Simon, Herbert

Working Paper

On the Behavioral and Rational Foundation of Economic Theory

IUI Working Paper, No. 115

Provided in Cooperation with:
Research Institute of Industrial Economics (IFN), Stockholm

Suggested Citation: Simon, Herbert (1983) : On the Behavioral and Rational Foundation of Economic Theory, IUI Working Paper, No. 115, The Research Institute of Industrial Economics (IUI), Stockholm

This Version is available at:
http://hdl.handle.net/10419/95108

A list of Working Papers on the last pages

No. 115, 1983

On the Behavioral and Rational Foundations of Economic Theory

by

Herbert Simon

Paper presented to the IUI Conference on:
The Dynamics of Decentralized (Market) Economies
Stockholm-Saltsjöbaden, Grand Hotel
August 28 - September 1, 1983

Sponsored by:
The Marcus Wallenberg Foundation for International Cooperation in Science
and organized jointly by the Industrial Institute for Economic and Social Research (**IUI**) and the Journal of Economic Behavior and Organization (**JEBO**).

December, 1983

ON THE BEHAVIORAL AND RATIONAL FOUNDATIONS OF ECONOMIC DYNAMICS

by
Herbert A. Simon

Paper presented to the IUI Conference on:
THE DYNAMICS OF DECENTRALIZED (MARKET) ECONOMIES

Stockholm-Saltsjöbaden, Grand Hotel
August 28 - September 1, 1983

Sponsored by:
THE MARCUS WALLENBERG FOUNDATION FOR INTERNATIONAL COOPERATION IN SCIENCE
and organized jointly by the Industrial Institute for Economic and Social Research (**IUI**) and the Journal of Economic Behavior and Organization (**JEBO**).

On the Behavioral and Rational Foundations of Economic Dynamics

Herbert A. Simon

Department of Psychology

Carnegie-Mellon University

On the Behavioral and Rational Foundations of Economic Dynamics

Herbert A. Simon
Carnegie-Mellon University

ABSTRACT

Existing uncertainties about the correct explanations for economic growth and business cycles cannot be settled by aggregative analysis within the neoclassical framework. Current disputes in theory rest largely on ad hoc, casually empirical, assumptions about departures from perfect rationality under uncertainty. Such disputes can only be settled by painstaking microeconomic empirical study of human decision making and problem solving.

Microeconomic research of the kinds that are required can receive powerful guidance from the theories of human thinking that have been developed and tested over the past twenty five years by cognitive psychologists.

The study of the movement of economic systems through time has focused on two phenomena of particular interest and importance: the business cycle and long-run changes in technology and productivity. One of the impressive features of Joseph Schumpeter's economic theories is his unification of these two phenomena, accounting for business cycles as the byproducts of discontinuous advances in technology. The task I have set myself in this paper, however, is not to argue for this, or any other, particular theory of the business cycle or of economic development. Instead, I wish to look at the logical foundations of such theories, and particularly the assumptions they make about the behavior of the human actors in the economic system.

If I may be permitted a personal remark, I observe that I am one of a very small number of pre-Keynesians present at this meeting. By a pre-Keynesian I mean someone whose habits of thought about economics were already formed, or at least partially formed, before the publication of the *General Theory* in 1936. By that year I had already studied Ely's textbook, taken Henry Simonds' course in price theory, and attended Henry Schultz's courses in econometrics. Of course, you cannot draw any strong conclusions about my economic views from those facts. Milton Friedman and Paul Samuelson are also pre-Keynesians, trained at the very same University of Chicago that I attended.

The significance of having a pre-Keynesian past is that it subjects one to occasional attacks of *deja vu*. Rational expectations theory, though innovative enough, does not come as a complete novelty to anyone who has reflected on Frank Knight's *Risk, Uncertainty, and Profit*. Reviewing the current explanations for the business cycle that are offered to us by monetarists, rational expectationists, and supply siders, we may even convince ourselves that there is a genuine fifty-year cycle — not a business cycle but a cycle of business cycle theories.

Given the current disarray of macroeconomics, and the bewildering multiplicity of theories purporting to derive macroeconomic phenomena from microeconomic assumptions, it may be less useful to offer new ideas than to ask how we can winnow out the many and conflicting ideas we already have. In this paper, I will discuss the logical structure of some quite conventional ideas about how an economy like ours *might* operate. My main concern will be with the question of how we can subject these ideas to empirical test and use them effectively in the formulation of social policy.

Finally, I am among the economic majority who regard the heresies of Keynes as small (albeit important) heresies. Just as Luther accepted the Bible while rejecting the Pope, so Keynes' modes of reasoning in the *General Theory* are only locally heretical. His general form of argumentation is the one that is standard in economics: what might be called "what would I do if I were a rational man" argumentation. So my remarks will cover Keynes as well as the classics.

Foundations of Classical and Neoclassical Theory

The economic theories that we call classical and neo-classical are based ostensibly on two tautologies and a postulate of rationality. The two tautologies are Say's Law (perhaps better referred to as the national income identity when intended as a tautology) and the monetary identity (alias the classical Quantity Theory of Money). The first of these says that, at any level of production, the income generated just covers the costs incurred. The second says that money exchanges hands at just the rate needed to cover total transactions.[1]

These laws can be thought of as describing a system in neutral equilibrium. A system governed by the national income identity can remain at full employment, or at a zero level of production. A system governed by the monetary identity can have stable prices or inflation, a high velocity of money or a low one. There is nothing in either law to produce a change in any of its variables — except, of course, in response to a change in one or more of the others. Nor is there anything inherent in the equations that forces the variables to remain constant.

---

[1] These brief statements are intended to identify the tautologies, not to characterize them with any precision.

We must be careful about thinking that, in labeling a law a tautology, we thereby make it innocuous. In fact, the tautological character of a law can only be determined in a context of the theory and observables in which it is embedded. Newton's Second Law, F = ma, is often referred to as a tautology — and so it is in a situation where only mass and acceleration can be observed. Then it is simply a definition of force. However, if independent operations exist for measuring the force applied to a body, then F = ma is an empirical law, for it asserts that the measured force must equal the product of the independently measured mass and acceleration. (See "The axioms of Newtonian mechanics," Chapter 6.1 in my *Models of Discovery*; and "The axiomatization of physical theories," Chapter 6.5 of the same volume.[2])

In much the same way, economic arguments can be, and frequently are, derived from Say's Law or the monetary equation by postulating something (often, *ceteris paribus*) about certain of the variables in the law and then deducing a relation among the others. Thus, we have the familiar argument that an increase in the quantity of money raises the price level (the velocity of money and the level of real product held constant). Say's Law lends itself to parallel arguments about an increased level of output "creating" new demand by generating income.

Whenever we encounter such an argument, "based on" Say's Law or the monetary equation, we must keep clearly in mind that the argument is really "based on" the ceteris paribus or other auxiliary assumptions that are being made. We should not be seduced into accepting the argument because of its tautological appearance without careful consideration of the empirical bases for these auxiliary assumptions. Such assumptions commonly have the effect of transforming tautologies into conditions of equilibrium.

The third component of classical theories, the postulate of rationality, can take many forms. It's job is to get markets cleared by providing an opportunity for profit or increased utility whenever supply and demand do not balance, and to make sure that resources are used up to the proper margins. For a wide range of assumptions, rationality implies that, in

---

[2]Simon (1977).

equilibrium, people will have no motivation to modify their behaviors, and resources will be fully employed. The equilibrium need not, of course, be static. Instead, variables, exogenous or endogenous, like innovation, capital accumulation and population increase may produce a constantly changing steady state, but always with full employment of resources.

What is notably absent from these classical foundations is any specification of the mechanisms that restore equilibrium when it is disturbed. In fact, the only endogenous variable that is at all productive of dynamics in classical theory is the volume of capital, which is determined by the equilibrium requirements, and which in turn moves the system to new equilibria. In this respect it is much easier to derive a theory of economic development from the classical postulates than a theory of the business cycle. (Whether the theory thus derived is correct is a different question.)

I need to qualify the assertion that classical theory provides no specification of the mechanisms that restore equilibrium. The theory does assert that if the demand for a commodity is, for instance, below the supply at the market price, then suppliers will reduce the price and/or restrict the supply. What the theory does not provide, without additional postulates, is information about the relative magnitudes of the two adjustments or the rates at which they will occur.

The incomplete specification of the adaptive mechanisms leads to a problem and an opportunity. The problem is that it is precisely these mechanisms that determine the possibility and structure of cyclical movements about the equilibrium. The opportunity is to extend the postulate of rationality to specify a rational mode and rate of adjustment. Economic search theory and the theory of rational expectations are examples of responses to this challenge.

The "Stylized Facts" of Dynamics

In his admirable little book, *Growth Theory*, Robert Solow reproduces the six "stylized facts" that, according to Kaldor, characterize the process of economic growth in advanced economies:

(1) Real output per man (or per man-hour) grows at a more or less constant rate over fairly long periods of time.

(2) The stock of real capital grows at a more or less constant rate exceeding the rate of growth of labour input.

(3) Moreover, the rates of growth of real output and the stock of capital goods tend to be about the same, so that the ratio of capital to output shows no systematic trend.

(4) The rate of profit on capital has a horizontal trend.

(5) The rate of growth of output per man can vary quite a lot from one country to another.

(6) Economies with a high share of profits in income tend to have a high ratio of investment to output.[3]

There is a rather high level of agreement among economists, although not complete agreement, that these "facts" do reasonably describe what has gone on in the economic growth of the advanced countries. A growth theory may be regarded as a plausible first approximation if it can predict (or retrodict) them; otherwise it is implausible. There is no guarantee that more than one theory will not pass the test, in which case new facts must be adduced that can discriminate among the competing theories, or some other criterion must be introduced that can select the "correct" one.

Robert E. Lucas (1977) has provided a similar list of "stylized facts" to describe what happens during business cycles. If his list will not command universal assent, it does not differ in important particulars from the lists that have been drawn up by others (e.g., by Mitchell in the essay whose title I have alluded to). Whatever the quarrels may be among business cycle theorists, they are not mainly quarrels about these facts.

> Technically, movements about trend in gross national product in any country can be well described by a stochastically disturbed difference equation of very low order. These movements do not exhibit uniformity of either period or amplitude, which is to say, they do not resemble the deterministic wave motions which sometimes arise in the natural sciences. Those regularities which are observed are in the *co-movements* among different aggregative time series.
>
> The principal among these are the following. (1) Output movements across broadly defined sectors move together. (In Mitchell's terminology, they exhibit high *conformity*; in modern time series language, they have high *coherence*.) (ii) Production of producer and consumer durables exhibits much greater amplitude than does the production of nondurables. (iii) Production and prices of agricultural goods and natural resources have lower than average conformity. (iv) Business profits show high conformity and much greater amplitude than other

[3] Solow (1970), pp. 2-3.

> series. (v) Prices generally are procyclical. (vi) Short-term interest rates are procyclical; long-term rates slightly so. (vii) Monetary aggregates and velocety measures are procyclical.[4]

A business cycle theory may be regarded as a plausible first approximation if it can predict (or retrodict) these facts; otherwise it is implausible. Again, we may have to introduce additional considerations if we find ourself faced with several theories capable of explaining these facts.

A growth theory or a business cycle theory cannot be regarded as veridical, then, merely because it reproduces the appropriate collection of stylized facts. The empirical claims contained in these lists are so general that many theories, many different sets of structural equations, might fit them. But different structural equations will lead to different policy recommendations, and, whenever structure changes, to different predictions. Why not test theories by comparing them with the aggregate empirical data instead of the derived stylized facts? The general answer is that any theory that will reproduce the stylized facts can very likely be fitted to the aggregate data with an R^2 value well over .9. (Nelson & Winter, pp. 218-230, provide an example and a discussion of the problem in the context of growth models. Adelman and Adelman constructed a classical illustration of how a Keynesian macromodel can fit the stylized facts describing the statistical structure of business cycles.) It seems likely also that the details of the empirical data that go beyond the stylized facts in situations like these are also well below the noise level, and contain little information that can be used for model identification or prediction. As Leontief and Orcutt, among others, have pointed out, the idea of validating economic theories with aggregative data is a will-o'-the-wisp that has led econometrics down the garden path of ever more sophisticated statistical methodologies. The ponderousness of contemporary econometric techniques simply crushes the fragile data to which they are applied.

In the physical sciences, when errors of measurement and other noise are found to be of the same order of magnitude as the phenomena under study, the response is not to try to

---

[4] Lucas (1977).

squeeze more information out of the data by statistical means; it is instead to find techniques for observing the phenomena at a higher level of resolution. The corresponding strategy for economics is obvious: to secure new kinds of data at the micro level, data that will provide direct evidence about the behavior of economic agents and the ways in which they go about making their decisions. (Support for this thesis comes from surprising directions. See Lucas, 1980, pp. 288-289.) A major purpose of this paper is to show how and why such empirical data are essential for the future development of economics.

The Formal Mechanisms for Dynamics

The mathematics of differential and difference equations delimits for us the range of mechanisms that are available for representing the dynamics of economic systems. The two most likely candidates are systems of linear equations subject to random shocks, and non-linear systems (with or without shocks) possessing limit cycles.

Linear systems call for exogenous shocks to drive them (e.g., sunspots, oil shocks). The systems themselves need to be stable, so that they will tend to return to equilibrium whenever displaced. The coefficients of all of the adjustment mechanisms are free parameters. They can, of course, be estimated from the empirical time series data themselves, but only with the loss of large numbers of degrees of freedom.

Non-linear systems have the attractive feature that they may behave in cyclical fashion, traversing so-called "limit cycles," even in the absence of exogenous shocks. To explain real data with them, not only their parameters but also the forms of their functions must be estimated — or postulated. Again, these estimates may be made from available time series data at the cost of some degrees of freedom.

In the past forty years, an enormous number of dynamic models of these kinds, small, medium, and large, have been fitted to empirical data. As we are all aware, these model-fitting exercises have not led to much consensus as to what is the right theory. A chief problem is that the parameter values turn out usually not to be very robust. As rational expectationists are fond of pointing out, coefficients estimated from past data often fail badly as predictors of

the future. (Rational expectationists are especially pleased with this non-robustness of the coefficients of non-equilibrium theories because their theory leads them to expect it. The robustness of their own coefficients has not yet been tested extensively.)

The Behavioral Sources of Long-Term Dynamics

We have now set the formal terms of our inquiry. We wish to know what kinds of assumptions will allow us to build dynamic models that possess the characteristics defined by the "stylized facts" we have listed. But we will also be closely concerned with the bases, logical or empirical, for these assumptions. First, we will discuss models of long-term growth, and then turn to models of the business cycle.

I have already indicated in very general terms some ways in which dynamic movement is produced in long-term growth models and in models of the business cycle. For long-term growth in classical theory, three variables are important: one of these (technical innovation) produces a continuing shift in the production function. The other two, population and capital, produce a continuing growth in the factors of production. In most models, the shift in the production function and population are exogenous, while the capital formation is endogenous. A thoroughgoing pursuit of the goals of classical theory would try to define the processes of innovation and population growth as well as capital formation in economic terms — that is, to turn all of them into endogenous variables.

In a formal way, it is perfectly feasible to produce a theory of technical innovation based on the postulate of rationality. Since innovation is presumably produced by the investment of human and capital resources, we introduce a new production function for innovation, and equate the value of the marginal product of innovation with its cost. From a formal standpoint, we have simply replaced the task of estimating an exogenous variable, the rate of innovation, with the task of estimating the parameters of a function, the production function for innovation. The only obvious gain from the replacement is that we can now rest comfortably in the knowledge that everything is proceeding rationally. Human rationality is now only bounded by the characteristics of the external environment: the quality of the ore

that is mined by the innovation production process. Of course, if we examine the metaphor too closely we see that "quality of the ore" is a euphemism for "effectiveness of the thought processes of the human beings who are doing the innovating." We have not removed the constraint from the human brain, just relocated it, so to speak, from one hemisphere to the other.

Something similar can be said about endogenizing the process of population growth. All we need here is something like the marginal utility of a child, and a reasonable respect for the technology of contraception. But Gary Becker and others have already built such a theory for us. By gently manipulating the part of the utility function that contains children, almost any kind of population growth function can be matched. Of course it might take considerable imagination to devise a function that would provide a rational explanation for fluctuations in the American birth rate over the past fifty years. Perhaps we would need, after all, some occasional alteration in the utility function, some changes in taste, to accommodate the data.

The conclusion that I draw is that to build an interesting and useful theory of long-term economic growth, even for developed countries, we have to go behind the principle of rationality. First, with respect to the production function we need an empirically based theory of innovation — a Schumpeterian component.[5] I say "Schumpeterian" because the theory has to explain not merely that most inventors and "risk capitalists" are motivated by the prospect of gain, but also has to specify just what the circumstances are under which those prospects will seem rosy and the circumstances under which they will seem bleak. Surely, a successful theory will have a large historical component — the prospects of gain from investment in genetic engineering are not independent of recollections of gain (one's own or others') from investments in electronics. A phenomenon that depends critically upon the strengths of beliefs in extremely uncertain events, as well as upon acquisition of esoteric and rapidly changing knowledge about new technological developments, must be influenced by psychological mechanisms that go far beyond the principle of rationality.

---

[5] Nelson & Winter (1982); and the papers by Nelson and Winter in this volume.

Second, the rationality principle will also do little to explain rates of population growth unless complemented by a historical and psycho-sociological theory of the determinants of the "utility" of children. If we object that such historicizing, psychologicizing, and sociologizing are not the business of economics, then we must conclude that the objector thinks that long-term growth theory is not the business of economics.

The Business Cycle: Shocks

There is no difficulty in specifying various sources of unpredicted shocks as a means for causing departures of an economy from equilibrium. The weather is a perennial source of such shocks for economies that have an important agricultural component. The Oil Shocks provided spectacular examples of another sort, while wars, revolutions, and even elections supply a continuing sequence of disequilibrating events.

If we were to carry the logic of neoclassical economics to its limit, we would want to predict even these events (omitting the weather) as endogenous products of rational cogitation. However, a more limited goal, and the one usually accepted by economic theory, is to introduce them as unpredictable, hence unexpected, events. Then, in rational expectations theory, they need only be reacted to, not foretold.

Shocks, defined in this way, can make a valuable contribution to the testing of economic theories. For if they are genuinely unpredicted, they can be used after the fact to identify economic models and improve our estimates of their parameters. If there has been any systematic discussion in the economic literature of methods of exploiting this information, it has escaped my attention.

In business cycle theory we are concerned with the ways in which economic agents respond when they are faced with departures from equilibrium, or with expectations of such departures. Rather than discuss the matter in general terms, I think it more useful first to take up some examples: the inventory cycle, responses to price changes, and "permanent" (or is it "natural"?) unemployment.

Inventory Cycles

A businessman may receive signals from the economic system indicating that he is currently supplying more of his product than can be sold in the market at the current price. The usual signals are that inventories of finished product are increasing in stores or warehouses, or, if the product is made to order, that new orders are arriving more slowly than products are being shipped. He may respond to these signals in a variety of ways — even ignoring them at his peril. He may reduce prices; he may reduce the production rate. If he reduces the production rate, he may reduce his labor force, put them on shorter hours, or simply divert some of them to maintenance work of some kind. In addition to price and product adjustments, he may increase (or decrease) his advertising, reorganize his sales efforts and sales organization, modify his product, or promise shorter delivery times. The actions he might take are limited only by his ability to imagine possible decision variables, and the range of values that each might take.

Moreover, there are many signals besides increasing inventories that might trigger the actions. The excess supply may be predicted prior to the actual accumulation of inventories or the depletion of orders, and the prediction, in turn, may be based on many kinds of information, ranging from astrology, through the pages of the *Wall Street Journal*, to the latest turn of the Wharton model.

A business cycle theory must, somehow, capture or sum up these reactions — not for any single businessman, but for the aggregate of enterprises. This is usually done by introducing aggregate equations that are copies, writ large, of what is imagined to be the response process at the micro level. Of course the copy is not supposed to reproduce all the detail and variety of the individual functions, but only their gross, most important, features.

Now since management scientists and operations analysts have been rummaging about in business firms for many years, and since production scheduling has been one of their favorite concerns, we have a great deal of information — although it is rather unsystematic information not easily summarized in statistical distributions — about how inventories and

production rates are actually controlled. I would like to make some generalizations, based on this knowledge, that I think could be verified by systematic sampling. First, production responses are more common than price responses, at least in the early stages of inventory buildup. This is probably not unrelated to the fact that prices are usually constructed by marking up costs. Second, in most business firms the response strategies are not formalized; responses are made "judgmentally" by executives without definite rules to guide them. Third, the decisions about rates of production are often made by different persons, using different procedures, than the decisions about prices. These are "stylized facts" at the micro level, accepted by most persons who have spent much time in business firms.

To avoid dropping into "anything-can-happen" mode, let me describe an actual formal decision rule that has been devised to govern the production-level decision, and which has had at least some modest application to real factory situations: the so-called "linear decision rule" devised by Holt, Modigliani, Muth and myself (1960) (two Keynesians, a rational expectationist, and a behavioralist!). Simply stated, the rule determines the desired level of production for the next scheduling period as a function of current inventories and expected values of sales for future periods. The level selected by the rule maximizes the present value of the expected profit stream for the given sales estimates, production function, and cost of holding inventories. (A generalized form of the rule also makes decisions about the level of the labor force.) The price of the product, and the possibility of changing the price, are entirely ignored in the decision; the problem is to produce at minimum cost the stream of product that will be required to to meet the predicted future sales.

Now I do not wish to claim that the linear decision rule solves the right optimization problem. But in my many conversations about the rule with practicing managers, I have never heard it criticized for its omission of price considerations, or as being over-simple. From this, I am tempted to conclude that the other rules used in industry are probably no more complex, and moreover, that there is probably a considerable variety of them, using different signaling information and different decision variables. Different decision rules and different forecasting

procedures imply different dynamic behavior of the firm, and of the economy when the behavior of many firms is aggregated. Moreover, the rules and forecasts that firms use are subject to learning and fashion. That would be no great matter if the changes in them were random and independent, but they almost certainly are not.

It would not require an unconscionable amount of research effort to obtain, by investigations carried out in individual firms, a fairly good picture of the strategies actually used by firms to modulate their production levels. Such a picture would certainly provide a more satisfactory basis for an aggregate production decision rule than a complete reliance on aggregate time series for selecting relevant variables and estimating parameters. It could also provide some information about the circumstances that might cause firms to change their response strategies, and hence some basis for anticipating structural changes in the aggregative model.

In investigating and describing the production decision rules used by business firms, one aspect of particular interest would be the use or non-use of forecasts of future sales, and the procedures for arriving at those forecasts. A decision rule that simply reacts to current inventory levels is a feedback system that is guaranteed to generate temporally damped cycles, but to amplify them each step backward in the production chain — from ultimate customers to producers of capital goods. Hence, if we found that such passive, reactive feedback systems were common (as I strongly suspect may be the case), we would have one source of explanation for Lucas' first and second stylized facts.

Introduction of sales forecasts into the decision rules could change the dynamics of a system drastically. If sales estimates were based on the extrapolation of recent history, they would not make a fundamental difference to the dynamic behavior of production rates. However, if expectations were formed in other ways (e.g., in whatever ways it is supposed that "rational" expectations are formed), the sales forecasts could be either strongly stabilizing or strongly destabilizing. Possibilities for destabilization would be especially great if many firms drew upon the same sources of information for their forecasts.

Since these conclusions follow from well-known general properties of feedback and feedforward control systems, I will not try to justify them in detail here. I simply wish to point out that we cannot be satisfied that a theory explains the observed facts of the business cycle without knowing a good deal about the precise forecasting methods that it assumes. That brings me to the topic of rational expectations. But instead of discussing it in the context of inventory cycles, I would like to consider how businessmen respond to changes in the price level, and the relation of these responses to all types of business cycles.

Responses to Price Level Changes

An attractive feature of theories that incorporate rational expectations is that they purport to give a general explanation of how businessmen estimate future sales and prices, and hence to foreclose the need for any empirical inquiry into forecasting. We can ask, even of these theories, what information they use to estimate the structural parameters that determine equilibrium, but I wish, instead, to finesse that question and explore in another direction.

If expectations are formed rationally, in the most clear-eyed sense of that phrase, then the corresponding economic systems tend to steer very rapidly toward equilibrium and it becomes very difficult to understand how they can experience consequential cycles of any kind. It is interesting to see how Lucas deals with this difficulty. In brief, he handles it by assuming that forecasters are rational, but not *very* rational. He starts with the proposition that "one would like a theory which accounts for the observed movements in *quantities* (employment, consumption, investment) as an optimizing response to observed movements in *prices*."[6] But "optimizing response" does not rule out delusions or illusions. For the critical assumption in Lucas's model, the assumption that creates the cycle, is that there is "confusion on the part of agents between relative and general price movements."[7] The unmoved mover is not the assumption of rationality, but an auxiliary assumption (unmotivated

---

[6] Lucas (1977), p. 14.

[7] Lucas (1977), p. 22.

by empirical data) of a very particular failure of rationality — a failure of the businessman to interpret correctly the price information that is known to him.[8]

It appears that rational expectations theory, thus emended to account for the business cycle, does not at all relieve us of the burden of empirical inquiry at the microeconomic level. Instead, it instructs us loudly and clearly that to obtain a validated theory of the cycle, we must know not only what price signals the businessman receives and responds to, but also what misapprehensions he suffers under while he is interpeting these signals. Without such empirical inquiry, we are faced with the most casual of empiricisms. Removing this difficulty would seem to call for a rapid convergence of the research program of rational expectations to the program of behavioralism.

The problem to which I have just called attention — the critical role of "irrationalities" in Lucas's business cycle theory — is not peculiar to that theory, or to rational expectationist theories, for that matter. Departures from rationality are endemic in classical and neo-classical economic theories. In earlier days, they were often described as "stickinesses." In partial equilibrium analysis, they take the form of ceteris paribus assumptions. In contemporary dynamic simultaneous-equation models, they most often show up as response rate parameters that determine which of the mechanisms in a system operate rapidly and which operate slowly. Whatever their form, conclusions about dynamic behavior and short-term equilibrium are extremely sensitive to them.

Since this last point is as important as it is obvious, I would like to perseverate upon it for a moment. In our research on near-decomposability, Albert Ando and I gave a formal rationale for partial equilibrium analysis, and for separating the short-term from the long-term dynamics of any system. The idea is that if a system can be "nearly decomposed" into discrete subsystems by ignoring small coefficients, then the short-term dynamics of each subsystem can be determined by examining it independently of the others. This very strong

---

[8]Of course, in neoclassical theory these failures are not treated as irrationalities, but as rational responses to imperfect information. Whatever they are called, they will be understood only if they are subjected to empirical study at the microlevel. They cannot be deduced as consequences of the rationality assumption.

conclusion, conversely, demonstrates the criticality of the choice of "small" coefficients for the inferences one draws about a system's dynamic behavior.

The formal analysis of near-decomposability teaches us that rationality assumptions by themselves cannot provide macroeconomics with a satisfactory microeconomics. In addition, there remains at the very least the empirical task of determining which of the parameters is small enough to be ignored in partial equlibrium or (what is the same thing) short-term analysis. And a behavioralist would argue that the best way to do that is to look at behavior in the small — at the level of the individual firm.

Rational expectations theory moves the locus of parameter estimation, but does not eliminate the task. Moreover, as the particular example of Lucas' business cycle theory shows, it usually requires also its own complement of "stickinesses" to make it fit the facts. The particular stickiness that Lucas proposes for his theory is a form of the money illusion. Let me try to put that illusion (as well as others) in a little broader psychological perspective.

The rationality assumptions of classical theory require implicitly that the economic actor attend to all of the important variables about which he has to make decisions or that can inform him in his decisions. Whether that is easy or hard to do — or even possible — depends on how many relevant variables there are in the world. In any realistic description of the environment of a human decision maker, the variables and information to which he might attend (and to which he must attend to satisfy the strict requirements of rationality) are innumerable. The hypothesis of bounded rationality claims that human beings handle this difficulty by attending to only a small part of the complexity about them. They make a highly simplified model of the world, and they make their decisions in terms of that model and the subset of variables that enter into it.

Now this approach may work very well (and may even give a good approximation to Olympian rationality) if the number of very important variables is small at any given time, *and* if this list of important variables does not change from time to time without the change being noticed. The decision maker of bounded rationality has both the problem of dealing with a

simplified model of the decision situation *and* of noticing when he needs to change his model. Attentional mechanisms are critical to the quality of his rationality.

If the price level is not changing significantly, then one does not suffer from the money illusion if one fails to discriminate between real and money prices. One good way to simplify the world model in a world of stable prices is to ignore the distinction. That is almost certainly what most of us did in our economic decisions twenty years ago. At some point in time, however, we became aware that prices were no longer stable, and we began to take the price level into consideration; we began to index our commitments. The discrepancies between real and monetary quantities became so large that our attention was drawn to them as important enough — among the myriad of things we could consider — for our consideration.

Here we have a major discontinuity in the economic process, a major structural shift taking place from one set of mechanisms to another. The dynamics of the economic system depends on the timing of this shift, which of course will not be made by all persons simultaneously. To handle this discontinuity, and others like it, in our theories, we have to choose among three alternatives. The first is to treat attentional shift as an exogenous random shock. Then we simply give up the attempt to explain or predict it. The second is to apply the familiar marginal arguments: We allocate just as much effort to scanning the horizon for newly important variables as is justified by the marginal value (in terms of improved decision making) of the scanning. This second alternative is vacuous, providing us with no clue as to how these marginal costs and benefits are to be estimated. The third alternative is to study human attention and shifts of attention empirically in order to develop a theory of individual and social determinants of focus of attention. This important task of empirical inquiry has not yet been addressed by the economics profession.

The Debate on Unemployment

As a final example of how auxiliary assumptions of irrationality, or rather, bounded rationality, complement assumptions of perfect rationality in economic theories, both classical and Keynesian, I should like to turn to the much discussed topic of unemployment. The

literature on this topic is so enormous that I don't really need to make any new arguments, but can simply to point to those that have been made and repeated many times. Let me start with the notion, currently popular in some circles, of "natural level of unemployment."[9]

The most familiar of Keynes' local heresies was to introduce high viscosity into the adjustment of the wage level in order to permit the economic system to reside for considerable periods of time in a condition of below-equilibrium employment. The radical (should I call it "ultra-classical"?) alternative to the Keynesian heresy is to insist that labor supply and demand are always in equilibrium, no matter what the official statistics of unemployment say. The level of unemployment is natural — all of those wanting employment at the current wage rate are employed.

The debate about natural levels of employment and unemployment can turn into a very refined philosophical discussion of the meaning of the phrase "wanting employment." Though I haven't seen it done, it might even be transformed into a classical discussion of free will versus determinism. But I think the debate misses the real issue, which has been pointed out by Thurow among others: Observed rates of unemployment fluctuate greatly from year to year. A theory that dismisses these fluctuations as irrelevant throws away an important aspect of the dynamics of the economic system. Why do these fluctuations occur, and what impact, in turn, do they have upon the other variables of the system?

A curious feature of the debate, nearly as true of the Keynesians as of their opponents, is that almost no one (Thurow is again a recent and notable exception) seems to question that there *is* a labor market in which workers offer their services at one or another price and/or employers simultaneously offer employment at one or another wage.

Thurow asks a very refreshing and revealing question: What *in fact* happens if an unemployed person presents himself to a business firm and offers to work (in a job for which

---

[9] If awards were made for outrageous audaciousness in the coining of terms, certainly the author of "natural level of unemployment" would deserve one, as would Jack Muth, the author of "rational expectations." The virtue of such terms is that they win the argument instantly by taking the breath away from would-be disputants, whose very skepticism now accuses them of "unnaturalness" or "irrationality," as the case may be.

he is qualified) at a wage below that being paid to the current occupant of a position? Here again, I would prefer to seek our answer by systematic empirical inquiry than by the methods of casual empiricism. Since I am not familiar with any inquiry into the question, I will hazard a casual answer. The offer would be refused, probably politely. If the jobseeker persisted, on the grounds that he was simply behaving as his economics textbook said he should, he would soon be referred for psychiatric examination.

For whatever good or bad reasons, employers during periods of high unemployment do not usually dismiss their current labor force and hire another at a lower wage. Sometimes, in extremity, they threaten to close down if their employees will not accept wage cuts. But I am reciting (stylized) facts that are familiar to all of us, no matter how indirect our information about labor markets.

Economic theory has been satisfied to substitute a hypothetical "labor market" — whether the sticky one of Keynes or the perfectly adjusting one of the classical theory — for an actual study of the decisional and behavioral processes that lead to employees being dismissed and to job offers being made and being accepted or rejected. In the hypothetical labor market, the actors are, in some definable sense, "rational." But the assumption of their rationality does not provide answers to the important economic questions about the market. The actual level of employment predicted by such theories derives not from the rationality assumption, but from the particular limits that are supposed to operate on that rationality — whether they be stickiness of wages (Keynes) or unexplained shifts in the balance of preference for work and leisure (natural rate of unemployment).

One does not have to be a Marxist to notice an assymetry between the roles of employee and employer in the real labor market. Before there can be an offer or acceptance of employment, there must be a *job* to be filled. And jobs exist in the minds of employers. Employers decide, from time to time, that they want a work force of a certain size and composition. To be sure, these decisions are not made in a vacuum; they are made in the anticipation of profit, and in the light of a host of expectations about prices of output, costs of

production, and the quality of labor that could be hired at various wage rates. However they are made, there is a genuine decisional process going on here, involving all kinds of guesses, hopes, and fears about the future, whose dynamics is central to the dynamics of the whole economic system. Since armchair conjecturing about what this process *must* be has only led to disagreement and confusion in the economics profession, it is probably not too soon to begin investigating empirically what the process actually is.

The picture I have just painted of the labor market is thoroughly Schumpeterian. Schumpeter focused mainly on those employers who were innovatively introducing new products, but the problem is not very different for employers in general. Employers must envision and "create" jobs, before labor markets can operate. And if the number of jobs created is less than the number of workers available at the going wage, the difference between these numbers will show up in the official statistics as unemployment. It is less a matter of explaining how there can be a departure from equilibrium than it is of explaining how a system so constructed manages to keep close to equilibrium (as it does) much of the time.

Again, I must stop short of stating the answer to the question I raise. For my basic point is that, to find the answer, we must study the decision process in much greater depth and with far greater thoroughness than it has ever been studied.

Studying the Bounds on Rationality

A few main themes recur repeatedly in the previous sections. I should now like to sum up the findings of those sections in a new set of "stylized facts" — this time, facts about the status and prospects of economic theory.

1. There is broad agreement, at the level of aggregate statistics, on the principal facts of economic growth in the developed countries, and the facts of business cycles, unemployment, and inflation.

2. It is not likely that important new facts can be obtained by applying sophisticated statistical techniques to aggregate time series. The residual fluctuations in the data are

mostly below the level of random noise.

3. The picture revealed by the agreed-upon facts about growth and cycles does not have high enough resolution to choose between major alternative economic theories. But different theories, all consistent with the aggregate data, lead to different policy prescriptions and to different predictions whenever structure changes.

4. The stylized facts of economic growth and business cycles cannot be derived from the rationality postulates of classical and neoclassical theory without introducing essential qualifications of those postulates. These qualifications take the form of assumptions of bounded rationality (e.g., the money illusion), or assumptions of exogenous shocks (e.g., unmotivated shifts in work-leisure preferences). Both neo-classical and Keynesian writings are mottled with such auxiliary assumptions, and their conclusions would not follow if the assumptions were removed. In fact, with removal of the auxiliary assumptions, which have mainly to do with the ways in which expectations are formed about uncertain future events, the neo-classical and Keynesian theories would become indistinguishable.

5. The auxiliary assumptions one finds in neo-classical and Keynesian analyses have no secure empirical base. They generally derive from the most casual of casual empiricisms. They are not adequately supported by aggregate data for the reasons already given. They are not adequately supported by micro-level data (data derived from actual studies of the decision-making processes of consumers and in business firms), because such data have not been gathered systematically for most of the phenomena under study.

A rather obvious conclusion follows from these facts: that the continuing progress of the economic theory of change and cycles requires massive empirical work at the level of the economic agents who make decisions — for example, the consumer and the firm. The dynamics of the economic system depend critically on just how economic agents go about making their decisions, and no way has been found for discovering how they do this that avoids direct inquiry into and observation of the processes.

This thesis, sometimes called Behavioralism, has been opposed on a number of

grounds. First, it has been opposed as an abandonment of the principle of rationality. I have tried to show here that the principle of rationality has already been abandoned, and abandoned in fundamental ways, by classicists, neo-classicists, Keynesians, and rational expectationists alike. No new abandonment is proposed; just a major effort to discover which modes of abandonment are empirically defensible.

A second objection (e.g., Blaug) is that Behavioralism has not yet produced a complete theory that can compete with the classical one. It is not a theory, but a set of hopes and promises of a future theory. Blaug argues that you cannot beat something with nothing; there is no reason to abandon the neoclassical theory until a new theory is in place. This is a Catch 22 argument: behavioralism can't be developed without being pursued empirically; but there is no reason for pursuing it since it does not offer, here and now, an alternative to classical theory.

Behavioralism does not hold forward the hope of a theory that can be arrived at in an armchair. The new theory can only be built through extensive empirical research — on the scale, let us say, of the empirical research of the last century or two in field biology or geology. Moreover, the required research is of a kind that is novel to most economists. It does not rest on statistical data derived from secondary sources, but calls for direct observation of human behavior in the market and in the firm. The reason for undertaking that research is that the classical program has failed: it patches the rationality principle with ad hoc assumptions of bounded rationality, but it has no way to choose among alternative patches without obtaining new data. And macroeconomic studies are not a viable source for such data.

Empirical research on human decision making at the microeconomic level need not be, and should not be, a blind accumulation of facts. Priorities among the areas of decision to be investigated can be determined by the needs of macroeconomics. But there is an even more powerful source of heuristics for guiding the research. During the past twenty five years there has been developed in cognitive psychology a theory of the information processes that

account for human problem solving and decision making for a wide variety of tasks.[10] The theory has by now reached the point where it has a good deal to say about complex, professional level human cognitive activities, like making medical decisions, using financial statements to identify and analyze corporate problems, or making investment decisions. In parallel with the theory, a methodology has been developed for using verbal thinking-aloud protocols and other verbal responses as empirical data for testing hypotheses about information processes, and a powerful technology for modeling the processes as computer programs and simulating their behavior. The substantive and methodological content of this work in cognitive psychology provide a theoretical framework for empirical work in microeconomics that was not available a decade or two ago. Lack of theoretical or methodological guidlines is no longer a credible reason for delaying the construction of an empirically based microeconomics.

Requirements for a Schumpeterian Dynamics

Let me end on a more substantive note, deriving from the earlier discussion of the labor market. In the language of classical economics there is little room for a fundamental asymmetry between the two parties to a bargain. There is no room for a concept like "initiative." In a Walrasian tatonnement it is irrelevant who makes the first bid. Similarly, there is no distinction in classical economics between action and inaction. Each time period, the actors must choose some value of each decision value, and there is no special value called "inaction." An investment level of zero is not qualitatively different in the theory from a level of 100.

In the everyday world, matters are quite different. We do distinguish certain states as states of inaction. In a baseball game, 0-0 is a possible score, but there is a difference between a tie game and a day on which no game was played. Inaction is the state that prevails when no positive action is being undertaken, and it is qualitatively different from action. Action has to be motivated. The actor must attend to some stimulus, external or

[10] See, for example, Newell and Simon (1972).

internal, that leads him to consider action relevant to it. In the absence of motivation, inaction prevails.

Applying these ideas to economic affairs, we arrive at a picture of them that looks very different from Walrasian equilibrium. In the Walrasian picture, there is at all times a fixed set of markets, each with its supply function and demand function and a price fluctuating around the equilibrium value. Markets are neither created nor destroyed. In an economy where actions must be positively motivated (let me call it a Schumpeterian economy), commodities are produced only after someone is motivated to consider producing them. Investments are made only when someone is motivated to pay attention to a potential investment opportunity and decides to invest. Job slots are created only when employers attend to the need for more workers and decide to try to employ them.

In a Schumpeterian world, human attention is a central endogenous variable, whose determination is a key to the direction and level of activity of the economic system, both in the long run and in the short. Attention, in turn, is a function of the information and communication flows in the society. There are habits of attention, a potent source of inertia and stability. There are also trends and fads in attention — societies as well as individuals may be said to have changing foci of attention.

There is a good deal of room for traditional economic variables in such a world. Economic actors may attend to prices, and they may become aware of, and be motivated to anticipate, the possibilities for price changes. One might even say that a Schumpeterian model is simply a next step in the same direction as that in which economic theories have been moving for forty years. During that time we shifted from an almost exclusive preoccupation with statics to a full concern with dynamics. We introduced expectations into the dynamic model, first describing those expectations as adaptations to the memory of the recent past. Then we (or the rational expectationists among us) took the leap of making expectations depend on an understanding of the economic system itself.

A very natural next step for economics is to maintain expectations in the strategic

position they have come to occupy, but to build an empirically validated theory of how attention is in fact directed within a social system, and how expectations are, in fact, formed. Taking that next step requires that empirical work in economics take a new direction, the direction of micro-level investigation proposed by Behavioralism.

Bibliography

Adelman, I. and Adelman, F.L., 1959, The dynamic properties of the Klein-Goldberger Model, Econometrica 27, 596-625.

Becker, G.S., 1981, A treatise on the family (Harvard University Press, Cambridge, MA).

Blaug, M., 1980, The methodology of economics (Cambridge University Press, Cambridge).

Hicks, J.R., 1950, The trade cycle (The Clarendon Press, Oxford).

Holt, C.C., Modigliani, F., Muth, J.F. and Simon, H.A., 1960, Planning production, inventories, and work force (Prentice-Hall, Englewood Cliffs, NJ).

Keynes, J.M., 1936, The general theory of employment, interest, and money (Macmillan, London).

Leontief, W., 1971, Theoretical assumptions and nonobserved facts, American Economic Review 61, 1-7.

Lucas, R.E., Jr., 1977, Understanding business cycles, in: K. Brunner and A. Meltzer, eds., Stabilization of the domestic and international economy (North-Holland, Amsterdam).

Lucas, R.E., Jr., 1980, Methods and problems in business cycle theory, in: Studies in business-cycle theory (The MIT Press, Cambridge, MA).

Muth, J.F., 1961, Rational expectations and the theory of price movements, Econometrica 29, 315-335.

Nelson, R.R. and Winter, S.G., 1982, An evolutionary theory of economic change (Harvard University Press, Cambridge, MA).

Newell, A. and Simon, H.A., 1972, Human problem solving (Prentice-Hall, Englewood Cliffs, NJ).

Samuelson, P.A., 1948, Foundations of economic analysis (Harvard University Press, Cambridge, MA).

Schumpeter, J.A., 1934, The theory of economic development (Harvard University Press, Cambridge, MA).

Simon, H.A. and Ando, A., 1961, Aggregation of variables in dynamic systems, Econometrica 29, 111-138.

Solow, R.M., 1970, Growth theory (Oxford University Press, Oxford).

Thurow, L.C., 1983, Dangerous currents: The state of economics (Random House, New York).

Turnovsky, S.J., 1977, Macroeconomic analysis and stabilization policies (Cambridge University Press, Cambridge).

WORKING PAPERS (Missing numbers indicate publication elsewhere)

1976

1. Corporate and Personal Taxation and the Growing Firm
by Ulf Jakobsson

8. Estimation and Analysis with a WDI Production Function
by Göran Eriksson, Ulf Jakobsson and Leif Jansson

1977

15. Pay Differentials between Government and Private Sector Employees in Sweden
by Siv Gustafsson

1980

25. On Unexplained Price Differences
by Bo Axell

34. Imperfect Information Equilibrium, Existence, Configuration and Stability
by Bo Axell

1981

36. Energi, stabilitet och tillväxt i svensk ekonomi (Energy, Stability and Growth in the Swedish Economy)
by Bengt-Christer Ysander

38. Utiliy in Local Government Budgeting
by Bengt-Christer Ysander

40. Wage Earners Funds and Rational Expectations
by Bo Axell

42. The Structure of the ISAC Model
by Leif Jansson, Tomas Nordström and Bengt-Christer Ysander

43. An Econometric Model of Local Government and Budgeting
by Bengt-Christer Ysander

44. Local Authorities, Economic Stability and the Efficiency of Fiscal Policy
by Tomas Nordström and Bengt-Christer Ysander

45. Growth, Exit and Entry of Firms
by Göran Eriksson

52. Swedish Export Performance 1963-1979. A Constant Market Shares Analysis
by Eva Christina Horwitz

56. Central Control of the Local Government Sector in Sweden
by Richard Murray

59. Longitudinal Lessons from the Panel Study of Income Dynamics
by Greg J. Duncan and James N. Morgan

1982

61. Var står den nationalekonomiska centralteorin idag?
av Bo Axell

63. General Search Market Equilibrium
by James W. Albrecht and Bo Axell
General Equilibrium without an Auctioneer
by James W. Albrecht, Bo Axell and Harald Lang

64. The Structure and Working of the ISAC Model
by Leif Jansson, Thomas Nordström and Bengt-Christer Ysander

65. Comparative Advantage and Development Policy Twenty Years Later
by Anne O. Krueger

67. Computable Multi-Country Models of Production and Trade
by James M. Henderson

69. Relative Competitiveness of Foreign Subsidiary Operations of a Multinational Company 1962-77
by Anders Grufman

71. Technology, Pricing and Investment in Telecommunications
by Tomas Pousette

72. The Micro Initialization of MOSES
by James W Albrecht and Thomas Lindberg

75. The MOSES Manual
by Fredrik Bergholm

76. Differential Patterns of Unemployment in Sweden
by Linda Leighton and Siv Gustafsson

77. Household Market and a Nonmarket Activities (HUS) - A Pilot Study
by Anders Klevmarken

1983

79. Energy Prices, Industrial Structure and Choice of Technology; An International Comparison with Special Emphasis on the Cement Industry
by Bo Carlsson

81. ELIAS - A Model of Multisectoral Economic Growth in a Small Open Economy
by Lars Bergman

84. Monopoly and Allocative Efficiency with Stochastic Demand
by Tomas Pousette

86. The Micro (Firm) Foundations of Industrial Policy
by Gunnar Eliasson

87. Excessive Government Spending in the U.S.: Facts and Theories
by Edward M. Gramlich

88. Control of Local Authority Expenditure - The Use of Cash Limits
by Noel Hepworth

89. Fiscal Containment and Local Government Finance in the U.K.
by Peter Jackson

90. Fiscal Limitations: An Assessment of the U.S. Experience
by Wallace E. Oates

91. Pricing and Privatization of Public Services
by George E. Peterson

94. Job Mobility and Wage Growth:
A Study of Selection Rules and Rewards
by Bertil Holmlund

96. The Machine Tool Industry - Problems and Prospects in an International Perspective
by Bo Carlsson

97. The Development and Use of Machine Tools in Historical Perspective
by Bo Carlsson

99. An Equilibrium Model of Search Unemployment
by James W. Albrecht and Bo Axell

100. Quit Behavior under Imperfect Information: Searching, Moving, Learning
by Bertil Holmlund and Harald Lang

102. Collecting Data for Micro Analysis.
Experiences from the Hus-Pilot Study
by N. Anders Klevmarken

103. The Swedish Micro-to-Macro Model
- Idea, Design and Application
by Gunnar Eliasson

105. Estimation of Wage Gains and Welfare Gains from
Self-Selection Models
by Anders Björklund and Robert Moffitt

106. Public Policy Evaluation in Sweden
by Bengt-Christer Ysander

108. Entry, Industry Growth and the Microdynamics
of Industry Supply
by John C. Hause and Gunnar Du Rietz

109. Capitalist Organization and Nationalistic Response;
Social Dynamics in Age of Schumpeter
by William Parker

110. A Nonwalrasian Model of the Business Cycle
by J.-P. Benassy

111. Disequilibrium Economic Dynamics -
A Post-Schumpeterian Contribution
by Richard Day

112. Schumpeterian Competition in Alternative Technological
Regimes
by Sidney Winter

113. The Industrial Finance Systems; Europe, U.S. and Japan
by Tad Rybczynski

114. The Microeconomics of Organization and Productivity
Change - The Use of Machine Tools in Manufacturing
by Bo Carlsson

115. On the Behavioral and Rational Foundations of Economic
Theory
by Herbert Simon

116. Schumpeterian Dynamics
by Erik Dahmén

117. Micro Heterogeneity of Firms and the Stability of Industrial
Growth
by Gunnar Eliasson

118. The Initialization Process - The Moses Manual, Part 2
by Fredrik Bergholm