REISMAN ON *CAPITALISM*

GEORGE REISMAN

The *Review of Austrian Economics* (*RAE*) recently published a review of my book *Capitalism: A Treatise on Economics* that, while praising my book to some extent, seriously misrepresents or altogether ignores major portions of it.[1]

Since a full analysis of the review would require twice as much space as the review itself, I will confine myself here to just a few instances of comparing the review's statements concerning my book with my book's actual content.

THE ALLEGED MISSING THEME OF SUPPORT FOR BUSINESSMEN

The first paragraph of the review reads as follows:

> George Reisman was a student of both Ludwig von Mises and Ayn Rand. Yet, Reisman's *Capitalism* discusses few typically Austrian or Objectivist themes. Amazingly, in a more than 1,000-page book, ostensibly influenced by both *Human Action* and *Atlas Shrugged*, there is no index entry for entrepreneurship. Nor will one find entries for the market process, subjective value, or evolution. Austrian business-cycle theory and welfare economics are unexpectedly under-represented. This list of lacunae is brought forward not as criticism, but to emphasize that, particularly in its most original aspects, *Capitalism* stands somewhat apart from any school or living tradition. Mises and Rand have influenced *Capitalism*, but surprisingly, Reisman's greatest influences are Adam Smith, David Ricardo, and John Stuart Mill.

It is difficult to interpret the third sentence of this paragraph other than to mean that my book ignores the strongly pro-business aspects of my ostensible intellectual heritage. What other "typically Austrian or Objectivist theme" can be meant if not the intellectual support of businessmen and capitalists that characterizes *Human Action* and *Atlas Shrugged?* Indeed, it really would be amazing, if that theme were absent from my book. But it is certainly not absent. True, the first printing of

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[1] Alexander Tabarrok, "Review of George Reisman's *Capitalism: A Treatise on Economics,*" *Review of Austrian Economics* 10, no. 2 (1997): 115–32.

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my book has no index entry for entrepreneurship. Despite my having worked several months on the preparation of the index, the index turned out to be less than perfect, and this entry was omitted. In future printings, this error will be remedied. An entry will appear that reads:"Entrepreneurs, See Businessmen and Capitalists." This will refer the reader to the more than nine column inches of index entries that appear under this latter heading.

What is truly amazing here is that the reviewer has claimed that the theme of intellectual support for businessmen and capitalists is not present in my book. To claim this, far more is involved than his failure to find an index entry for entrepreneurship. What is present is an apparent failure on his part to find a major portion of *the intellectual content* of my book. Frankly, I find it difficult to reconcile such lack of awareness with Mr. Tabarrok's actually having read these portions of my book. For if he had read them, it would have been impossible for him to claim that the theme of intellectual support for businessmen and capitalists was missing from my book.

Closely connected with this, I must immediately respond to the last two sentences of the review's first paragraph as well. Here the reviewer refers to the fact that "in its most original aspects, *Capitalism* stands somewhat apart from any school or living tradition." This much is true. But apart from citing my book's alleged omissions, the reviewer does not trouble to explain in what actual respects my book is distinctive, something he would seem obliged to do, especially since he explicitly refers to this fact. It is also true that the great classical economists he names have had a major influence on my thought, and precisely in the areas in which my book is most original. However, because the reviewer does not supply any specifics except these alleged omissions, what he implies is that the uniqueness of my book is defined by its unintelligible peculiarity, a peculiarity reinforced by its being subject to the influence of the classical economists, whom many contemporary economists, particularly the younger "Austrians," regard as the precursors of Karl Marx and thus as belonging to some kind of intellectual Dark Age and utterly unworthy of intellectual respect.

In order to make good the reviewer's deficiency here, allow me to refer to a major respect in which my book really does stand apart from any school or living tradition and does so precisely in its recognition of the productive role of businessmen and capitalists, and, moreover, precisely in a respect in which it is most heavily influenced by the ideas of the classical economists. This is the radical opposition between a major insight of my book and an erroneous basic assumption that is shared by, among others, Adam Smith, Karl Marx, and leading representatives of the Austrian School, notably, Eugen von Böhm-Bawerk and Murray Rothbard.

This latter idea, which is held by virtually all economists other than myself, I call *the primacy of wages doctrine*. It is the belief that wages are the original, primary form of income and that all other incomes, such as profit and interest, come into existence as a deduction from what is originally all wages. Profits appear, allegedly, only after the accumulation of capital and with the emergence of capitalists, as a deduction from what otherwise would be wages. According to

Karl Marx, the deduction is unjust and constitutes the exploitation of labor. Adam Smith thinks virtually the same thing. Böhm-Bawerk and Rothbard think the deduction is justified, on the basis of time preference, and thus does not constitute any kind of exploitation of labor.

In my view, however, profits are *not* a deduction from what is originally all wages. On the contrary, profits are the original and primary form of income, and profit earners, not wage earners, are and continue to be the primary producers. The productive status of the wage earners is best characterized by the word "help." They are the profit earner's helpers in the production of what fundamentally are *his* products, not theirs. The actual consequence of the emergence of capitalists and the accumulation of capital, I hold, is the creation of productive expenditure, wages, and costs, and the consequent reduction in the proportion of income which is profit. The interested reader can find my discussion of these and related points on pages 473–85.

This radically distinctive, pro-capitalist view of the status of profits and of the relationship between capitalists and wage earners, based precisely on ideas of the classical economists, above all, on John Stuart Mill's proposition that "demand for commodities is not demand for labor," appears to have entirely escaped the reviewer. And because it has escaped him, he confronts the influence of the classical economists in my book with nothing but knee-jerk rejection.[2]

Misrepresentation of My Use of Classical Economics' Concept of Demand and Supply

The reviewer's out-of-hand rejection of classical economics makes him blind to the extremely valuable concepts of demand and supply employed by the classical economists, and which my book adopted and used to achieve extremely rewarding results. He writes:

> A related annoyance is Reisman's use of the so-called classical theory of price determination. According to Reisman, the classicals conceived of demand "as an amount of expenditure of money, such as $1 billion, while supply is to be understood as an amount of a good or service offered for sale" (p. 152). To illustrate, Reisman presents the following equation:

[2] I must say that considerations of space prevent my answering the other criticisms in the review's opening paragraph, except to note that while I do not have an index heading of "subjective value," I do have very extensive entries under the heading "marginal utility, law of diminishing," including more than one major entry referring to pages that the reviewer appears not to have read, especially the pages containing a very lengthy quotation from Böhm-Bawerk that both acknowledges determination of price by cost of production and demonstrates how such determination represents the operation of the law of diminishing marginal utility.(The quotation spans pages 414–16 of *Capitalism*.) I also wish to note that my theory of the trade cycle is fundamentally the same as that of Mises, in that I attribute it to government-fostered credit expansion, not to the operations of the capitalist economic system. My treatment does not significantly follow along the lines of Hayek's analysis, however.

> $P = D/S$
>
> Where D is understood as an expenditure of money and S a quantity of goods.
>
> Whether classical or not, this "theory" of price determination is untenable. How can consumers decide how much to spend on a good without first knowing the price? Are we to believe that consumers choose to spend, say, $1 billion on Coca-Cola without first knowing the price of Coca-Cola? If, for some unspecified reason, the supply of Coca-Cola falls, will consumers continue to spend just as much on Coca-Cola as previously? (*RAE*, p. 120)

It is almost embarrassing for me to have to point out the lack of familiarity with my book this last paragraph displays. In the very same section of my book from which the reviewer quotes and which serves to familiarize the reader with the differing concepts of demand and supply employed by the classical and contemporary economists, I carefully explain why the classical concept of demand applies *only at the level of the economic system as a whole,* where the volume of spending can be related to the quantity of money in the economic system and can be assumed constant so long as the supply of and demand for money for holding remain the same. In that section and in subsequent sections of the same chapter, as well as elsewhere in my book, I explain and illustrate repeatedly and exhaustively why, in contrast, the expenditure for any individual product cannot be assumed to be constant, but is subject to powerful competitive forces operating between the various products and industries, which continually shift the expenditures for individual products. To confirm these points, the interested reader should see, for example, pp. 155–58, 160, 179, 561–68, and 899–901 of my book. In these pages, by means of the assumption of a constant aggregate expenditure, I relate changes in expenditure for individual products and industries to equivalent, offsetting changes in expenditure in the rest of the economic system.

The reviewer concludes the paragraph I just quoted from with the rather bold assertion that "Reisman's so-called classical theory [of demand and supply] explains nothing" (p. 121). He then goes on to declare, in his very next paragraph, that:

> Reisman accepts the classical doctrine of the wage fund which is simply the above equation [P= D/S] applied to the price of labor. (Take P as the price of labor, D as the demand for labor considered as a fixed monetary expenditure like $1 billion, and S as the fixed quantity of labor supplied.) This theory makes no more sense applied to wages than to any other price. Fortunately, Reisman doesn't take the theory too seriously and manages to argue to the correct conclusion that the average wage is determined by the average productivity of labor.

Here it is necessary for me to point out that I not only take the classical theory of demand and supply with total and absolute seriousness, but also consider it to be one of the most fruitful doctrines in the history of economic thought. A demonstration of its fruitfulness is provided precisely by the fact that I do not somehow, inexplicably, just "manage," as the reviewer puts it, to reach the

conclusion that average real wages are determined by the average productivity of labor. The actual way I reach this conclusion is by dividing the classical economists' demand and supply formula for wages by their demand and supply formula for prices. When this is done, the result is that the average money wage rate divided by the general consumer price level, i.e., the average real wage rate, equals the aggregate demand for labor over the aggregate demand for consumers' goods, times the aggregate supply of consumers' goods over the aggregate supply of labor. This last expression reflects the productivity of labor. Other things being equal, a rising productivity of labor, I show, raises real wages in direct proportion by reducing prices in inverse proportion, while the average money wage rate remains the same. This is a rather significant result for a theory that allegedly "explains nothing" (see pp. 618–22).

The fruitfulness of the classical economists' formulas for prices and wages is further evidenced by the fact that these formulas make it possible to see the effects, if any, of a rising productivity of labor (and also of a changing supply of labor) on aggregate profits as well as on prices and real wages. This is because aggregate profits can be represented, at least as a first approximation, as the difference between the classical economists' concept of the aggregate demand for consumers' goods and their concept of the aggregate demand for labor. Profits equal sales minus costs. The demand for consumers' goods in the classical sense is consumer sales revenues. The demand for labor in the classical sense can be taken as total wage costs. Since the demand for capital goods has been omitted from both sales and costs (later in my analysis it is brought in), the difference between the demand for consumers' goods and the demand for labor can be taken, as I say, as a first approximation to aggregate profits. Among the results of this framework of analysis is the realization that falling prices caused by increased production do not reduce the aggregate amount or average rate of profit in the economic system and thus that they do not constitute deflation. Deflation, it follows, is to be understood strictly as a phenomenon of monetary contraction. This framework of analysis, incidentally, is what underlies virtually every aspect of my exposition of Say's Law and makes the reviewer's positive appraisal of that portion of my book somewhat difficult to explain. (For my treatment of these points, see pp. 569–74.)

MISSTATEMENT OF MY DEFINITION OF ECONOMICS

I must mention the serious matter of the reviewer's misstatement of my definition of economics and his ignoring of my explicitly stated views on the applications of the subject. On the very first page of Chapter 1 of my book, I write: "I define economics as *the science that studies the production of wealth under a system of division of labor*" (p. 15. Italics in original). I then go on, at considerable length, to explain why the division of labor is "the essential fact that necessitates the existence of the subject of economics." I also point out that:

> Apart from the very survival of a division-of-labor society, and all that depends on it, the most important application of economics is to provide the knowledge necessary

> for the adoption of government policies conducive to the smooth and efficient functioning of such a society. [In the note that occurs at this point, I write: "Because of its primary application to government policy, it is understandable why the subject was originally known as political economy, which was its name from the time of Adam Smith to the last quarter of the nineteenth century, when the change to "economics" took place."] On the basis of the knowledge it provides, economics offers logically demonstrable solutions for politico-economic problems. For example, it explains very clearly how to stop such major present-day problems as inflation, shortages, depressions, and mass unemployment, and how to turn capital decumulation into capital accumulation and a declining productivity of labor into a rising productivity of labor. In addition, economics can very clearly show how to achieve economic progress all across the world, and is potentially capable of playing an enormous role in eliminating the intellectual and economic causes both of domestic strife and of international conflict and war. As I will show, the essential nature of the policies economics demonstrates to be necessary to solve all such problems is respect for property rights and economic freedom. (pp. 17–18)

The reviewer's translation of my definition appears in the following passage:

> Reisman's analysis of socialism belongs to the field of what most economists would call political economy or public choice. Thus, it is rather odd that Reisman insists that economics is *the science of wealth,* and that Mises is confused when he calls economics a branch of a larger field which studies human action (see, p. 42 and chap. 2, n. 12). The post-1950 rise and contributions of public choice, social choice, law and economics, and Chicago-school sociology make it eminently clear that Mises's understanding of economic method is correct. (*RAE,* p. 118, n. 1. Italics supplied.)

Is it really possible that the reviewer could have read my definition of economics, my reasons for that definition, and my integration of that definition with the leading applications of economics, and then still have written the above passage?

MISREPRESENTATION OF MY VIEWS ON TIME PREFERENCE

The reviewer equally misrepresents my explicitly stated views on time preference as a determinant of the rate of profit and interest, when he flatly declares: "Reisman rejects the time-preference theory of interest primarily because it explains a rate of return in terms of goods" (p.125, n. 11).

The truth is that I do *not* reject the essential role of time preference in determining the rate of profit and interest. Indeed, in my treatment of profit I explain at the outset that I

> trace the phenomenon of net consumption [i.e., the excess of the demand for consumers' goods from business over the demand for labor by business] back to time preference and show that net consumption is actually the vehicle by means of which time preference determines the rate of profit. (p. 723)

Later in the same chapter (chap. 16), there is a subsection with the title "Net Consumption and Time Preference " (pp. 743–44), which elaborates my views on

the relationship between time preference and the rate of profit and interest. It opens with the statement that:

> Net consumption is not an ultimate cause of profit. It itself reflects the operation of time preference. . . . Time preference determines the proportions in which people devote their income and wealth to present consumption versus provision for the future. The higher the prevailing degree of time preference, the higher is the proportion in which people devote their wealth and income to present consumption in comparison with provision for the future. The lower the prevailing degree of time preference, the lower is the proportion in which they devote their wealth and income to present consumption in comparison with provision for the future. In this way, time preference operates to establish the rate of net consumption. (p. 743)

It should be clear from the very title of my section "Exposition and Critique of the Time-Preference Theory in Its Traditional Form" that what I criticize is not the time-preference theory as such, but only the traditional version of it. To be specific, I criticize the claim that time preference *directly* determines the rate of profit and interest. I hold instead that its influence is indirect, via determining what I call the rate of net consumption. Furthermore, contrary to the reviewer, I do not reject the traditional version "primarily because it explains a rate of return in terms of goods." I reject it for a variety of reasons, among which is the fact that it fails to do what any valid theory of the rate of profit and interest absolutely must do, which is to explain the rate of profit and interest in terms of money, as well as in "real" terms. The scope of my criticisms is indicated by the titles of the various subsections in my critique, namely: "The Contradiction Between Böhm-Bawerk's 'First Cause' and the Doctrine of the Purchasing-Power Premiums," "The Discounting Approach," "The Disappearance of the Higher Value of Present Goods at the Margin: Böhm-Bawerk's Abandonment of the Time-Preference Theory." (The critique spans pages 792–97 of my book.)

AGGREGATE ECONOMIC ACCOUNTING AND "MACROECONOMICS"

My greatest disappointment with the review concerns its treatment of my views on aggregate economic accounting and "macroeconomics." The reviewer declares: "Reisman's discussion mostly recounts earlier work in the Austrian tradition without building much upon that work" (p. 128).

Here I must say that anyone who cares to take the trouble to read my book will easily be able to see that my treatment of aggregate economic accounting and "macroeconomics" goes far beyond that of earlier authors. For example, I go on to elaborate a new system of national-income accounting in which the very equality of national income and net national product, which is at the core of contemporary national-income accounting, serves to confirm that most spending in the economic system consists of *productive expenditure*, not consumption expenditure.

Productive expenditure, the concept of which has been absent from economics since the demise of classical economics, means expenditure for the purpose of

making subsequent sales. It is financed by the gross saving of businessmen and capitalists. I demonstrate that the net investment component of net national product is productive expenditure for capital goods and labor minus the very same costs that business firms deduct from sales revenues in arriving at their profits and thus that most spending in the economic system is concealed under net investment. (The virtually dollar-for-dollar positive relationship that is found to exist between changes in net investment and changes in aggregate profits serves later in my book in the overthrow of the Keynesian *IS* curve and the doctrine of unemployment equilibrium.) (On these points, see, pp. 699–707, 744–50, and 882–83.)

Even more importantly, my book explains how awareness of the separate, distinct demand for and production of capital goods leads to a radically new and different theory of capital accumulation and of the relationship of capital accumulation to the rate of profit and interest. In this theory, capital accumulation is seen to be the result not only of saving but also of practically *anything whatever* that serves to increase the ability to produce. An increase in the ability to produce includes an increase in the ability to produce capital goods and thus, given the productive consumption of capital goods, results in additional capital accumulation.

One major result of this perspective is the realization that technological progress, rather than playing the role of adding to the demand for capital while saving alone increases the supply of capital, is itself a leading *source* of capital accumulation. Another is the realization that increases in the supply of *labor* result in increases in the supply of capital goods, as does international free trade and, above all, economic freedom in general. On this foundation, I explain, for example, the inability of the former Soviet Union to accumulate significant capital. The explanation is the utter inefficiency of socialism, which resulted in the inability ever to produce many more capital goods than were used up in production except at the price of producing so few consumers' goods that mass starvation resulted. On the same foundation, I also develop an argument that free immigration into a laissez-faire capitalist society actually contributes positively to capital accumulation per capita, by virtue of increasing the ability to produce capital goods relative to the productive consumption of capital goods (see, pp. 622–42, 709–12, and 362–66).

My book shows that the role of saving in capital accumulation is fundamentally one of *force to acceleration*, rather than being one of force to motion. This becomes clear in the context of an economic system with a fixed quantity of money and a fixed aggregate demand for products, in which a rise in saving and fall in consumption serves to increase the demand for capital goods relative to the demand for consumers' goods and thus to bring about a more rapid rate of capital accumulation. A leading implication of this perspective is the realization that capital accumulation can occur with a stable relationship between the demand for capital goods and the demand for consumers' goods and thus without any fall in the rate of profit or interest (see pp. 813–20).

Another leading implication is that what necessitates a continuous rise in the demand for capital goods as a condition of capital accumulation is the increase

in the quantity of money and volume of spending, which constantly raises the demand for consumers' goods and thus requires a continuing rise in the demand for capital goods if the same ratio of demand for capital goods to the demand for consumers' goods is to be maintained. A corollary of this is the realization that the continuous net saving out of money income that goes on in the real world, far from being the source of a declining rate of profit and interest, is actually accompanied by a *positive addition* to the rate of profit and interest. This addition, I show, is constituted by the rate of increase in the quantity of money and volume of spending, which systematically raises sales revenues relative to costs, which are always largely historical. The rise in the quantity of money and volume of spending, if modest (as it almost certainly would be under a gold standard) and outweighed by the rate of increase in the production and supply of goods, can, of course, be accompanied by generally falling prices. And these falling prices, far from reducing the rate of profit and interest, are accompanied by this same positive addition to the rate of profit. This, of course, powerfully reinforces the conclusion named earlier that falling prices caused by increases in production are in no sense deflationary—that deflation is a phenomenon of monetary contraction, not of falling prices due to increased production (see pp. 762–75, 834–37).

The preceding several examples, along with the overthrow of the primacy-of-wages doctrine that I mentioned earlier, are important examples, but by no means the totality, of what I believe are my book's original and important contributions in the areas of aggregate economic accounting and "macroeconomics," but which simply went unnoticed in the review.

In this response, I have dealt with five instances of misrepresentation in the review: its claim that I ignore the essential theme of support for businessmen and capitalists, its misrepresentation of my use of classical economics' concept of demand and supply, its distortion of my definition of economics, its misrepresentation of my views on time preference as a determinant of the rate of profit and interest, and finally, its denial of my contributions to aggregate economic accounting and "macroeconomics." These five instances are merely a good sample. The review has other serious instances of misrepresentation as well, such as those concerning my treatment of the subjects of cost of production and marginal productivity, my criticism of the circularity in contemporary economics' concept of demand, and above all, my treatment of the positive theory of profit. Regrettably, lack of space prevents me from proving such further misrepresentations here. But I am confident that they will be obvious to any serious reader of both the review and my book.

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