

Austrian Economics Newsletter

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Viewpoints on the "New" Austrian Monetary Economics

by
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Dramatis Personae

TOM: a graduate student in economics, a Monetarist sympathizer

DICK: a graduate student in economics, basically a (modern) Keynesian

HARRY: a graduate student in economics, a believer in an obscure set of doctrines known as "Austrian" or "Subjectivist" economics.

[The action takes place on the campus of a respected Midwestern university.]

Summary of the Main Ideas

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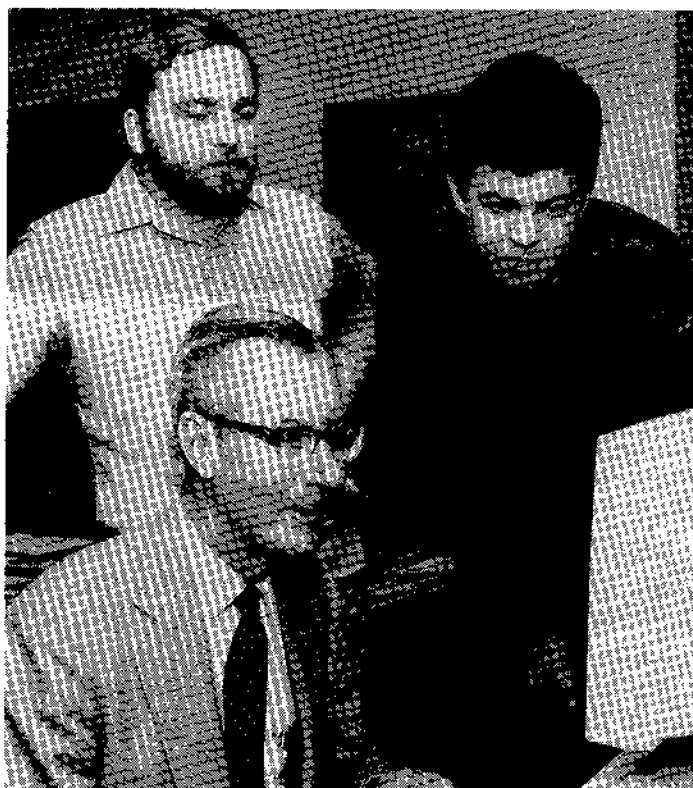
[Tom and Dick are sitting on a bench on the lawn in front of the library. The date is early Fall 1985.]

TOM: Well, Dick are you getting the hang of this Rational Expectations (RE) theory?

DICK: Yes, I think so. I must say some of the ideas are very compelling and the techniques used are quite impressive. I still believe though that RE is consistent with a case for discretionary policy. Differences in information and institutional rigidities—like implicit contracts—suggest that the government can still help avoid the social costs of unemployment.

TOM: Really Dick! I don't know how you can say that. Haven't you read Barro's empirical work or his work on contract formation under RE?

DICK: Sure, but his and all of the other empirical evidence presented in favor of the invariance postulate is highly ambiguous to say the least. And whether we like it or not,



Student editors Sven Thommesen and Mark Thornton look over the shoulder of Professor Leland B. Yeager.

implicit contracts that appear ex post to be inconsistent with RE do exist.

[Harry approaches from the library steps]

TOM: Here's Harry. Let's see what he thinks.

DICK: Tom, you know that Harry is a little weird. He thinks all this stuff is fundamentally flawed. Before you know it we'll be into some deep philosophical debate about methodology.

HARRY: Hi guys! I just received a copy of this great new book by O'Driscoll and Rizzo (O-R).¹

TOM and DICK: Who?

HARRY: They're two young economists who write on modern Austrian economics and they've produced this really interesting book; I think you should both read it. It is a fundamental challenge to most of mainstream economics.

DICK: Come on Harry! How are you every going to get your Ph.D.? You need to be concentrating on the important stuff. Tom and I were just discussing RE.

HARRY: Don't worry Dick, I'll get my degree. Actually in

chapter 9 of this book O-R discuss aspects of RE.

DICK: I can imagine.

HARRY: You've dismissed their arguments even before you know what they are. Talk about keeping an open mind!

TOM: OK, OK, let's not get excited. What do they say, Harry?

HARRY: Well, its very difficult for me to explain unless you are willing to let me fill in some background on Austrian economics. That could take awhile.

DICK: Here we go.

TOM: Look, I've got a proposition to make. We have a mid-term break coming up in a couple of weeks. Why don't you prepare a reading list for Dick and me on Austrian economics and we'll try to acquire some of the necessary background?

DICK: Come on Tom. Do you really think that's an efficient use of our time?

TOM: It may provide a fresh perspective on things Dick. And Harry's right. We do need to keep an open mind. Now, if that's agreed we'll meet after the mid-term break for a discussion.

HARRY: Great idea! Let me suggest that to keep it focused, we concentrate on chapter 9 of O-R. You can borrow my copy (though I really recommend you buy your own). I think you should read the whole book, but since you're both interested in monetary economics let's focus on that. Agreed?

TOM: Agreed.

DICK: Oh all right.

Scene ii — Methodology

[Later that term, in a study room in the basement of the library.]

TOM: If I'd known what I was getting into, I doubt that I'd have suggested it Harry. That Austrian stuff is rough going, especially at first before you catch on to the spirit of it. It really is quite different from what I'm used to. I'm not sure I know what to think.

DICK: Actually I was very pleasantly surprised. I found much of the discussion very refreshing, especially the emphasis on the importance of expectations—that's very Keynesian. I didn't like the policy implications though. Those seem very Monetarist.

TOM: Yes, perhaps that's why I'm confused.

HARRY: I really think it would help if we spent a little time discussing methodology.

DICK: Uh oh.

HARRY: You have to understand that from the Austrian perspective mainstream economic methodology is based on a view of the world that is very naive. In reality the hypotheses that emerge from theorizing in economics are seldom well formed or capable of decisive refutation. And, of course, as was recently pointed out by McCloskey²,

economists don't really take the falsification criterion seriously. Their rhetoric is different from their practice. The practice is to decide between theories or models on the basis of their intuitive appeal and mathematical elegance.

If you read the Mises chapter I put on the reading list, you know that, for Austrians, the soundness of a theory is not an "empirical" matter, but rather a matter of logic.

TOM: Yes, I did eventually understand the notion that theory and history yield different types of knowledge and that knowledge in social science theory is 'a priori' in nature—although apparently not all Austrians agree completely on this. I also understand that praxeology implies both subjectivism and methodological individualism. But having come this far it seems to me we have reached an impasse. Mainstream economists simply do not feel bound by the principles that Austrians consider inviolable.³ Rather as you know they consider the realism of the assumptions—including I suppose assumptions about the acquisition of knowledge or the formation of expectations—to be beside the point. What matters is whether or not the theory "works" in providing useful predictions.

HARRY: Yes, but how are we to know if the theory "works" when, as I've said already, most (if not all) hypotheses are effectively insulated from refutation.⁴

DICK: It is clear by the amount of space that they devote to methodological issues, that O-R are sensitive to these questions. After exploring at length the meaning and implications of subjectivism they attempt, I think, to go beyond their mentors and offer (in chapter 2 section II entitled the Dimensions of Subjectivism) an explicit criterion for deciding between theories. It is worth quoting:⁵

The covering-law model, therefore, does not provide an adequate form of explanation for dynamically subjectivist theories. An essential feature of these theories is that logical deducibility of the statement of the relevant decision or phenomenon is no longer possible. This, of course, does not mean that explanation is no longer possible but merely that one variety of explanation has been excluded. "Not deducibility but intelligibility constitutes the basic feature of the logic of explanation."... One clear and useful meaning we can attach to the intelligibility relationship is that the choice-theoretic explanatory schema must render the given phenomenon more likely than if the particular model had not been presented.

This intrigued me. Intelligibility itself is surely a subjective category whose content differs across individuals particularly as they appraise real-world situations corresponding to proffered theories—business cycles for example. So, in the final analysis what makes a theory "work" or which theory "works" best is itself a subjective decision. In that sense we surely have reached an impasse.

But, since my interest is now aroused, and I am anxious to discuss O-R's monetary economics I suggest we bear their intelligibility criterion in mind and press on. Later we

can decide whether we agree or not that their theory maximizes intelligibility.

TOM: Good idea. But before we leave this there is one other methodological point that's worth noting. For O-R subjectivism implies methodological dualism, that is, that the social and natural sciences must be treated differently. I found this unconvincing. The analysis of cyclical phenomena, for example, would appear to be amenable to mathematical techniques developed in the natural sciences.

HARRY: I don't think so Tom. My own view of the matter is as follows. It is not subjectivism per se that leads to methodological dualism. It is rather that subjectivism in human affairs implies disparate expectations. Obviously, all (scientific) investigation leads to results that must be subjectively perceived and interpreted. In what we call the hard or natural sciences there is a high probability of obtaining a consensus on the meaning of those individual perceptions. This is because the connection between the individual experience of the "data" and the mental constructs used to organize them for analysis and prediction vary little, if at all, across individuals. Thus, there is unanimous agreement that the atomic weight of hydrogen will continue to be what it was (acknowledged by all to be) historically. Predictions based upon this belief can be made. Expectations converge. There is a shared experience of the world and of ideas that is absent in the case of individuals experiencing business cycles for example.

Similar considerations apply to objections about the use of mathematics in economics. These objections should not be to the use of symbolic representations per se, but rather to the interpretation of the symbols as constants—perceived to be so by all the economic agents. These symbols, if they are anything, are necessarily expressions of the subjective (disparate) notions in people's minds as to how the world works or will work.

I think this will be important in our later discussion, and I am very glad you both feel that we should continue.

~~DICK: Well, having come this far, why not? I suggest you summarize the main ideas in O-R chapter 9 Harry.~~

HARRY: Ok, but why don't we do this over a beer? It's getting late.

Scene iii — Summary of the Main Ideas

[Later that evening in a favorite student meeting place]

HARRY: Well, as O-R tell us, this chapter is organized around two major topics: Menger's theory of the origins of money and the Mises-Hayek theory of economic fluctuations. Of the two I think the former is the less well known but, perhaps, the more important. O-R use it to illustrate the subjectivist approach to analysis and at the same time they illuminate two recurring themes—the distinction between typical and unique events and the development and role of institutions. Also, at the end of the chapter this topic is tied to the question of monetary regimes.

TOM: Why do you say this is the more important? At times I was at a loss as to its relevance.

HARRY: It has very important implications and it contains some ideas that challenge the very heart of neoclassical monetary economics.

TOM: You'd better explain.

HARRY: Firstly, there is the basic idea that money, as a social institution, was not designed by anyone. Like language it simply evolved. But, more important, like language it could not have been designed. The analogy with language is compelling. [page 13]. The use of a language presumes that others understand it. The use of a money instrument presumes that others value it. Money cannot be legislated. This should give pause to all those who dabble in theories about the optimum money supply or the most efficient monetary rule. Furthermore, money, like language, is universal. People invariably tend to use them. That is a pattern prediction. The precise form that both take (and we might also include the institution of law) is not predictable.

So, I think these ideas are important. But actually O-R's treatment is both less and more than Menger's.⁶ They touch rather lightly on the details of the process by which money evolves. Menger's own treatment is replete with historical examples. Also, when explaining the benefits from money that accrue to society, they did not explain as clearly as they might have that money facilitates both exchange and production. [See Lewin (1984)]. It does so by economizing on information and reducing risk. The latter, in particular, is important in facilitating production. Thus, when money malfunctions, both production and consumption activities are affected. This provides a link between the origins of money and the topic of economic fluctuations. Of course, this is all there is between the lines, and, perhaps, that is where it should be given that both Menger and Mises had treated the subject quite fully.

DICK: Well, then what is new about O-R's treatment?

HARRY: To some extent it is a matter of emphasis, but I think there are some new things here. One which I think may be overstated is the idea that the "subjective perceptions and market characteristics of goods not their physical attributes" [page 8] are important in determining the course of monetary evolution. I understand this as an attempt to strike a blow for subjectivism, but my reading of Menger suggests a close connection between the physical attributes of commodities and the value that people place on them as money qua money. Obviously, these attributes have no absolute or intrinsic value, but when coupled with place and circumstance they combine to suggest that some things make better money than others. I'm thinking of things like durability, divisibility, transportability, ease of identification etc. When put like this, O-R's point may appear less controversial. What comes to be used as money is indeed "partly an accident of history" [page 8] but (again

reading Menger) there are striking similarities in the type of monetary evolution that different societies at the same stage of development undergo—and this is true even for societies that develop in isolation of one another.

Another idea, which is new more in its emphasis than in its content, is that all other kinds of social evolution depend crucially on monetary evolution [page 9]. This is a very important idea that makes nonsense of any attempt to calculate the value to society of money in terms of some notion of consumer surplus. It illustrates quite clearly that one cannot think about money productively in terms of comparative statics. Monetary developments are part of a truly dynamic process.

TOM: What about the second topic—the theory of fluctuations?

HARRY: I find this much more difficult to characterize because I find myself more critical of their treatment than of the first topic.

DICK: Why don't you save your general criticisms until later and concentrate on outlining the main ideas now? You may assume that Tom and I are familiar with the Mises-Hayek theory.

HARRY: OK, that sounds reasonable. Perhaps the most important is the idea that fluctuations are manifestations of error. People make mistakes and when enough of them do so in a similar way we get a cycle. Cycles thus generate economic losses and opportunities. People make errors because information is decentralized and they get the "wrong" messages; for example, that the supply of loanable funds has increased when actually it has not. Thus, as a result of "incorrect" investments the capital stock is inappropriate to produce the consumption goods that people want to consume at prevailing prices. An important insight is that production is a complex structure that depends in a crucial way on the intertemporal complementarity of different production activities. So, when one set of activities is disrupted or shifted the reverberating effects are felt over time. Also, production instruments are often durable and, most important, depreciable only over time. This means that investment errors become embodied in specific capital goods. The capital structure at any point in time reflects not only prospective production capabilities, but also retrospective production decisions and errors.

O-R adopt a simplified I-S framework of exposition. But it is clear that their theory does include the rich complexity that I just mentioned. As such it is a fundamental challenge to prevailing cycle theory. In particular, it is a disequilibrium theory.

TOM: It seems to me it is also a Monetary theory not that different from some of the RE models.

HARRY: Actually I think it is more accurate to say that it is a money-capital theory. The capital part of it does make it quite different from all mainstream models. In particular, the use of broad aggregates in the latter is carefully

avoided by the Austrians.

DICK: So what's new about O-R's treatment?

HARRY: Once again there are new points of emphasis. But O-R also use a mode of expression that is more contemporary than Mises, Hayek, Rothbard, and others and this may help make it more accessible. I am thinking of the use of I and S schedules, present-value formulae and terminology like real interest rates, interest elasticity and so on. This allows them to make some graphic points (no pun intended) that illustrate differences from received theory.

DICK: For example?

HARRY: For example:

(1) that in any realistic account of fluctuations the numerator of the P.V. formula [page 27] will change as a result of other changes (like changes in r)

(2) that sunk costs make "bad" projects appear profitable [page 32]

(3) that cycles will include fluctuations in real interest rates.

The last-mentioned is particularly compelling in the light of the economic environment of the last few years with record high interest rates and persistently low levels of price inflation.

TOM: What did you make of the section on empirical evidence?

HARRY: I found it provocative, but wondered if O-R were attempting to claim more for the empirical "tests" than is consistent with their position. For example, the statement "Rothbard's thesis has since been justified . . ." [page 36]. However, in general I welcome this type of approach if carefully done, if only for strategic reasons.

TOM: What do you mean?

HARRY: Austrian economics for the most part has been thoroughly ignored for the past thirty years or so. I believe the degree of neglect it has experienced is out of all proportion to the contribution the Austrians have made to the foundations of modern economics including monetary economics. But I think the reasons for this neglect are understandable. First, much of Austrian literature appears very long on destructive criticism and very short on alternatives. Second, it is couched in terms unappealing to modern technically sophisticated student like you and Dick. Econometric investigations of Austrian themes may be a way to get your attention, especially if they perform no worse than the alternatives.

I think O-R have tried to address both these factors by providing alternative theoretical specifications where they could and not just criticizing the conventional wisdom and by addressing the "evidence" in modern terms. To this extent, I feel somewhat inhibited in criticizing their efforts.

DICK: You will do it though, won't you?

HARRY: Yes, but perhaps we should first stop and discuss the ideas I have summarized so far.

TOM: OK, but not tonight. Let's meet again in the library

next Wednesday.

Scene iv — Point-Counterpoint

[The following Wednesday in the library]

TOM: I've been thinking about what you said, Harry—that Austrian business cycle theory is a disequilibrium theory. Surely this is a semantic issue, one that may not be very important. After all, any disequilibrium situation can be recast as an equilibrium situation simply by respecifying the constraints to include the time and information available to agents. Use of the notion of equilibrium simply reflects the conviction that people do the best they can given the circumstances and that, therefore, their actions can be modelled and understood. What is gained by insisting on the disequilibrium character of fluctuations?

DICK: I disagree, Tom. One thing I liked about O-R's book is their emphasis on the difference between individual and general equilibrium. This is surely one of Keynes's most important points. The economic system may find itself at any time in a situation which from the vantage point of the ideal observer is a disequilibrium. I agree he confused matters by characterizing it as a less than full employment equilibrium. He really was dealing with what we should recognize as a disequilibrium.

HARRY: I think more than semantics is involved here. It is not just that there is a difference between individual and general equilibrium. It is that if the latter has any meaning at all, it is a very restricted meaning. I refer to Hayek's treatment of equilibrium in terms of the coordination of individual plans. So when we say that economic fluctuations are disequilibrium phenomena we should understand this to mean that they are the consequences of plan discoordination—that is of the disappointment or surprise to individual plans. To define disequilibrium away, in the manner you suggested, Tom, may be to sacrifice any understanding of how this discoordination can occur.

TOM: I see the point, but I don't think you are entirely correct. Even in terms of individual plans, any actual series of events (which may be part of an economic cycle) can be understood as a succession of individual equilibria. Every individual is in equilibrium at each point of time (or short period of time) relative to the circumstances, including knowledge, at that time. When something changes he or she moves to a new equilibrium.

HARRY: That's precisely the problem. You can't explain how the individual gets from one equilibrium to another—he or she must be in disequilibrium in the transition or else action becomes meaningless.

But notice that we have moved from macro to individual equilibrium. I think that's important. I have serious doubts about the applicability of the equilibrium concept at the macro level or at any level above the individual.

TOM: I have another point. On page 25 O-R characterize a typical sequence of the first part of a cycle. It occurs to me

that a RE invariance property can be derived from this setup. If the economic agents understand this sequence then surely they will immediately attempt to buy consumer goods. If they do, the prices of these goods will rise and the cycle will be avoided.

HARRY: If you read this chapter carefully you see that O-R anticipated this point, which has been made before of the Mises-Hayek theory. They make two points in reply: (1) While it may be true that agents understand the general pattern of cycles, they lack the ability to predict magnitudes and turning points. Thus, mistakes are still made. "In a decentralized economy, the kind of information acquired by agents is not sufficient to insulate them completely from the effects of monetary shocks." [page 29]. (2) Even if expectations in general were correct, cycles may exist. Remember the discussion about making Mr. Garrison a wealthy man.

But having said this I want to concede that it does raise an important point about the feasibility of cycle theory (any cycle theory). I hope I'll get a chance later to give you my own view.

DICK: Perhaps just one final point before you do. The Austrians seem to have much in common with both the Keynesians and Monetarists. To oversimplify, they share many of the Keynesian criticisms of the Monetarists (and the New Classicals) yet they end up with the same policy prescriptions.

HARRY: O-R touch on this though they don't expand on it. Austrians like the subjectivist elements of Keynes's analysis and in the hands of O-R can integrate his multiplier process into theirs. From a policy perspective, however, they reject both Monetarist and Keynesian approaches as being constructivist in approach. Thus, paradoxically they believe that the dark forces of time and ignorance lead in exactly the opposite direction from what the Keynesians think they do.

Scene v — Criticisms and Extensions

TOM: What were you going to say a moment ago about cycle theory?

HARRY: I'm afraid that since this is a major point I may be a little long winded.

Though I am in basic sympathy with it, I believe that Austrian cycle theory may be laboring under an inconsistency in the way in which it treats capital. To understand you have to remember that one of the earliest and most well known Austrian contributions was that of Bohm-Bawerk. Although aspects of his general approach were repudiated by the Austrian consensus, the spirit of his approach has been honored. Austrians writing in cycle theory, continue to cast capital concepts in terms of "longer" and "shorter" production periods. O-R are no exception, though they do so in very general terms. Still, I wonder if this approach is necessary or helpful.

Bohm-Bawerk, of course, was looking for a method of measuring capital. He zeroed in on "time" as being the most important element in capital theory. Production takes time. Some production processes take more time than others. Since people are impatient for the results they will only adopt longer processes if they yield more valuable results. Thus, to say that a society is more highly capitalized is really to say that its production processes are "longer" or more roundabout. There is a basic common-sense appeal to this line of reasoning, especially when we look at the real world with its highly complex, specialized activities.

However, when one tries to formulate it, as Bohm-Bawerk and others have done, one gets into problems. Basically, he tried to use time as a measuring device and came up with the concept "average period of production" as a measure of the size of a capital stock. Now, I need not repeat here the difficulties of sustaining this concept. The extensive literature to which it gave rise bears adequate testimony. I need say merely that in any actual economy where multiple outputs are continuously produced with multiple inputs it is impossible to calculate it. In fact any measure that attempts to compare processes in terms of the "time they take" is bound to fail. When inputs and outputs are heterogeneous they can only be compared using money prices as a yardstick. But these prices are the result of production (and consumption) decisions. "Longer" and "shorter" are not independent of the decisions they are supposed to determine. So we are in the curious situation of using concepts that rely on general equilibrium for their very meaning in order to enunciate a disequilibrium theory of the cycle. In fact, in disequilibrium "longer" and "shorter" don't have any clear meaning.

Actually, my criticism is more essential than this. O-R-Garrison attempt to salvage some use for "period or stage of production" by asserting that this concept is really a subjective one:

A common misunderstanding can be avoided, and the element of subjectivism can be reemphasized by pointing out that the notion of a "stage of production" is itself a subjective concept. That is, it has meaning only in terms of the relationship (as perceived by the producers) between capital goods that make up a stage of production and the future consumption goods ... The difficulty of concretizing the notion of stages of production does not render the notion meaningless or unhelpful. It only reminds us that there is coordination. Conceiving of the production process in terms of (subjectively defined) stages of production does help to identify the specific nature of the coordination problem and it points the way to a satisfactory solution to that problem. [Chapter 8, page 12].

Now, obviously it is true that different producers have production plans. These plans involve temporal se-

quences—relationships between inputs and outputs over time. To the extent that these plans are successfully completed they must be consistent with one another. If they are inconsistent there will be shortages, surpluses, delays etc. Thus, insofar as inconsistency of plans is not a dominant fact of production life, there will be some shared notions of some production activities having to occur before and after others. In that restricted sense one may identify "stages" of production. This also leads to the realization that there is an implicit intertemporal structure to the capital stock at any point of time. However, this intertemporal logic, like the hypothetical core of perfectly consistent plans, is never actually wholly perceived by anyone. No one is actually motivated by it. Individuals, at best, catch glimpses of it. What exists in individuals' minds, the plans they make, may bear a closer or more distant relationship to the ideal structure.

TOM: Where is all this leading, Harry?

HARRY: I can understand your impatience, but this is a tough subject.

TOM: OK, see if you can short circuit it though.

HARRY: What I'm getting to is that when O-R construct a cycle that starts with a fall in the interest rate that causes investors to shift away from "shorter" into "longer" production processes, they cannot be talking about anything that corresponds to readily identifiable objects. The whole apparatus is misleading and unnecessary. It is true that individual producers may be induced into investing in what they see as "longer" processes, but even the meaning of this is in doubt. And what if they are so induced? As long as there exists no perfect correspondence between these individual-mind constructs and the real world, we do not have a theory of a cycle-in-process in terms of a shift from one class of activities to another. That's it really. The defining characteristics of the proposed classes—longer, shorter—have no objective existence.

Let me give an example. One page 31 O-R try to explain why a monetary cycle is unlikely to be quickly self correcting. One of the two reasons (the lesser one) that they give is that the government will exacerbate the problem by investing in "capital—goods (i.e. 'smokestack industries')". The implication is that it is these industries that are (type-1) "longer" period industries. But, it is by no means clear that these are the industries that will appear profitable to private investors when the interest-rate falls. An industry that appears to embody large amounts of physical capital—any may be appealing to the government for political reasons—may not require large amounts of financial capital investment in order to increase output. If most of the fixed (specific) resources are already in place and production could be increased with a relatively small increase in the application of variable (general) resources (labor working overtime), then it is not necessarily these industries that will initially appear to increase the most in profitability. There appears to be fallacy of composition

here.

DICK: I am having some difficulty following this, Harry. Perhaps it is because I am used to thinking in terms of P.V. formulae. Can you relate it to that for me?

HARRY: Yes, actually this bears some resemblance to the reswitching problem.⁷ P.V. formulae are similar to period-of-production formulae in that they attempt to reduce multi-dimensional production processes to one dimension—present value. (They apply to flow and point output configurations rather than only to point output configurations. But the basic idea is the same.) Remember what I said the other day about subjectivism and the use of symbols. The point is that when O-R use that formula—admittedly only as a simplification device—they must know that the symbols refer to individual mental images and not to objectively verifiable phenomena. I'm glad you brought this up. Let's look at the P.V. formula more closely:

$$P.V. = \frac{S_1}{(1+r)} + \frac{S_2}{(1+r)^2} + \frac{S_3}{(1+r)^3} + \dots + \frac{S_n}{(1+r)^n}$$

Note that it involves three essential symbols or ideas, namely, S , r and n . Each of these (not particularly n , i.e. the length of the project) is subjective and will differ across individuals. There are an infinite number of ways to characterize any investment, especially when it involves a multiplicity of complementary processes. Certainly there is no sense in which they can be unambiguously divided into longer or shorter even at the individual level. As the discount rate falls, some projects will gain in relative attractiveness and some will fall. But this is not monotonic and it may switch and reswitch. It depends on the configuration that the individual has in his or her mind. There is another problem. When individuals appraise alternative investments they may look at more than just the present value. (In technical terms their utility functions may not be additively separable over time).

I guess I'm just reacting to what I perceive as an excessive formalism in this context.

DICK: Gee, Harry, you were critical of the Austrians for destructive criticism. Where does your view leave their theory?

HARRY: Actually, I do have a tentative alternative, a half-formed theory of fluctuations. But it's not really very concrete and I don't want to bore you.

DICK: Go on bore me Harry. Tom wants to hear.

TOM: Sure why not?

HARRY: As I see it, there are four basic ideas that must go into a cycle or fluctuations theory:

- (1) The identification and characterization of the cycle as subjective
- (2) Cycles are political phenomena
- (3) Cycles involve clusters of errors
- (4) Cycles are not generalizable at the level of movements of interest rates, prices, output or any general series.

I will try and elaborate briefly on each in turn. First,

what we call a cycle in modern economics may be in large part a statistical artifact. In a retrospective sense a cycle is an ex post identification of coherence among a set of aggregate time series. The composition (and meaning of the elements) of the set and the demarcation of the cycles can and do differ across analysts. The "cycle" that is relevant to the individual economic agent may be something completely different. Looked at prospectively, the cycle doesn't exist. It is in the process of being created by individual decisions and its realization will have unique aspects.

Second, cycles occur because of politics, money and uncertainty. In our modern political systems politicians buy votes with inflation [Lewin (1982), pages 639-40]. Inflation brings resource reallocation because it is the result of government expenditure on specific vote-raising projects. Those who receive the proceeds directly and, to some extent, indirectly perceive a gain. Those who experience the price inflation perceive a loss. The gains are more immediate and concentrated than the losses and are more likely to be seen as connected to specific expenditures. Hence, there is a well documented inflationary bias in our budgetary system. Everyone would like to see inflation stopped but no-one wants their pet project cut. However, cuts do occur for two reasons. One, in an inflation the real value of a government subsidy erodes with time and pressure builds up to accelerate the subsidy beyond tolerance limits. Second, and related, subsidies tend to occur in ailing or nonviable industries. Often these get worse over time or at least no better. The subsidy is, in fact, implicitly designed to prevent painful resource reallocation or to establish a pattern of resource allocation out of line with private consumption and production decisions. That is why they occur through the political process rather than through the market process. Thus, unless they are to accelerate without limit, these subsidies eventually get cut (or they increase at lower nominal rates). By this time they have substantial resources committed to them—some of which would have been elsewhere, perhaps in a different form,

but for the continuing subsidy. The relative cutback now causes a large reallocation. This implies unemployment and an income-constrained process.

Third, you see from this that errors will be made, the indirect effects of the inflation, the subsidy and the subsequent cutbacks are diffuse and uncertain. And the errors will be clustered in time and economic space (i.e. connected to each other by type of economic activity). Basically, this is because some resources are both durable and complementary to each other. There is a shared fate that is spread over time.

Finally, although fluctuations may appear as general cycles when, as is bound to happen, the politically exacerbated ups and downs in specific industries coincide, these cycles are not generalizable in terms of movements of economic aggregates. Interest rates do not necessarily play a pivotal initiating role. The monetary effects may work

directly on consumer goods industries. All that one can say, I believe, is that the pattern of production gets out of line with the more immovable pattern of consumption and oscillates around it. [cf. Hayek (1980) pages 20-21].

Scene vi - Epilogue

TOM: That's very interesting Harry. I think we'd better get back to studying the convergence properties of differential equation systems for the econometrics exam though.

HARRY: What did you think about my ideas and the issues raised in this book? Will economics ever be the same?

TOM: You know, in the final analysis, it comes down to choice between methodologies. That's where we started and that's where we were bound to end up. Methodological assertions really revolve around criteria of "proof" and, therefore, cannot be proved. (This is a case of Godel's theorem).

HARRY: What do you think Dick?

TOM: Dick's fallen asleep, Harry.

Peter Lewin is visiting assistant professor of economics at the University of Texas at Arlington.

Footnotes

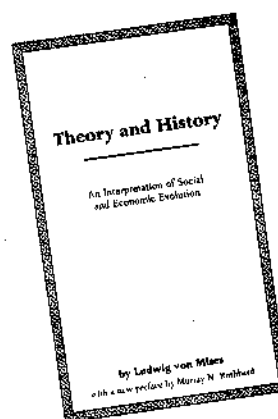
*With apologies to Maddock and Carter (1982). In order not to impede the flow of action references have been kept to a minimum.

1. Gerald P. O'Driscoll, Jr. and Mario J. Rizzo, *The Economics of Time and Ignorance*.
2. McCloskey (1983). See Klammer (1983).
3. See Lewin (1982).
4. See for example Blaug (1976).
5. O-R page 15 references omitted, emphases added.
6. Menger (1981), see also White (1984) chapter 2 and Lewin (1984) chapter 2.
7. See Samuelson (1966).

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