus for dealing with the relevant problems within the Theory of Money and Prices, such as is represented by the better versions of the "cash-balance approach." On the contrary, it is reasonable to suppose that he would have been given pause by a consideration of the consequences of the very contentions upon which the case for the usefulness of the "cash-balance approach" must be made to rest: namely, that economizing individuals *will* find it advantageous under some circumstances to "substitute" cash for non-cash assets, or *vice versa;* that in fact they do effect such substitutions; and that the task of "cash-balance analysis" is to provide a generalized account of the factors which *can* lead

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[97] The only respect, indeed, in which Mr. Keynes has *formally* "qualified" his sweeping proposition is that the absence of a "tendency to substitute some other factor" for "money" may be modified "to some trifling extent, where the money-commodity is also used in manufacture or the arts" (*General Theory,* 231). On the doubtful relevance of such a consideration in any case, see what is said above, p. 665, n. 89. Mr. Lerner, on the other hand, qualifies his apparently contradictory proposition that "the elasticity of substitution between cash and debts is very high" (see above, p. 667, n. 94) to the extent that he holds this to be true, not universally, but only "over a large range." Until the latter phrase is made more precise, it is impossible to evaluate Mr. Lerner's proposition from a substantive point of view; but it is not unfair to suggest that, as it stands, the very possibility of the occurrence of the special case over-generalized by Mr. Keynes indicates that Mr. Lerner's statement is likewise an over-generalization of special cases, although it does not represent as egregious a case of over-generalization as does the proposition advanced by Mr. Keynes.

to these substitutions, leaving for empirical investigation the determination of *which* of these factors were operative, if any were operative, in a given historical situation.[98]   Indeed, it is difficult to believe that Mr. Keynes would have advanced the proposition in question if, instead of being anxious to provide a new "application" to Money of the categories of the "general" Theory of Value, he had made use of the general concept which may be regarded as playing the rôle in his own Theory of Prices which has been played by the "cash-balance approach" in other versions of the Theory of Prices: namely, the concept of "liquidity preference." [99]   For if anything is clear with respect to this concept, it is that the case for its usefulness likewise rests upon the contention that economizing individuals will find it advantageous to put themselves in a *more* or *less* "liquid" position under varying circumstances; and this means nothing if it does not mean that these individuals will find it advantageous, under varying circumstances (one set of such circumstances being characterized by changes, actual and expected, in the "exchange value of money"), to "substitute" cash for non-cash assets, and *vice versa*.[100]

But the really interesting thing to be observed is that Mr. Keynes's proposition is belied by the very context in which it appears.   He tells us, for example, that when "the exchange value of money rises," and

[98] It will again be observed that it is precisely this task which could *not* be accomplished if the cash-balance approach were made to *rest upon* the proposition that the "utility" of money is "solely derived from its exchange value."   Cf. Volume I, 451, of the present work, and n. 100 thereto.

[99] It is unfortunately true, to be sure, that Mr. Keynes's determination to emphasize the rôle of "liquidity preference" in the determination of the *rate of interest* has sometimes led him to minimize its rôle in the Theory of Prices (cf. above, p. 578, n. 59); just as it has sometimes led him to overemphasize the effect, *upon* the degree of liquidity preference, of changes, actual and expected, in the rate of interest, at the expense of other factors which can be shown to be of equal, if not greater, influence upon the degree of liquidity preference. It is unnecessary here, however, to go beyond the fact that he himself has left room for changes in the degree of liquidity preference as a factor affecting money prices. See, for example, Chap. 21 of the *General Theory* ("The Theory of Prices"), p. 305, where, after having included e_d (the "elasticity of effective demand") in his list of factors determining "the response of money prices to changes in the quantity of money," he goes on to argue that "e_d stands for the liquidity factors which determine the demand for money in each situation." The general adequacy of such a statement as a description of the forces determining what Mr. Keynes calls the "elasticity of effective demand" is, of course, another matter.   See below, pp. 723 ff., 728 f.

[100] One can say this, of course, and still disagree with the relative *emphasis* placed, in the *General Theory's* discussion of the forces determining the degree of liquidity preference, upon (1) changes, actual and expected, in the "exchange value of money" which are not direct results of changes, actual and expected, in the rate of interest; and upon (2) changes, actual and expected, in the rate of interest itself.   See n. 99, immediately preceding.   Cf. also, however, the following paragraph of the text.

the "demand for it increases" (as a result, say, of expectations that its purchasing power will increase still more in the near future), money becomes "a bottomless sink for purchasing power." [101] What does this mean, if it does not mean that, under the circumstances indicated, individuals *will* attempt to substitute *cash* for *non-cash* assets? [102] And when, elsewhere in the *General Theory*, Mr. Keynes accepts the reality of the phenomenon usually described under the head of a "flight from the currency," what does this mean if it does not mean that circumstances may exist under which there *will* be a tendency to "substitute some other factor" for money, and that there *is* a "value," or series of "values," of money at which the demand for it will be "diverted . . . so as to slop over into a demand for other things"? [103] The very fact, surely, that Mr. Keynes is prepared to accept these propositions as descriptions of what happens in the world we know, at the same time that he insists that "money has an elasticity of substitution equal, or nearly equal, to zero," itself provides a commentary upon the degree to which a concept such as the "elasticity of substitution of money" is capable of obscuring issues which could not have been obscured if these issues had been approached less circuitously.

    ii. It is anything but clear, in the second place, that the concept of an "elasticity of substitution of money" could be regarded as being as adequate for the purposes of monetary analysis as other analytical weapons which are available, even if the concept itself were completely

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[101] *General Theory*, 231.

[102] In this case, obviously, the "elasticity of substitution of money" would be very greatly different from "zero," even though the correlation between the change in the "exchange value of money" and the rate of substitution of cash for non-cash assets would be positive. The mere fact that the correlation has a *sign* different from that which would be expected to apply, under most circumstances, in the case of most commodities other than money, is, however, not the decisive argument against the application of the concept of "elasticity of substitution" to money. In this connection, cf. what is said above, p. 643, n. 32, with respect to an analogous argument against the application of the concept of "elasticity of *supply*" to the supply of non-commodity money. Much more decisive is the fact that in both cases Mr. Keynes has advanced substantive propositions with respect to the functioning of money which it is difficult to believe that he would have advanced if he had stated these propositions without insisting upon using the concepts of the "general" Theory of Value.

[103] Contrast the *General Theory*, 231. For examples of Mr. Keynes's acceptance of "the reality of the phenomenon usually described under the head of a 'flight from the currency,'" see the *General Theory*, 207, 306, 329. Again it should be observed that there is no warrant for identifying a proposition to the effect that the elasticity of substitution of money is "equal, or nearly equal, to zero," with the proposition that the correlation between the changes in the exchange value of money and the rate of substitution of cash for non-cash assets is *positive*, in contrast with the "case of other rent-factors," where it is likely to be negative (*General Theory*, 231). See the preceding note.

dissociated from Mr. Keynes's unfortunate proposition that this elasticity is "equal, or nearly equal, to zero." Suppose, for example, that it is recognized (as it must be recognized) that the elasticity of substitution of money may have any one of a series of values, of which zero would represent only a single possibility.[104] Our problem then becomes that of *explaining* why this elasticity will be as large as it is. It should require only the slightest reflection to realize that a principal reason why this elasticity may change is that the rate at which individuals may be expected to substitute cash for non-cash assets, and *vice versa*, will depend *not only* upon changes in the "exchange value of money" (and on expectations of such changes), but also upon *other things as well*.[105]

Now, the concept of an elasticity of substitution of money, as defined by Mr. Keynes, tells us nothing as to the nature of these "other things": to this extent, as was pointed out above, it resembles the concept of an "elasticity of demand for money" itself.[106] To anyone, on the other hand, who is not blinded by the appearance of sophistication characterizing the concept of an "elasticity of substitution of money," it should be clear that it is precisely the supposedly less sophisticated devices of monetary theory which tell us what these "other things" are.[107] If, for example, the "demand for cash balances" which is under discussion is what has been called in this work the "absolute" demand for cash balances, it is clear that among the forces determining the rate at which the individuals in a given community will substitute cash for non-cash assets will be those summarized by the T of the Fisher Quantity Equation, as well as by the P of that equation (the "exchange value of money") and by those forces which are suggested by the

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[104] On the fact that an elasticity of substitution of zero *is* a possibility as itself providing a warning against over-generalizations with respect to a "high" elasticity of substitution of money, see above, p. 669, n. 97. It should be clear, on the other hand, that the fact that a "zero" elasticity of substitution of money is only a *single* possibility provides a further example of the extent to which Mr. Keynes's *"general"* theory" of Money may be said to represent an over-generalization of highly special cases.

[105] It should be clear that the argument which follows represents another example of the limitations attaching to the use of "elasticities" as a result of the fact that most of these elasticities are, in the words of Mr. Hawtrey, "only applicable to a function of one variable." Cf. above, p. 655, n. 62, and p. 667, n. 93.

[106] See above, p. 667, n. 93. On the extent to which the same charge can be levelled against Mr. Keynes's "liquidity function," see below, p. 673, n. 111.

[107] It is of considerable importance to observe that these "less sophisticated devices," unlike certain uses of the "Lausanne" proposition that a given economic magnitude is to be regarded as a function, not of one variable alone, but of several variables, actually undertake to tell us *what* these other variables are. See above, p. 656, n. 62, and also the following note.

conception of *expectations* of changes in P.[108]   And if the "demand for cash balances" which is under discussion is what we have called the "relative" demand, it should be clear that the forces determining the rate at which individuals will substitute cash for non-cash assets may be any or all of those included in our list of factors determining the size of cash balances held relatively to outlay (that is, the size of our K), of which *price changes* (changes in the "exchange value of money"), and the expectation of such changes, *are only one*.[109]   These are matters, surely, that would have been revealed at once to anyone for whom the glitter of the supposed achievement represented by the application, to the problem of the "demand for money," of the fashionable "elasticity of substitution" has not obscured the issues of analysis that are really important.[110]   They are matters, indeed, which were not obscured to Mr. Keynes himself when, instead of being anxious to provide a new "application" to money of the categories of the "general" Theory of Value, he discussed the facts with respect to the rate at which money is likely to be "substituted" for other things in terms of the concept which after all represents his own version of the "cash-balance approach": namely, the concept of "liquidity preference." [111]

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[108] Careful readers of this work should hardly need to be reminded that the T, for example, is itself in all cases to be taken as a covering term for its *components,* each of which, in turn, constitutes merely a rubric for further detailed analysis.   Cf. also the reference, in the following note, to Volume I of the present work, with respect to the nature of the analysis that must be held to lie behind the K of our formulation.

[109] See Volume I, 482 f.

[110] It is of course not denied that it is *possible* to present a concept of an "elasticity of substitution of money" with respect, not to the single factor of "changes in its exchange value," but with respect to *all* factors which can be shown to affect the "tendency to substitute some other factor for" money.   What is denied is that a mere translation of the relevant analysis in terms of the concept of an "elasticity of substitution of money" would in itself represent a substantive addition to our analytical equipment, even if it did not succumb to the temptation merely to *state* that many factors are involved, without specifying concretely what these factors are.   And what is affirmed is that even the short history of the concept of an "elasticity of substitution of money" has evidenced a tendency to ignore the limitations set upon the use of concepts which, in Mr. Hawtrey's words, are "only applicable to a function of one variable."   Cf. also what is said in the following note with respect to the implications of Mr. Keynes's expression $M = L(r)$.

[111] See above, p. 670.   As was pointed out in n. 99 thereto, it is true, of course, that Mr. Keynes's own treatment of the concept of "liquidity preference" is fairly open to the charge that it over-generalizes the cases in which the principal factor (if not the sole factor) affecting the degree of liquidity preference is held to be changes, actual or expected, in the *rate of interest.*   Yet a close examination of the *General Theory's* treatment of the *concept* of liquidity preference will reveal that the degree of liquidity preference is held to be capable of being affected by other factors than changes, actual or expected, in the rate of interest.   It is to be observed,

4. *Disguise of Ancient Platitude, or Positive Error?* From the argument thus developed, it should be clear that Mr. Keynes's application to Money of the concept of "elasticity of substitution" parallels in several respects the experience with the application to Money of the concept of "elasticity of demand": in its introduction of factitious issues; in its further obscuring of issues of substance; and (one may add, on the basis of what was said above under 3, ii) in its tendency, at best, to confuse the mere *statement* of a problem with progress toward its solution, in a greater degree than certain other devices of monetary theory can be said to have encouraged such a confusion. In one respect, however, Mr. Keynes's application represents a parallel, not to the best that has been written on the subject of an elasticity of demand for money, but the worst. The point involved may be best seen if one compares Mr. Keynes's proposition that "money has an elasticity of substitution equal, or nearly equal, to zero" with the proposition that money has an "elasticity of demand equal to unity."

As we have seen, the latter proposition, if taken literally, reduces algebraically to a form of the "quantity theory" of such a degree of crudity that it has ceased even to interest, to say nothing of command-

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for example, that all of the passages (cited above, p. 671, n. 103) in which Mr. Keynes commented on the phenomenon of a "flight from the currency" occur in the course of a discussion of the factors affecting liquidity preference—or, as Mr. Keynes sometimes puts it, the form of "the liquidity function." It is a serious question, to be sure, whether (1) the formal definition of "liquidity preference" as "a potential or functional tendency, which fixes the quantity of money which the public will hold when the *rate of interest* is given," and (2) a writing of this "function of liquidity-preference" (L) in the form $M = L(r)$—in which M is "the quantity of money" and r is the rate of interest (*General Theory*, 168)—is really as illuminating as the *list* of the factors affecting liquidity preference, comparable to the list of factors affecting the size of cash balances relative to outlay presented in Volume I, 482 f., of the present volume, which could be constructed upon the basis of other passages in the *General Theory*. For here again Mr. Keynes makes use of an expression relating movements in a given magnitude to a *single* variable (in this case, the rate of interest), in face of the fact that these movements are likely to be greatly affected by movements in variables other than the single variable thus chosen; and again we are left with the task of explaining changes in the *form* of the function relating it to the single variable *in terms of movements in these other variables*. The point in the text is merely that, despite the objections that can be raised to the details of Mr. Keynes's treatment of the forces affecting the degree of liquidity preference, these details ought in themselves to have been sufficient to give Mr. Keynes pause before advancing his substantive proposition with respect to the "elasticity of substitution" of money being "zero, or nearly zero." And the further point is that it is difficult to believe that he would not have been given pause if he had stated his substantive proposition with respect to changes in the "exchange value of money" and the "tendency to substitute" cash for non-cash assets without making formal use of the concept of an "elasticity of substitution of money."

ing the support of, any large number of competent monetary theorists.[112]   The most that could be said, therefore, of the alleged "discovery," by "post-war" theorists, that the elasticity of demand for money is *not* necessarily equal to unity is that it represented a belated rediscovery of a very ancient platitude.   But in the case of Mr. Keynes's proposition that "money has an elasticity of substitution equal, or nearly equal, to zero," we are thrown back, not to a restatement in unfamiliar terms of a very familiar platitude, but to a statement which, literally interpreted, is either false or represents an entirely unwarranted over-generalization of a highly special case.[113] The way is thus opened to another "post-war" "discovery": in this case, the "discovery" that money may have an elasticity of substitution which is *not* necessarily "equal, or nearly equal, to zero," and may even be said (in the words of Mr. Lerner) to have a *"very high"* elasticity of substitution.[114]   It should hardly be necessary to labor the point that the chief purpose of our discussion of Mr. Keynes's application to money of the concept of "elasticity of substitution" has not been to produce a "discovery" of this kind.   It has been rather to illustrate again the consequences of a failure to heed Lessons of Doctrinal History that would have been revealed by a study of the results obtained from earlier attempts to apply to the problem of the Value of Money certain "homely but intelligible concepts" of the "general" Theory of Value.

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[112] I am abstracting here, of course, from the possibility of an interpretation of the proposition that "the elasticity of demand for money is always equal to unity" which would regard all departures from the results suggested by the crudest of "quantity theories" as representing a *shift* in a demand curve of constant (and unitary) elasticity.   See above, p. 652, and the reference to Pigou in n. 54 thereto.   In any event, I can see no possibility of translating Mr. Keynes's proposition with respect to a "zero" elasticity of substitution of money in terms which would be both formally correct and substantively useful.

[113] In this connection, compare our findings, above, pp. 634 ff., with respect to the substantive correctness of Mr. Keynes's proposition that money has "both in the long and the short period, a zero, or at any rate a very small, elasticity of *production*."

[114] See above, p. 667, n. 94.   As a further commentary on the importance of such a "discovery," it may be observed that the relation of Mr. Lerner's proposition to that of Mr. Keynes is more than a little obscured by the heroic assumptions that would be required in order to establish an identity between an "elasticity of substitution" in terms of a response to changes in the "exchangeable value of money" (Keynes), on the one hand, and, on the other, to changes in the "relative valuation" of "cash and debt" (Lerner), as affected specifically by changes in the rate of interest.

Keynes's "Elasticity of Effective Demand"

WE HAVE considered thus far two different applications of the concept of "elasticity of demand" to the problems of monetary theory: first, its application to the problem of differential price change and to the broader problem of the rôle played by particular demand schedules of the Marshallian type in a general "synthesis" of monetary theory and the "general" Theory of Value designed to account for the determination of money prices; and secondly, its application to the "demand for money." We have seen that the two applications led to results of greatly differing heuristic value. We have seen also that Mr. Keynes, in rejecting the first type of application, rejected a type of analysis which can be shown to be of the utmost usefulness in itself, quite apart from any gain in "elegance" that might be attributed to a conscious application to monetary problems of the concepts of the "general" Theory of Value. On the other hand, Mr. Keynes has neither formally rejected the second type of formulation; nor (in his *General Theory,* at any rate) has he formally accepted it.[1]

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[1] For an example of an acceptance of the concept of an "elasticity of demand for money" by Mr. Keynes in his writings *prior* to the *General Theory,* see the references to Keynes's *Monetary Reform* given above, p. 647, n. 41. The instances, either in the *General Theory* itself, or in Mr. Keynes's writings subsequent thereto, in which he may *seem* to have made use of the concept of an "elasticity of demand for money," do not, upon closer examination, contradict the statement in the text. Mr. Keynes's reference, for example, in his essay on "The Theory of the Rate of Interest" (*Lessons of Monetary Experience: Essays in Honor of Irving Fisher* [1937], 152), to the "element of elasticity" which may be held to characterize "the desire to hold inactive balances," does not involve the establishment of a relation between the quantity of money demanded and the "Value" of Money in the sense of its purchasing power per unit. On the contrary, instead of corresponding to the "elasticity of demand for money" of Mr. Keynes's predecessors, and of Mr. Keynes himself in his *Monetary Reform,* the usage in question corresponds to loose expressions of the type

Given the amount of confusion engendered by this second type of application, or at best the paucity of positive results obtained from it, Mr. Keynes's apparent failure to attach any great importance to the application of the concept of "elasticity" to the demand for money might in itself have been an occasion for rejoicing. It must be remembered, on the other hand, that Mr. Keynes continues to regard a proper application of the "homely but intelligible concept" of "elasticity of demand" to the problems of monetary theory as a matter of considerable importance. It is necessary, therefore, to consider the implications, for the further development of monetary theory, of Mr. Keynes's own application of the concept of "elasticity of demand" to the theory of the determination of money prices, particularly when his own application is viewed in the light of the results, both positive and negative, obtained from the two earlier applications already discussed, as well as the results obtained in other sectors of monetary theory.

indicated above, p. 646, n. 37. Similarly irrelevant for our present purposes are the occasional instances, in the *General Theory* itself, in which Mr. Keynes speaks of "the elasticity of demand for liquid cash in terms of debts" (so, for example, the *General Theory,* 235). For it should be clear that in such instances Mr. Keynes has in mind elements affecting the degree of "elasticity" shown by the *demand schedule for loanable funds,* the "demand" for such funds being expressed as a function of the *rate of interest.* A detailed examination of the consequences of Mr. Keynes's insistence upon stating the problem of the determination of the rate of interest in terms of the "demand for *money,*" rather than in terms of the demand for loanable funds, must be left for another occasion. Here it is sufficient merely to observe again that the "elasticity of the demand for money" with which the earlier writers were concerned involved the establishment of a relation, not between the quantity of money demanded and the *rate of interest,* but between the quantity of money demanded and the "Value" of Money *in the sense of its purchasing power per unit.* It may be added, finally, that, *from the standpoint of algebra,* the particular concept of "elasticity" in Keynes's *General Theory* which corresponds most closely to the formula for the "elasticity of demand for money" used by earlier writers (see, for example, R. A. Lehfeldt, "Statistics of Extremely Depreciated Currencies," *loc. cit.,* 557)—namely, $e = Mdp/pdM$—is called by Mr. Keynes, not "the elasticity of demand for money," but "the elasticity of prices in response to changes in the quantity of money" (*General Theory,* 296, 305). On the significance of the latter "elasticity" for the general purposes of monetary theory, see Chapter Fourteen, immediately following.

I

Mathematics or Economics?

To begin with, it must be pointed out that if some significance does attach to the *mere fact that the concepts of the "general" Theory of Value are applied to the problems of monetary theory,* then Mr. Keynes's "elasticity of effective demand" (e_d) is even less "significant" than the application of the concept of "elasticity" to the demand for money. For it must be said that the latter does represent an application, to the special problem of the Value of Money, of the concept of "elasticity of demand" as it appears *within the "general" Theory of Value.* With respect to Mr. Keynes's "elasticity of effective demand," on the other hand, the following propositions are in order:

1. Mr. Keynes's formula for his "elasticity of effective demand" is $e_d = MdD/DdM$, in which M represents the "quantity of money" and D represents "effective demand" in one of the senses assigned to the latter term in the *General Theory.*[2] Clearly, this expression has the form of the

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[2] See the *General Theory*, 305. On p. 304 of the same work, D is defined by the expression $D = MV$, "where M is the quantity of money"; V is "its income velocity"; and the latter, in turn, is defined by the expression $V = Y/M_1$ (see the *General Theory*, 201, 209), in which Y is "income" and is defined by the expression $Y = OP$, in which, in turn, O and P are the "quantity" and the "price," respectively of "current output." (*General Theory*, 209; for the definition of M_1, see pp. 199 ff. of that work.) No reader who has wrestled with the various definitions, expressed or implied in the *General Theory*, of D and the other magnitudes involved in the expressions just cited, will need to be reminded that Mr. Keynes has imposed a formidable task of reconciliation upon his readers. For example, from the expressions $D = MV$ (or $V = D/M$) and $V = Y/M_1$, just cited, it would seem to follow that D, instead of being equal to Y, stands to Y in the relation of M/M_1—that is, $D:Y = M:M_1$. For a possible interpretation of this expression (on the assumption that the algebraic expressions used by Mr. Keynes were intended to be taken literally), see below, p. 696, n. 42. See also, however, p. 284 of the *General Theory*, where, from the expression $Op = D$, one would suppose either that (1) D is equal to Y, since $Y = OP$ (*General Theory*, 209); or (2) if the P of the expression $Y = OP$ is taken to represent *realized* prices, in contrast to the "expected" prices (p) of the expression $Op = D$ (see the *General Theory*, p. 284—though see also pp. 304 f., of the same work, where p would appear to refer to *realized* prices), one would suppose that $D:Y = p:P$, in which case we are dealing with an expression whose meaning is not only obscure in itself, but is made still more obscure when we write $D:Y = p:P = M:M_1$.

general mathematical expression for the elasticity of a function $y = \phi(x)$ at the point x—namely, $x/y \div dx/dy$, or xdy/ydx—and therefore has the same *algebraic* aspect as the "elasticity of demand" of the general Theory of Value. The same thing could be said, however, of the relation of Mr. Keynes's concept not only to the "elasticity of *demand*" of the general Theory of Value, but to any "elasticity" one might wish to adduce: elasticity of supply, elasticity of substitution, elasticity of expectations, and so on *ad infinitum*. The only respect, that is to say, in which Mr. Keynes's "elasticity of effective demand" resembles the

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To these difficulties is added a further set, arising from the fact that, on p. 299 of the *General Theory*, we are explicitly told that although "the ratio between the quantity of effective demand and the quantity of money closely corresponds to what is often called the 'income-velocity of money,'" the "effective demand" involved "corresponds to the income the expectation of which has set production moving, *not* to the actually realized income" (italics mine); and this despite the facts (1) that Mr. Keynes's expression $Y = OP$ is explicitly identified with a "stream" equation of the form $MV = OP$ (*General Theory*, 209, 289), which would certainly suggest that the "income" involved is *realized* money "income" (or *realized* money outlay from income, or *realized* money expenditure upon "real" income ["output"]); and (2) that the "quantity of money" which is to be compared with this "expected" "income" is presumably a *realized* quantity of money, so that, taking Mr. Keynes literally, we are asked to think of "income velocity" as a ratio between a *realized* quantity of money, on the one hand, and an *expected* level of "income," on the other. A still further set of difficulties is raised by the formal definition of "effective demand" given on p. 25 of the *General Theory*, where it is identified, not with D (as in the expressions cited above), but only with the *particular* "value of D at the point of the aggregate demand function where it [the 'aggregate demand function'] is intersected by the aggregate supply function." The difficulties, indeed, into which one is led by an attempt to reconcile Mr. Keynes's various definitions of the magnitudes involved in his concept of an "elasticity of effective demand" and the related expressions are such as to lead one to conclude (1) that least damage is done to this part of the argument of the *General Theory* by interpreting all of the magnitudes involved in the expression $e_d = MdD/DdM$ as *realized* magnitudes; (2) that the same thing may be said with respect to the expressions $Op = D$ and $Y = OP$, so that, for our present purpose, D may be taken as equal to Y, and as representing, despite the statement on p. 299 of the *General Theory* quoted above, a *realized* stream of money payments; and (3) that the respects in which Mr. Keynes's definition of "income velocity," as it appears in the crucial expression $MV = D$, differs from "the usual definition" (read: *some* of the *definitions*) of "income velocity" are entirely "minor" for our present purpose (cf. the *General Theory* itself, p. 304; and see also below, p. 681, n. 5; p. 687, n. 16; and pp. 707 ff.).

"elasticity of demand" of the "general" Theory of Value is that it is an "elasticity," defined by the "general" formula xdy/ydx. It follows that what is involved is an application to economics of *mathematics*, and not an application to the Theory of Money and Prices of a concept of the "general" Theory of Value.[3] An application of the latter type would be involved only if, in addition to this identity of *algebraic* aspect, the x and y of the general mathematical expression xdy/ydx referred to essentially the same things in Mr. Keynes's formula as in the "elasticity of demand" of the general Theory of Value.

2. This is in fact what may be said of the application of the latter concept to the problem of differential price change during periods of monetary expansion and contraction.   In effect, also, this is what is involved in the application of the concept of "elasticity" to the "demand for money": for in that case the variables x and y refer to the quantity demanded and the per unit *value*, respectively, of a given commodity (in this case, "money"), just as they do in the "general" Theory of Value.   In the case of Mr. Keynes's e_d, however, nothing corresponds to the "value" of a given commodity (or money): his e_d undertakes to compare the degrees of relative change not in quantity and per unit *value* of a commodity (or money), respectively, but in the quantity of money and the *amount of income generated* on the basis of the quantity of money.[4]

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[3] If support is needed for the proposition that the *general concept* of an "elasticity" is a matter of mathematics rather than of economics (even if the mathematicians themselves do not seem to have been the first to use the *term* "elasticity" in this connection), see R. G. D. Allen, *Mathematical Analysis for Economists* (1938), 251 ff., on "the elasticity of a function"; and cf. also Joan Robinson, *The Economics of Imperfect Competition*, 18: "The elasticity of a curve is a *geometrical* conception" (italics mine).

[4] It should be equally clear that the variables involved in Mr. Keynes's "elasticity of effective demand" are likewise not those which appear in the "income-elasticity of demand" of Hicks and Allen ("A Reconsideration of the Theory of Value," *loc. cit.*, 64 ff., 200 ff.). For the latter concept involves a comparison of the "relative increase in income" with the "relative increase in *demand*" for a given commodity, whereas Mr. Keynes's "elasticity of effective demand" involves a comparison of the relative increase in *income*, which is itself virtually *identified* with "demand" (cf. above, p. 678, n. 2, and also what is said below, pp. 694 ff.), with the relative increase in the *"quantity of money."*

II

SUBSTANTIVE SYNTHESIS OR TERMINOLOGICAL INNOVATION?

In effect, therefore, Mr. Keynes's e_d "closely corresponds," not to a "homely but intelligible concept" of the "general" Theory of Value, but, as Mr. Keynes himself admits, "to what is often called the 'income velocity of money.'"[5] It corresponds, in other words, to one of the "vaguer" (and also, presumably, less "homely" and "intelligible") concepts of *monetary* theory for which Mr. Keynes proposed to substitute a conceptual apparatus which would be closely related to such "notions" as the "elasticity of demand" of the general Theory of Value.[6] From this fact two conclusions may be drawn:

1. It follows, first, that the *substance* of the "homely but intelligible concept" of elasticity of demand, *as that concept has appeared within the "general" Theory of Value*, remains, so far as Mr. Keynes's "synthesis" is concerned, where it was before he undertook his "synthesis": namely, within the "general" Theory of Value. The *substance* of

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[5] See the *General Theory*, 299. It will be recalled that Mr. Keynes himself has characterized the *differences* between his own definition of "income velocity" (and therefore, by virtue of the expression $D = MV$, of his concept of an "elasticity of effective demand" $e_d = MdD/DdM$), and "the usual definitions" of the latter concept, as altogether "minor." See above, p. 679, n. 2, and especially the reference to the *General Theory* given at the end of that note. It should be observed also that on p. 258 of the *General Theory*, Mr. Keynes does not quarrel with the position of those economists who would argue "that aggregate demand depends upon the quantity of money multiplied by the income-velocity of money." On the contrary, the "type of analysis" with which he "fundamentally differs," in the passage indicated, has to do, not with the proposition summarized by his own expression $D = MV$, but with a specific contention with respect to the probable effect of a "reduction in money-wages" upon "aggregate effective demand" (D), by way of its effect (or lack of effect) upon either of the two variables (namely, the "quantity of money" and its "income velocity") which, by virtue of Mr. Keynes's expression $D = MV$, are to be regarded as the monetary *components* of "aggregate effective demand."

[6] For Mr. Keynes's inclusion of "income velocity" among the "vaguer phrases" of the Theory of Money and Prices which are alleged to represent the very antithesis of an application of the "homely but intelligible concept" of "elasticity of demand" of the "general" Theory of Value to monetary theory, see the *General Theory*, 292.

Mr. Keynes's "elasticity of effective demand," similarly, remains, after Mr. Keynes's "synthesis," where it was before: namely, within the Theory of Money and Prices. Mr. Keynes's supposed "synthesis," therefore, apart from the matter of mathematical expression discussed in Section I of this chapter, has to do solely with matters of terminological innovation within *monetary* theory; it is not in itself a substantive synthesis of material heretofore treated separately in the "general" Theory of Value, on the one hand, and the Theory of Money and Prices, on the other.

2. Writers prior to the Keynes of the *General Theory* had applied the general mathematical formula for "elasticity" (or the equivalent of this mathematical formula) to certain relations held to be important in *monetary* theory, apart from the relation between changes in the quantity of money and changes in its value.[7] They refrained, however, from

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[7] In 1921, for example, J. Marschak applied the mathematical concept of "elasticity" to the ancient question whether, and to what degree, a change in one of the variables of the familiar Quantity Equations might be expected to be accompanied by a change in the other variables ("Die Verkehrsgleichung," *loc. cit.*, 356 ff.). To be sure, Marschak did not make explicit use of the algebraic expression $\frac{dx}{x} \div \frac{dy}{y}$; but its use was of course implied by his identification of his "degrees of elasticity" with the concept of a differential coefficient (see p. 356 of the article cited). Essentially the same type of application was made in 1933 by E. Petersen, *Den moderne Kvantitetsteoris Gyldighet for Pengeverdiens Bestemmelse* (see the Preface, and especially 49 ff., 57 ff.; and cf. the same author's later *Macro-Dynamic Aspects of the Equation of Exchange*, 24 ff.). Petersen, like Marschak, did not state his "coefficients of elasticity" explicitly in terms of the standard formula $\frac{dx}{x} \div \frac{dy}{y}$. His actual formula for, say, the "elasticity" of prices (P) with respect to the quantity of money (M), was of the form $_M C_P = \frac{b_p}{b_m}$. (See *Den moderne Kvantitetsteoris Gyldighet*, 51 f.) In this expression, $_M C_P$ is a symbol for the particular "elasticity" in question, just as one might write $_D e_P$, in place of e_p, for Mr. Keynes's "elasticity of money prices in response to changes in effective demand" (*General Theory*, 285). To establish the essential similarity of Petersen's formula for "elasticity" to formulas of the general type $\frac{dx}{x} \div \frac{dy}{y}$, it is therefore necessary only to establish the fact that Petersen's b_p and b_m are designed to represent the same type of magnitude as $\frac{dx}{x}$ and $\frac{dy}{y}$. This is easily established. The term b_p represents the "number of times" *prices* (P) are increased as the

asserting that, in so doing, they were applying to monetary theory the concepts of the "general" Theory of Value.[8] In so refraining, they indicated a procedure that might have prevented unfortunate claims to "synthesis" of the kind associated with Keynes's "elasticity of effective demand." What is more important, however, is that their procedure made clear what Mr. Keynes's procedure has not made clear: namely, that if any significance attaches to the concept of "elasticity," when used in the way indicated, such significance must be found, not in a supposed application to monetary theory of concepts developed originally within the "general" Theory of Value, but in a demonstrated superiority of formulations making use of this type of concept to

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result of the change in M, whereas b_m represents the "number of times" M itself is increased (Petersen, *Den moderne Kvantitetsteoris Gyldighet*, 51). On the other hand, $\frac{dx}{x}$ may be taken as representing the "percentage" increase in the variable x. It should be clear, therefore, that, mathematically speaking, Petersen's "elasticity coefficients" represent essentially the same sort of device as is represented by the more usual formulation for "elasticity coefficients," since both are concerned with a comparison of the ratios of relative increase in two associated variables.

[8] In their respective discussions, both Marschak and Petersen *referred*, to be sure, to the concept of "elasticity of demand" as it appears in the "general" Theory of Value. See the references to Marshall and Schumpeter in Marschak, "Die Verkehrsgleichung," *loc. cit.*, 356, and the reference to "Cournot or Marshall's coefficients of elasticity of demand" in Petersen, *Macro-Dynamic Aspects of the Equation of Exchange*, 24 n. That Dr. Marschak, however, was aware that he was applying to the Theory of Money and Prices a concept of *mathematics* is evidenced by his comment that the phrase "degree of elasticity" was merely a "figurative expression" (*bildliche Ausdruck*) for the mathematical concept of a *differential coefficient* ("Die Verkehrsgleichung," *loc. cit.*). Similarly, the context in which Dr. Petersen presented his "theory of elasticity" shows that the "similarity" which may be said to exist between his "coefficients of elasticity," on the one hand, and "Cournot or Marshall's coefficients of elasticity of demand," on the other, was to be regarded as a *mathematical* similarity, and not as a similarity deriving from a supposed application to monetary theory of specific concepts of the "general" Theory of Value such as the Cournot-Marshall "elasticity of demand." It is noteworthy, indeed, that in the Preface to his earlier monograph Dr. Petersen advanced his "theory of elasticity," not as providing a "bridge" between the Theory of Money and Prices, on the one hand, and the "general" Theory of Value, on the other, but merely as an alternative way of dealing, within the Theory of Money and Prices, with the issues traditionally discussed under the head of the "quantity theory"; and that in the body of that monograph (pp. 49 f.) he referred, for analogies, not to the "general theory of value," but to certain simple problems in physics.

those formulations of received *monetary* theory of which such a usage represents merely a translation in less familiar terms.[9]

III

THE LESSONS OF DOCTRINAL HISTORY FURTHER APPLIED

From the conclusion stated in Section I of this chapter (namely, that Mr. Keynes's "elasticity of effective demand" is not an application of the "elasticity of demand" of the "general" Theory of Value even in the sense in which this may be said of the concept of an "elasticity of demand for money"), it does not follow that no lessons were to be learned from the application of the concept of "elasticity of demand" to the "demand for money," when the problem is that of ascertaining the precise degree of *substantive advance in monetary theory* which may be said to be repre-

[9] On Petersen's treatment, cf. the preceding note. Cf. Marschak, "Die Verkehrsgleichung," *loc. cit.*, 356: "We are given certain magnitudes M, V, T, P, connected by the equation of exchange. Let one of them be supposed to vary . . . ; how large will be the changes in all four magnitudes which are called forth by this variation in one of them. . . ?" The answer to this question, Marschak went on to say, would give us the "magnitude of the degree of elasticity" of each variable with respect to the others, at least in the sense that it would tell us the "direction of change" and also "which of the four magnitudes is the most elastic, and which is the most rigid." Whether the restatement of these familiar problems in terms of a series of "elasticities" represented a major substantive advance is, of course, a question which will be answered differently according to the amount of importance one attaches to (1) the *statement* of the problem of ascertaining relations of dependence and independence existing between the variables of the Fisherine equation, as opposed to (2) an examination, on the basis of both analytical and empirical considerations, of the factors *determining* these relations of dependence and independence. (Cf. what is said on this matter in Volume I, 26, of the present work.) Yet it is only fair to Marschak and Petersen to point out that neither of them can be accused of having created the impression that more was involved in their "elasticities" than a mode of restating a series of problems long familiar within monetary theory itself. The contrary is indicated by the fact that both writers, instead of *contrasting* their use of the mathematical concept of "elasticity" with analysis running in terms of "the quantity of money, . . . the velocity of circulation relatively to the volume of transactions, . . . *et hoc genus omne*"—in other words, in terms of the variables of the familiar Quantity Equations—made this use of the concept of "elasticity" *part* of their treatment of the Quantity Equations. The contrast with the practice of both the *General Theory* and certain of the earlier champions of the concept of an "elasticity of demand for money" (see above, p. 652, n. 56) should be obvious.

sented by Mr. Keynes's concept of an "elasticity of effective demand." On the contrary, it can be argued that the reasons for the disappointing nature of the results obtained from the use of the latter concept are in all essentials the same as those which we found operative in the case of an "elasticity of demand for money." [10]   Specifically, it can be shown (1) that there is no important respect in which Mr. Keynes's alchemy can be said to have led to a chemistry the substance of which was not already in existence; (2) that in certain respects Mr. Keynes's use of the concept of an "elasticity of effective demand" represents merely a disguised statement of ancient platitudes; (3) that in other respects it is such as to obscure, rather than to illumine, issues of substance in monetary theory; and (4) that, unless one is to be content with a monetary theory which is distinctly retrograde as compared with the best that has been available, the use of the concept of an "elasticity of effective demand" must be *supplemented* by the use of a number of precisely those concepts of the Theory of Money and Prices which Mr. Keynes has rejected on the double ground that they are "vaguer" than the concepts which he sponsors, and that they have no analogue within the "general" Theory of Value.

Again these conclusions may be established *seriatim*.

1. *From Alchemy to Chemistry?*   No instructed student of monetary theory could deny that crucial importance attaches to an emphasis upon the study of the causes and consequences of movements in what Wicksell called the "moneyed demand." [11]   But such a student could no more accept the contention that this emphasis was a peculiar discovery of Mr. Keynes, as a result of his concern with the concept of an *"elasticity"* of effective demand, than we were able to accept the implication that the emphasis upon the concept of a demand for money for "holding" purposes was a peculiar discovery of those who were concerned with the concept of an *elasticity* of "demand for money." [12]

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[10] See above, pp. 649 ff.

[11] For examples of Wicksell's use of the term indicated, see the reference given above, p. 95, n. 12, to Volume I of the present work.

[12] On the latter point, see above, pp. 662 ff.   It will be observed that the parallel extends even to the point that *both* the concept of a "demand for money for holding purposes" and the concept of a "general [money] demand" antedate literally by centuries the association of each concept with an *"elasticity* of demand."   Cf. above, p. 663, n. 83, and also the following note.

On the contrary, emphasis upon the importance of "moneyed demand" goes back almost to the beginning of economic literature; at no time did it really disappear from that literature; and, as we have seen, it has played a particularly vital rôle, in our own generation, in the writings of Mr. Hawtrey, who even applied the *term* "effective demand" to the concept of a "moneyed demand," though he certainly did *not* make use of the *term* "elasticity of effective demand," in the sense in which Mr. Keynes makes use of this term.[13] It follows, therefore, that the alchemy of Mr. Keynes's "elasticity of effective demand" was *not* a necessary condition for the historical emergence of the chemistry of the concept of an "effective" money "Demand."

2. *The Disguising of Ancient Platitudes.* It might be argued, to be sure, that Mr. Keynes's concept of an *"elasticity* of effective demand" does very much more than emphasize the importance of the concept of a "moneyed demand"; that what it does is to point out (i) that the *magnitude* of the stream of money demand is related in important

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[13] For examples of Hawtrey's use of the expression *"effective* demand," as applied to the concept of "general ['money'] demand," see *Good and Bad Trade,* 4 f., 6 f., 78, 224; *Currency and Credit,* 3; *Trade and Credit,* 90, 94, 106; *The Art of Central Banking,* 205; *Trade Depression and the Way Out,* 101. On the rôle played by the concept of a "general [money] demand" in Hawtrey's work generally, see above, p. 120, and the references given in n. 73 thereto—references, it may be added, which provide their own commentary on Mr. Keynes's failure, at any point in his *General Theory,* to comment explicitly on Hawtrey's treatment of "effective [money] demand" (see, for example, the oblique comments, on p. 76 of the *General Theory,* on what is cited in the index to that work [p. 388] as referring to "Demand and Hawtrey's theory"); though Mr. Keynes does think it worth while to cite Karl Marx, Silvio Gesell, and Major Douglas as representatives of those "underworlds" of economic thought in which alone, he suggests, the concept of "Effective Demand" continued to "live on furtively below the surface" (*General Theory,* 32. See also p. 294 of the same work, on the alleged "newness" of the "ideas" involved in the type of construction represented by Mr. Keynes's "effective demand"; and cf. Keynes's article, "The General Theory of Employment, *loc. cit.,* 219, where it is alleged that "the theory of effective demand, that is the demand for output as a whole," has been "entirely neglected for more than a hundred years.") A full demonstration of the absurdity of these statements, when judged in the light of the plain facts of the history of economic doctrine, must be deferred to my later *Money and Production,* which will present evidence in support of the contrary propositions advanced in the text. In the meantime, apart from the references to Wicksell and Hawtrey already given, it should be sufficient to call attention to the references to Tooke and Newmarch (Volume I, p. 314, n. 33, and above, p. 96, n. 15); to Cairnes (above, p. 46, n. 124, and p. 96, n. 16); to Newcomb (above, p. 106, n. 37); and to Schumpeter (above, p. 117, n. 65); as well as to what is said above (p. 46, n. 123; p. 47, n. 129; p. 104, n. 36; and p. 117, n. 65) with respect to the historical connection between the concept of a "moneyed demand" and "stream equations" of the general Fisherine form, or their analytical equivalent.

ways to the *quantity of money;* and at the same time (ii) that this relation is not necessarily constant (in the words of Mr. Keynes, that the "elasticity of effective demand" is not necessarily equal to unity).[14] In fact, however, the statement of these propositions in terms of an "elasticity of effective demand" represents the same kind of disguise of ancient platitudes that was represented by the statement, in terms of an "elasticity of demand for money," of the ancient proposition that changes in prices need not be in all cases strictly proportional to changes in the quantity of money.[15]

With respect to the proposition indicated under (i), for example, it should be observed that, by Mr. Keynes's own admission, the association indicated by his e_d "closely corresponds" to the type of association discussed by earlier writers under the heading of the concept of "income velocity." [16] Even the *formalization* of the association of changes in the magnitude of the stream of money demand with changes in the quantity of money, therefore, cannot be said to constitute a novelty.[17] There have been writers, to be sure (Tooke and Aftalion are examples),

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[14] On the second point, in particular, see the references to the *General Theory* given below, p. 691, n. 31.

[15] See above, pp. 658 ff.

[16] See above, p. 681, n. 5, and the references to the *General Theory* there given. It should be observed also that Mr. Keynes does not list, among the differences between his e_d and "what is often called the 'income velocity of money,'" the fact that his e_d relates the "quantity of money" to aggregate *demand* (or *outlay* from income), whereas certain variants of the concept of an "income velocity of money" relate the "quantity of money" to aggregate money *income,* in the literal sense of a sum total of money *received as income* (money payments "into income"). And it is well that he does not do so. For, in the first place, it is precisely a characteristic of the *General Theory's* treatment of the relation between "income" and "demand" that its definition of "income," like the definition of "income" used by certain sponsors of the concept of "income velocity," is such as to obfuscate the distinction between *income* and *outlay* from income ("demand"). (See Volume I, 378 ff., of the present work and the references given in nn. 76–81 thereto, and also the references given on p. 359, n. 33. On the relevant aspects of Mr. Keynes's concept of an "elasticity of effective demand," see what is said below, pp. 694 ff.) And, in the second place, certain definitions of "income-" or "circuit-velocity" were specifically couched in such terms as to make it clear that the numerator of the ratio representing this "velocity" was *outlay* from income ("demand"), rather than "income" in the sense of income received in the form of money (money payments "into income"). Cf. Volume I, 359, n. 33, and 381, n. 82.

[17] I have italicized the word "formalization" in order to emphasize the fact that, as a matter of doctrinal history, recognition of the existence of important relations between changes in the "quantity of money" and the level of money incomes was implicit in the analysis of a considerable number of "income theorists" who themselves made no formal use of the concept of "income velocity." See Volume I, 306 ff., 317 f., 320 ff., 332 ff., 340 f., and especially 349 ff.

who implied that an emphasis upon the importance of studying movements in money *income* necessarily involves a minimization of the importance of the other term in Mr. Keynes's e_d—namely, the "quantity of money." [18]   It can be shown, however, that these instances represent a departure from the main line of "classical" tradition.[19]   It is not unfair to point out, moreover, that one of the most notable instances, in recent years, of such a departure from "classical" tradition was represented by those parts of Mr. Keynes's *Treatise* in which an attempt was made to *minimize* the importance of changes in the "quantity of money," on the ground, among others, that these changes were of no great importance for the explanation of changes in *income* (and therefore of outlay from income).[20]   To those who accepted uncritically the argument of the *Treatise* on this head, the rediscovery of the proposition that there *may* be very significant relations between changes in the quantity of money, on the one hand, and the level of income and of outlay from income, on the other, would, of course, represent anything but a platitude.   But this would represent a proof, not of the substantive novelty of Mr. Keynes's "elasticity of effective demand," but of the influence on men's minds of those aspects of the argument of the *Treatise* which were least happy, on both the critical and the constructive sides.[21]

Precisely the same thing must be said of the proposition that the relation between changes in the magnitude of the stream of money demand and changes in the quantity of money is not necessarily constant (or, again in Mr. Keynes's words, that the elasticity of effective

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[18] See Volume I, 343 ff., and the references there given to Aftalion (p. 344, n. 1; p. 348, n. 9; and p. 352, n. 18) and Tooke (p. 346, n. 4). It is worth recalling, however, that both Aftalion and Tooke were forced, at some stage in their respective arguments, to admit the existence of an important relation between changes in the "quantity of money" and the level of money "income." See Volume I, p. 352, n. 18, and the references to Tooke given at the end of n. 19 to p. 150, above; cf. also what is said in n. 20, below.

[19] See Volume I, 349 ff., and the references there given.

[20] See Volume I, 348, of the present work; though see also the comment, in n. 9 thereto, on the differences between Mr. Keynes's "practice" and his "preachment" in the *Treatise,* and the forward reference there given to the later discussion, in Volume I, of the *Treatise's* analysis of the "process by which changes in the money stock are related to changes in the level of money incomes" (cf. Volume I, 405). The parallel in this respect between the Keynes of both the *Treatise* and the *General Theory,* on the one hand, and Tooke and Aftalion, on the other, is itself instructive. See note 18, above, and also the following note.

[21] Unhappily, and despite Mr. Keynes's own use of his "elasticity of effective demand," some aspects of the argument of the *General Theory* have tended to perpetuate the unfortunate influnce of the *Treatise* in this respect.   See Volume I, 29 ff., 33 ff., of the present work, and especially the references given on p. 30, n. 56, and p. 34, n. 61; and cf. also Joan Robinson in the *Economic Journal,* XLVIII (1938), 510.

demand is not necessarily equal to unity). Mr. Keynes himself, in suggesting that "in the simpler discussions it seems that ['effective'] demand [must have become] proportional to the quantity of money," did not specify the "simpler discussions" he has in mind.[22] If he means to refer to the writings of crude inflationists who may have argued that a given increase in the quantity of money will result in a proportional increase in income or in outlay from "income," the citations (if they could be found) would be interesting on their own account.[23] They would also, however, be quite irrelevant when used to justify the argument of a book "chiefly addressed" to Mr. Keynes's "fellow economists."[24] It would be necessary, rather, to refer to "simpler discussions" enjoying the support of a considerable body of authoritative opinion among those "fellow economists," in which it was actually argued that the equivalent of Wicksell's "moneyed demand" would be expected to be "proportional to the quantity of money." And the only unequivocal examples of a comparable statement which I have been able to find even in the "simpler discussions" by economists of standing are those in which writers such as Irving Fisher have been *charged with* (but not convicted of) having "confused" the "quantity

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[22] Cf. the *General Theory*, 292.

[23] One of the aspects of such a list of citations which would be not least interesting would be an indication of just where one would draw the line between the "simpler" discussions, on the one hand, and the "more sophisticated" discussions, on the other. One would certainly have thought that the *Money* of Foster and Catchings, for example, would rank as one of the "simpler discussions" of the relation of money to "demand" (see, for example, *Money*, Chapter XVII ["Money in Consumption"], and Chapter XIX ["The Annual Production-Consumption Equation"]). Yet even Foster and Catchings cannot be charged with having assumed that "demand" is "proportional to the quantity of money." There are, to be sure, some unguarded statements in their writings. The statement, for example, that "the flow of consumers' incomes is increased whenever there is an increase in the total volume of money in circulation" (*Money*, 289) is certainly not true as it stands, even though it does not say that incomes (and therefore "demand") will be increased "in proportion" to the "quantity of money." In fairness to Foster and Catchings, on the other hand, it should be made clear that their central position was that although the "expenditure for consumers' commodities ['demand'] . . . depends mainly on the size of consumers' incomes," and "the size of these incomes depends *mainly* on the total volume of money in circulation, . . . this is not the only factor." "The size of consumers' incomes," they insisted, "is also determined in part" by what they called "circuit-velocity," the purpose of the latter concept being precisely to take account of variations in the degree of proportionality shown by changes in the quantity of money, on the one hand, and the total of expenditures on consumers' goods ("demand"), on the other. Cf. *Money*, p. 298 of the second (1924) edition. On the place of the "circuit-velocity" of Foster and Catchings in the history of the income approach, see Volume I, p. 341.

[24] Cf. the Preface of the *General Theory* (page v).

of money" with "money income." [25]   Even these examples, therefore, instead of supporting Mr. Keynes's generalization, prove nothing beyond the fact that the misrepresentation of the work of earlier writers has been, and is, the commonest of literary malpractices.

"In the more sophisticated discussions," on the other hand, continues Mr. Keynes in his attempt to state the substance of received opinion concerning the degree of "proportionality" between "effective demand" and the quantity of money, "we are lost in a haze where nothing is clear and everything is possible." [26]   This is an extremely strong statement.   In any case, when it is applied to the question whether "the

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[25] See, for example, the references to B. M. Anderson and Joan Robinson given in Volume I, 347, n. 7, of the present work. The reader really interested in what Professor Fisher *has* said with respect to the relation between the "quantity of money" and "money income," should consult, for example, Fisher's *Elementary Principles of Economics,* 63, on the need to "distinguish carefully three money items: (1) *money on hand* at an instant of time [the "quantity of money"] . . . ; (2) *the receipt* of money during a period of time which is an example of income . . . ; and (3) *the expenditure* of money during a period of time, which is an example of outgo" (italics Fisher's; contrast the sharp distinction thus made between money *income* and money *outlay* with the treatment of the "Relation between Income and Demand" in Keynes's *General Theory,* as discussed below, pp. 694 ff.). To be sure, even so generally fair and well-instructed a critic as Professor H. S. Ellis has charged a writer of no less stature than Professor Schumpeter with having advanced an equivalent of the proposition that "effective demand" (or "income") will be strictly "proportional to the quantity of *money*" (or, more specifically, that the "efficiency" of money—that is, its "income velocity"—will be "absolutely fixed in the nature of the case"). See Ellis's *German Monetary Theory,* 132 ff. But (subject to correction by Professor Schumpeter himself) I must say that the passages cited by Professor Ellis in support of his charge leave me unconvinced as to Professor Schumpeter's guilt (in this connection, see the reference to Schumpeter given in Volume I, 365, n. 51, of the present work). It may be admitted also that occasionally even so generally careful a writer as Mr. Hawtrey has used a type of *expression,* with respect to the "proportional" relation between the "aggregate of money incomes" and the "stock of money" (cf. the references to Hawtrey's *Good and Bad Trade,* in Volume I, 351, n. 15 of the present work) that can be characterized only as misleading. I cannot see, however, how anyone really conversant with Mr. Hawtrey's analytical system could believe that anything more than a matter of "expression" *is* involved in these cases. Cf. the comment, in Volume I, 351, n. 15, on an analogous instance of unfortunate exposition in Mr. Hawtrey's writings. In this connection, cf. also what is said below, p. 691, n. 31, concerning Mr. Keynes's own proposition that "if we have a short period of time in view . . . we can treat [income-] V as nearly enough constant."

[26] *General Theory,* 292. The reader will observe that we are here testing the validity of one of the specific charges upon the basis of which Mr. Keynes is prepared to reject virtually the whole of received doctrine on the subject of the Theory of Money and Prices. Cf. Volume I, p. 1, of the present work.

more sophisticated discussions" have concluded that "demand" is "proportional to the quantity of money," it is entirely misleading. That this is so will become clear if, as in the case of the application of the concept of unitary elasticity to the demand for money, we translate the issues involved into concepts long familiar within *monetary* theory.[27]

The particular concept of monetary theory which, again by Mr. Keynes's own admission, is relevant to the question of the relation between "effective demand" and the "quantity of money" is "income-" or "circuit-velocity." [28] If, therefore, it were actually true that it had been generally assumed that "effective demand" would be "proportional to the quantity of money," we should expect to find the authors concerned insisting that "income-" or "circuit-velocity" would under all circumstances be constant. In fact, of course, they have insisted upon the direct opposite.[29] For, as we saw in Volume I of this work, one of the very *purposes* of the concept of "income-" or "circuit-velocity" is to call attention to the fact that the relation between the "quantity of money" and "income" or "outlay from income" ("effective demand") could *not* be expected to be constant.[30] It was hardly necessary, therefore, for Mr. Keynes not only to suggest that "what we are being taught" leads to the conclusion that the "elasticity of effective demand" must have become equal to unity, but also to reiterate, as if he were facing contradiction, the proposition that "there is no reason to expect that . . . [income-velocity] will be constant." [31]

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[27] Cf. above, pp. 650 ff.

[28] See above, p. 681, n. 5, and p. 687, n. 16.

[29] On apparent instances to the contrary, see what is said above, p. 690, n. 25.

[30] See Volume I, 365, of the present work, and especially the references to Schumpeter and Pigou given in nn. 51 and 52 thereto; also the reference given above, p. 689, n. 23, to Foster and Catchings on the concept of "circuit-velocity."

[31] *General Theory*, 201, 299. Not a little irony attaches to the fact that, in the very same paragraph in which he first states this proposition, Mr. Keynes informs us that "if we have a short period of time in view and can safely assume no material change in any of these factors" which are held to determine the magnitude of income velocity, "we can treat [income-] *V* as nearly enough constant." For the validity of the assumption of a "constancy" of income-velocity over even "a short period of time" depends entirely upon whether we "can safely assume no material change" in the factors which affect it; and since it is anything but clear that we *can* make such an assumption in all cases, *even* for "a short period of time," it follows that if we were to apply to Mr. Keynes the same canons of criticism that he has applied to other users of the concept of "income-velocity," we should have to hold him responsible for arguing that "effective demand" will be "proportional to the quantity of money" in a greater degree than most sponsors of the concept of "income-velocity" can be held responsible for having advanced such an argument. Cf. also p. 307 of the *General Theory*, where it is suggested that over a *long* "period of time," there is a "stable proportion between the national income and the

On the other hand, if Mr. Keynes, in suggesting that the "more sophisticated" discussions of the relation between the "quantity of money" and "demand" have resulted in the conclusion that "everything is possible," means to say that the consensus of informed opinion would hold that the relation of changes in "effective demand" to changes in the quantity of money may assume any one of a number of degrees of proportionality or disproportionality all of which are equally "possible," he is of course correct.[32] This, however, is precisely the conclusion to which he is led by his own positive argument. It is also the conclusion which is summed up by his very concept of "elasticity of effective demand." And of course it is the conclusion which is summed up by the concept of "income-" or "circuit-velocity." Surely, therefore, it is not unfair to suggest that, in insisting upon stating his argument with respect to the relation between the quantity of money and the quantity of "effective demand" in terms of an "elasticity" of this demand, Mr. Keynes has merely provided a further example of the ease with which one can be deceived into thinking that one has succeeded in establishing a proposition (in this case, the proposition that the "elasticity of effective demand" need not be equal to unity) which is somehow in *substantive* conflict with propositions widely accepted by those who did not happen to use the particular *terms* involved in the latter statement (in this case, the term "elasticity of effective demand"). In this

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[total] quantity of money to which the psychology of the public tends sooner or later to revert"—that is, a long run "stability" characterizes the ratio discussed by a number of earlier writers under the head of "income-velocity." Actually, of course, Mr. Keynes is most fairly interpreted as really holding that "there is no reason to expect that it ['income-velocity'] will be constant," *even* over "a short period of time," precisely because he believes that it depends on many factors which are not only "complex," but may be expected, under certain conditions, to be extremely "variable" (cf. the *General Theory*, 299; though see also what is said below, pp. 719 ff.). The ground on which Mr. Keynes is here being criticized is not that he should have suggested that "income-velocity" *may* be constant, over "a short period of time," under certain circumstances. He is criticized, rather, on the ground that, despite his own admission that his "elasticity of effective demand" "corresponds closely" to the concept of "income-velocity," he went on to suggest (1) that the aspect of the concept of "income-velocity" which "obscures . . . the real character of the causation, and has led to nothing but confusion" (*General Theory,* 299) is its implied association with the thesis that income-velocity is always *constant;* and (2) that, in dealing "with the case where income-velocity is not constant," a gain of substance is represented by *substituting* the *term* "elasticity of effective demand" for the old term "income-velocity" (cf. the *General Theory,* 304 f.).

[32] Actually, of course, the abler writers on the subject (such as Mr. Hawtrey) have gone far beyond this simple statement, and have developed an analytical technique the purpose of which is precisely to enable us to determine *why* we get the precise degree of "proportionality" that we do get in any concrete case. On this matter, see what is said below, pp. 713 ff., 719 ff.

respect, indeed, we have a virtually complete parallel to the "contribution" supposed to inhere in a demonstration that the elasticity of the demand for money need not be unitary; for in both cases what was supposed to be an important contribution to the substance of monetary theory as a result of the application of certain devices of the "general" Theory of Value has turned out to be a mere restatement, in a disguised terminology, of propositions long regarded as platitudes within monetary theory itself.

3. *The Obscuring of Issues of Substance.* The argument just presented amounts, obviously, to the contention that Mr. Keynes's "elasticity of effective demand," instead of carrying us beyond analytical results made perfectly familiar by the popularization of the concept of "income-velocity," represents, at best, merely the provision of a new terminology for the restatement of these perfectly familiar results.[33] There are cases, however, in which (in the words of Mr. R. F. Kahn) "terminological innovation" may be regarded as "legitimate."[34] This will be so (again in the words of Mr. Kahn) whenever such "terminological innovation . . . helps us to understand causation."[35] As it happens, Mr. Keynes himself has contended that his term "elasticity of effective demand" is superior to the term "income-velocity" precisely on this ground.[36] It is of some importance to demonstrate, therefore, that, instead of representing a substantive advance in this direction, Mr. Keynes's usage must be said at best to

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[33] It is of some importance to stress the words *"at best."* In this connection, see especially what is said below in Sec. IV (pp. 730 ff.) of this chapter ("A Vista Blocked").

[34] See the *Economic Journal*, XLVIII (1938), 334.

[35] *Loc. cit.*

[36] Cf. the *General Theory*, 299, where it is argued that "the 'income-velocity of money' is, in itself, merely a name which explains nothing," and it is further suggested that "the use of this term obscures . . . the real character of the causation, and has led to nothing but confusion"; whereas, it is implied, this cannot be said of his concept of "elasticity of effective demand" (or, as he puts it, the "ratio between the quantity of effective demand and the quantity of money"). In the light of what was said above, pp. 687 ff., nothing further need be said here with respect to the fact that the only reason adduced by Mr. Keynes, even by implication, for his dissatisfaction with the term "income-velocity" is its alleged carrying of the connotation that this "income-velocity" is "constant" (see again the *General Theory*, 299, 304 f.). It *is* worth pointing out, however, that the confusion which was engendered by Mr. Keynes's various statements of his position with respect to the concept of "income-velocity" in his *General Theory* can hardly be said to have been lessened by his later statement that he finds "the term 'income-velocity of circulation'" one under which certain "business, banking, and personal technique[s] and habits . . . *are conveniently summarized,*" particularly since he gives no indication as to *which variant* of the concept of "income-velocity" it is, with which he is thus willing to make his peace (see the *Economic Journal*, XLVIII [1938], 322; italics mine).

obscure substantive issues associated with "causation" in *precisely the same degree in which this can be said to have been true of certain variants of the concept of "income-velocity" and the corresponding variants of the "income approach" generally.*[37]

i. *The Relation between "Income" and "Demand."* No instructed student of monetary theory would deny that the concept of money "income" derives its chief importance, for a wide range of analytical purposes, from its association with the element of money Demand.[38] Such a student, to be sure, could hardly regard this proposition as a novelty; for, as we have seen, the association of the two has been a cardinal element in the best variants of the "income approach" to the problems of monetary theory that have come down to us.[39] The

[37] In the light of what is said above, p. 678, n. 2, it should be evident that there are aspects of Mr. Keynes's concept of an "elasticity of effective demand" which can be said to have obscured issues of substance in even *greater* degree than this can be said of most versions of the concept of "income-velocity." See, for example, what is said above, p. 678, n. 2, with respect to the uncertainty of the relation between "expected" and *realized* "effective demand" (*D*); and cf. also the remarks of Mr. Robertson, "Some Notes on Mr. Keynes' General Theory of Employment," *loc. cit.*, 168 ff., on the *General Theory's* failure, in this instance, to take "full account of the potentialities, for good as well as evil, of that contrast between the realized and the expected which, at some moments 'vital for causal analysis,' at others seems forgotten" (p. 170 of the article cited; cf. also below, p. 696, n. 42). The obscurities of the *General Theory* on this point, moreover, can hardly be said to have been removed by the further ambiguity pointed out above, p. 678, n. 2: namely, that deriving from the fact that although "effective demand" is often identified outright with all values of *D*, it is *formally defined* as coinciding only with the *particular* "value of *D* at the point of the aggregate demand function where it [the 'aggregate demand function'] is intersected by the aggregate supply function." For even if the latter statement were interpreted to mean that "effective demand" refers only to that demand which "becomes effective" (cf. the *General Theory,* 55) in the sense that its *realization* is made possible by the coincidence of aggregate demand price with aggregate supply price, we are still confronted by those explicit statements in the *General Theory* according to which "effective demand" is not only defined as an "expected" magnitude, but is actually *contrasted with* "realized" "income." It is difficulties of this kind which lead one to the conclusions stated at the end of p. 679, n. 2; and what follows in the text above is based upon the assumption that these conclusions will be accepted for purposes of the present argument.

[38] I have in mind here, of course, the relations between Income and "aggregate" (or "general") Demand. On the rôle of Income in the determination of the demand for particular commodities, see above, pp. 202 ff.

[39] It should be sufficient here to remind the reader of what has been said in this work concerning this aspect of the analytical structures of Tooke, Wicksell, Schumpeter, and Hawtrey, respectively. On Tooke, see Volume I, 311, 314 (including the references given in n. 33 thereto). On Wicksell, see Volume I, 326 f., and the references given on p. 327, n. 75; also above, pp. 95 ff. On Schumpeter, see above, p. 117, and especially

important point to be made, however, is that it is one thing to say that important analytical connections exist between two magnitudes; it is quite another to adopt a terminological usage which is such as to *identify* these two magnitudes. For the issue involved in any choice between two terminological systems on the basis of the relative amount of help which each provides in "understanding causation" is precisely whether it is wise to *identify*, by *definition*, two terms which, in ordinary language, refer to *things* that are not only substantively different, but also stand to each other in a type of *analytical relation in clock time* which can be shown to be of the utmost importance for an understanding of the steps involved in an unfolding economic process.

From Volume I of this work, we know it to be precisely a characteristic of certain variants of the so-called "income approach" generally, and of certain variants of the concept of "income-velocity," in particular, that they have suffered precisely from a failure to distinguish adequately between "Income" received in the form of money, on the one hand, and *Outlay* from that income ("Demand"), on the other.[40] If, therefore, Mr. Keynes had a clear foundation for his claim, at one point in his *General Theory*, to have introduced an emphatic *"contrast"* between effective demand and income"—a distinction which, at this point in the *General Theory*, he was prepared to regard as *"so vital for causal analysis"*—the *General Theory's* treatment of the relations between "Demand" and "Income" could be regarded as representing a definite step in advance, as compared with the treatment one found in certain variants of the concept of "income-velocity" current at the time the *General Theory* was published.[41]

n. 65 thereto. On Hawtrey, see above, pp. 120 ff., and especially the references given on p. 120, n. 73. If one were to go outside this list of writers of established standing, it would of course be extremely easy to cite additional instances. See, for example, what is said in Volume I, 334, and especially n. 103 thereto, on the treatment of "income" and "demand" by N. Johannsen.

[40] For an example of a variant of the "income approach" which is open to criticism on this ground, see Volume I, 354 ff., of the present work, on Aftalion; also what is said on pp. 379, n. 76; 404, n. 39; and 412, n. 58 of that volume, concerning the implications of the expression $E = M_1 V_1$, as used in Keynes's *Treatise*. On the treatment of the relation between income and outlay from income by certain sponsors of the concept of "income velocity," see Volume I, 359, n. 33; 364, 379 ff. It should hardly be necessary to remind readers of Volume I, on the other hand, that not all earlier writers can be charged with having failed to face the problem of the relation between *Income,* on the one hand, and *Outlay* from that income, on the other. The most notable instance to the contrary is provided, of course, by Hawtrey. See Volume I, 354 f., 407 f., and the references to Hawtrey given on p. 354, n. 21, and 359, n. 33. Cf. also the references to Pigou's *Theory of Unemployment* given in Volume I, 381, n. 83.

[41] For the claim indicated, see the *General Theory*, 78 f. It is something of a commentary upon the relation of this claim to Mr. Keynes's general analytical system that, as Mr. Robertson has remarked ("Some Notes on Mr. Keynes' General Theory of Employment," *loc. cit.,* 169, n.

Unfortunately, however, the basis for such a claim is precisely what must remain a mystery, in the light of at least three indisputable facts.[42] The first fact is that the *algebraic* usage of the *General Theory*, instead of being such as to *"contrast"* Income (Y) with "Effective Demand" (D), is often such as virtually to *identify* the two.[43] The second fact is that this algebraic usage is corroborated by the repeated presentation of verbal propositions in which "Income" is again virtually identified with "Demand." [44] The third fact (and the most impressive one) is a

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2), the claim itself appears quite "suddenly," and without adequate preparation of the reader. See also the following note.

[42] It should be observed, in addition, that commentators upon the *General Theory* have found Mr. Keynes's very statement of his claim so ambiguous that they have ventured to interpret it in such a way as to make it mean something quite different from that distinction between realized money income and realized money outlay from income the importance of which is stressed here. Mr. Robertson, for example, interprets Mr. Keynes's claim to have introduced a sharp "contrast between effective demand and income" as meaning nothing more than a contrast between *realized* income and *expected* income ("Some Notes, etc.," *loc. cit.,* 169 f.); and it is noteworthy that Mr. Keynes himself has not disavowed this interpretation of his claim. It should be observed, in any case, that Mr. Robertson's interpretation runs afoul of those passages in the *General Theory* (see above, p. 678, n. 2) according to which (1) "effective demand" (D) could mean only *realized* "effective demand"; and (2) "income" (Y) is explicitly described as referring to *expected* income. Indeed, less difficulties would be created for an interpretation of Mr. Keynes's claim if it were interpreted as involving a "contrast" between realized money income, on the one hand, and realized effective demand, on the other, in the sense of the amount of total expenditure out of that income upon *consumption*—in other words, as involving a reference to Mr. Keynes's "psychological law" with respect to the "propensity to consume" (see, for example, the *General Theory,* 29 f.). But since Mr. Keynes explicitly includes in his "effective demand" (*General Theory,* 29) not only the amount of expenditure (actual or expected) upon *consumption* (D_1), but also upon "new investment" (D_2), this interpretation brings a further set of difficulties of its own. If, finally, one were to take literally the expression $D:Y = M:M_1$ (cf. above, p. 678, n. 2), it might be possible to argue that Mr. Keynes's "contrast between effective demand and income" had reference to those aspects of the phenomenon of "liquidity preference" which are summed up by the difference between M and M_1 or M_2 (see the *General Theory,* 199). This, of course, would be a suggestion of genuine substance (cf. what is said on this matter in the following paragraph of the text). It could still not be said, however, to remove the difficulties raised in the following sentences of the text.

[43] See above, p. 678, n. 2.

[44] For evidence of such virtual identification, one need not go beyond (1) Mr. Robertson's suggestion (not rejected by Mr. Keynes) that the "contrast between effective demand and income" has to do with the contrast between realized *income* and expected *income;* and (2) the fact that actually Mr. Keynes's usage, in this instance, is not consistently such as to distinguish sharply between expected and realized magnitudes. See note 42, above; and cf. also below, p. 704, n. 66, and p. 730, n. 126.

fact that must be familiar to anyone who has followed the discussion in our technical journals ever since the appearance of the *General Theory:* namely, that members of the Keynesian group have regarded, as one of the great advantages of the terminological apparatus of the *General Theory,* that it insists that no individual can make a change in the level of his *disbursements* out of income ("demand"), without affecting the "income" receipts of some other individual, so that we may say that Income and Outlay from Income (Demand) are simply the same magnitudes looked at from the standpoint of a disburser of a money payment and the recipient thereof, respectively.[45]

Each reader must decide for himself just how much of an achievement is represented by the latter type of proposition, particularly when its novelty is tested in the light of (1) the endless recurrence, in discussions of alleged "uselessness" of "stream" equations of the general form $MV = PT$, of the proposition that one of the very few things we are told by such equations is that "money spent must be equal to money received"; and (2) the emphasis, in descriptions of the economic "circuit" from the Mercantilists and the Physiocrats to Walras and Schumpeter, on "the dependence of the receipts of one person on the expenditures of others."[46] Each reader must decide for himself, also, between the respective merits of an analytical system, on the one hand, which chooses, as in the case of Mr. Keynes's *General Theory,* to deal with *discrepancies* between the "income" of one individual and the *subsequent* disbursements of *that individual* in terms of concepts such as "liquidity preference" and certain forms of "leakage"; and an analytical system, on the other hand, which chooses to deal with these discrepancies either in terms of the homely Marshallian proposition (itself of very great antiquity) that "though men have the power of purchase they may not choose to use it," or in the more formal terms of a discrepancy between

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[45] See again Lerner, "Some Swedish Stepping Stones in Economic Theory," *loc. cit.,* 575, where this type of proposition is actually juxtaposed with the proposition that "the total income of society is equal to the total demand for goods and services, or the amount of money spent on them." A statement of the latter type, which avowedly purports to summarize the relevant aspects of the argument of the *General Theory,* can hardly be regarded as presenting an emphatic "*contrast* between effective demand and income." On the objections to the statement itself, see what is said below, pp. 704 ff.

[46] The latter statement is quoted literally from the discussion, by E. von Bergmann, *Geschichte der Nationalökonomischen Krisentheorien* (1895), 3 ff., of the fact that "the authors of the seventeenth century, in viewing economic life . . . emphasized that the expenditures of one group constitute the receipts [*Einnahmen*] of another, so that a close and necessary connection is established between economic units." On the rôle of this proposition in the development of the concept of an "economic circular flow" *(Wirtschaftskreislauf),* see what is said above, pp. 352 ff.; and on the use of the proposition that "money spent must be equal to money received" in discussions of the familiar Quantity Equations, see Volume I, 90 ff., of the present work.

Saving and Investment (as in the case of Mr. Robertson), or of a discrepancy between "consumers' income" and "consumers' outlay," and the relation of such a discrepancy to cash-balance administration (as in the case of Mr. Hawtrey).[47]   What must be clear is that if any aspect of the *General Theory* does emphasize the "distinction, so vital for causal analysis," that is represented by a "contrast between effective demand and income," it is certainly not the *General Theory's* formal treatment of the relation between "Effective Demand" and "Income." For proof that this is so, indeed, one does not have to go beyond the statements of defenders of the *General Theory* itself. For it is they who have seen virtually nothing in the distinction between "payments into income" and "payments out of income" (effective demand, in one of the most significant senses of the term) but a "tiresome question of definition." [48]   And it is they who have argued that in any case the "difficulty" with which this "tiresome question of definition" has been concerned is one that "does not arise for long-period theory," and therefore cannot seriously disturb the Olympian vision of those for whom, despite their claim to be concerned precisely with "processes of change," no interest attaches to a tracing of the successive steps of "processes of change" realized, in the "short period," in the world we know.[49]

ii. *"Demand," the Money Value of "Real Income," and "Proceeds from the Sale of Output."*   The parallel between the treatment of the relation between "Income" and "Demand" in the *General Theory,* on the one hand, and the less satisfactory variants of the "income approach"

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[47] On Hawtrey, see again the references given above, p. 695, n. 40. A full discussion of the concept of "leakages" must be left for a later treatment of further aspects of the problem of the Generation and Utilization of Money Income; and the same thing must be said of the concept of "liquidity preference" as used in this context (cf. above, p. 696, n. 42). On the "homely Marshallian proposition" referred to in the text, see above, p. 616, n. 132. A detailed presentation of the evidence for the contention that this proposition is in fact one of "very great antiquity" would require far too much space for it to be included here. It is sufficient here to call attention to the emphasis by eighteenth-century writers, as summarized by von Bergmann, *Geschichte der Nationalökonomischen Krisentheorien,* 7 ff., upon the fact that "expenditures need not occur precisely at the same moment as the receipts [out of which the expenditure is made]," with the result that "the whole progression" will be "interrupted" (Bergmann, *loc. cit.*).

[48] Cf. the passage cited above, p. 616, n. 131, from B. P. Whale. Since Mr. Whale's comment was evoked by the argument, presented in Volume I of this work, for distinguishing sharply between "payments out of income and payments into income," the reader's attention is invited particularly to pp. 354 ff., 379 ff., 404, and 431, n. 50, of that volume.

[49] See again the passage from Mr. Whale cited above, p. 616, n. 131. Actually, of course, it is anything but clear that the "difficulty" in question "does not arise for long-period theory," if what is meant by the latter proposition is that "payments out of income" (or money payments of any kind) "must finish up as *somebody's* income." See below, pp. 704 ff.

and of the concept of "income-velocity," on the other, extends, moreover, to the methods and assumptions which have been used to justify expressions of the general form $D = Y = OP$, or their equivalent.

One of these methods, for example, is to identify "money income" with OP, or its equivalent, on the ground that OP measures the money "value" of "current output," or "real income." [50] The stream of money payments ("Demand," in one sense of the term) used to purchase this "real income" may then be regarded as representing "payments into income" (Y), in the sense that they "enter into" the *real* income" (OP) of the *disburser of the money funds* used to purchase this "real income." If the set of concepts involved is rigorously adhered to throughout, there can, of course, be no greater objections to it on purely *formal* grounds than there can be to any conceptual apparatus which is internally consistent and is strictly followed in application.[51] Again, however, it must be remembered that the basis for choice between two sets of conceptual apparatus is not their respective degrees of formal internal consistency, but the extent to which each set of concepts "helps us to understand causation"; and from this point of view,

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[50] Cf. the *General Theory,* 38, where "money-income" is identified with "the *value* of output," which in turn is contrasted with "the *volume* of current output or real income"; and see the comments on the *General Theory's* treatment of the relation between "income" and "output" in Hawtrey, *Capital and Employment,* 174 ff. From Mr. Keynes's reference to Pigou in the context first cited, one might suppose that the definition of "money income" thus presented is one that has been consistently sponsored by the latter. See, however, Pigou's *Theory of Unemployment,* 192 f.; and cf. also the reference to Pigou's *Industrial Fluctuations* given in Volume I, 381, n. 81, of the present work. It cannot be denied, on the other hand, that definitions of "money income" similar to that quoted from the *General Theory* have been employed, explicitly or implicitly, in the writings of certain protagonists of the "income approach" in general, and of "income-velocity," in particular. See, for example, the references to Schumpeter in Volume I, 376, n. 70, 379, n. 76; and see also the comments on Robertson's use of the term "income" in Volume I, 380 f., and the references to Robertson given in nn. 78, 79, 81. (From these references, it should be clear that the statement of Professor Haberler [*Prosperity and Depression,* 178] that "Professor Robertson and others use 'income' in the sense of actual money income involving monetary transactions (a transfer of money)" is—unfortunately—less accurate as applied to Robertson than to "others"; and it should be clear also that, as a result, Professor Haberler's justified warning against assuming that "actual money income," as so defined, is necessarily "quite the same thing as income in the sense of the money value of the output as a whole," is less applicable to certain of these "others"—such as Pigou—than it is to Robertson.)

[51] It is worth observing here that, unfortunately, not all of the writers who have made use of this type of conceptual construction have in fact shown the same degree of care in adhering rigorously to the terms of the construction itself. In this connection, contrast, for example, what is said with respect to Schumpeter's usage in Volume I, 379, n. 76, with what is said with respect to that of Robertson, *ibid.,* 381, n. 81.

it is anything but clear that the type of conceptual apparatus indicated is to be put on a par with that type of apparatus, for example, for dealing with the processes involved in the generation and utilization of money income which, for want of a better term, may be characterized as "Hawtreyan." [52]

Specifically, the objection to the former type of apparatus is that it leaves us completely unenlightened as to the nature of the forces which make the level of *money* payments "into income" (in the sense of a sum of money payments received as income) as large as they are.[53] When we are told the magnitude of the "payments into" the *real* income of the disburser of the money funds, we are told neither (1) how large was the *money* "income" (in the literal sense of money payments received as income) out of which these disbursements were made; nor (2) how much money income (again in the sense of money payments received as income) was realized by the *recipient* of this disbursement.[54] The latter shortcoming is obviously the crucial one; for *if* it could be assumed (i) that the amount of money disbursed by one set of individuals (the "demanders") represented, in its entirety, "income" to another set of individuals (the sellers of "current output"); and (ii) that the sales of current output represented the *only* source of money income, we should then have a method for ascertaining the magnitude of *money* income in the sense indicated above under (1).[55]

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[52] That the type of apparatus in question (which is also the type of apparatus sponsored in the present work) does deserve the appellation "Hawtreyan" would be denied by no one aware of the profound debt which all contemporary monetary theory owes to Mr. Hawtrey's labors in this field. To say this, of course, is not to deny that the separate *ingredients* of the "Hawtreyan" apparatus go far back in economic literature. See, for example, Volume I, 312, 314, 317, 333 f., 340, 407. Nor is it to say that there are no respects in which Mr. Hawtrey's own analysis or exposition, for all their brilliance and power, are incapable of amendment or further development at certain points. See Volume I, 341; 351, n. 15; 376, n. 70; 382 f. The point is merely that, as a simple matter of doctrinal history, Mr. Hawtrey's contributions, when judged from the standpoint of articulation and comprehensiveness, go so far beyond those of any other single writer on the matters under discussion that he can hardly be said to have a serious rival in the field.

[53] See Volume I, 379 f., of the present work, and 354 f. of the same volume; also nn. 54 and 55, immediately following.

[54] The "payments into" "real income" are to be represented, by definition, as equal to $(OP)_{s \cdot t_n}$, in which the subscript s represents the amount of output "*sold*" in the period t_n (see Volume I, 133 ff.; and cf. also what is said below, p. 702, at the end of n. 57). What is desiderated under (1), on the other hand, is the magnitude of $(PT)_{I \cdot t_{n-1}}$; and what is desiderated under (2) is the amount of the contribution to $(PT)_{I \cdot t_n}$ which is represented by $(OP)_{s \cdot t_n}$.

[55] If we assume that the amount disbursed (D) by the "demanders" is disbursed entirely out of "income," it would be represented, in our notation, by the expression $M_i V_{i \cdot t_n}$: this first assumption being equivalent to the

It is at this point that we find the most striking parallel between the apparatus presented in the *General Theory*, on the one hand, and certain variants of the income approach, on the other.[56] For in both cases the method adopted for resolving the difficulty was simply to *assume* two propositions that must be demonstrated if we are to accept the implications of the expression $OP = Y$: namely, (a) that the *only* source of money income (Y) is the sale of current output (OP); and (b) that the sales proceeds of current output represent, in their entirety, "income" to the recipients of these proceeds.[57] Yet it is precisely these

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assumption that $M_{ni}V_{ni}$ is equal to zero (cf. Volume I, 369 f.). The further assumption indicated under (i), however, demands also that $M_iV_{i \cdot t_n}$ be equal to $(PT)_{I \cdot t_n}$; and this in turn would require that not only $M_{ni}V_{ni}$, but also $(PT)_{NI \cdot t_n}$ would be equal to zero (cf. Volume I, 383 [equation 8]). Similarly, the assumption indicated under (ii) demands that $(OP)_{s \cdot t_n} = (PT)_{I \cdot t_n}$; and this requires that all possible components of $(PT)_{I \cdot t_n}$ other than $(OP)_{s \cdot t_n}$ be equal to zero. If, on the other hand, we *were* justified in making these assumptions, we could write expressions comparable to $M_iV_{i \cdot t_n} = (OP)_{s \cdot t_n} = (PT)_{I \cdot t_n}$ for each clock-time period, including the period t_{n-1}; and we should then have obtained the value of $(PT)_{I \cdot t_{n-1}}$, which is the sense of "money income" indicated under (1). It will be observed that, even when we are given the magnitudes of $(PT)_{I \cdot t_{n-1}}$ and $M_iV_{i \cdot t_n}$ (which is assumed to be equal to $(PT)_{I \cdot t_n}$), we are still faced with the problem of establishing an analytical relation between the two On the various devices proposed for dealing with this problem (obviously crucial for the central element in the "dynamics" of the process of income generation, which must be concerned with the analytical relations between the magnitudes attained by the significant variables in *successive time periods*), see what is said above, p. 697 f. For the type of device sponsored in this work as a method for dealing with this problem, see Volume I, 382, n. 85, and 383, n. 88; and cf. above, pp. 366 f. and 485 ff.

[56] The closeness of the parallel will be evident if it is observed that the expression $Y = OP$ is the exact algebraic equivalent of "income equations" of the type of Aftalion's $R = PQ$ or (if the "real income" involved is confined to the volume of *consumers'* goods) Lindahl's $E(1-s) = PQ$. Cf. Volume I, 328, n. 78, and 339, n. 112. In the present instance, of course, we are concerned with the implications of the expression $Y = OP$ when it is read as $OP \rightarrow Y$. Cf. the reference to Aftalion given in Volume I, 130, n. 65. But it should hardly be necessary to labor the point that the very fact that there is some ambiguity as to how the expression $OP = Y$ *is* to be "read" itself provides a commentary on the relative precision of an apparatus, on the one hand, which provides no continuing analytical control over such expressions, and an apparatus, on the other hand (such as that sponsored in the present work), which by the use of expressions of the type M_iV_i and $(PT)_I$, and of clock-time period subscripts, can be said to provide just such a continuing control at all steps in the analysis.

[57] In terms of our own notation, these assumptions amount to supposing (a) that $(PT)_I$ contains no items other than $(OP)_s$—that is, that there are no "non-output" components of $(PT)_I$; and (b) that $(OP)_s$ contains no other items than $(PT)_I$—that is, that $(OP)_s$ contains no elements that

two propositions whose general validity is open to serious question.

It is anything but clear, in the first place, that the sale of current output is the *only* source of money income.[58] It is clear, on the contrary (as Mr. Hawtrey has pointed out) that "there are [money] incomes which correspond to no output."[59] We have no reason to assume that these money incomes which "correspond to no output" cannot in any case represent net additions to the total money spending power, and therefore the "effective demand," of the community. Nor should it be necessary to labor the point that this difficulty becomes increasingly serious in the degree that we are prepared to contemplate (as Mr. Keynes and his group are certainly prepared to contemplate) the use of government spending, and particularly government spending financed by inflationary methods, as a means of affecting the level of "Income" and thereby "Effective Demand."[60]

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are properly to be included under $(PT)_{NI}$. See above, p. 700, n. 55. I am of course abstracting here from the further difficulties raised by the fact that not *all* of the "output" of a given period may be *sold* within that period. See Volume I, 133 ff.; and cf. what is said at the end of n. 56, immediately preceding, with respect to the bearing of such matters on the relative merits of the respective sets of apparatus under discussion.

[58] It should again be clear that there can be no formal objection to *defining* "money income" in such a way as to *exclude* "all money receipts other than those made against [that is, received in payment for] . . . services" of the "factors of production." See, for example, Pigou, *The Theory of Unemployment,* 191 f. The point of the present argument, however, is precisely that not all sponsors of expressions of the type $OP = Y$ have either (1) made clear that they *are* using such a definition of "money income"; or (2) shown an awareness of the fact that the implicit use of such a definition in *one* part of their argument calls for modification of *other* parts of their argument. Cf. nn. 59 and 60, immediately following.

[59] Cf. Hawtrey, *Capital and Employment,* 175. According to Mr. Hawtrey *(loc. cit.),* "upon the methods of treating these incomes Mr. Keynes is silent." It would be more accurate, however, to say that "Mr. Keynes is silent" on the method of reconciling the cases indicated *with the implications of his expression $OP = Y$.* For he is quite explicit in stating that "if changes in M are due to the Government printing money wherewith to meet its current expenditure . . . , the new money accrues to someone's income" *(General Theory,* 200). The difficulty is that he fails to specify, at the same time, that we are to exclude from the "current expenditure" of government those expenditures which cannot by any reasonable stretch of terminological usage be said to be in payment for "productive" services. On other ambiguities, apart from the question of the dole, in the *General Theory's* treatment of what is to be regarded as "output" (and therefore is involved in the question of the relation between $(OP)_s$ and $(PT)_I$), cf. Hawtrey, *loc. cit.*

[60] It should be clear that, insofar as the "government spending" involved *is* "financed by inflationary methods," it is hardly open to us to adopt either of the two methods suggested by Mr. Hawtrey *(Capital and Employment,* 175) as a method for helping Mr. Keynes out of his dilemma: namely, the method of saying (1) that the "income" of "an old age pensioner" is not "income" at all (on the ground "that the old age pensioner

The respect, however, in which the apparatus of the *General Theory* represents a really serious retrogression as compared with, say, a developed "Hawtreyan" technique for tracing the processes involved in the generation and utilization of money income, has to do with the second of the assumptions indicated above: namely, that the proceeds from the sale of current output represent, in their entirety, "income" to the recipients of these proceeds. It is, of course, a cardinal point in the "Hawtreyan" technique that only a part (and by no means necessarily the greater part) of these proceeds will represent "income" to the first recipient of these proceeds; the rest, it is argued, will represent "traders' receipts," or "traders' turnover," the *relation of which to the subsequent generation of money income must be separately traced.*[61] Yet it can be shown that it is precisely this point which is obscured by the *General Theory's* treatment of the relation between "income," "effective demand," and sales "proceeds."

Mr. Keynes, to be sure, defines "the income of the *entrepreneur*" as being equal, not to the total of his sales proceeds, but as being equal to "the *excess* of the value of his finished output sold during the period *over his prime cost.*"[62] It must be remembered, however, that the

is merely an agent for spending part of the income of the taxpayer"); or the method of saying (2) "that the income of the old age pensioner is a charge upon that of the taxpayer, whose income is diminished by that amount." It should be equally clear that the net effect of the third method suggested by Mr. Hawtrey—namely, that of saying "that the old age pensioner's income is paid to him for a fictitious output"—would be to deprive the concept of "output" of all realistic meaning, and to destroy a large part of whatever significance would otherwise attach to the expression $OP = Y$. The difficulty in question, characteristically enough, was one that had been raised by critics of the *Treatise* (see Volume I. 131, n. 69, of the present work, and the references there given), and properly so; for it is a difficulty which must attach to any use of the term "Investment" as a substitute for a frank concern with the causes and consequences of changes in the dimensions of streams of *money expenditure*. See Volume I, 279 ff., of the present work; and cf. what is said above, p. 476, n. 29, with respect to the analogous problems raised by the use of the term "Investment" in the *General Theory*. It would be laboring the obvious to stress the relevance of the general point involved for those defenses of government spending which rest upon the tacit assumption that all of such spending, regardless of the tangible "real" results directly obtained, represents "investment," and that every recipient of government bounty is to be regarded as contributing directly to "output."

[61] This is, of course, one of the aspects of the "Hawtreyan" technique which has had precedents of long standing. See above, p. 700, n. 52, and the references to Volume I there given.

[62] *General Theory*, 53 (italics mine). The differences between Mr. Keynes's "entrepreneurial income" and his "entrepreneurial *net* income" (cf. the *General Theory*, 57), while they are extremely important in other contexts, including contexts relevant for the problem of the generation of money income, need not concern us here. On the question of who is to be regarded as an "entrepreneur," see Hawtrey, *Capital and Employment*, 175; and cf. the *General Theory*, 62.

crucial expression, for our present purpose, is $D = OP = Y$; and that therefore the test of the fitness of the apparatus presented in the *General Theory* for dealing with the elements included in Hawtrey's "traders' receipts" (or "traders' turnover") is its treatment of the relation of the magnitudes D, OP, and Y to each other, particularly from the standpoint of their sequential relations in clock time. "The effective demand" (D), writes Mr. Keynes, "is simply the aggregate income [Y] (*or proceeds*) [OP] which the entrepreneurs expect to receive." [63]   And it is here that we come to the cruical point in the argument. These "proceeds," we are told, "are inclusive of [that is, are made up, not only of the 'income of the entrepreneurs,' but also of] the *incomes which they [the entrepreneurs] will hand on to the other factors of production.*" [64]   That is to say, the reason why we are supposed to be justified in regarding the whole of the "proceeds" from the sale of current output (OP) as entering into "income" (Y) is that these proceeds either represent income to the first recipient, *or will be "handed on" as "income" to others.*

To some of Mr. Keynes's followers, this argument has seemed irrefutable. According to Mr. Lerner, for example, the statement that "the total income of society [Y] is equal to the total demand [D] for goods and services, or the amount of money spent on them [OP]" calls for only very "slight amplification" in order to demonstrate its universal validity.[65] For "while . . . not all receipts can be called income" (since "some of the proceeds from the sale of goods and services may constitute not *income* for the seller but *costs* to him incurred in producing the goods and services"), nevertheless "these costs are paid out to others, and constitute *their* income, or are paid out by them as costs to a third or fourth party, so that ultimately all the payment for the first-mentioned goods and services must finish up as *somebody's* income." It should require only slight reflection, however, to observe that there are at least two very serious objections to dismissing, in this cavalier fashion, the whole series of problems raised by the "Hawtreyan" concept of "traders' receipts."

The first objection, of course, is that nothing in the "sale of goods and services" provides any assurance that the *proceeds* of this sale *will* be promptly disbursed by the first recipient of these proceeds.[66] They may,

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[63] *General Theory,* 55.

[64] *General Theory,* 55. It is clear that it is a proposition of this type which is supposed to justify statements, elsewhere in the *General Theory,* such as that changes in M which are represented by newly mined gold are "directly associated" in their entirety "with changes in Y," on the ground that all of the value of this new gold "accrues as someone's *income*" (*General Theory,* 200; italics mine).

[65] See Lerner, "Some Swedish Stepping Stones in Economic Theory," *loc. cit.,* 575 n.

[66] Cf. above, p. 615, n. 130. It will be observed also that the present instance provides a further illustration of the looseness with which the relation between "expected" and "realized" magnitudes is treated in the *General Theory,* and of the consequences of this looseness (cf. above,

for example, be kept in the form of an "idle" cash balance.[67] And even if they are "disbursed" promptly, they may be "disbursed," not as income to those whom Mr. Keynes calls "the other factors of production," but in the repayment of debt to commercial banks, who may then refuse to lend out the money represented by these proceeds, so that no further income will be generated at all.[68] These are, to be sure, special cases. What matters for our present purpose, however, is that they are *possible* cases. For if they *are* possible cases, then the assumption of Mr. Keynes and Mr. Lerner that sales proceeds *"will be"* "handed on" as incomes to others, itself represents an assumption applicable only in special cases, and not a proposition of the degree of generality required to support the implications of the expression $D = OP = Y$, when the latter is regarded as summarizing the central contention of Mr. Keynes's followers: namely, that we can deny that "the total income of society [Y] is equal to the total demand for goods and services [D], or the amount of money spent on them [OP]," only by denying the obvious truism that "total payments and total receipts are merely different names for the same transactions, distinguishing merely whether they are viewed from the paying or the receiving end." [69]

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p. 696, n. 42). For example, the "aggregate income (or proceeds)" of the entrepreneurs are designated by Mr. Keynes as "expected" proceeds; whereas it is stated that they *"will* hand on" these "expected proceeds" (*General Theory*, 55). The result of this ambiguous usage is of course to leave us in doubt both as to the relative magnitude of, and the time relations between, the *realized* receipt of proceeds and the *realized* "handing on" of money sums equivalent to that part of these "proceeds" which does not directly represent "income" to the entrepreneur. Mr. Lerner's exposition, on the other hand, is such as to suggest that all the magnitudes involved are *realized* magnitudes. Mr. Keynes must, of course, be supposed to claim, for his apparatus, that it is capable of accounting for the successive steps involved in *realized* processes. There can be no objection, therefore, to our assuming, as Mr. Lerner does, that the "sales proceeds" involved are *realized* sales proceeds, and then passing to an examination of the subsequent stages in the realized processes of income generation.

[67] Cf. above, p. 615, n. 130. It should be observed that this type of possibility is fully taken into account in the type of apparatus for dealing with the processes of the generation of money income which is sponsored in the present work. See, for example, what is said on this matter above, pp. 485 ff. From the account there presented, it should be clear also that our apparatus permits the observation and explanation of developments of the type indicated in the text (as well as of the type indicated in the following sentence of the text) at *any* stage in the process of income generation (that is, at the stage represented by the second recipient, the third recipient, and so on), and the *dating* of these developments in clock time, with the consequences, for the magnitude of the amount of money income generated within a given clock-time period, which are indicated in the following paragraph of the text.

[68] Again this is a possibility which is taken fully into account in the apparatus sponsored in the present work. See above, p. 491, n. 68.

[69] See again Lerner, "Some Swedish Stepping Stones," *loc. cit.*, 575.

It is of the utmost importance, moreover, to advance a further contention: namely, that to dismiss the problems raised by the Hawtreyan concept of "traders' receipts" solely on the ground that "all the payments for finished goods and services must *finish up* as somebody's income" is to forget two propositions that ought to have been regarded as axiomatic in all discussions of the generation of money "income." [70] The first proposition is the elementary one that the "amount of income" generated has no meaning unless we specify the *clock-time period* over which this "amount of income" is held to have been generated. The statement, for example, that a billion dollars of "income" has been generated by an initial cash disbursement, without specifying the clock-time period over which this billion dollars was received as income, has no more meaning than would a statement that a given individual receives an "income" of $1,000, without specifying whether that individual receives an "income" of $1,000 per week or $1,000 per year.[71] If this be granted, then a second proposition follows: namely, that it is vital, in tracing the effect of a given money disbursement on the subsequent generation of income, to deal with the factors bringing it about that a given disbursement may represent the generation of money income, not *immediately*, but only after a *considerable lapse of clock time;* and that therefore the "amount of income" generated within a given clock-time period by a series of initial cash disbursements may vary very greatly according to the proportions in which these initial disbursements are divided between direct payments into income (our $(PT)_I$), on the one hand, and payments into "traders' receipts" (our $(PT)_{NI}$), on the other. It is precisely a virtue of an adequately developed "Hawtreyan" technique for dealing with the processes of the generation of money income—with its explicit introduction of the element of "traders' receipts," and its relation of this element to the generation of money income in clock time—that it is perfectly capable of dealing with complications of this kind, in all desired detail and with all desired accuracy.[72] It is precisely a vice of the technique of the *General Theory,*

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[70] See above, p. 615, n. 130. It may be observed in passing that further obscurity attaches to the statement quoted, by reason of its implication that the *only* payments which "must finish up as somebody's income" are payments for *"finished" "goods and services."* The difficulties raised by such an implication are of course the same in kind as those raised by all attempts to derive a figure for money "income," in the literal sense of a stream of "income payments" in the form of money, from a figure representing the value of "output." See above, pp. 702 ff., nn. 59 and 60.

[71] It is to be remembered that users of "stream" equations of the general "Fisherine" form, from the day of Lubbock onward, have pointed out that the terms involved in these equations all refer to a specific length of clock time. See Volume I, 65 (and the references given in n. 69 thereto) and 425 f.

[72] On the designation of the type of technique indicated as a "developed Hawtreyan technique," see above, p. 700, n. 52. On the method employed by this technique as applied to the point under discussion (a method

on the other hand, that it is such as to obscure the issues of substance which are raised by the "Hawtreyan" distinction between payments into income and payments into "traders' receipts." And again one is justified in asking whether these issues would have been obscured if, instead of regarding the substitution of the *term* "elasticity of effective demand" for the earlier "income velocity" as a matter of substantive significance, Mr. Keynes had preceded his analysis of the processes involved in the generation of money income by a critical examination of the substantive limitations attaching to certain of the usages (including the unsupplemented use of the concept of "income velocity") which had already been proposed as weapons for dealing with precisely this problem.

iii. *"Income Velocity" in Keynes's* **Treatise** *versus "Income Velocity" in the* **General Theory,** *in Relation to the Concept of an "Elasticity of Effective Demand."* It is typical of the paradoxes involved in any attempt to evaluate the significance of the *General Theory* for further developments within the Theory of Money and Prices that its argument with respect to "income velocity" can be said to represent a retrogression, not only as compared with the best available in the writings of authors other than Mr. Keynes, but even as compared with Mr. Keynes's own *Treatise,* when both the *Treatise* and the *General Theory* are judged from the standpoint of a critical examination of the type just desiderated, and all that such an examination might have done to lessen the degree to which issues of substance have been obscured by the less fortunate uses of the concept of "income-velocity." It is hardly surprising that this fact should have been hidden from those readers of the *General Theory* who have accepted uncritically its suggestion that the real significance of the concept of an "elasticity of demand" is that it constitutes a new and fruitful "synthesis" between the "general" Theory of Value and the Theory of Money and Prices. It should not be hidden, however, from those readers of the *General Theory* who have realized that neither Mr. Keynes's substitution of the *term* "elasticity of effective demand" for the *term* "income-velocity," nor his continued adverse comments upon certain variants of the *concept* of "income-velocity" can alter the fact that his "elasticity of effective demand" *is* a variant of the concept of "income-velocity" under another name, and is therefore subject to all the tests which should have been applied to received variants of the latter concept.[73] For such readers, on the contrary, the real questions one must ask are (1) whether the particular "minor respects" in which, by Mr. Keynes's own statement, his own *formal* definition of "income-velocity" may be said to differ from the more common variants of the concept of "income-velocity"

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which amounts to a summing of the successive clock-time dated $(PT)_I$'s realized over the period of clock time during which the "income" is held to be generated), see above, pp. 494 ff.

[73] For examples of "continued adverse comments" on the concept of "income-velocity" in the *General Theory,* in addition to the comments cited above, p. 691, n. 31, and p. 693, n. 36, see the references to the *General Theory* given below, pp. 711 f., nn. 85 to 88.

are such as to free his own concept of "income-velocity" from the limi-
tations to which, according to the argument of Mr. Keynes's own
Treatise, most variants of the concept are subject; and (2) what light
is thrown upon his concept of an "elasticity of effective demand," *when
it is judged simply as a variant of the concept of "income-velocity,"* by
the differences between the definition of "income-velocity" *formally*
advocated in the *General Theory* and certain of the earlier definitions
of the concept.[74]

The objections raised by Mr. Keynes, in his *Treatise,* to the concept
of "income-velocity," had to do essentially with the *denominator* of the
ratio measuring this "velocity." [75]   In Volume I of the present work,
it was argued that when our problem is that of *tracing in all required
detail the steps involved in the processes of the generation and utilization
of money income,* it is undoubtedly true that serious limitations upon
the use of most variants of the concept of "income-velocity" have in the
past derived from the nature of its denominator; and it was further
suggested that the very fact that Mr. Keynes based his rejection of
the concept of "income-velocity," in his *Treatise,* primarily upon the
nature of its denominator, would justify the characterization of the
Treatise's argument on this head as an important milestone on the road
to a more adequate understanding of the mechanics of the generation
and utilization of money income.[76]   The further fact, therefore, that
one of the "minor respects" in which Mr. Keynes's own concept of "in-
come velocity" differs from certain other variants of the concept is in
its definition of the denominator of the ratio representing "income
velocity," might lead one to suppose that the argument of the *General*

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[74] For the definition of income-velocity *"formally* advocated" (that is,
advocated in the *name* of a supposed improvement of the concept of
"income-velocity," as opposed to the definition presented under the head
of an "elasticity of effective demand"), see below, p. 709, n. 78.

[75] See Volume I, 388, 395, of the present work, and the references to
the *Treatise* there given.

[76] See Volume I, 402 ff. It is, of course, no minimization of the im-
portance of the relevant aspect of the *Treatise's* argument as a "milestone"
of the type indicated, to point out, as was pointed out in Volume I (403 ff.),
(1) that Mr. Keynes was a "traditionalist *malgré lui*" with respect to the
broad outlines of the solution of the problem; and (2) that in a num-
ber of important respects, Mr. Keynes's analysis had already been sur-
passed by that of Mr. Hawtrey (see Volume I, 407 ff.). It should be
pointed out also that, despite observations to the contrary by commentators
on my first volume, I did *not* follow Mr. Keynes in arguing that the con-
cept of "income-velocity" is of "no particular significance" for certain broad
problems of monetary theory (cf. Volume I, 388). On the contrary, I
argued only that the *unsupplemented* use of the concept of "income
velocity" is inadequate for the solution of the problem indicated by the
words italicized in the text. Cf. above, p. 476, n. 30, and the references to
Volume I there given.

Theory on this head provides one of the instances in which it is possible, despite superficial appearances to the contrary, to find evidence, not of "a confusing change of view" as between the two works, but "a natural evolution in a line of thought" common to both.[77]

Unfortunately, however, the particular amendment now proposed by Mr. Keynes to the concept of "income-velocity"—namely, the elimination, from its denominator, of that part of the "quantity of money" which is held to satisfy the "speculative motive"—does not represent further progress in the direction adumbrated by the argument of the *Treatise*.[78] On the contrary, it represents a parallel to certain suggestions made by other writers prior to the appearance of the *General Theory*, including some suggestions which were intended to make the concept of "income-velocity" impervious to the criticisms advanced in

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[77] Cf. the Preface (p. vi) to the *General Theory*. The reader is again reminded that, in what follows, I am discussing the *General Theory's* treatment of the concept of "income velocity" *expressis verbis,* and not the definition of "income-velocity" presented under the head of an "elasticity of effective demand." Cf. above, p. 708, n. 74; also below, pp. 723 ff.

[78] The proposed "amendment to the concept of income-velocity" is to be found on p. 201 of the *General Theory*, where Mr. Keynes, having remarked that "it is not always made clear whether the income-velocity is defined as the ratio of Y to M or as the ratio of Y to M_1," announces that he proposes to use the concept of income velocity "in the latter sense." On the meaning of M_1 and M_2 in the expression $M = M_1 + M_2$, see again p. 199 of the *General Theory*. For the argument which follows in the text, it is of some importance to observe that the formal definition of "income-velocity" thus presented (it is designated as the formal definition of the concept in the Index [p. 396] to the *General Theory*, under "Money—income-velocity of") is in conflict with the comment on p. 195 of the *General Theory*, where it is argued that the only "connection" in which the "concept of the income-velocity of money is strictly appropriate" is that in which, for the purposes of expressing a "velocity," *"income"* is related directly to the volume of deposits *held to satisfy the "income-motive."* The latter comment, indeed, comes much closer to the position of the *Treatise*, according to which only *"income* deposits" were to be related directly to "income" in terms of a "velocity" concept (see again Volume I, 395, of the present work, and the reference to the *Treatise* there given; though see also what is said on this matter below, p. 711, n. 87). The definition of "income-velocity" *formally* sponsored in the *General Theory*, on the contrary, specifically includes in its denominator the cash held to satisfy the "business motive," which in turn had been *distinguished* not only from the "speculative motive" (*General Theory,* 196 f.), but also from that "income-motive" to which alone, according to Mr. Keynes on p. 195 of the *General Theory*, the "concept of the income-velocity is strictly appropriate." The careful reader of the *General Theory* should have no difficulty in observing the source of this particular confusion: it derives, in large part, from the virtual identification of "the value of current output" (with which the "business motive" is held to be associated) with "current income" (cf. the *General Theory,* 196). See below, pp. 712 ff.

the *Treatise,* but which can be shown to have missed one of the central issues raised by those criticisms.[79]

That issue is the possibility of improving our methods for dealing with the range of phenomena covered by the concept of "income-velocity" by subjecting to separate examination two distinct sets of phenomena: namely, (1) those phenomena of monetary "velocity which are amenable to direct explanation by the use of the *"cash-balance approach"* as applied to the problem of the forces determining the size of cash balances held *relative to outlay;* and (2) phenomena which must be regarded as falling under the head of changes in specific components of the Fisherine T.[80] Mr. Keynes's *Treatise* can hardly be said either to have stated this issue with all possible articulateness or to have provided a detailed solution, based upon the implications of this issue, which could be said to be superior to the best type of apparatus for tracing the processes involved in the generation and utilization of money income available at the time the *Treatise* was published.[81] It did, however, point the way to such a solution. And this is precisely what cannot be said of the particular amendment to the concept of "income-velocity" now proposed in the *General Theory.* On the contrary, the concept of "income-velocity" thus presented must be regarded as inferior, from the standpoint of the requirements of adequately *"micro*economic" analysis, not only to the best of the devices developed in this field by other writers, but also to the relevant portions of the argument in the *Treatise.*[82] The same thing must be said, moreover, of the effect of Mr. Keynes's proposed amendment to the concept of "income-velocity" when it is compared even with other variants of the concept of "income-velocity" from the standpoint of their fitness for the only type of analysis in which the concept might be said to be directly useful: namely, analysis of a broadly *"macro*economic" character involving the problem of the relation between changes in "income," on the one hand, and the "quantity of money," on the other.

A. *The "Microeconomic" Problem: Income Velocity as a "Hybrid Conception," and the Argument of Keynes's* **General Theory***.* As we

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[79] See Volume I, 389 ff. It is to be observed also, however, that a usage similar to that sponsored by Professor Pigou in his *Theory of Unemployment,* according to which only the "active" part of the money stock was to be included in the denominator of the "income-velocity" ratio, had been proposed as early as 1917 by Professor Schumpeter, who suggested that the denominator should include, not the *total* "stock of money," but only the quantity of money "in circulation." See Volume I, 359, n. 32, of the present work, and the references to Schumpeter there given.

[80] See Volume I, 369 ff., 395 ff.

[81] Cf. above, p. 708, n. 76.

[82] On the single passage in the *General Theory* which may be said to represent a continuation of the *Treatise's* argument with respect to the requirements of adequately *micro*economic analysis, and its relation to the definition of "income-velocity" formally sponsored in the *General Theory,* see above, p. 709, n. 78.

have seen, Mr. Keynes's essential objection to the concept of "income-velocity," in his *Treatise*, was that it was a "hybrid conception," by virtue of the fact that it undertook to relate directly to "income" the *total* volume of cash balances, instead of the particular segment of that total which Mr. Keynes, at the time the *Treatise* was written, believed to be alone *directly* related to "income": namely, the volume of "income deposits." [83] It is clear, however, that from this standpoint, the definition of "income-velocity" formally advocated in the *General Theory* is as much of a "hybrid conception" as the definitions criticized in the *Treatise*. For even if, as Mr. Keynes now proposes, we subtract from the "quantity of money" the amount of cash held to satisfy the "speculative motive," we still have in the denominator of the ratio represented by "income-velocity," not only the "income deposits," but also the "business deposits," of Keynes's *Treatise*, as well as that part of "savings deposits" which is held to satisfy what he now calls the "precautionary motive." [84] It can be shown that a principal effect of this usage, as in other cases in which the concept of "income-velocity" has been used, has been to inhibit a close ("microeconomic") analysis of the nature of the forces affecting the "demand for money as a whole" in relation to movements in something called "income."

"The term 'income-velocity,'" writes Mr. Keynes, "carries with it the misleading suggestion of a presumption in favor of the demand for money as a whole being proportional, or having some determinate relation, to income." [85] In fact, however, he goes on to say, "this presumption should apply . . . only to a *portion* of the public's cash holdings." [86] From a passage on the following page of the *General Theory*, one expects to find Mr. Keynes arguing, as he did in the *Treatise*, that the "portion of the public's cash holdings" which may be presumed to be "proportional," or to have "some determinate relation, to income" is that "portion" which is held as income deposits—or, as he now puts it, is held to satisfy "the income-motive." [87] "One reason for holding cash," he writes on the page in question, "is to bridge the interval between the receipt of income and its disbursement"; and he adds: "it is

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[83] Cf. above, p. 708, n. 75, and p. 709, n. 78.

[84] Cf. above, p. 709, n. 78, and the references to the *General Theory* there given.

[85] *General Theory*, 194. It should be observed that the context of Mr. Keynes's discussion demands that the phrase "some *determinate relation to* income" be interpreted as meaning a "virtually *constant* relation to income." On the extent to which earlier users of the concept of "income-velocity" can fairly be charged with having encouraged the belief in such a presumption, see what is said above, p. 690, n. 25, and also below, p. 715, n. 96.

[86] *General Theory*, 194. On the validity of such a "presumption," *even* as applied to the ratio between "income" and the volume of "*income* deposits," see what is said above, p. 691, n. 31; also n. 87, immediately following, and below, pp. 713 ff.

[87] See the *General Theory*, 195, where it is stated (1) that Mr. Keynes's "transactions motive" "broadly corresponds to the former classification

in this connection that the concept of the income-velocity of money is strictly appropriate." [88]

As we have seen, however, Mr. Keynes's own use of the concept of "income-velocity" would suggest that the "portion" of the "public's cash holdings" which he regards as directly related to "income" is not *merely* the volume of cash held to satisfy the "income motive," but also that which is held to satisfy what he now calls "the business motive." [89] Clearly, there is confusion here. It is not difficult, moreover, to discover whence the confusion arises. It arises from the fact that the usage now proposed by Mr. Keynes, like that proposed by earlier users of the concept of "income-velocity," conceals the fact that changes in the relation between a given aggregate of cash balances and the level of "income" may be due to one of *two* separate types of controlling force: namely, (1) the decisions of cash-balance administrators with respect to the size of their cash holdings *relative to their own outlay* (the reciprocal of Fisherine V); and (2) factors affecting the proportion which is borne by the absolute volume of transactions against which cash must be held by *non-income recipients* (the $(PT)_{ni}$ of our formulation), to the absolute volume of transactions against which cash must be held by *income recipients* (the $(PT)_i$ of our formulation). [90]

[that is, the classification of the *Treatise*] of income deposits and business deposits"; and (2) that cash balances held to satisfy "the transactions motive" "can be further classified" as balances held to satisfy "the income-motive" and "the business-motive, respectively." From the further context (*General Theory,* 195), it would appear that Mr. Keynes would include the cash balances held by *income* administrators to satisfy the "precautionary motive" with the *Savings* Deposits of the *Treatise,* and not with the Income Deposits. Actually, however, in the *Treatise* itself, he had specifically included, in "Income Deposits," money held *"to provide against contingencies"* and money held as "personal savings"—the only part of the cash holdings of income administrators relegated to "Savings Deposits" being such cash holdings as are regarded by their owners "as an investment" (*Treatise,* I, 35 f.). It should hardly be necessary to labor the point that on *such* a definition of "Income Deposits," the "presumption" of "proportionality" of even "Income Deposits" to "income" is anything but a valid presumption under all circumstances.

[88] Cf. above, p. 709, n. 78.

[89] Cf. above, p. 709, n. 78.

[90] Cf. the references to Volume I of the present work given above, p. 710, n. 80. For examples of a tendency, by earlier users of the concept of "income-velocity," to suggest that only the *first* of the two elements indicated is involved, see Volume I, 397, n. 22. For an example of the same tendency by supporters of the argument of the *General Theory,* see Joan Robinson in the *Economic Journal,* XLVI (1936), 693, where it is suggested that the way in which "the Quantity Theory *(sic!)* truism is fulfilled, when effective demand alters" (that is, presumably, the way in which a change in the level of "effective demand," relative to the quantity of money, is to be represented in terms of the "quantity *equations*") is by "changes in the *velocity of circulation*"—with no indication that anything is meant by the latter other than *Fisherine* "velocity of circulation" ($= 1/K$).

That this is so will be clear if it is remembered, in the first place, that the particular stream of money receipts which confronts those whom Mr. Hawtrey calls "consumers" (apart from receipts from the sale of owned wealth) is the stream of money income; and the particular stream of money outlay for which these "consumers" are responsible (apart from the outlay from funds obtained by the sale of owned wealth or a previously accumulated cash balance) is outlay from income.[91] It is, indeed, for this reason that one must assent to Mr. Keynes's proposition that the strength of what he calls the "income-motive" for holding cash "will chiefly depend upon the amount of income and the normal length of the interval between its receipt and its disbursement"; and it clearly it was this type of consideration that Mr. Keynes had in mind when he suggested that "it is in this connection that the concept of income-velocity of money is strictly appropriate." [92]

By the same reasoning, however, the stream of money receipts with which those administrators of cash balances whom Mr. Hawtrey calls "traders" are concerned is not the stream of money *income*, but the stream of what Mr. Keynes himself calls "sale-proceeds," which have no necessary connection whatever with anything properly called "income." [93] Similarly, the stream of money *outlays* with which these traders are concerned (apart from outlays from the proceeds of the sale of owned assets) are not primarily outlays from income, but are

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[91] It should again be observed (1) that the expression "outlay from income" is an elliptical expression for "outlay from cash balances which have been alimented by the receipt of money income"; and (2) that this fact is made clear by the apparatus sponsored in this work, according to which movements in "outlay from income" will be reflected in $M_i V_i$. See above, p. 615, n. 130.

[92] It should be clear that the latter proposition would be put in other terms by those who, like myself, would leave room for the concept of "income-velocity" (cf. above, p. 476, n. 30, and p. 708, n. 76) *at the same time that they stress its limitations* for the purposes of a *close ("microeconomic") tracing of the steps involved in the processes of the generation and utilization of money income.* Specifically, it would be argued that the "connection" indicated by Mr. Keynes more closely approximates the "connection" in which, it is here argued, it is "appropriate" to use a concept of "velocity" which is *strictly consistent with the methodological principles underlying" the body of analysis designed to account for the size of cash balances relative to the outlay against which the cash balances are being held* (see Volume I, 368, 372 ff.); and that the remaining phenomena subsumed under the head of "income-velocity" are to be studied under the head of categories other than "velocity," in the strict (Fisherine) sense of the term.

[93] Cf. the description, in the *General Theory* (p. 195) of the "business motive" for "holding cash": "Cash is held to bridge the interval between the incurring of *business costs* and that of the receipt of the *sale-proceeds*" (italics mine). Cf. also the *Economic Journal*, XLVIII (1938), 319, where Mr. Keynes referred to that "part" of "the active demand for cash" which is "due to the time-lags . . . between the receipt by entrepreneurs of their sale-proceeds and the payment by them of wages, etc."

those associated with what Mr. Keynes calls "the incurring of business costs," which likewise need not be disbursed from anything properly called "income." The conclusion to which one comes, therefore, upon the basis of the methodological assumptions underlying the "cash-balance approach," is that if we are to understand the movements in the size of cash balances held relative to outlay (which is *one*—though only one—of the elements determining the relation of the "demand for money as a whole" to "income"), the magnitude with which we must compare the volume of business deposits is not *income*, but what Mr. Hawtrey has called "traders' turnover"—that is, a segment of the $(PT)_{ni}$ or $(PT)_{NI}$ of our formulation.

In this respect, clearly, Mr. Keynes's proposed definition of the concept of "income-velocity" represents a retrogression not only compared with the single passage, in the *General Theory*, in which he has argued that a concept such as income-velocity is "strictly appropriate" only to balances held to satisfy "the income-motive," and *not* to those balances held to satisfy the "business-motive," but also as compared with the position with respect to the concept of "income-velocity" that he had adopted in his *Treatise*. It is a retrogression because Mr. Keynes's further use of the concept of "income-velocity," as formally defined in the *General Theory*, illustrates, as clearly as one could wish, the dangers of oversimplification and even of positive error which one incurs as soon as one abandons the firm ground of analysis suggested by those forms of the cash-balance approach which are best suited for the purpose in hand: namely, those forms which have insisted upon separate analysis, with the use of suitably specialized techniques in each case, of (1) the forces determining the ratio of cash balances to *the outlay against which these cash balances* are being held (the reciprocal of Fisherine "velocity"), on the one hand; and (2), on the other, those forces determining phenomena summarized by the specific components of the Fisherine T.

Consider, for example, the implications of Mr. Keynes's statement that *not only* the volume of cash balances held to satisfy the "income-motive," *but also* the volume of those that are held to satisfy the "business-motive" may be "presumed" to be "proportional," or to have some "determinate relation to, *income*." [94] This is certainly not a

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[94] See the passages in the *General Theory* cited above, p. 711, nn. 85 and 86, from which it is clear that Mr. Keynes believes that, although it is "misleading" to suggest that there is a "presumption in favor of the *demand for money as a whole* being proportional, or having some determinate relation to, income," this "presumption" *may* be regarded as applying to the "*portion* of the public's cash holdings" which is represented by total cash holdings *minus* cash held to satisfy the "speculative-motive." On the consequences, for Mr. Keynes's argument, of the fact that both the "Income Deposits" and the "Business Deposits" of his *Treatise* included not only cash held to satisfy what he now calls the "business-motive," but also what he now calls the "precautionary-motive," see below, pp. 719 ff., and also what is said above, p. 712, n. 87.

conclusion which follows directly from the considerations suggested by the cash-balance approach, according to which the variables held to affect the demand for cash balances must be shown to be those with which the administrator of a cash balance is himself confronted.[95] Of the suggestion that the amount of cash held to satisfy the "income motive" may be expected to be in some degree "proportional," or to have "some determinate relation to, income," it can at least be said that the facts with respect to *income* are one of the important sets of facts which actually bulk large in the minds of the administrators of the "income-deposits" of the *Treatise* (Hawtrey's "consumers' balances").[96] As we have seen, however, the variable which, in the case of "business-deposits" (Hawtrey's "traders' balances"), plays the rôle assigned to "income" in the case of consumers' deposits, is not "income," but Mr. Keynes's "sale-proceeds" (Hawtrey's "traders' turnover"). Why, then, should we assume that the movements in "business-deposits," like those in "income-deposits," may be presumed to be "proportional" to "income"?

Mr. Keynes's answer to this question provides as good an example as one could wish of the consequences of a failure to realize that the magnitude of "income velocity," as ordinarily defined, will be very largely affected by the magnitude of components of the Fisherine T

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[95] It should be observed that this holds true regardless of whether the "demand for cash balances" with which we are concerned is the *"relative"* demand for cash balances (that is, the demand for a cash balance of a given "size" [K] *relative to outlay*), or the *"absolute"* demand for cash balances (that is, for a cash balance of a given "size" in terms of an absolute number of money units—a "demand" which will be affected *not only* by the forces summarized by our K, but also by our $(PT)_i$ or $(PT)_{ni}$; for "in both cases, it is individuals [or the *agents* of financial *units*, such as governments or government agencies] who demand a cash balance" (see Volume I, 447 f., of the present work). That this statement does *not* mean that the approach which is here recommended is an exclusively "atomistic" approach (cf. the references to B. P. Whale given above, p. 476, nn. 30 and 31), in the sense that it would *ignore* the "institutional" factors involved, follows from the fact that "institutional" factors must be said to affect the magnitude not only of our T, but also of our K's. (See Volume I, 483; and cf. also what is said on pp. 433 ff. and 444 ff., of the same volume, on attempts to make the "cash-balance approach" carry more than it can bear.) The contention is merely that, if we are to understand the functioning of the economic process, we must be prepared to relate market results to the actions of "individuals," as these individuals may be expected to operate *in a given "institutional" setting*. See above, pp. 465 ff., 499 f.; and cf. what is said in the following paragraphs of the text.

[96] It should be observed that the statement that "income-deposits" may be expected to have "some determinate relation to, income" should *not*, in strict accuracy, be interpreted as meaning that they may be expected to bear a *fixed* (constant) relation to income (cf. above, p. 711, n. 85). For the size of the stream of income receipts is only *one* of the factors that can be shown to affect the size of "income-deposits" relative to the outlay of income recipients. See again Volume I, 482 f., of the present work.

which deserve the most careful study on their own account. The "strength of the demand for cash" held to satisfy the "business motive," he tells us, may be presumed to be "proportional" to movements in "income," because it will "chiefly depend on the value of current output (*and hence on current income*)." [97]   That there are very serious objections on other grounds to the identification of "income" with the "value of current output" should be clear from what was said above under 3, ii.[98]   For the present, however, our major interest lies in the following proposition: namely, that it is difficult to believe that Mr. Keynes would have come to the conclusion that the volume of business-deposits may be presumed to be "proportional" to "income," if he had insisted more strongly upon the fact that it is not the volume of the money "income" of traders which is important for the decisions of "traders" with respect to the amount of cash held to satisfy the "business motive," but the money value of their *"turnover."* For then he might have gone on to an examination of the nature of the *forces determining* the amount of this "turnover," instead of supposing that it will necessarily be proportional to the amount of "income"; and it is difficult to believe that he would have failed to discover that what is involved is a whole range of complicated phenomena of the type summarized under the several components of the (*PT*) of a "total transactions equation." [99]

It would be unfair to Mr. Keynes to suggest that he has been completely unaware of the difficulties involved in an identification of "output" with "income," and of both with "traders' turnover." In publications subsequent to the *General Theory*, for example, he has suggested that what is to be watched in connection with the demand for cash arising out of an increase in employment and general productive activity is not only the level of "income," but also something called "business activity," and the "business habits" under which such

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[97] *General Theory,* 195 f. (Italics mine; on the remainder of the sentence in the *General Theory,* which has to do with "the number of hands through which output passes," see below, pp. 717 f.).   See also Mr. Keynes's later article, "The General Theory of Employment," *loc. cit.,* 217, where the "amount of . . . [money] required in the active circulation for the transaction of *current business*" is characterized as "mainly depending on the level of *money-income.*"   On the basis of passages in the *General Theory* such as that cited above, p. 699, n. 50, one is forced to conclude that the *"and hence"* of the passage quoted in the text above is to be taken as meaning "which is equal by definition to."   An alternative interpretation, according to which the words "and hence" would be taken to mean "which is *determined by* the level of *expenditure out of* money income" would, of course, force a reopening of all the issues arising out of the *General Theory's* virtual *identification* of "income" and "demand" which were discussed above under 3, i (pp. 694 ff.), *as well as* the issues discussed above under 3, ii (cf. the following note).

[98] See above, pp. 698 ff.

[99] Cf. the algebraic summary of these components given in Volume I, 599, n. 58.

"activity" is carried on.[100]   Even in the *General Theory*, moreover, he made at least one important concession to the position that even if one could identify movements in the "value of output" with movements in the amount of *income received in the form of money,* one cannot regard movements in "output" as solely determining the demand for cash held to satisfy what he called the "business-motive": one had to consider, he pointed out, not only "the value of current output," but also *"the number of hands through which output passes."* [101]

It is of the utmost importance, therefore, to observe the consequences of admissions of this type for Mr. Keynes's analysis.   In the first place, this fact alone makes it impossible to argue with any assurance that the "strength" of the demand for cash to satisfy the "business-motive" may be "presumed" to be "proportional" to "income." Still more important, moreover, is a second conclusion: namely, that the very concept of "the number of hands through which output passes" shows us that we are dealing with the whole complex of problems summarized by the components of the T of a "total transactions equation."   On the basis of the chapters in Volume I of this work devoted to an analysis of these components of T, it should be clear that justice is most emphatically not done to the relation of variations in the volume of "current output" ("income," in one of Mr. Keynes's meanings of the term) to variations in the total volume of "transactions," by the addition of the single element of "the number of hands through which output passes." [102]   What Mr. Keynes has done, there-

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[100] See, for example, the *Economic Journal,* XLVIII (1938), 319, 322; and cf. also the *General Theory,* 196, where it is stated that "in normal circumstances"—whatever that may imply—"the amount of money required to satisfy the transactions-motive and the precautionary-motive" is mainly a resultant not only of the "level of money income" but also of "the general activity of the economic system."   Relevant also in this connection are Mr. Keynes's later concessions with respect to the demand for "cash" for purposes of what he has called "finance" ("Alternative Theories of the Rate of Interest," *loc. cit.,* 247 f., and also p. 319 of the issue of the *Economic Journal* cited at the beginning of this note).   For the effect of such concessions is to open the way to a consideration of the influence, upon "the demand for money as a whole," of *all* forms of what were called, in Volume I of this work, "non-output transactions."   See Volume I, 525 ff.; and, on "The Rôle of Transactions in *Securities,*" in particular, see pp. 576 ff., 595 ff., of the same volume.   Cf. also below, p. 721, n. 111.

[101] *General Theory,* 196.

[102] See Volume I, Chapters XVIII and XIX; and cf. the reference to the algebraic summary of the components of the Fisherine T given above, p. 716, n. 99.   The reader is invited to compare the type of analysis presented in the chapters indicated, from the standpoint of the requirements of adequate analysis of the "institutional" factors involved (see above, pp. 464 f.), and from the standpoint of the requirement of an adequate *"mechanics"* of the process of income generation (see above, pp. 475 f.), with Mr. Keynes's dismissal of the relevant problems by loose references to business "activity" and "business habits" (cf. note 100, above).

fore, is merely to add a further example, to the long list that can be culled from the writings of those who have worked with "income-velocity" as an analytical device, of the consequences of a failure to recognize a point that is as simple as its neglect has been widespread: namely, that there is hidden in the concept of "income-velocity," not only the whole complex of problems associated with changes in the size of cash balances held relative to outlay (the reciprocal of Fisherine V), but also the whole series of difficult and intricate problems associated with the disentangling of the elements included under the "volume of transactions" (T) which appears in equations of the "total transactions" type. *Given* a clear recognition of the fact that the concept of "income velocity" is a "hybrid conception," in this sense of the term, there can be no objection to its use, for certain broad and elementary purposes of monetary theory, by theorists aware of the necessity for studying simultaneously the behavior of the *parents* of the "hybrid" (specifically, movements in the Fisherine V's and in the components of (PT)), whenever what is involved is something that deserves to be regarded as *close analysis* of the forces determining the relation between a given aggregate of cash balances and the level of "income." The point made here is merely that in this respect the treatment now accorded by Mr. Keynes to the concept of "income-velocity" evidences even less aware-ness of the importance of this proposition than can be imputed to his rejection of the concept of "income-velocity" in his *Treatise* on the ground that it is a "hybrid conception." [103]

B. *The "Macroeconomic" Problem: The Reasons for, and the Con-sequences of, the Elimination of "Speculative Motive" Balances from the Denominator of "Income-Velocity."* The argument against "in-come-velocity" on the ground that it is a "hybrid conception" has to do only with the limitations that must apply to such a concept when what is involved is *close analysis* of the nature of the processes involved in the generation and utilization of money income and the rôle played in those processes by the "quantity of money." It is an argument, therefore, which becomes less serious in the degree that the sponsors of the concept of "income-velocity" abate their claims on behalf of

[103] Of particular interest in this connection is Mr. Keynes's essentially sympathetic reference to the concept of "income-velocity" in a publica-tion subsequent to the *General Theory*, cited at the end of n. 36 to p. 693, above. It should hardly be necessary to emphasize again that little ob-jection could be taken to Mr. Keynes's newly discovered sympathy for the concept of "income-velocity" as a term under which the effects of certain "business, banking, and personal technique[s] and habits . . . are con-veniently summarized," *if* he had accompanied such an expression of sym-pathy with some indication of an awareness of the necessity for *an ade-quate analytical treatment* of these "business, banking, and personal tech-niques and habits," and their relation to the covering concept of "income-velocity." The point made here is merely that both in the *General Theory* and in his subsequent writings, he has shown even less awareness of this necessity than he did at the time he wrote his *Treatise*.

it as a device useful for purposes of detailed ("microeconomic") analysis, and show themselves sympathetic to the use of supplementary devices (including the relevant variants of Fisherine V and the relevant components of the Fisherine T) that can be shown to throw light upon particular processes which an *unsupplemented* use of the concept of "income-velocity" might otherwise have tended to obscure.[104]   In what follows, therefore, it will be assumed that nothing more is claimed on behalf of the concept of "income velocity" by its sponsors than was granted to it in Volume I of the present work: namely, that it may be regarded as useful for the broad ("macroeconomic") purposes of establishing certain elementary propositions of monetary theory. These propositions were (1) that, for the purposes of monetary theory, a special importance attaches to those particular money payments which represent "income payments," as contrasted with the payments involved in transactions other than "income" transactions; (2) that the magnitude of the stream of money income may be greatly affected by changes in the stock of money; and (3) that there is no *a priori* reason for supposing that the relation between the stock of money and the total of income payments will remain constant, in such wise that we could use changes in the quantity of money as indicative of changes in the magnitude of money income, and *vice versa*.[105]

Our problem is now to ascertain which of the two concepts of "income-velocity" under discussion is most helpful for these broad ("macroeconomic") purposes: the concept sponsored, in effect, by Mr. Keynes in the *General Theory*, which would eliminate from the denominator of the "income-velocity" ratio the particular balances held to satisfy the "speculative-motive"; or the variants of the concept of "income-velocity" which would *include* these balances.   In dealing with a problem of this type, it should be clear that what is involved is not a choice between "right" and "wrong" concepts, but between concepts which are *more* or *less* likely to obscure important analytical issues.   It should be equally clear that any judgment on the latter head must be based largely upon the experience represented by the analysis provided in connection with the use of the one or the other type of concept.   With these propositions clearly in mind, attention may be called to the following aspects of Mr. Keynes's discussion of the relation between "income," on the one hand, and the "quantity of money," on the other:

(a) *The Presumption of "Proportionality" in the Movements of the* *"Quantity of Money" and of "Income," respectively, in relation to the*

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[104] That it is the *Fisherine* V which must be invoked if we are to understand why "income-velocity" is as high as it is, instead of the other way around, is, of course, precisely what has been obscured by the argument of some defenders of the concept of "income-velocity."   See Volume I, 397, and the reference given in n. 24 thereto.

[105] See Volume I, 364 ff.

Elimination of "Speculative-Motive" Balances from the Denominator of "Income-Velocity." As far as I have been able to discover, Mr. Keynes's principal reason for eliminating from the denominator of "income-velocity" balances held to satisfy the "speculative-motive" is that these balances cannot be presumed to bear a constant ratio to "income." [106] Yet it must be evident that if this were *all* that is involved, there would be little basis for choice between the particular concepts of "income-velocity" which are here under discussion: after all, the absence of a presumption in favor of a "proportional" relation between "income," on the one hand, and the "quantity of money," on the other, is precisely what gives *all* variants of the concept of "income-velocity" one of their chief claims to significance.[107] The difficulty arises from the implications of Mr. Keynes's argument with respect to the nature of the relation between "income" and the "quantity of money" *minus* the amount of balances held to satisfy the "speculative motive." For, as we have seen, he has argued (though not without some qualification) that *this* "quantity of money" *may* be "presumed" to bear a fairly constant proportion to "income." [108]

It is precisely such "presumptions," however, which cannot be accepted without serious misgivings. In one respect, to be sure, what is involved is an empirical question; and, for reasons adduced by Mr. Keynes himself with respect to the difficulty of empirical segregation of balances held to satisfy the "speculative motive" from other types of balance, it is not easy to provide unequivocal empirical evidence which

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[106] Cf. above, p. 711, and the reference to the *General Theory* given in n. 85 thereto. Mr. Keynes's further comment, in the same context, to the effect that earlier writers on the nature of the forces affecting "the demand for money as a whole" have "overlooked" the "part played by the rate of interest," is one the accuracy of which is to be tested only by an examination of the analysis of these earlier writers. Such an examination must be left for my later *Money and Interest.* I can state here only my own conclusion: namely, that although *some* earlier writers on the problem of the forces determining the demand for cash balances can fairly be charged with having *under*emphasized the "part played by the rate of interest," other writers can be charged with having *over*emphasized it, by the use of expressions of the type $M = L(r)$, and the context in which such expressions appear (see, for example, the *General Theory,* 168). I am inclined, indeed, to include Léon Walras among those who may be charged with such overemphasis upon the "part played by the rate of interest" in the determination of the "demand for money as a whole," although certainly not in the degree in which, in my opinion, the *General Theory* can be charged with such overemphasis. Cf. my "Léon Walras and the Cash-Balance Approach," *loc. cit.,* 581, n. 29; and on Walras's general treatment of "the demand for liquidity" as "a function of the rate of interest," see Lange, "The Rate of Interest and the Optimum Propensity to Consume," *loc. cit.,* 20 f.

[107] See above, p. 691, and the references to Volume I given in n. 30 thereto.

[108] Cf. above, p. 691, n. 31; p. 711, n. 86; and p. 714, n. 94.

bears directly upon the validity of Mr. Keynes's "presumption." [109]   In other respects, however, we are not so helpless. We know, for example, that the denominator of Mr. Keynes's "income-velocity" includes one type of balance—namely, "traders' balances"—which cannot be "presumed" to bear a constant proportion to *"income,"* particularly when account is taken of the effect, on the amount of such balances, of possible changes in the strength of the "precautionary motive" over the period of the trade cycle.[110]   And we know also that the absolute amount of these "traders' balances" will be directly affected by factors having nothing to do directly either with the "speculative-motive," in Mr. Keynes's sense of the term, or with movements in "income": factors, for example, such as changes in the "number of middlemen's sales," the "rate of sale of goods," and a considerable part of the volume of "financial" transactions.[111]   *Given* a clear recognition of these reasons

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[109] For Mr. Keynes's statement of the reasons indicated in the text, see the *General Theory,* 195.

[110] The "precautionary motive," it will be recalled, is defined by Mr. Keynes as the "motive for holding cash" in order "to provide for contingencies requiring sudden expenditure and for unforeseen opportunities of advantageous purchases, and also to hold an asset of which the value is fixed in terms of money to meet a subsequent liability fixed in terms of money" (*General Theory,* 196). In his *Treatise* (I, 35), Mr. Keynes specifically included cash held for such purposes (as "a margin against contingencies") in his *Business* Deposits, despite his statement, in the *General Theory* (p. 195) that balances held to satisfy the "precautionary motive" "broadly correspond" to the *"Savings* Deposits" of the *Treatise.* (Cf. what is said above, p. 712, n. 87, with respect to the comparable treatment of cash held by *income*-administrators to satisfy the "precautionary motive.") It is of course true, as had been suggested by earlier writers, and as is suggested also by Mr. Keynes (*General Theory,* 196), that the amount of cash held to satisfy the precautionary motive, in particular, may be greatly affected by the existence of possibilities for "temporary borrowing" (cf. Volume I, 483, of the present work, under point 4); and it is therefore *conceivable* that institutional and conjunctural practices with respect to the administration of "traders' balances," in particular, might show smaller variations, both absolutely and relatively, than might have been the case otherwise. From such empirical evidence as is available, however, it is anything but clear (despite occasional statements to the contrary) that the net effect of these institutional and conjunctural factors is such as to assure a virtual "constancy," *over the period of the trade cycle,* in the relation between (1) the amount of balances held to satisfy the "transactions-" and "precautionary-motives," on the one hand, and (2) the level of "income," on the other. Cf. the comments of Mr. Keynes himself in his *Treatise,* I, 246.

[111] The *"part"* of the volume of 'financial' transactions" to which reference is made corresponds essentially to that against which Mr. Keynes, in his *Treatise* (I, 244 ff.) regarded his "Business Deposits *B*" as being held. The whole discussion of this matter in the *Treatise,* when compared, for example, with the oblique comments in Mr. Keynes's later discussion of the rôle played by "finance" in the demand for cash balances (see above,

for a possibly *dis*proportional variation in "income," on the one hand, and in the "quantity of money" *minus* balances held to satisfy the "speculative-motive," on the other, there could hardly be serious objection to the use of Mr. Keynes's proposed definition of "income-velocity" or of other definitions of that concept which may be said to resemble it analytically.[112] Yet it can hardly be argued that such recognition is evidenced in Mr. Keynes's own treatment of the problem. It remains a serious question, therefore, whether the proposed amendment is in fact superior to those variants of the concept of "income-velocity," rejected by Mr. Keynes, of which it can at least be said that the very *lack* of "presumption" of constancy in the ratio between the magnitudes they relate provides a kind of protection against loose "presumptions" which can hardly be said to be an adequate substitute for precise analysis and adequate empirical investigation based upon such analysis.

(*b*) *The Phenomenon of "Seepage" and the Composite Demand for Cash Balances.* If we bear in mind that the question of the desirability of eliminating "speculative-motive" balances from the denominator of "income-velocity" is not a question of right or wrong, but of the *more* or the *less helpful,* a further point may be raised upon the basis of a discussion that grew out of Mr. Keynes's rejection of the concept of "income-velocity" in his *Treatise.*

In that discussion, Mr. D. H. Robertson undertook not only to defend concepts of income-velocity whose denominator includes the *total* of cash balances, but also to argue for their superiority to the alternative proposed by Mr. Keynes in his *Treatise,* an element in which was the use of the ratio of "income" to the amount of *"income* deposits."[113] The particular thesis advanced by Mr. Robertson in this connection was one which he regarded as "of the utmost importance": namely, "that under certain conditions money which has been imprisoned in what Mr. Keynes calls the 'saving deposits' . . . may seep out [and] raise the aggregate of . . . 'income deposits.'"[114] Given this fact, Mr. Robertson argued, it is dangerous to confine any analysis of the relation between an aggregate of cash balances and the level of money income solely to a *segment* of the total volume of balances (in this case, that represented by the volume of "income deposits"). It is much preferable (so Mr. Robertson's argument ran) to include

p. 717, n. 100), provides a further commentary upon the position of those for whom any discussion of the details of the argument of the *Treatise* has been made "otiose" by the argument of the *General Theory.*

[112] Cf. the references to Schumpeter and Pigou given above, p. 710, n. 79. It should be observed that neither Schumpeter nor Pigou, to my knowledge, rested the case for the usage they sponsored, upon the presumption of a constant ratio between the portion of the money stock included in the denominator of "income-velocity" and the level of "income." (On the interpretation of Professor Schumpeter as having assumed a constancy in this ratio, see what is said above, p. 690, n. 25.)

[113] See Volume I, 398 ff., 410, of the present work.

[114] Cf. Volume I, 410, and the reference to Robertson given in n. 51 thereto.

in the denominator of the "income-velocity" ratio *all* cash balances, for otherwise we should not have an adequate account even of the reasons why the ratio of "income deposits" to "income" is as large as it is.

In Volume I of this work, it was pointed out that on this matter Mr. Robertson was less than fair to the argument of the *Treatise;* that in fact the apparatus presented in the *Treatise,* like that of other writers who have evidenced no high regard for the concept of "income-velocity" as a device useful in the analysis of the processes involved in the generation and utilization of money income, *did* undertake to show how the volume of "deposits" other than "income deposits" would be related to the volume of "income deposits," and therefore to the level of the money income generated upon the basis of a given "quantity of money"; and it was further argued that the method of the *Treatise* (which itself has an ancestry much longer than that of "income-velocity" and has received its classic statement in our own day at the hands of Mr. Hawtrey) is in many respects greatly superior to the type of *unsupplemented* use of the concept of "income velocity" which has become so common in recent years, whenever what is involved is *close analysis of the processes involved in the generation and utilization of money income.*[115] There was no attempt, however, in our earlier discussion, to minimize the proposition which Mr. Robertson himself had characterized as "of the utmost importance": namely, "that under certain conditions money which has been imprisoned in what Mr. Keynes calls the 'saving deposits' . . . [read here: "balances held to satisfy the 'speculative motive'"] may seep out and raise the aggregate . . . of 'income deposits.'" On the contrary, Mr. Robertson's proposition was accepted as a direct, and inescapable, application of the argument underlying the concept called, in Volume I of the present work, the "composite demand for cash balances."[116]

It is of considerable interest to observe, therefore, that the terms of Mr. Keynes's exposition of the relation between *his* definition of "income-velocity" and his concept of an *"elasticity of effective demand"* would suggest that he himself might be prepared to admit that in this particular respect his concept of "income-velocity" is inferior even to the concepts of "income-velocity" which he had rejected in the *Treatise* and which he continues formally to reject in the *General Theory.* For a collation of the passages in the *General Theory* dealing with "income-velocity" and the "elasticity of effective demand," respectively, shows (if the passages are to be taken literally) that one of the respects in which the formal expression for the latter differs from the formal expression for the former is that the "quantity of money" involved in the "elasticity of effective demand" is the quantity of money *inclusive*

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[115] On the significance of the words italicized, see above, p. 708, n. 76, and the references to Volume I there given; and cf. what is said in the same note with respect to the place of Keynes's *Treatise* in the history of attempts to deal with the process of the generation and utilization of money income.

[116] See Volume I, 521 f.

of "speculative balances," whereas the denominator of his "income-velocity" would *exclude* such balances.[117]   In other words, the passages in question, if interpreted literally, would suggest that Mr. Keynes has presented, in his *General Theory*, not *one* concept of "income-velocity," but at least two—the second of these concepts being disguised by the

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[117] It will be observed, for example, that the M in Mr. Keynes's formula for e_d (*General Theory*, 305), is *not* defined in such a way as to eliminate the volume of cash balances held to satisfy the "speculative motive," whereas the denominator of his explicit formula for "income-velocity" is M_1—that is, the stock of money *minus* the volume of cash balances (M_2) held to satisfy the "speculative-motive" (cf. above, p. 678, n. 2, and p. 709, n. 78, and the references to the *General Theory* there given).   It should be added, however, that a curious light is thrown upon this "literal" interpretation of Mr. Keynes's meaning by a footnote on p. 209 of the *General Theory*, according to which, "if we had defined V [that is, "income velocity"] not as equal to Y/M_1, but as equal to Y/M, then, of course, the Quantity Theory is a truism which holds in all circumstances, *though without significance*" (italics mine).   For, since Mr. Keynes's "elasticity of effective demand" itself amounts to a definition of "income-velocity" in which the denominator is M, rather than M_1, this would amount to saying that Mr. Keynes's own "generalized statement of the Quantity Theory of Money" (*General Theory*, 305) is itself a "truism . . . without significance" —despite the fact that it is presented as exhibiting "the extreme complexity of the relationship between prices and the quantity of money, when we attempt to express it in a formal manner."   (On the "generalized statement" thus indicated as a framework for the Theory of Money and Prices, see Chap. Fourteen, immediately following.)   Actually, of course, expressions of the *general form* $MV = OP$ can be made to be "truistic" whether we define V as equal to Y/M_1 or as equal to Y/M.   For if the expression $Y = OP$ is regarded as "true" by definition, it follows that any expression which *is made equal to Y by definition will also be made equal to OP by definition*.   Thus, we may write $MV = OP$, when $V = Y/M$. But we may also write $M_1V = OP$, when $V = Y/M_1$.   The only type of expression that will *not* necessarily be true "by definition" is an expression of the form $MV = OP$ in which M refers literally to the "whole" stock of money, whereas Y is defined by the expression Y/M_1.   In that case, of course, as Mr. Keynes points out (*General Theory*, 209), the expression $MV = OP$ would be formally valid only when M_2 is equal to zero, so that (since $M = M_1 + M_2$) we should have $M = M_1$.   (Cf. Volume I, p. 70, n. 81, where this aspect of the argument of the *General Theory* is discussed briefly in connection with the general methodological issues raised by certain criticisms advanced in Keynes's *Treatise* against the familiar "Quantity Equations.")   I know of no instance, however, in which a writer of standing other than the Keynes of the *General Theory* has presented under the name of "the Quantity Theory of Money," or under any other name, a proposition such as that which Mr. Keynes now presents as being "much the same as the Quantity Theory of Money in its traditional form," solely in order to be able to announce triumphantly that such a "theory" would be valid only "in a static society or in a society in which for any other reason no one feels any uncertainty about the future rates of interest," so that "the Liquidity Function L_2, or the propensity to hoard

appellation "elasticity of effective demand." [118] For our present pur-
poses, therefore, only two comments need be offered. The first com-
ment is that, from the standpoint of what was called above the *"micro-
economic"* problem, the version of the concept of "income-velocity"
which is now presented under the name of an "elasticity of effective
demand" is in no respect superior to the concepts of "income-velocity"
which Mr. Keynes had himself rejected in his *Treatise* and continues
formally to reject in the *General Theory*. The second comment is that
the concept of "income-velocity" which he formally advocates *under that
name* in the *General Theory* offers no genuine analytical gain from
the standpoint of the *"micro*economic" problem, when compared with
those variants of the concept of "income-velocity" which are the
equivalent of the *General Theory's* "elasticity of effective demand";
and it may be regarded as even more open to criticism, from the
standpoint of the requirements of *"macro*economic" analysis, than were
the concepts of "income-velocity" which were (1) criticized in the
General Theory on the ground that they undertook to relate the *whole*
"stock of money" to the level of "income," only to be (2) reintroduced
in the form of an "elasticity of effective demand" which does exactly
the same thing. For it is results of this type which support the charge
that Mr. Keynes, in failing to discuss the substantive issues raised by
his concept of an "elasticity of effective demand" as problems of *mone-
tary* theory, in the narrower sense of the term, has succeeded only in

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(as we might term it) will always be zero in equilibrium" (*General Theory,*
208 f.). Indeed, the only instance known to me in which a proposition
of the type rightly rejected by Mr. Keynes as of limited validity has been
presented as being of perfectly *general* validity, is that provided by Mr.
Keynes in his *General Theory*, when he writes $D = MV$ (*General Theory,*
304), and then proceeds to incorporate D, as so defined, in what purports
to be a *"generalized* statement of the Quantity Theory of Money," in a
context which would suggest that this "Quantity Theory of Money"
(read: version of the "Quantity Equations"?) is one of universal validity
(*General Theory,* 305). For if (1) M continues to be defined as the
whole "quantity of money" (*General Theory,* 304); but (2) the denominator
of the V ratio also continues to be defined in such a way as to eliminate
M_2 (cf. the *General Theory,* 304, with pp. 201 and 209 of the same work);
then it follows (3) that any expression, such as Mr. Keynes's expression
for *"e* without suffix" (*General Theory,* 305), which can be shown to reduce
to an expression of the general form $D = MV = OP$ (cf. Chap. Fourteen,
immediately following), will necessarily be formally valid only when M_2
is equal to zero.

[118] The insertion of the words "at least" is necessitated by uncertainty
as to whether a third concept of "income-velocity" is to be regarded as
implicit in Mr. Keynes's statement with respect to deposits held to satisfy
the "income-motive" as providing the only "connection" in which "the
concept of the income-velocity of money is strictly appropriate" (see
above, p. 709, n. 78). Cf. also what is said above, p. 693, n. 36, with respect
to the uncertainty as to *which* concept of "income-velocity" Mr. Keynes,
in a later article, regards with sympathy as a "convenient" summarizing
device,

leaving these substantive issues in greater obscurity than he left the related issues in his *Treatise,* which made no pretense at having effected a new and significant "synthesis" between the "general" Theory of Value, on the one hand, and the Theory of Money and Prices, on the other.

4. *Substitute or Complement?* From the argument just presented, it should be clear that Mr. Keynes's concept of an "elasticity of effective demand," insofar as it is not *identical* with certain variants of the concept of "income-velocity," can be regarded only as *complementing* certain other variants of the latter concept, including the variant advocated under the *name* of "income-velocity" by Mr. Keynes himself in his *General Theory.* It must be clear also (though this fact can hardly be said to have been emphasized, if, indeed, it has ever been pointed out, by either Mr. Keynes or his disciples) that the concept of an "elasticity of effective demand," when it is itself regarded as a variant of the concept of "income-velocity," is to be regarded as *complementing* the use of other devices—the various "multipliers," in particular—which have been proposed by the members of the Keynesian group as weapons for dealing with the problem of the generation of something called "income." [119]

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[119] The reason why this fact has been obscured by Mr. Keynes and his disciples has of course been that, following the example set by Mr. Keynes in his *Treatise,* they have refused categorically to admit that it is desirable, or indeed even possible, to translate a given argument with respect to "investment" and "income" into terms of the variables of the familiar Quantity Equations, even when the more highly developed forms of the latter are used. Cf. Volume I, 30, and the references given in n. 56 thereto. It should be clear that no proof of the impossibility of such translation is provided by the fact that some writers have been guilty of a completely *erroneous* translation—as when the concept of the "multiplier" is implicitly *identified* with the concept of "income velocity" (cf., for example, A. D. Gayer, "Fiscal Policies," *American Economic Review,* XXVIII [1938], Supplement, 101). On the contrary, the very possibility that such an identification could be made is to be taken as *arguing for* a careful statement of the relation between the various "multipliers," on the one hand, and, on the other, the concept of "income-velocity" (or, if one wishes, Mr. Keynes's "elasticity of effective demand"). And, from the standpoint of a localization of the responsibility for such confusion as has arisen with respect to the relation between the various "multipliers," on the one hand, and the concept of "income-velocity," on the other, it is of considerable importance to note that both Mr. Keynes and his disciples have done their best to avoid such a careful statement, despite the fact that the *General Theory* makes use *not only* of the concept of a "multiplier," *but also* of the concept of "income-velocity" and its variant, the "elasticity of effective demand." (Cf., for example, the cursory treatment presented on pp. 298 f. of the *General Theory* of the problem of the relation between the "investment" multiplier and the degree of "proportionality" that may be expected to exist between the "quantity of effective demand," on the one hand, and the "quantity of money," on the other.) A statement of the kind indicated must be left for the treatment of the concept of "multiplier" which I have reserved for a later study on *The Generation*

Two further questions now arise, however. The first question is whether Mr. Keynes's "elasticity of effective demand," when regarded (as it must be) simply as a variant of the concept of "income velocity," does not itself need to be supplemented by a further set of analytical devices, when our purpose is that of providing adequate analytical weapons for "a *close study*" of the problems of monetary theory; and the second question is what relation these further devices bear to concepts, such as "the velocity of circulation . . . the volume of transactions . . . *et hoc genus omne*," which Mr. Keynes himself has formally rejected.[120] We know, for example, that a close examination of the concept of an "elasticity of demand for money," instead of leading to the conclusion that we could dispense with these supposedly useless concepts, can lead only to the conclusion that the concept of an "elasticity of demand for money" requires the reintroduction of these supposedly "useless" concepts whenever, in the words of Professor Pigou, we undertake to provide an adequate "anatomy" of the "demand for money." [121] Up to a certain point, to be sure, Mr. Keynes's introduction of the concept of an "elasticity of effective demand" does represent an acceptance of the latter conclusion; for his "elasticity of effective demand" (or "income velocity") was introduced as part of the *ex-*

and Utilization of Money Income. It is sufficient here to point to what may be taken as a virtual admission by Mr. Keynes that use of the concept of a "multiplier" must be *supplemented* by the use of analysis running in terms of the variables of the Quantity Equations: namely, the very fact that he has found it necessary to introduce a concept such as his "elasticity of effective demand" and other variants of the concept of "income-velocity" into his *General Theory, in addition to* the concept of the "multiplier." On this matter, see also what is said above, pp. 476 f., nn. 29, 30, and 32, and p. 504.

[120] This fact in itself provides a commentary upon the looseness of the statement with respect to *"et hoc genus omne"* (*General Theory*, 292) to which reference has so often been made. For the very fact that Mr. Keynes puts *"income*-velocity" *on a par* with concepts such as the Fisherine "velocity of circulation" and the "volume of transactions" means that he lumps together a series of concepts which are themselves of greatly differing degrees of fitness for the type of "close study" of the problems of monetary theory desiderated in the text. The comment obviously applies *a fortiori* to Mr. Keynes's inclusion, along with concepts such as the Fisherine "velocity" and the "volume of transactions," of concepts such as "hoarding," "forced saving," and "inflation and deflation"; and the irony attaching to Mr. Keynes's practice in this respect is not lessened by the fact that he himself makes use of the term "inflation" in the very chapter (see the *General Theory,* 301 ff.) in which he had rejected the term on the ground of its "vagueness" (*General Theory,* 292; cf. also p. 119 of the same work). For examples of the way in which the variables of the familiar Quantity Equations can be used to remove the "vagueness" that would otherwise attach to a concept such as that of "hoarding," see Volume I, 459 ff., 525 ff., 534 ff., 566 f., and above, p. 653, n. 58; and cf. the results of the controversy between Professor Haberler and Mr. Kahn in the *Economic Journal,* XLVIII (1938), 323 ff., 334.

[121] Cf. above, p. 655, and the reference given in n. 61 thereto.

planation of the magnitude of that particular "elasticity" in the *General Theory* which, as a matter of algebra, corresponds most closely to the "elasticity of demand for money"—namely, his "elasticity of prices in response to changes in the quantity of money" ("*e* without suffix").[122] To this extent, indeed, Mr. Keynes's own positive analysis represents a retraction of his inclusion of the *concept* of "income-velocity" among those concepts of the "Theory of Money and Prices" with which it is possible to dispense.[123]   For, as we have seen, his "elasticity of effective demand" is itself only another *name* for the concept of "income-velocity."

It is of the first importance, however, to recall a further conclusion reached in Volume I of this work: namely, that even the best variants of the concept of "income velocity" (and therefore also Mr. Keynes's "elasticity of effective demand") can at most be regarded as "useful shorthand summaries of the final effect of all rates of spending [sc.: and of all *types* of spending] affecting incomes"; and that the concept of "income-velocity" must itself be *supplemented* by further analysis when our problem is that of devising analytical weapons adequate for a *"close study"* of the process of the generation of money income.[124] This much is virtually admitted by Mr. Keynes himself when he states that his e_d "stands for the liquidity factors which determine the de-

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[122] See the *General Theory*, 296, 305; and cf. below, p. 742.

[123] See above, p. 726, n. 119.

[124] For the quotation with respect to the rôle to be assigned to "even the best variants of the concept of 'income-velocity,'" see Volume I, 404. I am prepared to admit that my position would have been less open to misinterpretation if, instead of referring, in the passage indicated, to "the *substitute* which must be put in place of 'income-velocity' whenever 'close study of the problem of money' is involved," I had referred to the type of analysis which must be used to *complement* the concept of "'income-velocity' whenever 'close study of the problem of money' is involved" (though see also what is said on this matter above, p. 476, n. 30, and p. 708, n. 76). As to the *nature* of this "complement," however, there should be no obscurity. The essential elements are, again: (1) a sharp distinction between money income in the sense of income received in the form of money-media, and "money income" in the sense of the money value of "output" ("real income"); (2) an equally sharp distinction between money income received ("consumers' income") and outlay from that income ("consumers' outlay"); (3) the establishment of the relation of "consumers' income" to "consumers' outlay" in clock time which is given by a study of the facts of cash-balance administration; (4) a sharp distinction between "consumers' income," "consumers' outlay," and "consumers' balances," on the one hand, and "traders' turnover" and "traders' balances," on the other; (5) a study of the interrelation between these two broad types of category in terms of a study of the "diffusion" of cash balances throughout the system, conceived of as a study of a sequence of realized events dated in terms of clock time; (6) the formalization of (4) and (5) by means of a series of subscripts differentiating between "payments *in*" and "payments *out*," on the one hand, and payments into and out of *income* and payments which are *not* into and out of *income,* on the other, together with a series of time-period subscripts permitting the dating of all realized events in terms of clock time; (7) the

mand for money in each situation." [125] For all that this can mean is that we require an adequate *"anatomy"* of the "demand for money in each situation"; or, if one prefers, an adequate "anatomy" of the phenomenon of "liquidity preference." And it should be regarded as demonstrated that Mr. Keynes's own "anatomy" of the latter phenomenon points as clearly to the need for the use of the concepts of "velocity of circulation, . . . the volume of transactions, . . . *et hoc genus omne*" as did earlier attempts to provide an adequate "anatomy" of that "demand for money" the discussion of which, by Mr. Keynes's own statement, has dealt with "substantially the same subject" as that with which his own discussion of the "motives to liquidity preference" was intended to deal.[126] In this respect, therefore, Mr. Keynes's

establishment of categories making it possible to relate all realized events to the decisions of economizing *individuals* acting under the influence both of conjunctural "expectations" and the institutional setting in which' they are compelled to operate, at the same time that full justice is done to the *"macro*economic" aspects of the process (the process "as a whole"); and (8) the establishment of the relation of the whole process of income-generation and income-utilization to the processes involved in the pricing of *particular commodities and services,* with all that this means with respect to the analysis of the *structure* of prices, of incomes, and of output, as well as of the *"scale"* of prices, of incomes, and of output "as a whole."

[125] *General Theory,* 305. Cf. also p. 309 of the same work, where it is stated that "the long-run relationship between the national income and the quantity of money will depend on liquidity preferences." It will be observed that, apart from the ambiguity introduced by the limitation of this statement to the "long run" and the use of the term "national income," the latter statement provides a further illustration of Mr. Keynes's virtual identification of "effective demand" with "income." The same thing, incidentally, must be said of the passage, on p. 306 of the *General Theory,* where it is stated that "if the public hold a constant proportion of their *income* in money," then "$e_d = 1$"—despite the fact that the variable which is related to the "quantity of money" by the formula for e_d is not *income* (Y) but "the effective demand" (D). Cf. above, pp. 694 ff.

[126] Cf. the *General Theory,* 194. On the relation of the "concepts of 'velocity of circulation, . . . the volume of transactions, . . . *et hoc genus omne'*" to the provision of an adequate "anatomy" of the *demand for money,* see above, pp. 654 ff. As for the demonstration of the applicability of the proposition stated in the text to Mr. Keynes's treatment of "liquidity preference," it should be sufficient to call attention to the argument presented above, pp. 707 ff., particularly when this argument is interpreted in the light of the fact that the only "anatomy" of "liquidity preference" formally provided by Mr. Keynes himself is represented by (a) his expression $M = M_1 + M_2 = L_1(Y) + L_2(r)$, presented on p. 199 of the *General Theory,* and (b) his discussion of the "motives" underlying the "total demand for money" (or the "incentives to liquidity") under the fourfold heading of the "income-motive," the "business-motive," the "precautionary-motive," and the "speculative-motive." For it should be clear (1) that an adequate "anatomy" of even that part of the "demand for money" which is summarized by the expression $L_1(Y)$ demands the introduction of the whole argument with respect to V factors and (PT)

"elasticity of effective demand" provides us with a further example of what was designated above as the eighth of our Lessons of Doctrinal History: namely, that one of the reasons for the disappointing nature of the results obtained from earlier attempts to "synthesize" the "general" Theory of Value with the Theory of Money and Prices has been an unreasonable *exclusivism*, which has taken the form of a failure to see that supposedly different approaches to a given problem, instead of being mutually exclusive or even contradictory, are actually *complementary*.[127]

IV

A VISTA BLOCKED

It must now be observed that the Lessons of Doctrinal History illustrated in the preceding section of this chapter, strictly applicable and important though they are, are not the lessons which are *most* important from the standpoint of an evaluation of the extent to which Mr. Keynes's "elasticity of effective demand" may be regarded as representing a genuinely fruitful "synthesis" between the Theory of Money and Prices, on the one hand, and the *"general" Theory of Value*, on the other. For, in effect, what they show is the crudity of certain of Mr. Keynes's final results *within monetary theory itself,* when these results are compared with the best of the results obtained within monetary theory, in the narrower sense of the term, by earlier writers, including those who not only advanced no formal claim to having effected a new and fruitful type of "synthesis" between the two bodies of theory, but also have viewed much that has been done in the name of such "synthesis" with a distinctly critical eye.[128]

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factors presented above, pp. 710 ff.; (2) that the same proposition holds with respect to an adequate "anatomy" of the various "motives" distinguished by Mr. Keynes (see above, pp. 711 ff., 721 f.); and (3) that an adequate treatment of the *time* aspects of the processes involved in the determination of the relation between the "quantity of money" and the level of "effective demand" requires full use of the type of apparatus summarized above, p. 728, n. 124.

[127] See above, p. 128.

[128] In the latter category, for example, would fall Mr. Hawtrey (cf. above, pp. 119 ff.), to whom, more than to any other single writer, we owe so much of our understanding of those processes associated with the generation of "income" and "demand" with which the concept of an "elasticity of effective demand" was ostensibly designed to deal.

The Lessons of Doctrinal History which are most important for the purpose indicated, on the other hand, are rather these: (1) that in some cases the exclusivism born of an alleged concern with a substantive "synthesis" of the two bodies of doctrine has been represented by a *failure to face* problems which are not only of the utmost importance in themselves, but also provide opportunities for a genuinely useful application of the categories of the "general" Theory of Value to the problem of the forces determining money prices and the channels through which these forces operate; and (2) that the most fruitful results of attempts at "synthesis" have come about when, instead of using the devices of the "general" Theory of Value to restate results already familiar within the Theory of Money and Prices, these devices have been introduced for the solution of problems *to which they alone can provide an answer.*[129]

For it must not be forgotten (1) that Mr. Keynes's "elasticity of effective demand" was associated with an argument designed to prove that the concept of "elasticity of demand" as used within the *"general" Theory of Value* cannot be applied to problems which are of significance for the Theory of Money and Prices and of Output as a Whole; (2) that the effect of this argument, if accepted, would be to block any further attempt to face the problem of *how* to incorporate the "particular demand schedules" of the "general" Theory of Value, with their special property of "elasticity," into the theory of the forces determining the structure of money prices and the level and the structure of Output as a Whole; and (3) that the effect of such a development, in turn, would be that we should be asked to abandon a type of analytical device, developed originally within the "general" Theory of Value, to which *none* of the devices of monetary theory, in the narrower sense of the term, is capable of providing an answer.[130]   In short: the most serious condemnation of Mr. Keynes's treatment of what he calls the "elasticity of effective demand" is that the use of this concept, if accepted in the context in which Mr. Keynes presented it, would block

[129] See above, pp. 129, 131.
[130] See above, pp. 154 ff.

off completely one of the most inviting vistas opened to us by earlier writers on the Theory of Prices—writers who, if they did not succeed in resolving all of the issues raised by the problem indicated, did at least recognize the existence of the problem and did contribute to its ultimate solution; whereas the argument of Mr. Keynes's *General Theory* amounts only to a proposal that we either refuse to recognize the problem or regard it as incapable of solution altogether.

CHAPTER FOURTEEN

Keynes's "Elasticity of Prices" and the Framework of Monetary Theory

I

THE NEED FOR A FRAMEWORK

IT IS a simple fact of doctrinal history that the extreme complexity of the problem of the determination of money prices has led to the provision of formal algebraic *frameworks* for the study of this problem. From Volume I of the present work, we know that the abler sponsors of these frameworks have been under no illusions as to how far these frameworks, in and of themselves, carry us on the highroad toward what must be regarded as our ultimate goal: namely, as nearly complete an explanation as economic analysis is able to provide of why realized money prices, and the quantity of objects sold at those prices, are what they are.[1] These writers have made it clear, for example, that their object (in the words of the Keynes of the *General Theory*) was to provide, not "a machine, or method of blind manipulation," but simply an analytical device, or series of analytical devices, which would provide "*an organized and orderly method of thinking out particular problems.*"[2] And they have been quite aware, as the Keynes of the *Treatise* was aware, that these frameworks take on life and significance only "*when we have vitalized them by the introduction of extraneous facts from the actual world.*"[3]

But if the sponsors of these frameworks have been aware of their limitations, they have been quite insistent upon an

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[1] See Volume I, 82 f., and especially the references to Fisher and Pigou given in nn. 22 and 23 thereto.

[2] *General Theory,* 297.

[3] Cf. Volume I, 83, of the present work, and the reference to Keynes's *Treatise* given in n. 24 thereto.

adequate recognition of their virtues. They have insisted that we *do* need analytical "skeletons," of the type which these frameworks represent, if (again in the words of the Keynes of the *Treatise*) we are "to analyse and arrange our material in what will turn out to be a useful way of tracing cause and effect," and if we are to be able (in the words of the Keynes of the *General Theory*) to exhibit "the extreme complexity of the relationship between prices, [the quantity of objects sold at these prices], and the quantity of money." [4] They have argued, that is, as the Keynes of the *General Theory* has argued, that, *given* an "organized and orderly method of thinking out particular problems," such as is represented by an adequate algebraic framework for the study of the Theory of Money and Prices, we are able, "after we have reached a provisional conclusion by isolating the complicating factors one by one, . . . to go back on ourselves and allow, as well as we can, for the probable interactions of the factors among themselves"; but they have also argued (again as the Keynes of the *General Theory* has argued) that *without* such an analytical framework for applying our "formal principles of thought," we "shall be lost in the wood." [5]

It is a cardinal contention of the present work that this judgment has been vindicated a hundred-fold by the history of our subject.[6] But this work has also presented two further contentions. The first of these further contentions is that, of *all* the formal algebraic frameworks that have been proposed thus far, the one that has shown the greatest

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[4] Cf. the *Treatise*, I, 138, and the *General Theory*, 305. I have inserted the words "the quantity of objects sold at these prices" by way of making explicit what was certainly implicit not only in Mr. Keynes's description of the purpose of his formula for the "elasticity of *prices* in response to changes in the quantity of money," but also (despite statements to the contrary by members of the Keynesian group) in the familiar Quantity Equations (cf. above, p. 550, and the references given in nn. 1 and 2 thereto). The use of the analogy of the "skeleton" is taken directly from Pigou. Cf. Volume I, 83, of the present work, and the references given in n. 23 thereto.

[5] Cf. the *General Theory*, 297.

[6] Since a detailed specification in support of this contention would require a reiteration of a very large part of the substance of these two volumes, I must be content to refer here to the examples cited below, pp. 769 f., nn. 95 and 96.

flexibility, the greatest comprehensiveness, and the greatest possibilities for further constructive elaboration, is also the *oldest* of these algebraic frameworks: namely, the type of framework represented by a series of "stream" equations of the general Fisherine form.[7] And the second contention is that the power and comprehensiveness of this type of formulation is demonstrated by a further consideration: namely, that although the alternative frameworks *have certainly brought illumination to, and have certainly necessitated elaborations and refinements of, the "oldest" type of framework just indicated*, the superiority of the latter type of framework is evidenced by the fact that it has been able, with one notable exception, to *incorporate* all that is essential in the alternative formulations, and, at the same time, and with no exception whatever, to provide a necessary *analytical control over* these alternative frameworks.[8] The

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[7] The statement that these equations do represent the "oldest" type of framework will not be challenged when it is observed that we are speaking here of formal *algebraic* frameworks. For algebraic equations of the *Fisherine* form go back as far as the later eighteenth and early nineteenth centuries (see Volume I, 10 ff.); whereas the explicit presentation of algebraic equations of the "cash-balance" type do not go back further than Walras's presentation, in 1886, of an equation of the general form of the $n = pk$ of Keynes's *Monetary Reform* (see my "Léon Walras and the Cash-Balance Approach," *loc. cit.*, 573, 580 ff.), while explicit algebraic equations of the "income" type do not go back further than Schumpeter's presentation of his "income equation" in 1917 (see above, p. 104, n. 35, and the references there given). It is of the utmost importance, however, to observe that the statement with respect to the "superiority" of equations of the Fisherine type is to be interpreted strictly in the light of the following two sentences of the text, and of what is said in nn. 8 and 9, immediately following.

[8] On the "one notable exception," see the following paragraph of the text. But with respect to the *other* "alternative frameworks," I cannot emphasize too strongly the further proposition italicized in the text: namely, that *these* "alternative frameworks have certainly brought illumination to, and have certainly necessitated elaborations and refinements of, the 'oldest' type of framework just indicated." For to miss the significance of this further proposition is to miss what I regard as one of the major results of the work of "synthesis" within the field of the Theory of Money and Prices that I believe Volume I of this work represents. With respect to the particular "alternative frameworks," on the other hand, that may be said to be represented by the statement of the Theory of Prices in terms of (1) the concept of "liquidity preference"; (2) the relation between Saving and Investment; and (3) the "multiplier," it should be pointed out that while the nature of the demonstration of the applicability of the claim made in the text to the concept of *Liquidity Prefer-*

contention, that is, is that this "oldest" framework not only can, *after suitable elaboration and development,* do all that the alternative frameworks can do, but also can do *more* than any one of these frameworks, taken by itself, can do.[9] And this fact, it is argued, may in itself be regarded as providing a parallel, within the field of economic theory, to the kind of combination of happy "disillusion" and further "enlightenment" which representatives of the natural sciences have found in the history of their own subject—"disillu-

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ence should be clear from the brief remarks made above, p. 729, n. 126 (cf. also the backward references there given), and while something has been said with respect to the applicability of the claim indicated to a statement of the Theory of Prices in terms of the relation between *Saving and Investment* (see above, p. 476, and the reference to Volume I given in n. 29 thereto), I have purposely contented myself in this work with only the most general statements with respect to the applicability of the claim in question to the concept (or concepts) of the "multiplier" (see above, pp. 476 f., and p. 726). The reason for this procedure is again that I wish to discuss the relation of the "multiplier" (as well as of certain aspects of Savings-Investment techniques) to the general analytical apparatus presented in this work in more detail in a later publication on *The Generation and Utilization of Money Income* (see the Preface to the present volume).

[9] On the significance of the phrase italicized, see the preceding note. In the light of what is said in that note with respect to the work of "synthesis" within the field of the Theory of Money and Prices that I believe Volume I of this work to represent, it will be clear that I should myself regard as essentially unprofitable a dispute with those who might argue that if the *alternative* frameworks are *also* subjected to "suitable elaboration and development," they, too, may be regarded as capable of doing not only "all that the alternative frameworks" (including the framework represented by a system of stream equations of the general "Fisherine form") "can do, but also *more* than any one of these frameworks, taken by itself, can do." For even an admission that this is so would be perfectly consistent with the claim that the effect of the "elaboration and development" of these other frameworks would be to reduce them to a framework of the type advocated in the present work. (Cf., for example, what is said in Volume I, 449, n. 96, with respect to a "total transactions" equation of the *cash-balance* type, as represented by the expression $M = P \cdot kT$, which is of course the exact algebraic equivalent of $MV = PT$, when k and V are properly defined.) On the other hand, the nature of the reasons why I myself would support the statement advanced in the text should be clear from what has been said in this work (1) with respect to attempts to make the cash-balance approach carry more than it is capable of bearing (see above, p. 476, and the references to Volume I given in n. 31 thereto); and (2) with respect to attempts to argue that the use of "income" equations (or indeed any type of "partial" equation) makes it possible to *dispense with* "total transactions" equations of the general Fisherine form (see above, p. 502, and the references given in n. 97 thereto).

sion," in the form of a recognition that the process of later discovery, productive as it has been, has amounted primarily to an articulate emphasis upon "facts which were long before known and even instinctively perceived, our present recognition being simply more distinct and more definite"; and "elucidation," in that a *proper integration of the results of later discoveries into a consistent whole* "enables us to see everywhere throughout the most complicated relations the same simple facts." [10]

Readers of Volume I of the present work will have no difficulty in identifying the "one important exception" to the general rule that it has been found possible to *incorporate,* within an elaborated variant of the "oldest" of algebraic frameworks for the study of the Theory of Money and Prices, what is essential to the alternative frameworks.[11] The "important exception" is, of course, the framework represented by the Fundamental Equations of Keynes's *Treatise,* when these equations are interpreted as having attempted to provide *simultaneously* a "stream" formulation of the forces determining realized money prices and the quantities of objects sold at these prices, on the one hand, and what has been called in this work a "cost-profit" formulation, on the other.[12] It was the very possibility that an "important exception" *might* be involved that recommended

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[10] The quotations are from E. Mach. See Volume I, 83 f., of the present work.

[11] This statement is, of course, based upon the assumption that what is "essential" (in the sense of *peculiar*) to the Fundamental Equations of the *Treatise* is their attempt to perform *simultaneously* the two tasks indicated in the following sentence of the text. On the basis of the argument presented above, pp. 439 ff., on the other hand, it should be clear that the apparatus presented in this work is perfectly capable of "incorporating" a type of formulation *analogous to* the formulation of the *Treatise, in the special cases in which such a formulation is formally valid.* And it should be pointed out, finally, that the claim that the type of framework here advocated provides a *"necessary analytical control over"* the alternative frameworks is a claim that is advanced *"with no exception whatever"* (see above, p. 735); and that therefore it applies not only to the details of those parts of the *Treatise's* argument which were not indissolubly associated with the Fundamental Equations, but also to the use of the *framework* represented by these Fundamental Equations themselves. For examples of the type of "analytical control" indicated, see Volume I, 68 ff., 206 ff., 231 ff., 280 ff., 409 ff., 525 ff., 564 ff., 595 ff., as well as above, pp. 439 ff.

[12] See especially Volume I, 124 ff., of the present work; and cf. the preceding note.

a close examination, in two chapters of our first volume, of
the formal validity of the type of formulation represented
by the Fundamental Equations of the *Treatise*.[13]　For if it
had been possible to establish the formal validity of this
type of formulation under all circumstances, then a demon-
stration of the impossibility of incorporating its essential
features within even the most highly elaborated versions of
what was characterized above as the "oldest" of received
frameworks for the Theory of Money and Prices would have
convicted the latter of insufficient precision and compre-
hensiveness in the same degree as most of the alternative
frameworks.[14]　In Volume I of this work, however, it was
argued that the type of formulation represented by the
Fundamental Equations of the *Treatise* is *not* one that can
be regarded as formally valid under all circumstances; and
in the present volume it has been argued that when the type
of formulation indicated is restricted to the special cases
in which alone it would be formally valid, it is perfectly
capable of incorporation within the general analytical

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[13] It should be remembered that the reasons advanced in the *General
Theory* itself for the abandonment of the Fundamental Equations of the
Treatise were concerned neither with the formal validity of these equa-
tions under all circumstances, nor with the particular objections advanced
in Volume I of this work against the claim for such formal validity.　See
Volume I, 138 ff.　It should be remembered also that the episode repre-
sented by the *General Theory's* rejection of the Fundamental Equations
of the *Treatise* is not the first in which Mr. Keynes has advanced the
wrong reasons for abandoning a position he had previously defended.　His
"abandonment," in the *Treatise*, of the *cash-balance approach* of his *Mone-
tary Reform* is a case in point (cf. Volume I, 415 ff., of the present work);
and the very fact that the cash-balance approach thus "abandoned" in
the *Treatise* has taken on a new lease of life in the *General Theory* under
the disguise of the appellation "liquidity preference" (cf. above, p. 576,
n. 57), provides a further commentary, if such a commentary is necessary,
upon the contention of defenders of the *General Theory* that a concern
with the positions of the *Treatise* supposedly "abandoned" in the *Gen-
eral Theory* is necessarily "otiose" (cf. the references to Whale and
Kaldor given above, p. 157, n. 31; p. 464, n. 4; p. 611, n. 125).

[14] It must again be insisted that the charge of "insufficient precision
and comprehensiveness," as applied to "most of the alternative frameworks,"
is emphatically not to be interpreted as implying a charge that these alterna-
tive frameworks have themselves been without constructive effect upon the
framework advocated in the present work.　See above, p. 735, n. 8.

framework which has served as the foundation for the present work, and which is also the oldest of all the frameworks that have come down to us.[15]

This result takes on still greater significance when cognizance is taken of two further facts.   The first fact is that the argument of the *Treatise* was not only an argument *for* the type of framework represented by its Fundamental Equations, but also an argument *against* the type of framework upon which these two volumes have been built.[16]   The second fact is that, in the *General Theory*, Mr. Keynes has formally abandoned the type of framework advocated in his *Treatise*, with the result that the Fundamental Equations of that work will, in all probability, come to be regarded by the historians of our subject as a kind of economic *mule*, in the sense in which one historian of opera has characterized Debussy's *Pélléas and Mélisande* as "a sort of glorified *musical* mule: without pride of ancestry or hope of posterity." [17]   For these two facts raise at once the following question:

What *is* the formal algebraic framework for the study of the Theory of Money and Prices which is presented in the *General Theory*, in which Mr. Keynes has shown himself to be as insistent upon the need for such a framework as he had shown himself to be in his *Treatise*, but in which he has formally abandoned the framework which he had sponsored in the *Treatise*?

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[15] See again Volume I, 124 ff., and above, pp. 439 ff.

[16] Cf. Volume I, 12 ff., of the present work, and the references to the *Treatise* there given.

[17] It need hardly be emphasized that this statement is to be applied with the same degree of qualification (but no more) with which the statement of Ernest Newman (*Wagner as Man and Artist*, 324), quoted in the text, would have to be applied to the history of opera.   For examples of particular *details*, in the argument of earlier writers, which might be taken as providing a kind of "ancestry" for the Fundamental Equations of the *Treatise*, see Volume I, 130 ff., of the present work; and for an example of an attempt to *modify* these Fundamental Equations in such wise as to give them some "hope of posterity," cf. the reference to D. Hammarskjöld given above, p. 445, n. 95.   On the extremely important matter of the reasons advanced *by Mr. Keynes himself* for "abandoning" the Fundamental Equations, cf. what is said above, p. 738, n. 13.

II

THE HIGHROAD REGAINED

The answer to this question can come as a surprise only to those who have taken seriously (1) the *General Theory's* continued unfavorable comments upon formulations making use of "the quantity of money, . . . income velocity, . . . *et hoc genus omne*"; and (2) the practice, followed consistently by avowed defenders of the *General Theory,* of heaping ridicule upon just such formulations.[18] For the answer is this: that the expression formally presented in the *General Theory* as a framework for the study of "the relationship between prices and the quantity of money"— namely, $e = e_d \, (1 - e_e e_o + e_e e_o e_w)$—*is itself nothing more than a disguised variant of those very "Quantity Equations" which Mr. Keynes had formally rejected in his "Treatise" and continues formally to reject in the "General Theory."*

That this is so should be clear from a consideration of the following propositions:

1. The very description of the subject matter of the Theory of Money and Prices which Mr. Keynes now presents—namely, "the analysis of the relation between changes in the quantity of money $[M]$ and changes in the price level $[p]$, with a view to determining the elasticity of prices [the e ("without suffix") of the expression $e = M dp/p dM = e_d \, (1 - e_e e_o + e_e e_o e_w)$] in response to changes in the quantity of money"—represents a complete reversal of the position of the *Treatise,* in which Mr. Keynes was at such pains to disavow even the appearance of "reverting to the old-fashioned 'quantity of money' approach to the problem of price-determination."[19]   A major purpose, moreover, of the familiar Quantity Equations was, of course, precisely the presentation of a list of *"terms . . . upon which* the effect

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[18] For an example of the latter, see the references to R. F. Kahn in Volume I, 30, n. 56, 82, n. 20. Similar examples could, of course, be provided from the writings of any one of the better known among the avowed disciples of the *General Theory.* See, for example, the references to Joan Robinson given above, p. 83, n. 77, and to Kaldor, p. 517, n. 131.

[19] Cf. Volume I, 29 f., of the present work.

on prices of changes in the quantity of money *depends*." [20]
When, therefore, Mr. Keynes presents an algebraic formula
for "the elasticity of prices in response to changes in the
quantity of money," which purports to represent such a list
of "terms," and which in fact reduces to a simple Quantity
Equation, he must be regarded as accepting the type of
framework for the study of the Theory of Money and Prices
which is represented by these familiar Quantity Equations,
in a degree in which he certainly cannot be said to have
accepted it in the *Treatise*.[21]

That this result should have been obscured from many readers of
the *General Theory* may be regarded as due primarily to the unneces-
sarily forbidding algebraic aspect of the final formula presented as
measuring the "elasticity of prices in response to changes in the quan-
tity of money." [22]   In some degree, however, it may be due also to

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[20] For Mr. Keynes's characterization of the "four terms e_d, e_w, e_e, and
e_o" as the elements "upon which the effect on prices of changes in the
quantity of money depends," see the *General Theory*, 305; and on the
analogy between the procedure thus involved and the procedure by which
the familiar Quantity Equations were built up historically, see below, pp.
751 ff., under (2), and especially p. 753, n. 55.

[21] On the interpretation of those passages in the *Treatise* which might
be regarded as "occasional evidence of a willingness to put what he re-
garded as his newer type of analysis in terms which were essentially those
of the older 'quantity equations,' " see Volume I, 14 ff., of the present work.

[22] It should hardly be necessary to emphasize the point that even this
"unnecessarily forbidding algebraic aspect" would not have misled readers
of the *General Theory* if it had not been accompanied by formal denuncia-
tions of the familiar Quantity Equations as entirely useless or misleading.
There have been other instances in which a framework amounting essen-
tially to an elaborated "quantity equation" have taken on an algebraic
aspect that may be regarded as "forbidding." See, for example, the
"double equation" (equation 114) presented by E. Petersen, *Macro-Dynamic
Aspects of the Equation of Exchange*, 95.   But confusion was avoided in
these instances by an explicit statement that these more elaborate formula-
tions were in fact the result of an attempt to "split up into subsidiary factors"
"the traditional P, T, M, V concepts of the Fisher equation," and that,
by "reversing the analysis, we will be able to build up the Fisher con-
cepts . . . from our, so to speak, 'second-order' factors" (Petersen, *op. cit.*,
97).   Moreover, a very significant difference exists between (1) a formula-
tion whose "forbidding algebraic aspect" is due to the fact that it *carries
the analytical break-down further than the simpler formulations did;* and
(2) a formulation whose algebraic aspect may be characterized as "*unneces-
sarily* forbidding" because its complications are due, not to the number of
factors of analytical importance to which it gives separate notation, but
solely to such matters as (a) the use of "elasticity" coefficients and (b) the
process of translation and retranslation which is necessitated by the ex-

a lack of consistency, and even formal error, evidenced in Mr. Keynes's treatment of certain of the steps involved in his algebraic derivation.

To begin with, it is difficult to believe that the relation, to the familiar Quantity Equations, of Mr. Keynes's formula for the "elasticity of prices in response to changes in the quantity of money" ("e without suffix") would have been so greatly obscured if all of his readers had observed that the simpler formula for this "elasticity"—as opposed to the "final" formulation $e = e_d(1 - e_e e_o + e_e e_o e_w)$—is $e = \dfrac{Mdp}{pdM} = e_p e_d$, and that the simpler formulas, in turn, for e_d and e_p, respectively, are $e_d = \dfrac{MdD}{DdM}$ and $e_p = \dfrac{Ddp}{pdD}$; that is, $e = \dfrac{Mdp}{pdM} = \dfrac{MdD}{DdM} \cdot \dfrac{Ddp}{pdD}$. [23] For what the latter expression tells us is that, if we are to understand why the "response of money prices to changes in the quantity of money" (represented by $e = \dfrac{Mdp}{pdM}$) is as large as it is, we must attack the problem by two stages: namely, (1) a study of the reasons why the "response" of "effective demand" to changes in the "*quantity of money*" (represented by $e_d = \dfrac{MdD}{DdM}$) is as large as it is; and (2) why the "response" of money prices to changes in "effective demand" (measured by $e_p = \dfrac{Ddp}{pdD}$) is as large as it is. We know, of course, that, despite Mr. Keynes's failure to make the point clear, his "elasticity of effective demand" ($e_d = \dfrac{MdD}{DdM}$) is nothing but a variant of the concept of "income-velocity." [24] As far, therefore, as this part of the derivation of the formula for "the elasticity of prices in response to changes in the quantity of money" ($e = \dfrac{Mdp}{DdM}$) is concerned, it should be equally clear that Mr. Keynes's procedure is exactly the same as that followed by those earlier writers who undertook to insert the "missing term,"

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pression of some of the variables "in terms of the wage-unit" and of other of the variables "in terms of money."

[23] All of these expressions will be found on p. 305 of the *General Theory,* with the exception of the expression $e_p = \dfrac{Ddp}{pdD}$. For the latter expression, see pp. 285 and 304 of the *General Theory;* but see also what is said in the following paragraph of the text with respect to the divergent variants of the "more elaborate" definitions of e_p given in the two passages just cited.

[24] See above, pp. 681 ff. It is worth noting that Mr. Keynes introduced his presentation of the expression $e_d = \dfrac{MdD}{DdM}$, on p. 305 of the *General Theory,* with the specific statement (p. 304) that the concept is introduced in order to "deal with the case where *income-velocity is not constant*" (italics mine).

"velocity" (in this case, *income*-velocity), in expressions which included only terms for "the quantity of money" (M) and "prices" (p), respectively.[25] Thus, the only ambiguities and inconsistencies that appear thus far are those, associated with the relation between "effective demand" (D) and "income" (Y), and between "expected" and "realized" Demand and Income, respectively, which were discussed in the preceding chapter.[26] And if, as was suggested above, we proceed upon the assumption that least harm is done to this part of the argument of the *General Theory* by interpreting it as (1) being concerned only with *realized* magnitudes; and (2) involving a definition of "income" (Y) which would make "income" equal to "effective demand" (D), the difficulties of interpretation are not serious.[27]

Greater difficulties arise when we pass from the "simpler" formula for Mr. Keynes's "elasticity of money-prices in response to changes in *effective demand*" (namely, $e_p = \dfrac{Ddp}{pdD}$), to the more elaborate formulas for e_p which Mr. Keynes presents in order to be able to obtain his "final" formula for the "elasticity of prices in response to changes in the quantity of money"—that is, $e = e_d(1 - e_e e_o + e_e e_o e_w)$. For, to begin with, Mr. Keynes presents, not *one* "more elaborate" formula for e_p, but *two*. Thus, in one passage in the *General Theory*, we find $e_p = 1 - e_o(1 - e_w)$; whereas in another passage, we find $e_p = 1 - e_e e_o (1 - e_w)$.[28] Clearly, there is confusion here; and it is therefore hardly possible to blame the readers of the *General Theory* if they found it difficult to understand just how the "final" expression $e = e_d(1 - e_e e_o + e_e e_o e_w)$ was derived, and what it is supposed to mean.

Let us begin, however, with the *first* of the "more elaborate" formulas for $e_p = \dfrac{Ddp}{pdD}$: namely, $e_p = 1 - e_o(1 - e_w)$. The meaning of this expression is not difficult to ascertain if it is remembered (1) that it is derived from the expression $e'_p + e_o = 1$, in which $e_o = \dfrac{dO}{dD_w} \cdot \dfrac{D_w}{O}$; and (2) that the difference between e'_p and e_p is merely that e_p measures

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[25] See below, pp. 751 f., under (2).

[26] See above, p. 678, n. 2, and 694 ff.

[27] For the "suggestion" indicated, see above, p. 679, n. 2; and see also below, p. 744, n. 31.

[28] The first of these expressions for e_p is to be found on p. 285 of the *General Theory*; the second is to be found on p. 305. As far as I have been able to discover, Mr. Keynes made no attempt, at any point in the *General Theory*, to explain the discrepancy. The fact is the more striking in view of the circumstance that the term e_e, whose presence in the "more elaborate" formula for e_p presented on p. 305 is what differentiates it from the "more elaborate" expression for e_p presented on p. 285, from which e_e is absent, was introduced into the discussion on p. 282, only three pages before the presentation of the formula for e_p from which e_e was omitted.

"the elasticity of money prices in response to changes in effective demand measured in terms of *money*," whereas e'_p represents the "elasticity of prices in response to changes in effective demand" in terms of "wage-units." [29]   If, further, we assume, as we have assumed hitherto, that the "wage-unit" is here used merely as a kind of *numéraire*, there is no serious difficulty in interpreting the meaning of $e'_p + e_o = 1$.[30]   For, as Mr. Keynes himself makes clear, this expression is derived from an expression of the form $D = Op$; and if we remember that, by Mr. Keynes's own statement, D is equal to MV, it should be clear that the expression $D = Op$ is nothing more than a simple Quantity Equation of the general form $MV = Op$.[31]   Given this simple fact, the expres-

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[29] For the expression $e'_p + e_o = 1$, see the *General Theory*, 285. For the expression $e_o = \dfrac{dO}{dD_w} \cdot \dfrac{D_w}{O}$, see p. 283, where Mr. Keynes writes $e_{or} = \dfrac{dO_r}{dD_{wr}} \cdot \dfrac{D_{wr}}{O_r}$, and cf. p. 285, where it is stated that "the elasticities without a suffix r apply to industry as a whole." The statement with respect to the difference between e_p and e'_p follows from (1) the expression $e'_{pr} = \dfrac{dp_{wr}}{dD_{wr}} \cdot \dfrac{D_{wr}}{p_{wr}}$, presented on p. 284, when it is interpreted in the light of the statement on p. 285 with respect to the possibility of applying "the same line of argument" to "industry as a whole" simply by stating the argument in terms of "elasticities without a suffix r"; and from (2) the expression $e_p = \dfrac{Ddp}{pdD}$, presented on p. 285.

[30] On the "wage-unit" as a *numéraire*, see above, pp. 597 f., and the references given in n. 101 thereto.

[31] For the derivation of the expression $e'_p + e_s = 1$ from an expression of the general form $D = Op$ (or, in Mr. Keynes's expression, the derivation of the expression $e'_p + e_o = 1$ from the expression $e'_{pr} + e_{or} = 1$, which in turn is derived from the expression $O_r p_{wr} = D_{wr}$), see the *General Theory*, 284 f. For the expression $D = MV$, see again p. 304 of the *General Theory*. The difficulties introduced into Mr. Keynes's exposition by his careless treatment of the relation between *expected* and *realized* magnitudes are illustrated by the fact that the p of the expression $O_r p_{wr} = D_{wr}$ is defined (p. 284) as "the *expected* price in response to changes in effective demand," and the p of the expression $e_p = \dfrac{Ddp}{pdD}$ is likewise defined (p. 285) as "the *expected* price of a unit of output as a whole in terms of money"; whereas the expression e_p itself is defined (p. 285) as "the elasticity of *money-prices* in response to changes in effective demand in terms of money," and this e_p is juxtaposed with the expression $e_w = \dfrac{DdW}{WdD}$, defined as "the elasticity of *money-wages* in response to changes in effective demand in terms of money," W being defined as "the money-wages of a unit of labor," without any specification that it is to be conceived of as an "expected" magnitude. Cf. also p. 304, where it is stated that "the condition $e_w = 1$ means that the *wage-unit in terms of money* [not, it will be observed, the "expected"

sion $e'_p + e_o = 1$ is seen to represent a proposition which is at once extremely simple and extremely familiar: namely, that *if the "elasticity of effective demand"* (or the cognate variant of "income-velocity") *involves a definition of "demand"* (or "income") *which makes this "demand" (D) necessarily equal to expenditure upon* (or equal to the "value of") *output sold (Op)*, it follows that variations in this "demand" must be reflected in variations either in the amount of "output" (*O*), or the prices (*p*) at which this output is sold, or in both.[32] If the variation in "effective demand" is reflected *entirely* in variations in *prices*, we have e_o equal to zero, and, by the expression $e'_p + e_o = 1$, we then have $e'_p = 1$.[33] If the variation in effective demand is reflected entirely in variations in the quantity of *output* sold, we have e'_p equal to zero, and (again by virtue of the expression $e'_p + e_o = 1$), we then have $e_o = 1$. If, however, the variation in effective demand is reflected in variations in *both* the quantity of output sold and in the prices at which this output is sold, the values assumed by e'_p and e_o, respectively, will reflect the degrees in which the effects of these variations in demand will be distributed between effects on prices, on the one hand, and effects on output sold, on the other. Despite statements to the contrary by defenders of the *General Theory*, this is hardly a proposition which can be regarded as a novelty by instructed users of simple Quantity Equations of the general form $D = MV = OP$.[34]

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wage-unit in terms of money] *rises* in the same proportion as the effective demand, since $e_w = \dfrac{DdW}{WdD}$" (italics mine.)

[32] Cf. the *General Theory*, 285: "Effective demand spends itself, partly in affecting output and partly in affecting price, according to this law [namely, the "law" $e'_{pr} + e_{or} = 1$]." It will be observed that, despite the definition of p_{wr} as "the *expected* price" (p. 284; cf. the preceding note), there is no suggestion that the "law" is *not* directly applicable to *realized* changes in "effective demand," "output," and "price." On the significance of the clause italicized in the text, with respect to the *definition* of "effective demand," see below, p. 749.

[33] Cf. the *General Theory*, 286: "If $e_o = 0$ . . . .. output will be unaltered and prices will rise in the same proportion as effective demand in terms of money." Cf. also p. 306: "Now $e = 1$, . . . if $e_d = 1$ and $e_o = 0$."

[34] For an example of the type of statement indicated, see above, p. 481, n. 38; and cf. also the forward references there given. A full demonstration, on the other hand, of the absurdity of regarding as a novelty the suggestion that a change in the flow of money payments may affect not only "prices" but also the "volume of goods sold," would require so extensive a list of citations, from writers of the eighteenth century to Mr. Hawtrey, that it must be left for another occasion. It should be sufficient here to call attention (1) to what is said in Volume I, 94 f., of the present work, concerning *Hume* (cf. also above, p. 38, n. 103); and (2) to the kind of passage which can be found even in Fisher's *Purchasing Power of Money*, whose treatment of the effect of monetary expansion and contraction upon output can hardly be regarded as typical of the best available in economic literature at the time it was written, or even as typical of Professor Fisher's

But, *given* the definition of "demand" as equal to *OP*, it is hardly a proposition with which one can quarrel.

We have now to establish the relation between the expression $e'_p + e_o = 1$, on the one hand, and the expression $e_p = 1 - e_o(1 - e_w)$, on the other. Algebraically, of course, we have $e_p = 1 - e_o + e_o e_w$, or $e_p + e_o - e_o e_w = 1$, and $e'_p + e_o = 1$—that is, the expression $e_p + e_o$ will be equal to $e'_p + e_o$ whenever e_w (and therefore $e_o e_w$) is equal to zero.[35] The *economic* meaning of this proposition, on the other hand, should become clear once it is remembered that the difference between e'_p and e_p is merely that the former is expressed in terms of "wage units," while the latter is expressed "in terms of money." [36] That is, if variations in "effective demand," *even when the latter is measured in terms of wage units,* are accompanied by *no* variations in the "wage unit," it makes no difference whether we use expressions of the form $e_p + e_o = 1$ or $e'_p + e_o = 1$.[37] But *as long as one of our variables* (namely, the D_w of our $e_o = \dfrac{dO}{dD_w} \cdot \dfrac{D_w}{O}$) *is measured in terms of wage units*, it is necessary to provide an additional term (namely, the "elasticity of money wages in response to changes in effective demand in terms of money," or $e_w = \dfrac{DdW}{WdD}$) which will have the effect of correcting the D_w of the expression $e_o = \dfrac{dO}{dD_w} \cdot \dfrac{D_w}{O}$ in such a way as to make it equivalent to the D of the expression $e_p = \dfrac{Ddp}{pdD}$.[38] That is, instead of $e_p + e_o =$

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own later position. Contrast, for example, the passage from *The Purchasing Power of Money* cited above at the end of n. 46 to p. 110, with passages such as the following from the same work: "The surplus money is first expended at nearly the old price level. . . . In the meantime, the volume of purchases will be somewhat greater than it would have been had prices risen more promptly. In fact, from the point of view of those who are selling goods, it is the possibility of a greater volume of sales at the old prices which gives encouragement to an increase of prices" (p. 62). "Profits increase, loans expand, *and the Q's increase*" (p. 63; italics mine).

[35] Cf. the *General Theory*, 306, where a similar conclusion is drawn with respect to Mr. Keynes's "final" formulation $e = \dfrac{Mdp}{pdM} = e_d(1 - e_e e_o + e_e e_o e_w)$.

[36] See above, p. 744, n. 29.

[37] Cf. the *General Theory*, 306, where the condition $e_w = 0$ is translated into the condition that "money wages are fixed."

[38] For the expression $e_w = \dfrac{DdW}{WdD}$, see the *General Theory*, 285. The statement in the text obviously provides the economic meaning also for Mr. Keynes's statements (*General Theory*, 286, 304), based on the algebra of the expression $e_p = 1 - e_o - e_o e_w$, that "output will be unaltered and prices will rise in the same proportion as effective demand in terms of

1, we have $e_p + e_o - e_o e_w = 1$, or $e_p = 1 - e_o(1 - e_w)$.[39] Each reader must decide for himself as to the degree of illumination which is provided by this use of the "wage-unit," in contrast with a formulation resting on a definition of e_o which, like that of e_p, would relate changes in "output" to "changes in effective demand in terms of money" (D), rather than in terms of the "wage-unit" (D_w).[40] Each reader must

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money"—that is, e_p will be equal to unity—*not only* "if $e_o = 0$," *but also* "if $e_w = 1$"; and his similar statement (*General Theory*, 306)—based on the algebra of the expression $e = e_d(1 - e_e e_o + e_e e_o e_w)$—that "$e = 1$, if $e_d = 1$ and $e_w = 1$." For what these statements mean is that as long as e_o is defined as $e_o = \dfrac{dO}{dD_w} \cdot \dfrac{D_w}{O}$, the value of e_o may change *not only* as the result of a change in the relation of $\dfrac{dO}{O}$ to $\dfrac{dD}{D}$, but also as a result of a change in D_w which is due, not to a change in the D of the expression $D = D_w W$ (cf. the *General Theory*, 285, n. 1) or $\dfrac{D}{W} = D_w$, but merely to a change in the W of the latter expression. That is, in order to ascertain the response of "output" to changes in "effective demand *in terms of money*" (D), it is necessary to allow for the changes in the value of e_o which are due solely to those changes in the value of the D_w of the expression $e_o = \dfrac{dO}{dD_w} \cdot \dfrac{D_w}{O}$ which in turn are due, not to changes in D, but to changes in W. On the difficulties raised by the fact that, elsewhere in the *General Theory* (p. 304), Mr. Keynes defines e_o, not as $e_o = \dfrac{dO}{dD_w} \cdot \dfrac{D_w}{O}$ but as $e_o = \dfrac{DdO}{OdD}$, see what is said in note 40, below.

[39] Cf. the algebraic derivation of the latter expression presented on p. 285, n. 1, of the *General Theory*. It should be clear that all of this algebra would have been unnecessary if e_o had been defined as Mr. Keynes in fact defines it on p. 304 of the *General Theory*: namely, as $e_o = \dfrac{DdO}{OdD}$ (cf. the following note). For in that case, it would have been entirely proper to write $e_p + e_o = 1$.

[40] It is something of a commentary on the "degree of illumination" brought by Mr. Keynes's practice in this respect, that although he later writes a formula for e_o of precisely the type indicated (cf. nn. 38 and 39, immediately preceding), he neither points out that a change in definition *is* involved, nor undertakes the modification of his "final" formulation for $e = \dfrac{Mdp}{pdM}$, presented on the very next page (p. 305) of the *General Theory*, which is called for if e_o is defined as it is on p. 304 rather than as it is on pp. 283 ff. For it is clear that if e_o is defined as $e_o = \dfrac{DdO}{OdD}$, the introduction of e_w into the "final" formulation is not only superfluous, but actually erroneous. That is, *given* the definition $e_o = \dfrac{DdO}{OdD}$, the introduction of

decide for himself, also, whether enough is gained by the translation, into a series of "elasticities" of the form $e_p = 1 - e_o(1 - e_w)$, of what is after all only a very simple variant of the familiar Quantity Equations, to compensate for the degree of obfuscation which was bound to result with regard to the relation between an expression of the type $e_p = 1 - e_o(1 - e_w)$, on the one hand, and these Quantity Equations, on the other.[41] Here, however, we are concerned only with the internal consistency of the processes whereby Mr. Keynes derived his "more elaborate" formulas for his "elasticity of prices in response to changes in effective demand" $\left(e_p = \dfrac{Ddp}{pdD} \right)$; and, specifically, our problem is to establish the implications of the fact that at one point Mr. Keynes wrote $e_p = 1 - e_o(1 - e_w)$, whereas at another point he wrote $e_p = 1 - e_e e_o(1 - e_w)$.[42] The difference between the two expressions, it will be observed, is that the second expression includes a term for the "elasticity of employment" $\left(e_e = \dfrac{dN}{dD_w} \cdot \dfrac{D_w}{N} \right)$, purporting to measure "the response of the number of labor-units employed [N] . . . to changes in the number of wage-units [D_w] which are expected to be spent on purchasing its output," whereas the first expression includes no such term.[43]

Now, as a matter of algebra, it is clear that the expressions $e_p + e_o - e_o e_w = 1$ (or $e_p = 1 - e_o(1 - e_w)$) and $e_p + e_e e_o - e_e e_o e_w = 1$ (or $e_p = 1 - e_e e_o(1 - e_w)$) will *both* be accurate only if $e_e = 1$.[44] But of

the term $e_w = \dfrac{DdW}{WdD}$ would be permissible only if, as is suggested in the text below, "effective demand" is defined as expenditure upon output *and employment,* and the whole expression for $e = \dfrac{Mdp}{MdM}$ is regarded as a translation of a quantity equation of the form $D = MV = Op + NW$, rather than as a translation of a quantity equation of the form $D = MV = Op$.

[41] Again it should be observed that such obfuscation need not have resulted from the mere translation of these familiar Quantity Equations into a series of "elasticities," if Mr. Keynes had followed the example of earlier users of such "elasticities" in making clear that what was involved *was* just such a translation, instead of preceding his exposition by a diatribe against the use of such equations *"et hoc genus omne."*

[42] See above, p. 743, n. 28. As far as I have been able to discover, Mr. Keynes nowhere even comments upon the difference between the two formulas for e_p, to say nothing of undertaking to explain either the origin or the meaning of the difference.

[43] For the expression $e_e = \dfrac{dN}{dD_w} \cdot \dfrac{D_w}{N}$, see the *General Theory,* 282.

[44] Cf. the similar remark on p. 306 of the *General Theory,* on the basis of the algebra of the expression $e = e_d(1 - e_e e_o + e_o e_w)$, to the effect that

course there is no reason whatever for assuming that e_e will be equal to unity under all circumstances; so that Mr. Keynes must be accused of formal error in his algebraic derivation of his "final" formula for

$$e = \frac{Mdp}{pdM} = e_d(1 - e_e e_o + e_e e_o e_w).^{45}$$

The *source* of this formal error, on the other hand, is easily discovered if it is remembered that the crucial expression in the derivation of the formula $e_p = 1 - e_o(1 - e_w)$ is $e'_p + e_o = 1.^{46}$ For we know that the latter expression will be generally valid only when our definition of the "elasticity of effective demand" involves a definition of "demand" which makes this "demand" necessarily equal to expenditure upon (or equal to the "value of" *output*) sold $(Op).^{47}$ If, on the other hand, "demand" is defined as necessarily equal to expenditure upon *output* sold *plus* expenditure upon the number of labor units "sold" ("employment")—that is, if D is defined as equal, not to O_p, but to $O_p + NW$—then we require, not $e'_p + e_o = 1$, but $e'_p + e_o + e_e = 1$, or at best $e'_p + e_e e_o = 1.^{48}$ We are forced, therefore, to choose be-

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"$e = 1$, if $e_d = 1$ and $e_w = 1$." I am, of course, assuming here that Mr. Keynes, despite his definition of e_o on p. 304 of the *General Theory* as $e_o = \frac{DdO}{OdD}$, intended to define it as it is defined on pp. 283 ff., namely, as $e_o = \left(\frac{dO}{dD_w} \cdot \frac{D_w}{O} \right)$, so that it will be comparable to his definition of $e_e \left(= \frac{dN}{dD_w} \cdot \frac{D_w}{N} \right)$ in terms of "wage-units," and will therefore require the use of the "corrective" factor e_w (see above, p. 747, n. 40). Since, however, we are speaking here of what follows from Mr. Keynes's formula "as a matter of algebra," it should be pointed out that at least one of Mr. Keynes's own conclusions certainly does not follow "as a matter of algebra" from his own formulation. From that formulation, it would not follow that "$e = 1$, . . . if $e_d = 1$, $e_w = 0$, and $e_e e_o = 1$" (*General Theory*, 306). Under these conditions, on the contrary, e would be equal, not to 1, but to zero. For e to be equal to unity, $e_e e_o$ would itself have to be equal to zero.

[45] The charge of "formal error" is of course based upon the fact that Mr. Keynes himself refers to the *earlier* steps in his algebraic manipulation as the basis for his "final" expression $e = \frac{Mdp}{pdM} = e_d(1 - e_e e_o + e_e e_o e_w)$. See the *General Theory*, 285, 304 f.

[46] Cf. above, p. 746.

[47] Cf. above, p. 745, and n. 32 thereto.

[48] The admission of the possibility that we may write $e'_p + e_e e_o = 1$, as well as $e'_p + e_o + e_e = 1$, is an admission made only for the sake of reducing the objections to Mr. Keynes's "final" formulation to the absolute minimum *for our present purposes*. For one has only to consider the details of Mr. Keynes's own argument to observe that expressions including a term such as $e_e e_o$ are particularly subject to the dangers to which we are exposed when, instead of using a type of analysis which enables us

tween the expression $e_p = 1 - e_o(1 - e_w)$, on the one hand, and the expression $e_p = 1 - e_e e_o(1 - e_w)$, on the other.[49]   And the choice between the two expressions will depend upon our choice between the definition of "demand" given by the expression $D = Op$ and the definition of "demand" given by the expression $D = Op + NW$.[50]   Thus, if

to "know all the time what we are doing and what the words mean," we find ourselves "blindly manipulating" our algebraic symbols in such a way as "to lose sight of the complexities and interdependencies of the real world in a maze of pretentious and unhelpful symbols" (*General Theory*, 297 f.).   Consider, for example, the implications of Mr. Keynes's statement that "if there is full employment *either* of labor or of equipment, $e_e e_o = 0$" (*General Theory*, 306; italics mine).   That this follows *algebraically* from the very expression $e_e e_o$, there is, of course, not the slightest doubt.   But what is the *economic* implication of Mr. Keynes's statement?   Suppose, for example, that while e_e is equal to zero, e_o is greater than zero (the case realized when our production functions are such as to yield a greater amount of output with the same amount of labor [employment]).   If we make use of the expression $e'_p + e_o + e_e = 1$, we see at once that, although e_e is equal to zero, e'_p will *not* be equal to unity, because e_o will be positive; whereas if we make use of the expression $e'_p + e_e e_o = 1$, we are forced to conclude—quite wrongly—that $e'_p = 1$.   The case in question, it will be observed, is different from that represented by the expression $e_p e_d$ (cf. above, p. 742, and the reference to the *General Theory* given in n. 23 thereto).   For in the latter case, we are entitled to assume, *as a matter of economics*, (1) that if there is a change in the quantity of money

$\left(\text{the } M \text{ of the expression } e = \dfrac{Mdp}{pdM} \right)$, it is capable of affecting prices

only if the additional money is used to exert a new "demand," so that, if e_d is zero, we may conclude that e will also be zero; and (2) that if the new "demand" does not result in an increase in prices (that is, if $e_p = 0$), then the new money used to implement this "demand" will likewise not result in an increase in prices—that is, e also will be equal to zero (cf. also, however, what is said below, p. 765, n. 87).   In other words, it is difficult to believe otherwise than that, in making use of the implied expression $e'_p + e_e e_o = 1$ rather than the expression $e'_p + e_o + e_e = 1$, Mr. Keynes was misled into the conclusion that "if there is full employment *either* of labor or of equipment," e'_p will be equal to unity, because he failed to "keep 'at the back of his head' the necessary reserves and qualifications" which he has sometimes made in discussing the relations between movements in "output," on the one hand, and "employment," on the other (see above, p. 534, and the references given in n. 28 thereto).

   [49] I am of course abstracting here from the difficulties raised by Mr. Keynes's divergent usage with respect to the definition of e_o (cf. above, p. 747, nn. 39 and 40)—that is, I am assuming here that e_o, as well as e_e, involves a definition of "effective demand" in terms of *wage-units* (D_w), rather than in terms of *money* (D), so that both e_o and e_e require the degree of "correction" represented by the insertion of the term e_w.   Otherwise, of course, the choice could be said to lie between $e_p = 1 - e_o$ (or $e_p + e_o = 1$) and $e_p = 1 - e_e e_o$ (or $e_p + e_e e_o = 1$).   Cf. above, p. 747, n. 39.

   [50] It will be observed that an expression of the latter type, like the expres-

we accept the definition of "demand" implicit in the expression $e'_p + e_o = 1$, the "final" expression for the "elasticity of prices in response to changes in the quantity of money" $\left(e = \dfrac{Mdp}{pdM} \right)$ should be, not $e = \dfrac{Mdp}{pdM} = e_d(1 - e_e e_o + e_e e_o e_w)$,   but $e = \dfrac{Mdp}{pdM} = e_d(1 - e_o + e_o e_w)$.[51]
The points made here are merely (1) that, in writing the former expression, Mr. Keynes has been guilty of inconsistency in his algebraic manipulation; (2) that it is difficult to believe that Mr. Keynes would have been guilty of this inconsistency if, instead of working with his cumbersome "elasticities," he had worked with simple Quantity Equations of the general form $D = MV = Op + NW$ or (when D and V are subjected to the required change in definitions) $D = MV = Op$; and (3) that a further effect of his failure to work with expressions of the latter type has been to obscure the fact that his expression for the "elasticity of prices in response to changes in the quantity of money," despite its terrifying algebraic aspect, is nothing more than a particularly simple variant of precisely those Quantity Equations which both Mr. Keynes and his disciples continue formally to reject as useless or misleading for the general purposes of monetary theory.

2. The procedure followed by the *General Theory* in *building up* the full expression for "the elasticity of prices in response to changes in the quantity of money" from the preliminary expression $e = Mdp/pdM$ is the very procedure by which, as a matter of doctrinal history, the familiar Quantity Equations were built up out of more primitive formulations.[52]   For, in writing $e = \dfrac{Mdp}{pdM}$, Mr. Keynes begins, as monetary theorists from Bodin to the sponsors of the concept of an "elasticity of demand for money" have begun, by *recognizing the existence* of a significant "*relation between* changes in the quantity of money and changes in the price level."[53]   And when he goes on to elaborate this

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sion $e'_p + e_o + e_e$, is free from the difficulties raised by the use of the expression $e_e e_o$ whenever *either* the e_e or the e_o varies, but not both. Cf. above, p. 749, n. 48; and see also what is said below, p. 765, n. 87.

[51] Or, if "effective demand" is defined in terms of *money* (D), rather than in terms of wage units (D_w), so that we may write $e_p + e_o = 1$, or $e_p = 1 - e_o$, the "final" expression for the "elasticity of prices in response to changes in the quantity of money" would be $e = \dfrac{Mdp}{pdM} = e_d e_p = e_d(1 - e_o)$.

[52] See Volume I, 90 ff., of the present work.

[53] It should be observed that if we substitute, for "the price level" *(p)*,

formulation by inserting a set of "terms . . . *upon which*
the effect on prices [*p*] of changes in the quantity of money
[*M*] *depends*," he is following exactly the same procedure
which was followed by successive generations of monetary
theorists.[54]   For we know, from Volume I of the present
work, that a large part of the work of these earlier theorists
consisted precisely of the "insertion of missing terms" in the
more primitive formulations with respect to the nature of
the forces determining "the relation between changes in the
quantity of money and changes in the price level," with the
result that their work either (1) converted "a proposition
which was at best true only under definite assumptions, and
at worst was generally and literally false, into a proposition
that can be shown to be capable of the widest possible appli-
cation and of passing the most exacting scientific scrutiny";
or (2) converted into a *useful* proposition a proposition
which, although "true" enough as it stood, was not suffi-
ciently *useful* in the analysis of the causes of price change,
because it failed to *specify the nature* of the forces upon
which the *degree* of price change associated with a given
change in the quantity of money "*depends*," by presenting a

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its reciprocal, "the purchasing power ['value'] of money," Mr. Keynes's
formula for the "elasticity of prices in response to changes in the quantity
of money" ("*e* without suffix") is the exact algebraic equivalent of the
formula for the "elasticity of demand for money" (see, for example, the
reference to Lehfeldt given above, p. 677, n. 1). It is worth observing also
that although Bodin's recognition of "the existence of a significant relation
between changes in the quantity of money and changes in the price level"
certainly represented a major achievement considering the state of monetary
theory in his day (cf. Volume I, 93, of the present work), he himself did
not stop with this "recognition," but went on to emphasize that the *ex-
planation* of the precise *degree* of "relation" found to prevail would have to
include a consideration of the factors *other* than changes in the "quantity
of money" which can be shown to be capable of affecting prices. Cf. Volume
I, 93, and the references given in n. 50 thereto.

[54] For the statement in quotation marks, see the *General Theory*, 305.
For proof of the statement with respect to the "procedure which was fol-
lowed by successive generations of monetary theorists," cf. the reference to
Volume I given above, page 751, n 52, and also what is said above, page 655,
n. 62; page 661, n. 78; and page 661, n. 80, with respect to the relation between
the use of the *concept* of an "elasticity of demand for money," on the one
hand, and the use of the familiar Quantity Equations, on the other hand, as a
framework for the *explanation* of why this "elasticity" is as large as it is.

list of the *concrete elements* which would *account for* the degree of price change evidenced in each concrete case.[55]

3. The *particular* "concrete elements" introduced by Mr. Keynes in his developed formula for the "elasticity of prices in response to changes in the quantity of money," by way of indicating the nature of the forces which will determine the magnitude of this "elasticity" (namely, the elements of money "demand" [D], "output" [O], "employment" [N], and the price per unit of employment [the "wage unit," W]) have all been included in earlier variants of the familiar Quantity Equations.

If, for example, it is remembered that Mr. Keynes himself has written $D = MV$, it follows that the equivalent of D has been included in all those "income equations" in which "income" or "income velocity" have been given definitions which would make them equivalent to the D and the V, respectively, of Mr. Keynes's expression $D = MV$.[56] Similarly, the term O has been included not only *implicitly*, in "stream" equations of the "total transactions" type, but also *explicitly* in "stream" equations of the "partial" type, and particularly of the "income" type.[57]

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[55] See Volume I, 55 ff., 90 ff., 98. It is clear, of course, that the procedure followed by Mr. Keynes in passing from the rudimentary formulation $e = \dfrac{Mdp}{pdM}$ to his more elaborate list of "terms . . . *upon which* the effect on prices [p] of changes in the quantity of money [M] *depends*" represents the type of procedure indicated under (2), rather than that indicated under (1). The fact, however, that it *does* represent the type of procedure indicated under (2) provides a further commentary upon Mr. Kaldor's triumphant question, "Why $MV = PT$? Why not $A = B$?" (cf. above, p. 517, n. 131). One might ask, with as much (or as little) reason, "Why $e = e_d(1 - e_e e_o + e_e e_o e_w)$? Why not $A = B$?"

[56] On the expression $D = MV$, see above, p. 678, n. 2; and on the relation between "income" (Y) and "effective demand" (D) in Mr. Keynes's own treatment, see above, pp. 694 ff. For examples of earlier "stream equations" in which "income" (and therefore "income-velocity") have been given definitions which would make "income" equivalent to *outlay* from income ("effective demand"), see above, p. 695, n. 40.

[57] See, for example, the references to the equations of Schumpeter, Pigou, Aftalion, and Lindahl, respectively, given in Volume I, 339, n. 111; 416, n. 4; 339, n. 112; and 328, n. 78; also what is said above, p. 701, n. 56, with respect to the relation of Keynes's $Y = OP$ to equations such as Aftalion's $R = PQ$. The fact that, in some of the cases cited (as in the cases of Schumpeter and Lindahl), the volume of "output" (or the "social product," or "real income") was identified with the output of *consumers'* goods is of course not important in the present context, particularly in the light of (1) Mr. Keynes's own argument, in his *Treatise*, with respect to the form of equation required if we are to measure "the purchasing power of money" (see Volume

Likewise, a term for "employment" (N) has been included not only *implicitly* in stream equations of the "total transactions" type, but also explicitly in those "stream" equations in which separate notation was given for the "quantity" of *labor "services" sold* within the time period covered by such an equation.[58] And finally, a term for the "price per unit of employment" (the "wage-unit") has not only been included implicitly in stream equations of the "total transactions" type (including those whose sponsors argued specifically that "wages" are to be regarded as a particular type of "price" whose movements might often differ considerably from the movements of many *"commodity"* prices), but has also appeared explicitly in those variants of a "total transactions" equation making use of a "plurality of price levels," of which the "price level" of the "services" of labor ("wage or salary") was one.[59]

More significant for our present purpose, however, is the fact that the unsupplemented use of a "stream" equation of the type indicated, to which Mr. Keynes's formula for the "elasticity of prices" (*"e* without suffix") reduces, *can hardly be said to represent the most advanced type of formulation of the Theory of Prices* which can be constructed on the basis of "stream" equations of the general Fisherine form. It is subject, for example, to all the limitations attaching to formulations including a term for "income-velocity" (the equivalent of Keynes's "elasticity of effective demand") which have made no attempt to provide an analytical breakdown of this concept, of a kind which would make clear the distinction between (1) *income* (our "payments into income," $(PT)_I$), and (2) *outlay* from income (our "payments out of income," $(PT)_i$); to say nothing of the further possibility, opened by such a distinction, of relating "income" to "outlay" from that income by means of the use of clock-time period subscripts and the study of the facts with respect to cash-balance administration.[60] It is subject, likewise, to all the limitations of formulations which have made no attempt to relate "output" either to "output *sold*" or to the various

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I, 485 ff., of the present work) ; and (2) the argument presented in Volume I, 485 ff., 516 ff., with respect to the relation between "consumers' goods equations" and other types of "stream equations." On the term O as being *"implicit* in stream equations of the 'total transactions' type," see Volume I, 514 ff., 599.

[58] In addition to the reference to C. F. Roos given in Volume I, 571, n. 5, see, for example, E. Petersen, *Macro-Dynamic Aspects of the Equation of Exchange,* 37, 99; and cf. also the general discussion of the treatment of "services" in an equation of the "total transactions" type in Volume I, 571 ff., including the references given on p. 572, nn. 6 and 7.

[59] See the references given in the preceding note; and for an example of a treatment of "wages" as a particular type of "price," in a context concerned specifically with the problem of "The Dispersion of Prices," see Fisher, *Purchasing Power of Money,* 186 f.

[60] See above, pp. 694 ff., and especially p. 728, n. 124, and the references to Volume I of the present work given above, p. 701, n. 55.

types of "nonoutput transactions" whose magnitude can certainly be shown to be relevant for the magnitude of the money stream available for the purchase of "output." [61] And it is subject, finally, to all the limitations of formulations which have made no attempt to break down the category of "the price level of output" into a *"plurality* of price levels" attaching to various *categories* of "output" sold.[62]

To be sure, there can be no denying that insofar as we are justified in translating Mr. Keynes's "final" formulation for the "elasticity of prices in response to changes in the quantity of money" into a "stream" equation of the general form $MV = D = Op + NW$, we are dealing here with *one* set of "plural" price levels—the "price levels" in question being, respectively, (1) the "price level" of "output" sold; and (2) the "price level" of "employment" (the "wage-unit").[63] But it is certainly possible to deny validity to the particular charge levelled by Mr. Keynes against all earlier users of "stream" equations of the general Fisherine form: namely, that, the formulations presented by these writers do not adequately "distinguish between changes in prices which are a function of changes in output, and those which are a function of changes in the wage-unit." [64] Unfortunately, as so often

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[61] See above, pp. 698 ff., 714 ff., and the references to Volume I given above, pp. 716 f., nn. 99 and 100, and p. 717, n. 102.

[62] Contrast the examples given in Volume I, 428, 513 f., 516 ff., of the present work. It should be remembered that in this respect, as in a number of others, the argument of the *General Theory* is to be regarded as inferior even to Keynes's own *Treatise.*

[63] On the possibility, and the analytical consequences, of interpreting the formulation in question as *other* than the equivalent of a "stream" equation, see what is said below, pp. 761 ff.

[64] *General Theory,* 209. Unfortunately, this is one of the passages in which Mr. Keynes's meaning was still further obscured by his use of the term "Quantity *Theory*" in a context which fails to distinguish sharply between the "Quantity *Theory*" and the "Quantity *Equations*"— or, as they are called in the text, "stream equations of the general Fisherine form." On this aspect of the *General Theory,* which in this respect imitates the practice of the *Treatise,* see Volume I, 33 ff.; and on the ambiguity introduced by this type of usage in the particular passage under discussion, see especially Volume I, 33, n. 59; 36, n. 65; and 37, n. 67, as well as what is said below, p. 757, n. 68. The justification for assuming, in the argument that follows, that Mr. Keynes meant, in this instance, to refer to the Quantity *Equations,* is twofold. In the first place, on the very same page of the *General Theory* (p. 209), the Quantity *Equation* $MV = OP$ is referred to as being "much the same as the Quantity Theory of Money in its traditional form"; and Mr. Keynes appended a footnote in which he promised that his argument with respect to the "wage-unit" and the "price-level" would be "further developed in Chapter XXI" of the *General Theory*— the chapter, that is to say, in which Mr. Keynes presented his "generalized statement of the Quantity Theory of Money" in the form of the series of "elasticities," the relation of which to the received Quantity Equations is here under discussion. In the second place, and in accordance with the general position adopted in this work, what is defended here is only the

in the case of Mr. Keynes's charges against the substance of received doctrine within the Theory of Money and Prices, a special set of difficulties is created by the fact that the very meaning of the charge is itself not clear.[65] As in the other cases considered in this work, therefore, it is necessary to provide a series of possible translations of the charge, and to consider its validity according to each of these translations in turn.

1. *The Definition of Units of "Output."* A first rendering of Mr. Keynes's charge would take the form of an accusation that, in the received Quantity Equations, the units of "Output" are *defined* in such a way that it is impossible to say, of a given change in "prices" which is held to be due to a change in the volume of "output," whether the price change is due literally to a change in the physical volume of output, or to a change in the *figure* of the "volume of output" which is in turn due merely to the fact that the "unit of output" is *defined in terms of the wage-unit,* and that the wage-unit has changed.

If this were a fair criticism of the received Quantity Equations, it would be a serious one. It happens, however, that the most explicit example of the usage to which Mr. Keynes is now interpreted as taking exception is to be found, not in the writings of the most important defenders of the Quantity Equations, but in the writings of Mr. Keynes himself which appeared after the publication of the *Treatise.*[66] Until, therefore, it can be shown that Mr. Keynes was in this respect only making explicit what must be regarded as implicit in the older Quantity Equations, there can be only one conclusion with respect to Mr. Keynes's charge (when so interpreted) that the Quantity Equations do not "distinguish between changes in prices which are a function of changes in output, and those which are a function of changes in the wage-unit." This conclusion is simply that, on this interpretation of his charge, he is fighting with a demon which he himself has conjured into existence, and which has had little if any attraction for those who have preferred to work with the older apparatus which Mr. Keynes, both in the *Treatise* and since, has subjected to so sharp an attack.[67]

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best forms of the "Quantity *Equations,*" and not the heterogeneous mass of propositions, greatly differing in validity, which have been presented under the name of "the Quantity *Theory*" (cf. Volume I, 22 ff., of the present work, and also the references given in the Index [p. 612] to that volume, under "Quantity Equations and Quantity Theory.")

[65] Cf., for example, Volume I of this study, 40 ff., 179 ff., and 268 ff.

[66] Cf. Volume I, 122 f., and especially p. 123, n. 57.

[67] It is perhaps not unreasonable to see evidence of Mr. Keynes's pre-occupation with this demon in his insistence upon expressing movements in *output* in terms of a response to effective demand *as expressed in wage-units,* with the result that it then becomes necessary to "take out" the element, affecting the relation between movements in "demand" and movements in output, that is represented by a change in the "wage-unit" (see above, pp. 744 ff., and the references given in nn. 29 and 37–40 thereto). Each reader must decide for himself whether this procedure is either a necessary

2. *The Assumption of "Full Employment."* If, however, it were not for the example provided by Mr. Keynes himself in his writings subsequent to the *Treatise*, it is doubtful whether the particular rendering of Mr. Keynes's proposition just examined would have suggested itself as the most reasonable one. In any case, another rendering suggests itself on the basis of the specific context in which Mr. Keynes's proposition now appears. The "explanation" which Mr. Keynes himself suggests for the alleged failure of received doctrine to deal with the distinction between "changes in prices which are a function of changes in output and those which are a function of changes in the wage-unit" is "to be found in the assumption . . . that there is always full employment." For in this case, Mr. Keynes suggests, "O being constant . . . , it follows, if we can take V also as constant, that both the wage-unit and the price-level will be directly proportional to the quantity of money." [68] A possible translation of this "explanation" would run as follows:

As an example of the familiar Quantity Equations, let us take, as Mr. Keynes himself did, an expression of the form $MV = OP$.[69] It will be observed that *no special notation is given to either the quantity of employment or the price of the unit of "employment"—that is, the wage-unit.* If, then, we are interested in variations in the wage-unit, we are forced either (1) to regard P as *including* the "price" of a unit of "employment," the quantity of which will be included in the term

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way or the best way of dealing with the effects upon "output" of changes in the "wage-unit." Here I need point out only that even if it is regarded as the best way of dealing with the problem, there is no reason why the procedure in question could not be applied directly to equations of the general "Fisherine" form, by simply expressing the MV of Mr. Keynes's expression $D = MV$ in terms of "wage-units" (that is, we may write $(MV)_w$, just as Mr. Keynes himself writes D_w). For it is then seen that the issue raised by Mr. Keynes has nothing to do with either the validity or usefulness of "stream" equations of the general Fisherine form *as such*.

[68] *General Theory*, 209. The passages omitted have to do with the assumption that "there is no propensity to hoard," so that a value of zero will attach to the M_2 of Mr. Keynes's formulation—that is, the "amount of cash held to satisfy the speculative motive." The introduction of this particular "assumption," however, is obviously the result of Mr. Keynes's failure to distinguish sharply between the Quantity *Equations*, on the one hand, and the Quantity *Theory*, on the other, and therefore need not concern us here. Cf. above, p. 755, n. 64.

[69] For our present purpose, it is of no significance that the V of Mr. Keynes's "equation" is defined in a very special way (see again p. 209 of the *General Theory*, and cf. above, p. 678, n. 2; p. 709, n. 78; and p. 724, n. 117). The argument would be the same if V were defined in a way which, according to Mr. Keynes, would make the expression $MV = OP$ "a truism which holds in all circumstances" (*General Theory*, 209 n. 1), or were defined in such a way as to be free from the implications of any of the widely used variants of the concept of "income-velocity." On Mr. Keynes's own variant of the latter, see above, pp. 707 ff.

for "output" (O); or (2) to regard P as providing a satisfactory index of the variation in the wage-unit—that is, in the "price" of a unit of "employment," the magnitude of the variation in the quantity of this "employment" being likewise regarded as satisfactorily indicated by the variations in the volume of "output." [70]   This, however—so Mr. Keynes would be interpreted as arguing—is permissible only if we assume "that there is always full employment": for only in that case, in which there would be no variation in either employment or output, should we be freed from the necessity of considering the consequences of differential changes in output and employment, and therefore the possibility of differential changes in "prices" and the wage-unit.

If this is Mr. Keynes's meaning, then it must be said at once that he is much too generous to the argument he criticizes when he implies that an adequate "explanation" of its weaknesses can be found in the supposed fact that it would really be a sound argument if we were to assume the specific condition of "full employment." [71]   For the type of "explanation" which he provides for the alleged failure of earlier writers to distinguish between "changes in prices" which are a function of changes in output and those which are a function of changes in the wage-unit" could be regarded as a *justification* only if one were prepared to ignore a series of considerations which (paradoxically enough) Mr. Keynes himself has tended to ignore, both in his *Treatise* and in his later writings.   Specifically:

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[70] It is only fair to point out that, if this is Mr. Keynes's meaning, he is accusing earlier users of "stream" equations of vices which are attributable with much greater justice to other aspects of his own argument in the *General Theory*. See, for example, what is said above, p. 754, n. 59, with respect to the treatment of movements in "wages," when the latter are regarded as special kinds of "prices," in Fisher's *Purchasing Power of Money;* and contrast what is said above, pp. 584 ff., with respect to the frequent assumption, in the *General Theory,* that "prices," being "*governed by*" the "wage-unit," will "change in almost the same proportion" as the "wage-unit," as well as what is said above, p. 533, n. 28, and below, pp. 759 f., with respect to the *General Theory's* treatment of the relation of movements in "output" to movements in "employment."

[71] It should be observed again that here, and in what follows, unless otherwise indicated, I am abstracting entirely from the complications associated with (1) the assumption of the absence of any "propensity to hoard" or of a constant "V" (cf. above, p. 757, n. 68); and with (2) the definition of "full employment" as a "state of affairs" characterized by "the equality of the real wage to the marginal disutility of employment"—that is (according to the argument of the *General Theory*), a state of affairs in which, although there may be "frictional" and "voluntary" unemployment, there is no "involuntary" unemployment (*General Theory,* 15 f.). The reason for abstracting from the second set of complications, as well as from the first, is that these complications have nothing to do with such matters as the assumption that "full employment" means "constant output." On the implications of the alternative definition of "full employment" given on p. 303 of the *General Theory,* on the other hand, see the following note.

(a) It is *not* true, despite Mr. Keynes's clear implication to the contrary in both the *Treatise* and the *General Theory*, that "full employment" necessarily means *constant output*.[72] For one thing, "full employment" will not be the same thing as a *"constant* amount of *employment"* whenever the total supply of labor is increasing.[73] For another, as we have seen, to assume that output will remain constant as long as employment remains constant is to blind ourselves to some of the most elementary propositions of the theory of production, as the latter appears within the "general" Theory of Value.[74] The assumption of "full employment" therefore provides no assurance whatever of that constancy of employment and output which is the very minimum required if we are to conclude that, given a constant "velocity" of money, the changes in "both the wage-unit and the price level will be directly proportional to [changes in] the quantity of money."

(b) Even if it were true that "full employment" is necessarily the same thing as a "constant volume of employment," and that a constant volume of employment assures a constant volume of output, it would still not be true that such changes in the wage-unit and in the "price level" as might result from changes in the quantity of money would *each* be "directly proportional" to the change in this quantity, if by this is meant (as Mr. Keynes, on the interpretation of his argument now under discussion, would be understood to mean) that "the wage-unit" and the "price level" would change in the *same* proportion. On the contrary, this would not be true even if (1) we ignore the fact that entrepreneurial *demand* may be distributed in different proportions as between materials produced by others (a segment of "output"), on the one hand, and in employing *labor,* on the other; and even if (2) we accept also Mr. Keynes's implied condition that there is no possibility

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[72] For references to such an implication in the argument of the *Treatise,* see Volume I of this study, 42, 76 f., 201. So far as the *General Theory* is concerned, see especially p. 303 of that work, where it is stated that "we have full employment when output has risen to a level at which the marginal return from a representative unit of the factors of production has fallen to the minimum figure at which a quantity of the factors sufficient to produce that output is available." It is of course obvious that when the relation between expected returns and costs is such as to make an increase in output impossible, *output* must necessarily be assumed to be "constant." It is equally clear, however, that to *define* full *employment* as a condition in which a further expansion of *output* is impossible (and it is worth noting that the compiler of the Index to the *General Theory* [p. 389] has characterized the statement quoted above as a "definition" of "full employment") is to assume away one of the issues involved in the very argument under discussion.

[73] Cf. Volume I of this study, p. 76, n. 13.

[74] See above, p. 533, and especially n. 28 thereto. It may be observed that this fact was recognized by earlier writers on the theory of production who made no attempt to couch their argument in terms making use of a "modern" definition of "production functions." See, for example, J. S. Mill, *Principles,* Book I, Chap XII, sec. 2 (p. 177 of the Ashley edition).

of further increase in the "supply" of either "output" or "employment." [75] For it should be evident that the effect upon price of two "supplies" which are equally limited with respect to the absolute amount of each available, will be different whenever the *supply curves* involved are different.[76] There is, of course, no reason whatever for supposing that the supply curve of labor offering itself for employment will show the same conformation as the supply curves for the various subdivisions of "output." [77] This, indeed, is so obvious that it is difficult to believe otherwise than that we are here confronted with an example of what may be expected to follow from the type of treatment accorded by the *General Theory* to the problem of the relation between "employment" and "output" generally, in contrast with the type of treatment made possible by an adequate utilization of the devices of the "general" Theory of Value which were designed precisely to deal with this relation and with the problem of "supply" generally.[78]

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[75] The consequences of a differential change in the *demand* for labor, on the one hand, and in the demand for that part of "output," on the other hand, which represents materials of production produced by others than the joint demander of labor and materials, is discussed below, p. 761, under (c).

[76] We are dealing here, of course, with another example of the dangers inherent in identifying the phenomena of *supply* with the phenomena of *production*. See above, pp. 553 ff.

[77] This is virtually implied by Mr. Keynes himself (though, for reasons which are by no means self-evident, he seems to imply that the point is relevant only to situations in which something less than "full" employment prevails), when he attributes the "discontinuity" of the rise in the wage-unit to "the psychology of the workers and . . . the policies of employers and trade unions" (*General Theory*, 301)—that is, to factors which would lead to strictly parallel movements in the supply prices of the various subdivisions of "output" *only* if one were prepared to argue that the prices of "output" (1) would *in all cases* reflect changes in the wage-unit; and would (2) change *only* in response to changes in the wage-unit. On both of these propositions, see above, pp. 583 ff.

[78] It is the more important to emphasize this, in view of the fact that Mr. Keynes has more than once advanced propositions which would be true, if they would be true at all, only upon the basis of arbitrary assumptions that, according to Mr. Keynes, are introduced only to "elucidate the ideas involved." On p. 295 of the *General Theory*, for example, we are told [1] that "an increase in the quantity of money will have no effect whatever on prices, *so long as there is any unemployment*," and that "employment will increase in *exact proportion* to any increase in effective demand brought about by the increase in the quantity of money"; "whilst [2] as soon as full employment is reached, it will thenceforward be the wage-unit and prices which will increase *in exact proportion* to the increase in effective demand." The reader must be left to decide whether the degree of "elucidation" which is thus brought to "the ideas involved," when we "allow ourselves" to "simplify our assumptions" in the degree required if these propositions are to be made even *prima facie* plausible, is worth the danger of overlooking even Mr. Keynes's statement of the "assumptions" required,

(c) The conclusion that, under conditions of constant employment and output, the wage-unit could be expected to vary in the same proportion as "the price level" becomes still less true, obviously, when account is taken of the fact that a given aggregate money "demand" need not necessarily be distributed in equal proportions as between the purchase of "output," on the one hand, and of labor, on the other. It is self-evident that two ordinary "commodities" evidencing precisely the same supply curves will not experience the same rise in price if the shift in the demand curve upwards for one commodity is greater than the shift upwards for the other.[79] And we have no reason whatever for drawing a different conclusion in the case of the "commodities" making up "output," on the one hand, and the "commodity" represented by employed labor, on the other.[80]

3. *The "Determination" of Prices by the "Wage-Unit."* Thus far our argument has proceeded on the assumption that Keynes's formulation summarizing the forces determining "the relation between changes in the quantity of money and changes in the price-level" in terms of a series of "elasticities" is to be regarded as in all essentials a rewriting of a "stream" equation of the familiar type, and should therefore be regarded as carrying all the analytical implications attaching to this

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and his list of the "possible complications which will in fact influence events" (*General Theory*, 295 f.). The only comment needed here is that it is a strange reading of "tradition" which would suggest that, in dealing with the relations between employment, output, the wage-unit, and commodity prices, this "tradition" would be "satisfied" by "introducing a sufficient number of simplifying assumptions to enable us to enunciate a *Quantity Theory of Money*" (cf. what is said on this matter in Volume I, 37 f., of the present work). From the argument in the text, it should be clear that the "tradition" which must be "satisfied" is a "tradition" which, while making full use of Quantity *Equations* of the "stream" type, would make no "simplifying assumptions" whatever, but would undertake to deal with the problem in terms that would do full justice to (1) the distinction between "full" employment and *constant* employment; (2) the distinction between constant *employment* and constant *output;* (3) the distinction between changes in the amounts of output and labor (employed and unemployed) *in the community,* on the one hand, and the *supply curves* of the different *types* of labor and "output," respectively; (4) the factors which make the relation between the "quantity of money" and the quantity of "effective demand" what it is; and (5) the *distribution* of "effective demand" between the purchase of labor and the purchase of materials, on the one hand, and between different *types* of labor and materials, on the other.

[79] The respective *conformations* of the demand curves would obviously be relevant also. This much has been implicitly granted by Mr. Keynes himself. See above, p. 317, and the references to the *General Theory* given in n. 207 thereto.

[80] Again it is to be observed that the proportion in which entrepreneurial demand will be distributed as between labor and other "commodities," respectively, will depend chiefly upon the production functions chosen by the entrepreneurs in a given "conjuncture." See above, pp. 533, 600.

type of equation.[81] To say that the former *should* be so regarded,
however, is not to say that it is likely to be so regarded in all cases;
and in fact we are confronted here with an aspect of Mr. Keynes's
treatment which may be regarded as throwing further light upon the
wisdom of translating the substance of the familiar Quantity Equations
into an "elasticities" formulation of the Keynesian type, whenever
such translation is not accompanied by evidence of an explicit aware-
ness of the fact that, in the last analysis, all the important implications
of the familiar "stream" equations continue to apply to these formula-
tions in terms of a series of "elasticities."

The aspect in question will be brought out most clearly if attention
is again called to the fact that the argument of the *General Theory* is
at several points such as to suggest that, when Mr. Keynes speaks of
"changes in prices" as a *"function* of changes in the wage-unit," he is
thinking of the "changes in prices" as being *directly* "governed" by
changes in the wage-unit.[82] Interpreted in the light of such a concep-
tion of the determination of realized prices, the charge that earlier
writers on the Theory of Money and Prices did not "distinguish be-
tween changes which are a function of changes in output, and those
which are a function of changes in the wage-unit" reduces to a charge
of a nature quite different from that of the charges discussed thus far.
Specifically, it reduces to the charge that earlier writers did not dis-
tinguish (in the words of Mr. Keynes himself) between those changes
in *price-determining "costs"* which are due, on the one hand, to "the
shapes of the physical supply functions" (which will be affected by
the "scale of output" under given degrees of "efficiency in the productive
system"), and those which are due, on the other hand, to the mere
fact that there has been a change in the level of *money rewards per unit*
of a given amount of factors involved in the production of a given
amount of output.[83] With respect to this contention, only two com-
ments are necessary:

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[81] The conception of price-determination underlying a "stream" equation,
it will be agreed, is that price is thought of (in Mr. Robertson's words) as
resulting from "the mutual impact of the relevant flow of money and the
relevant flow of goods." It is therefore worth calling attention to those
passages in the *General Theory* which would certainly suggest the conception
of such a "mutual impact," quite apart from the fact that Mr. Keynes has
implicitly made use of the "stream" concept in writing $D = MV$. See, for
example, (1) Mr. Keynes's definition of "the elasticity of output or produc-
tion" as measuring "the rate at which output . . . increases when more effec-
tive demand . . . is *directed towards* it" (*General Theory*, 282 f.); (2) his
statement (*ibid.*, 284) that "the extent to which prices . . . will rise . . .
when *money expenditure* is increased, depends on . . . the elasticity of
output in response to *expenditure*"; and (3) his proposition (*ibid.*, 285) that
"effective demand spends itself, partly in affecting output and partly in
affecting price."

[82] See above, pp. 583 ff.

[83] Cf. the *General Theory*, 173, 294, 300, 302, 309, 328. It should be ob-

(*a*) There is nothing in Mr. Keynes's formula for the "elasticity of prices in response to changes in the quantity of money" ("*e* without suffix") *in itself* which says anything whatever with respect to the "determination" of "prices" by "costs," whether changes in the level of these "costs" are due to changes in the wage-unit or to any other reason whatever. All that the formula tells us is that "output," "employment," and the "wage-unit" may change in different degrees in response to changes in the quantity of money and the amount of "effective demand" generated on the basis of such a change in the quantity of money.[84] To be sure, by segregating one item—namely, the level of the *wage-unit* in terms of money—which can be shown to be relevant *to the level of "cost" itself*, Mr. Keynes's formula does make it possible to observe the movements in this particular *cost*-affecting factor. But *this is equally true of any "stream" equation of the general Fisherine form which gives special notation to the "wage-unit" as one "price" among others*. In both cases, the question of the way in which the change in this *cost*-affecting factor will affect the *realized selling prices* of the commodity whose production involves these costs, is *left for separate analysis*.[85]

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served, in addition, that some of Mr. Keynes's disciples, in their attempt to provide beginning students with "some help in assimilating Mr. Keynes' *General Theory*," have presented the interpretation under discussion as if it were the *only* interpretation possible. In Joan Robinson's *Introduction to the Theory of Employment* (56 ff.), for example, the three "groups of causes" of "changes in the general level of prices" enumerated are (1) the changes in *costs per unit of output* which are associated with changes in the level of employment and output; (2) changes in money-wages; and (3) changes in the "efficiency" of production—the element of "demand" as a cause of changes in prices being brought in only incidentally, and then often in a context (for example, p. 56) which would suggest that "demand" affects "prices" by affecting the level of output and therefore "cost per unit of output," which *in turn* affects "prices."

[84] Cf. the *General Theory* itself (p. 305 f.): "e_w [stands for] the . . . factors . . . which determine the extent to which money-wages are raised as employment increases, and e_e and e_o for the physical factors which determine the rate of decreasing returns as more employment is applied to the existing equipment."

[85] The *nature* of the "separate analysis" of this problem which would be strictly consistent with the *analytical implications* of the "stream" conception of the determination of realized money prices should be abundantly clear from the earlier argument of this volume. Specifically, (1) such analysis would distinguish sharply between the *realized* wage-costs that appear in the "stream" equations, on the one hand, and, on the other, the *ex ante* "wage-costs" that may be expected to *affect entrepreneurial supply prices*, which are the supply prices involved in the market supply curve $q = \Phi(p)$ of our expression $D = pq = p\Phi(p)$. Moreover, (2) such analysis would distinguish these changes in prices due to entrepreneurial adjustments on the side of *supply* (q) as a result of changes in wage rates, actual or expected,

(b) It follows that the validity of Mr. Keynes's formulation for the "elasticity of prices in response to changes in the quantity of money" (e without suffix) is completely independent of Mr. Keynes's contentions with respect to the "governing" of costs by "wage-" costs, or any other kind of cost. By those who have accepted the argument presented above in Chapter Eleven, this will be regarded as a saving grace of Mr. Keynes's algebraic formulation, and not as a shortcoming. It must be remembered, however, that one of the virtues of equations of the general Fisherine form, when they are regarded as "stream" formulations representing the "mutual impact of relevant flows" of money and of objects sold for money, respectively, is that they have provided a continuing reminder of the validity of a conception of price-determination which is at once (1) the very antithesis of the conception of the "governing" of prices by "costs" (including wage costs); and (2) completely consistent with the findings of "modern" value theory with respect to the rôle of *Demand* in the determination of prices actually realized in the market.[86]   Insofar, therefore, as the effect of Mr. Keynes's translation of the substance of these "stream" equations into a series of "elasticities" has been to obscure the fact that

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from those changes in prices due to changes in the level of *money expenditure* ($MV = \Sigma D$) which can be shown to result from changes in wage rates, actual or expected. Specifically, for example, the analysis would be concerned with (a) the effect of changes in the wage rate, actual or expected, upon the profitability of "investment," which can affect realized prices on the "demand" side, if it affects them at all, only by affecting the dimensions of the stream of *monetary expenditure;* and this, in turn, will be affected by the amount of entrepreneurial borrowing from commercial banks (and therefore the quantity of bank money) or by the rate of spending by entrepreneurs, and therefore the velocity of circulation of money. (On the relation, to the familiar Quantity Equations, of the problem of tracing the steps involved in the process of "Investment," see Volume I, 280 ff., of this study. It is significant, on the other hand, that Mr. Keynes has on occasion allowed his conception of the determination of price by "costs" to lead him to minimize the effects of "costs," actual and expected, upon the level of *money expenditure,* and therefore the importance of tracing the steps involved in expansions or contractions of such money expenditure. See, for example, the *General Theory,* 328, where it is specifically argued that "the rise in prices is not essentially due to the increase in investment;— it is due to the fact that in the short period *supply price* usually increases with increasing output, on account either of the physical fact of diminishing return or of the tendency of the cost-unit to rise in terms of money when output increases" [italics mine].) And the analysis would be concerned, in particular, (b) with an adequate treatment of the relation of changes in wage *rates* to changes in wage *incomes,* with all that such a treatment would involve with respect to (i) the difference between the *asking* price (*ex ante* supply price) of labor and the *realized* price of labor; and (ii) the rôle played by both in an adequate apparatus for tracing the steps involved in the generation and utilization of money income which would run from first to last in terms of the flow of money payments.

[86] See above, pp. 570 ff.

what is involved *is* a "stream" equation, with all the analytical connotations that have been attached to such equations by their ablest sponsors, it must be said that the translation itself has not only carried us no further in our progress toward the ultimate goal of the Theory of Prices, but has actually left us far behind the findings, for an adequate Theory of Prices, that have long been available within the best versions of *both* monetary theory, in the narrower sense of the term, and the "general" Theory of Value.

4. There is no novelty even in the suggestion that we may use a series of "elasticities," of the general form $\frac{x}{y} \cdot \frac{dy}{dx}$, associating movements in one of the included variables with movements in another of the included variables, in order to call attention to the fact that relations of *dependence* (or "connexity," as Mr. Keynes put it in his *Treatise*) may exist between these variables.[87] And, despite statements to the contrary by both Mr. Keynes and his followers, still less novelty can be credited to the mere reminder that such relations of dependence (or "connexity") do exist—that is, that the variables included in the familiar Quantity Equations

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[87] In the *Treatise*, Mr. Keynes applied the term "connexity" to the problem of the relations of "independence" or "dependence" between *prices* (cf. the reference to the *Treatise* given above, p. 583, n. 69); but the term is obviously applicable also to the problem of the relations of "independence" or "dependence" between the variables of the Quantity Equations. On the use of "elasticities," prior to the appearance of the *General Theory*, to express relations of "connexity" between the individual variables of the Quantity Equations, see the references to Marschak and Petersen given above, p. 682, n. 7. It is worth observing, moreover, that both of these writers' applications of the "elasticity" formula (or its equivalent) to the variables of the Quantity Equations was more thoroughgoing than was Mr. Keynes's application in his *General Theory*, in that both of them included "elasticities" expressing the "responses" of the variables to changes in variables *other* than the *quantity of money* or "velocity" (changes in the latter two variables being, by virtue of Mr. Keynes's expressions $e_d = \dfrac{MdD}{DdM}$ and $D = MV$, equivalent to changes in "effective demand"). An advantage of this procedure, of course (assuming that "elasticities" are to be used altogether), is that it enables us to deal also with those cases of price change in which there is *no* change in either the quantity of money or in "effective demand." It enables us, for example, to deal with the case in which changes in prices may be due solely to changes in components of the Fisherine *T*, while both the quantity of money and effective demand remain unchanged, with the result that both the $e_d = \dfrac{DdM}{MdD}$ and the $e_p = \dfrac{Ddp}{pdD}$ of Mr. Keynes's expression $e = e_d e_p$ would be equal to zero.

are by no means necessarily to be regarded as independent variables.[88]

In short, Mr. Keynes, in presenting his formula for dealing with the forces determining the degree of "elasticity of prices in response to changes in the quantity of money," has merely regained the highroad toward further progress in the Theory of Money and Prices which is represented by the explicit use of "Quantity Equations" as a framework for the study of the problem, and which he had wilfully abandoned in his *Treatise*.

III

THE WAY STATIONS COMPARED

The result just described may be taken as providing its own commentary upon the suggestion, by champions of the *General Theory,* that the latter work has brought about "an unprecedented rate of obsolescence" in economic theory in general, and in monetary theory in particular.[89]   But to

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[88] See Volume I, 24 ff., of the present work, including the references to Fisher on p. 24, n. 43, and to Pigou on p. 26, n. 48. Contrast the statement of Keynes quoted on pp. 25 f. of Volume I; also the statements from Whale and Kaldor quoted below, p. 769, n. 94, as well as the statements of Mr. Lerner ("Some Swedish Stepping Stones in Economic Theory," *loc. cit.,* 581 f.) that "the use of the quantity equation (whether $MV = PT$, or $M = PKR$, or any other variant) is harmful as an exercise for elementary students" because "they [elementary students] inevitably assume that V (or K) and T (or R) can be taken as constant and then they have to try to unlearn that"; and that "it is only natural that they should do so, however much they are warned, for if V and T cannot be taken as independent, there is no sense in separating them out in the equations" (cf. also the statement from Lerner quoted above, p. 284, n. 132).

[89] See, for example, Lerner, "Some Swedish Stepping Stones in Economic Theory," *loc. cit.,* 586; also the comparable statement from the same article cited above, p. 613, n. 126. The full irony of Mr. Lerner's observations when they are judged in the light of the argument presented in Sec. II of this chapter will be appreciated (1) if particular note is taken of his remarks on the "use of the quantity equation" cited in the preceding note; and (2) if one applies his generalizations with respect to the "rate of obsolescence" *not* to "economic theory" or "monetary theory" in general, but only to the successive publications of *Mr. Keynes.* For it was *Mr. Keynes* who, in his *Treatise,* announced the "obsolescence" of the version of the cash-balance approach presented in his earlier *Monetary Reform* (cf. Volume I, 414 f., of the present work), only to reintroduce the cash-balance approach in his *General Theory* under the guise of the concept of "liquidity preference" (see above, p. 576, n. 57). And it was Mr. Keynes who, in his

stop here would be to betray the spirit in which earlier generations of workers have struggled forward on the high-road which, if the argument of the present work is sound, is represented by the framework that is now common to both Mr. Keynes and the "traditionalists" on whom he has declared war.

For the mere fact that the highroad stretches far behind us is no proof that it does not stretch far ahead of us as well.[90] Nothing, therefore, could be more disastrous for the future of our subject than a complacency whch would rest on no firmer basis than the fact that even some of the most eminent of those who have strayed from the highroad have in the end managed to find their way back to it.[91] For what matters, in the end, is not that we should be *on* the high-road, but that we should be *further along* this highroad than were our predecessors. It is a question, in other words, of asking ourselves *which* of the several *way stations* on the road to our ultimate goal is the *furthest advanced* toward that goal, and which represent way stations long since left behind by the more energetic and far sighted of the earlier travelers. And it is the contention of this work that a considerable part of the Theory of Prices presented as such in the *General Theory,* instead of having *introduced* "an unprecedented rate of obsolescence" among the way stations

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Treatise, announced the "obsolescence" of the Quantity Equations as a framework for monetary theory, on the ground that the Fundamental Equations of the *Treatise* provided a superior framework, only to announce, in his *General Theory,* the "obsolescence" of the Fundamental Equations and his own adherence to a framework which amounts in substance to the type of framework represented by the Quantity Equations.

[90] The reader is invited to consider again some of the "vistas" contemplated in Chapters Eight and Nine of the present volume (see, for example, pp. 470 f., 509 ff.). But no one familiar with the development either of the natural sciences or of economics could cherish any illusion that the end of the highroad can be envisaged from where we now stand. For it is a commonplace of the history of intellectual achievement that the attainment of each new vantage point has disclosed vistas undreamed of by earlier investigators. Cf. the comments of Marshall, in his "Note on Method in Economic Study" (*Industry and Trade,* 673 f.), on the effect of progress toward "certainty," in both "the physical sciences" and economics, in opening "new ground in which uncertainties abound and certainties are rare."

[91] Cf. Volume I, 30 ff., of the present work.

erected by earlier workers in the field, is itself to be re-
garded as characteristic of way stations that have themselves
been made obsolete by progress already registered or already
capable of being envisaged in reasonably sharp outline.[92]

This is the *assertion* of a claim, not its demonstration.
The demonstration itself is one of the tasks that this work
has undertaken to perform by undertaking (1) a close ex-
amination of the successive way stations established by the
history of our subject; (2) a similarly close examination of
the architectural details of Mr. Keynes's own way stations;
and (3) a comparison of the latter not only with stations
that are seen, from a later perspective, to have lain along
the main highway, but also with those for which little can
be said beyond the fact that they resulted from excursions
which demonstrated that, wherever the road to further prog-
ress lay, it was not here.[93]

It will be the workers of another generation, possessed of
a later and broader perspective than our own, who will de-
cide where victory lay in the "Keynesian controversy"—
one of the greatest, if not the greatest, of the internecine
controversies that have ever split the ranks of economic
theorists.   It will be for such readers to decide, for example,
how far the Theory of Prices presented in Keynes's *General
Theory,* even when it undertook to re-establish the gain
represented by acceptance of the equivalent of the familiar
Quantity Equations, may be said to have cancelled this gain
by its continued confusion of the Quantity *Equations* with
something called "the Quantity *Theory,*" and the reintro-
duction of all the outworn controversies associated with the

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[92] On the extent to which the *General Theory* may be regarded as having
rendered "obsolete" certain positions advanced by *Mr. Keynes himself* in
his earlier writings, see above, p. 766, n. 89.

[93] No reader of this work can doubt that those who have strayed from
"the main highway" must also be regarded as having contributed to progress
on the highway itself, by having made it possible for "later investigators,
warned by the mistakes of their predecessors," to avoid the by-paths into
which "pioneers wandering in territories as yet unexplored are almost cer-
tain to be drawn"—*providing* these later investigators "are prepared to
study with care and to learn from the experiences of these predecessors."
Cf. Volume I, 204, of the present work, and also p. 297 of the same vol-
ume: "It is no sin to err, *if* the error is committed in fields largely un-
charted."   It is in this *"providing"* and this *"if"* that my quarrel lies with
the temper and the method of most of current heterodoxy.

latter.[94]   It will be for such workers to decide how far the
issues were confused by the *General Theory's* failure to es-
tablish an *articulate relation* not only between the disguised
version of the Quantity Equations presented in the *General
Theory* and the versions of the Quantity Equations explicitly
presented as such by earlier writers, but also, and much more
importantly, between these Quantity Equations and those
various "approaches" to the central problems of monetary
theory which, presented by earlier writers under the heading
of the "cash-balance approach" and the "income approach,"
have reappeared in the *General Theory* under the disguises
of "liquidity preference," on the one hand, and "effective
demand" and the "elasticity of effective demand," on the
other.[95]   It will be for such workers to compare the *details*
of these alternative formulations, from the standpoint of

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[94] Cf. what is said on this aspect of the *General Theory* in Volume I,
33 ff., of the present work. The reader must provide his own comment on
the position of those defenders of the *General Theory* who would justify
the confusion in question on grounds no more cogent than that "most
readers" of current works on monetary theory "will continue to associate
the quantity equations with the quantity theory, and thus be guilty of the
same confusion as Mr. Keynes" (cf. N. Kaldor in the *Economic Journal*
for September, 1939, p. 496). And the reader must provide his own com-
ment also on the position of those who would justify the perpetuation of
the "confusion" which is implied in the proposition that "the utility of
equations of the Fisher or Cambridge type appears . . . to be limited to
circumstances in which the factors other than P can be regarded as mutually
independent variables, each influenced in its turn by separate sets of con-
ditions" (so B. P. Whale in *Economica* for February, 1940, p. 90; cf. also
Kaldor, *op. cit.*, 497). On the relation of this type of proposition to the
confusion of the Quantity *Equations* with the Quantity *Theory,* see again
Volume I, 24 f., of the present work, and especially the reference to Fisher
given on p. 24, n. 43.

[95] See above, pp. 576, n. 57, and the references there given; and Chapter
Thirteen. The same comment applies to the failure of the *General Theory*
to provide an articulate account of the relation, to the variables of the
Quantity Equations (or the variables included in Mr. Keynes's formula for
the "elasticity of prices in response to changes in the quantity of money"),
of its argument with respect to the "marginal efficiency of capital" and
"investment," just as the *Treatise* had failed to relate, to the variables of
the Quantity Equations, its argument with respect to the "natural rate of
interest" and "investment" (cf. Volume I, Chaps. Seven to Ten). The com-
ment applies, likewise, to Mr. Keynes's statement, in his *General Theory,*
of the argument with respect to "the way in which changes in the quantity
of money work their way into the economic system" (*General Theory,* 173;
cf. Volume I, 171 ff., on the comparable aspects of the argument of the
Treatise). See also the following note.

articulation and precision, with the details of the apparatus proposed by earlier users of the "cash-balance approach" and earlier writers on the problem of the generation and utilization of money income.[96]

Above all, however, it will be for such workers to judge the net benefit from the impact of the *General Theory* upon the present state of the Theory of Money and Prices, when this impact is judged from the standpoint of (1) the accuracy of its statements with respect to what the "traditional" position has in fact been with respect to the relation between the Theory of Money and Prices, on the one hand, and the *"general" Theory of Value,* on the other; and (2) the comprehensiveness and the power of its own proposed "synthesis" of the two bodies of doctrine, as compared with the various "syntheses" that have either been already proposed, or can be constructed upon the basis of the disparate results obtained by individual workers over a very wide and unequally tilled area. It is these later workers who will decide, for example, what degree of good has actually come from the type of "synthesis" represented by Mr. Keynes's use of the *term* "elasticity of effective demand" to designate what amounts only to a variant of the concept of "income-velocity," and his application of the concepts of "elasticity of substitution" and of "elasticity of supply" to the demand for and supply of *money.*[97] It is they who will evaluate Mr. Keynes's treatment of the *demand* side of the problem

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[96] On a later occasion, I hope to be able to show that this comment applies with particular force to that theory of the *generation and utilization of money income* which must be regarded as implicit in all versions of the "multiplier" concept, when the latter is not *supplemented* by an apparatus for tracing the successive steps in the process as they unfold themselves in clock time as the result of the market action of individuals who carry on their *ex ante* calculations in a given institutional and conjunctural setting. In the meantime, see what is said on this matter above, pp. 476 f. Since an essential part of this supplementary apparatus is the use of a set of clock-time "period" subscripts, the reader himself must be allowed to compare the degree of precision attainable by the use of such time-period subscripts with the implications with respect to "different time functions" which have been alleged to be inherent in the use of "elasticities" of the type employed in Mr. Keynes's formula for the "elasticity of prices in response to changes in the quantity of money" (cf., for example, Petersen, *Macro-Dynamic Aspects of the Equation of Exchange,* 95).

[97] See above, pp. 628 ff., 663 ff., 681 ff.

of the determination of *individual money prices,* as compared with a type of treatment which undertakes to coordinate the best available on the subject of "demand" in analysis of both (1) the "particular equilibrium" and the "general interdependence" type within the *"general" Theory of Value;* and (2) the best available in those versions of the *Theory of Money and Prices* which, working with a "plurality" of "stream" equations (all of whose variables are dated in terms of clock time), representing the impact of realized money "demands," would undertake to provide an apparatus for dealing with a Moving System of Economic Quantities in which full justice is done to both the microeconomic and the macroeconomic aspects of those problems of *structure* and *process* which are indissolubly connected in analysis and reality.[98]   It is they who will evaluate Mr. Keynes's treatment of the *supply* side of the problem of the determination of individual money prices, with its treatment of realized prices as "governed" by "costs," and its avowed "sympathy with the labor theory of value," as contrasted with a treatment which is fully consistent not only with the advances made within the "general" Theory of Value in protest against the crudest versions of the "labor theory of value," but also with the advances made within the relevant sectors of "monetary" theory in the narrower sense of the latter term.[99]   And it is they who, by undertaking, with the help of all the empirical data that can be assembled, to *apply* the rival sets of apparatus to the explanation of events realized in the world we know, will decide which apparatus really provides the better combination of (1) an adequate *mechanics* of the economic process; (2) an adequate analysis of the *motives* which lead to individual market action; and (3) an adequate treatment of the effects, upon the functioning of the economic process, of economic *institutions,* from commercial banks to business monopolies and the "institution" of government.[100]   For *this* is what we must have if we are to hope for better results from economic *policy.*[101]

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[98] See above, Chaps. Four to Nine.
[99] See above, Chaps. Ten and Eleven.
[100] See above, pp. 462 ff., 471 ff., 505 ff.
[101] See above, pp. 512 ff.

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