THE MECHANISTIC FOUNDATIONS OF ECONOMIC ANALYSIS*

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DEBATES over methodology mark all sciences. Sometimes they are productive, more often merely pontifical. Disputants point accusatory fingers, argue past each other, and employ chopped logic in defense of their pet paradigm. Such contributes to the bad name and general neglect of epistemology among economists. Yet no economist worthy of the name can have failed to have dipped into the literature on the method of the social sciences and to have expressed concern over the state of the discussion. No discipline can remain coherent and advance until it has its basic methodology straight.

Prevailing orthodoxy about economics as a social science runs something like this. Economics is one of the more successful of the social sciences when judged by the canons of contemporary science. Economists share a reasonably well-defined method and body of theory. The discipline values objectivity and eschews ideology. Its analysis aims at explanation and prediction and gives rise to testable hypotheses. Deductive reasoning, model building, and empirical testing are the hallmarks of economics. Such is a bald sketch of the neoclassical paradigm as initially codified in the '30s by Lionel Robbins and T. W. Hutchison and, more recently, as restated in a popular and influential essay by Milton Friedman.[1]

Ultimately, of course, the success of a science can only be gauged by the relative accuracy of its predictions, and on this criterion economics has enjoyed a measure of success. Economics is not only successful, in this sense; it is imperialistic. Economists are everywhere plying their trade in adjacent and far vineyards. They have made major contributions in the allied fields of applied mathematics and statistics. They are bringing some semblance of order to such diverse areas as political science and criminology. They are rewriting history and redesigning educational systems. They are bumping up against psychology, law, and history. Even anthropology is not the unknown territory it once was. Indeed,

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it is this far-flung activity, this ferreting out of economic behavior in all manner of nook and cranny, that has given a new meaning to the phrase "economic imperialism." Economists seem bound to search for new problems and data sets as grist for their theory.

Throughout its ascension to present-day orthodoxy, the neoclassical paradigm has been subjected to a variety of methodological criticisms. Without the pretense of a thorough review, which would needlessly sidetrack the discussion, the substantive attack on the positivist foundation of the paradigm turns on four issues. (1) Can a philosophically satisfying distinction be made between normative and positive questions in economics? (2) Is economics an empirical science? (3) Can meaningful macroeconomic theorems be derived employing holistic constructs, or must economic theory be rooted in methodological individualism? (4) Does the neoclassical paradigm have a way of limiting the kind of questions raised within the discipline and of preshaping the analytic response to those that are raised?

Whether an affirmative answer to the first question is justified, the fact is that most economists, even those who differ sharply on other methodological issues, believe that the normative/positive distinction is useful.[2] As to the accusation, made by "radical economists," that neoclassical economics represents elaborate apologetics for private property and a capitalist economic order, we observe that "it takes a theory to beat a theory." The polylogism of the Marxist and self-styled radical economists is itself a philosophically suspect position. The burden remains on the radicals to develop a consistent theory of superior explanatory value to neoclassical theory.

Questions (2) and (3) have been raised and pursued most rigorously by the Austrian school of economics, notably by Ludwig von Mises and F. A. Hayek.[3] Mises argues that economics is a purely a priori science. Its theorems, like those in mathematics, are logically deduced from a few fundamental axioms.

> First among them is the axiom of purposeful human action. Supplementary axioms are that human beings are diverse in tastes and ability, that all action takes place through time, and that people learn from experience.[4]

Since the axioms are self-evidently true, barring errors in logic, theorems derived from the axioms are true. There is no need to subject them to "tests" of empirical falsification. Moreover, such statistical tests are impossible: one, because purposive action (as opposed to an event) contains a counterfactual element that is in

principle unobservable and, two, because there are no constants in economic relations amenable to specification by econometric techniques. Statistical studies, however useful, represent history, not economics, according to Mises. Another implication of the Austrian method is that so-called macro theory that does not trace its derivation back to the purposive actions of individuals is unacceptable.[5] Statistical regularities among macro aggregates do not and cannot reveal causal relationships.

In spite of the growing interest in Austrian economics, particularly among a number of talented young scholars, these methodological views have not as yet had a significant impact on the discipline. The vast majority of economists continue to reject what T. W. Hutchison refers to as "the dogmatic and extreme apriorism of Professor Mises."[6]

Proponents of neoclassicism have answered the first three questions,[7] at least to their own satisfaction; their defense has left the orthodox view largely intact. Economics is seen as a coherent discipline with a systematic methodology and theoretical foundation. It has squarely addressed the issues raised above and has enjoyed a measure of success as an explanatory social science. As any science, economics is progressive in the sense that it builds on, refines, and discards earlier work in cumulative fashion. That fundamental questions remain need hardly be denied. But if this is a pretty, even flattering, picture, it is neither a particularly praiseworthy one nor an occasion for a moment of incestuous backslapping—for the success that economics presently enjoys may in large part stem from *the nature of the questions it addresses.* Thus the significance of question (4).

To borrow a phrase from Robert Solow, "Economists are determined little thinkers." Their method is to reduce, simplify, and isolate. They have become adroit at framing and answering relatively simple questions. At this point, there is no need to confuse the sophistication of technique with the profundity of the questions addressed. Having adopted a modest agenda, economic theorists are seemingly content to rediscover the downward sloping demand curve, to transform economics into an internally consistent set of formal propositions of logic, and to reanswer the narrow range of questions that fall within the confines of the traditional paradigm. The very success of the paradigm serves to reinforce this limitation on the range of questions addressed and to lead economists to cast a jaundiced eye at alternative paradigms.

It is no accident that economists are increasingly turning to allied vineyards for problems to solve. Economists have never

been bashful about stepping over the artificial demarcation between disciplines; they have their share of intellectual imagination, and they do possess a powerful paradigm for discovering patterns of rational behavior. But there may be another reason behind these forays abroad: diminishing returns at home. At the margin, it may be easier to take our well-oiled set of tools abroad to work virgin territory than to stay at home and think about the necessity or means of redesigning (not merely refining) our tools. The basic theory of consumer behavior is employed to aid our understanding of political and criminal behavior; the theory of the firm becomes a tool for understanding the inner workings of the church, the government bureau, and the private club. The results are at least interesting and often provocative. The continued success of the traditional paradigm on home turf and in adjacent vineyards, plus the heady challenge of reducing that paradigm to an axiomatic system, has led economists to do less hard thinking about the fundamental nature of economic phenomena. In short, economists have not squarely addressed the complexity of economic processes and systems.

Modern welfare analysis, which at least raises some of these fundamental questions, is sterile as an informing theory of policy. Modern macro analysis is rife with ad hoc theorizing and appears very much to be stalled along the way. Price theory, for the sake of internal consistency and mathematical elegance, has been shorn of a meaningful analysis of the competitive process. Indeed, the summary statement of *conditions prevailing in market equilibrium* has been elevated to the status of normative standard by which many forms of competitive behavior, in a world of uncertainty and incomplete information, are condemned. The challenges of addressing the questions raised by Schumpeterian dynamics, of developing a theory of income distribution, and of developing a theory of long-run evolution of economic systems remain unheeded.

All the above highlights the primacy of question (4)—that of understanding the analytical predisposition of received theory—in marshalling a methodological assault on the neoclassical paradigm. No scholar has contributed more to our understanding of this issue than has Hayek. He has argued persuasively that economics must not merely emulate the natural sciences; that though economics is scientific in the same sense as are the natural sciences, it must develop its own distinctive methodology appropriate for the analysis of social phenomena. Hayek's contributions in this regard have been to clarify the distinction between the "facts" presented by natural and social phenomena, to develop

a theory of economic processes grounded on purposive behavior, and to demonstrate that socially useful institutions and patterns of social order are rarely products of deliberate design but more often evolve out of the voluntary interactions of individuals. These ideas do not fit comfortably within the traditional paradigm, and students of methodology owe an intellectual debt to Hayek's seminal work in the area.[8]

Though Hayekian in spirit, this essay is not a review of Hayek's ideas about the proper methodology of the social sciences. It is at one and the same time less and more ambitious—less ambitious in that it does not outline an alternative paradigm, as Hayek has attempted; more ambitious because it seeks to trace more explicitly the *conceptual origins* and concomitant limitations of the neoclassical paradigm. In particular, it seeks to demonstrate that the received paradigm is a mechanical analogue borrowed virtually intact from classical physics. In the next section the basic preconceptions of mechanics and their adoption in economics are reviewed, and this is followed by a critique of their applicability to economics.

The View from Physics

The weltanschauung of economics owes a major intellectual debt to classical physics. The economists' notions and ways of thinking about equilibrium, market forces, change, friction, and inertia are borrowed from mechanics, as is the distinction between statics and dynamics. More fundamental are the concepts of an ordered universe, the desirability and possibility of a unified theory, the distinction between positive and normative analysis, and the absolute nature of time and space. (The latter are so basic and raise so many questions that they are precluded from the discussion that follows. It is ironic that just as economics was embracing the weltanschauung of nineteenth-century physics, a revolution within physics was raising serious objections to each of these fundamental concepts.) Such economists as Mill, Jevons, Fisher, and, down to the present day, Knight and Samuelson, have expressed admiration for the methods of physics.[9] What follows is a brief review of some of its basic methodological preconceptions.[10]

Reduction

Physical phenomena are analytically broken down, reduced to their constitutive building blocks. A complex machine is interpreted as a collection of simple mechanical devices such as inclines, levers, and pulleys; forces are broken down into vectors; substances are reduced to molecules and, further, into atoms. The

relationships between these units are analyzed in terms of cause and effect, and the properties of the whole phenomenon are reconstructed from the properties of its constitutive units. An appreciation of the whole flows from a study of its parts. Because mechanical processes are independent in isolation, and hence additive, the method of reduction is admirably well suited to physics.

Economics too has its units of analysis. These are stylized consumers and firms, the atoms of economics. Meaningful statements about economic phenomena are derived from assumptions about the behavior of these decision-making units. Concern about these units carries over into aggregate economic analysis in the form of aggregation theory. (Of course, the influence of physics on this score is not confined to economics. The reductionist tradition is found in cellular and molecular biology, analytical chemistry, and the stimulus-response model of psychology.)

Consider the way a physicist would set up a simple trajectory problem. He is given the muzzle velocity and weight of the projectile, the height and angle of elevation of the rifle above the horizontal; he assumes no wind and no air friction; and he employs vector analysis to reduce all forces on the projectile to their simple vertical and horizontal dimensions. Such an approach allows our physicist to predict the distance from the muzzle where the projectile will strike the ground, its maximum apogee, and its velocity at 200 yards.

Compare the above with the way an economist might address a question concerning the consequences of an increase in the price of gasoline. He is given the present price and quantity consumed per period; he assumes consumer preferences remain unchanged and that money income is constant; and his vector analysis allows him to distinguish between the income and substitution effects of the price increase. The economist is now in a position to predict the new rate of consumption of gasoline, for example. This is the method of reduction.

Reversibility

In any strictly mechanical process the course of events is in principle reversible. Mechanical processes are therefore ahistorical. The notion of reversibility most often appears in economic thinking in one of two instances. The first is the argument that we can reverse an undesirable economic development. For example, should a competitive market structure evolve into an oligopolistic organization, it is believed that divestiture or dissolution can restore competition—and old marketing patterns, product lines,

technologies, and distribution networks. The second example is the romantic and currently popular notion that, if we only wanted to, we could return to a more simple life. Having accumulated much wealth and having achieved a high material standard of living, we could opt for less by retreating to a more primitive economic organization based on a greater degree of self-sufficiency and less energy-intensive means of production. Each of us has flirted with this private dream, but its current status among economists (particularly those arguing for zero economic growth) can only be explained by the mechanical paradigm underlying contemporary economic theory. The argument that "we can go back" is logically unassailable once you accept the premises of the traditional paradigm.

Isolation

Physical phenomena are analyzed in isolation from their surrounding environment. Strictly speaking, the laws of classical mechanics and thermodynamics are valid only for closed systems. Physics has more to say about a projectile falling in a vacuum cylinder than about a leaf blown from an oak tree. This method of abstraction is not a significant limitation, however, because physics is an experimental science capable of constructing closed systems within which physical phenomena can be analyzed. Where physics is not experimental, it takes the universe as a laboratory and deals with astronomical distances and ultrahigh velocities. These conditions approximate those of a closed system.

Of course, systematic abstraction is one of the most important vehicles of advance for any science. Although the principle of isolation, useful in experimental sciences, is not a readily applicable guideline for abstraction in economics, economics developed its own methods of isolation for simplifying the complex systems it seeks to analyze. Indeed, the very concept of economic activity as something apart from other social activities is an abstraction. The methods of abstraction, within the subject area, generally take one of three forms: (1) simplification of the relationships among elements of large complex systems, e.g., assuming fixed coefficients in an input-output matrix; (2) aggregation of a large number of relationships into a much smaller number, e.g., the consumption behavior of a large number of individuals is collapsed in an aggregate consumption function, or the investment decisions on the part of firms become an investment function; and, most important, (3) employment of the "method of Marshall"—*ceteris paribus*: factors judged to be at the periphery of the

economic process under study are assumed constant and variables are taken as datum; with "all other things constant," the effect of a given change (cause) can be deduced—the process is isolated by assumption.

Equilibrium

For our purposes, the final and probably the most important methodological characteristic of physics is to frame questions in terms of equilibrium. Physical processes are seen as unfolding until a balance of forces is struck, i.e., an equilibrium is reached. In a closed system the time path of adjustment and the equilibrium position are unequivocably determined by the initial conditions. The system may be at rest or moving at constant velocity, depending on whether the system is static or dynamic. Equilibrium is identified as the solution of the system. As Robert Kuenne maintains, "One of the most fruitful of the many economic adoptions from the field of mechanics is the concept of economic equilibrium, or a specific solution characterized by a state of balance between opposed forces acting upon economic variables."[11]

The concept of equilibrium is central to economic analysis. Economics is conventionally, and usefully, defined as the science of scarcity, by which is meant that human wants are unbounded while the resources necessary to satisfy those wants are finite. That the implied theory of choice should take the form of a constrained maximization problem is the natural outcome of looking toward classical mechanics for a basic paradigm. As the physicist Henri Poincare has observed, "Any system that involves a conservation principle [given means] and a maximization principle [optimal satisfaction] is a mechanical analogue."[12]

The kernel of most economic models thus becomes an equilibrium condition or set of conditions. These take two forms. The first involves an explicit maximization postulate: idealized consumers are assumed to maximize utility, and firms are assumed to maximize profits or net wealth. The equilibrium condition is then stated in terms of the equalization of the marginal rate of substitution of good X for Y with the ratio of their relative prices or the equalization of the marginal rate of technical substitution of factor A for B with the ratio of their relative wages. In the second form, the equilibrium condition is stated as a definition. Such conditions are typical of macro models; e.g., $Y = C + I + G$. Some models, aggregate growth models, for example, employ both forms of equilibrium condition.

For our purposes, those are the major elements in the method of classical physics—a paradigm that has been very instrumental in

the development of economic theory. Without belittling the success with which economists have applied it to economic problems, we can, however, raise questions about its limitations. What sacrifices are made to obtain such analytical rigor?

A Critique

It is not clear, on the face of it, how analytical dissection à la classical mechanics is appropriate for coping with economic systems. For the latter involve human actions and are inherently complex.

Reversibility

The first hitch is with the notion of reversibility. Mechanical processes are reversible—ahistorical, nonevolutionary, or in Samuelson's terms, "dynamical and causal." That is a very strong property. It means not only that the valid laws of mechanics are unchanging with respect to time but that a mechanical process, as it unfolds, is qualitatively unchanged. After it has run its course and reached equilibrium, it can be reversed until the initial conditions have been reestablished without qualitative change. But surely the hallmark of an economic process is adaptive, purposive behavior; and that of an economic system, evolutionary change. As Mises and then Hayek have so cogently argued, new knowledge generated as an economic process unfolds implies that initial conditions can never be restored even if an elaborate effort is made to replicate the initial incentive structure (set of relative prices, real incomes, etc.). People learn from experience and act upon that new knowledge. And that is only one of many factors barring reversibility in social phenomena. A mere mechanical analogue will not suffice, for a valid economic theory must be able to explain qualitative change. In this respect, we have not fully recognized the significance of Marshall's observations about the evolution of the firm or the irreversibility of long-run supply.

Equilibrium

Economic systems are almost always conceived of in terms of equilibrium processes, and this is a second problem. The juxtaposition of supply and demand forces determines equilibrium price and quantity in the market; monetary and fiscal policy tools are instruments for changing the equilibrium level of GNP; or further investment in education is predicted to increase the equilibrium growth rate of national income. The image of the given system moving toward equilibrium, whether static or dynamic, is pervasive

in the technical literature. As Professor Chipman has observed, "Equilibrium—meaning the balance of opposing forces—is a concept as fundamental in economics as it is in physics."[13]

There are two broad reasons for its importance in economics. First, all comparative statements in economics turn on an evaluation of differing equilibria. If a set A, where $[C_1 \ldots Cn \in A]$, leads to equilibrium E_1, and set B, where $[C_1 \ldots C_n \in B]$ and $[C_2 \notin B]$, leads to E_2, then any statement about the importance or desirability of C_2 involves a comparison of E_1 and E_2. If C_2 represents a proposed gasoline tax and the policy objective is to raise the price of gasoline, the merits of C_2 will be evaluated in terms of the predicted before and after equilibrium price of gasoline. This is a favorite kind of question for economists, for in this case C_2 represents a reasonably well-defined once-and-for-all exogenous change. The second reason is that equilibrium is a powerful image for organizing an analytical assault on complex systems. It gives the economist something to hang a solution on and, indeed, suggests a whole kit of mathematical tools for deriving that solution.

But the economist should not become too comfortable in conceiving of equilibrium in just the way physicists do. In the rigid deterministic world of nineteenth-century physics, the climax state and adjustment path of an isolated mechanical process are uniquely determined by the initial conditions. The process is reduced to a cause-and-effect relationship. A precisely defined set of conditions C′ invariably produces equilibrium E′, a process that can be replicated by independent observers. Reversibility guarantees that $[E' \rightarrow C']$ also. Change either the initial conditions or the process, and a different end state will result.

The statement $[C' \leftrightarrow E']$ needs to be modified in two ways, one trivial and the other basic. There may exist a set of simple transformations of C′ that also lead to E′; e.g., all distances and velocities are changed equiproportionately. This is loosely equivalent to a common assertion in monetary theory that, *ceteris paribus*, an equiproportionate change in the money supply and all money wages and prices will leave the long-run equilibrium unaffected. In other words, it is no longer strictly true that $[E' \rightarrow C']$ (although, at this point, the converse $[C' \rightarrow E']$ remains true).

It may seem tempting to search out other examples of this in economics where it is conventionally thought (and certainly taught) that the same result (equilibrium) can often be obtained by alternative means. A common chalkboard demonstration of multiplier analysis shows how full-employment equilibrium GNP

can be reached with expansionary monetary policy, tax cuts, or increases in government spending—or, in an appeal for eclecticism at the end of the lecture, a combination of all three. Assume the demonstration is correct. At this point the mechanical analogue becomes problematic, because the alternative policies are emphatically not simple transformations of each other. And if they are defined to be—e.g., the money multiplier is 2.3 times as large as the government-expenditure multiplier—the question is begged. There is a dimension to economic processes that is wholly absent in mechanical ones—purposive, adaptive, goal-seeking behavior—and herein lies an explanation of how different sets of economic conditions can lead to the same long-run equilibrium.

There is a further problem here. Returning to the chalkboard, the demonstration does not imply that all else—income distribution or rates of output of various industries in the economy, for example—remains invariant under the different policy measures. One man's equilibrium is another man's structural change. Nor is the problem resolved by appealing to the distinction between general and partial equilibrium, for it is not a question about the equilibrium of a subsystem versus the equilibrium of the economy as a whole, but about the attributes covered by the definition of the economy's equilibrium.

The second modification of the statement [$C' \leftrightarrow E'$] is more substantive. Modern statistical mechanics recognizes that C' does not determine a unique E' but a probability distribution of outcomes [$E_1 \ldots E_n$]. Physics is thus reduced to predicting the most likely course of events given a set of initial conditions. This is so because at the atomic level, physics has no explanation of the movement and position of individual particles. It could not predict, for example, when an individual gas molecule would strike the wall of its container, although, given a large number of molecules, it might predict the resultant gas pressure of an average number bounding against the wall of the container. It might also predict how this average and, hence the pressure, varied with temperature. Similarly, the economist cannot identify the next customer to purchase a can of tomatoes in a supermarket, although he might have something to say about the average number sold during the course of a week and how that number varies with price. The calculation of these averages is, of course, a statistical problem. The recognition that mechanical processes have stochastic elements and that uncertainty marks both physical and economic processes makes the analogue between mechanical and economic systems more appealing, but it does not save it entirely.

A stochastic shock to a mechanical process, however small or unlikely, will change the final equilibrium, however slight or infrequent. But in an economic system a stochastic shock might have no influence at all on the final equilibrium because of adaptive behavior. The purposive behavior of individuals implies that economic processes can exhibit self-correcting tendencies never demonstrated by purely mechanical processes. Suppose a number of gas molecules, for some unexplained reason, did not bombard the wall of the container on schedule; the equilibrium pressure would fall. Now suppose a number of customers suddenly decided not to buy tomatoes this week; the resulting surplus of tomatoes might induce the grocer to cut his price, thereby attracting additional new customers or heavier purchases by regular ones. The change in behavior of the first group, in a sense, prompted the compensating actions of the second group. The equilibrium quantity of tomatoes sold per period remained unchanged. Such compensating behavior can never arise within a mechanical system. The point is this: a public-policy question (say the advisability of a tax hike) cannot be decided by focusing on the proximate policy goal (lowering the equilibrium rate of inflation) without reference to the policy's influence on the income distribution, composition of industrial output, future income growth, and the like. Thus, substantive guidelines for public policy must be based on an appreciation of the adaptive nature of economic processes within the relevant system as a whole.

The Maximization Assumption

As suggested before, another reason for the economists' fascination with the idea of equilibrium is that an equilibrium condition or set of conditions serves as a point of departure and, depending on its form, suggests a way of deriving the equilibrium position of the process, which becomes identified as the solution. As noted above, the most popular equilibrium conditions in the literature involve a maximization principle.

There is a whole literature in economics on equilibrium and extremal methods. In commenting on the importance of equilibrium in economic analysis, Sir John Hicks reminds us that though "we need the equilibrium assumption, it does not follow that we have a right to it. And indeed, as soon as we allow ourselves to question it, it becomes obvious that it needs much justification."[14] The questions Professor Hicks has in mind are the ones he has addressed so skillfully during his career: questions of the existence, uniqueness, and stability of economic equilibrium. These ques-

tions have become conventional, as have certain observations about the limitations of the use of mathematics in economics. The latter include complaints that assumptions are often made for mathematical convenience rather that economic relevance; that some assumptions, though explicit and seemingly innocuous, are in fact crucial to the behavior and solution of the model in unseen ways; and that some economists are really disguised applied mathematicians who would reduce economics to a narrow set of internally consistent propositions of logic. (It is not clear why anyone would shrink before the epithet "applied mathematician," and in any event, there is no surprise in the fact that some men value their tools more highly than they do their work.) These observations do make a point and probably have been cast aside or ignored too cavalierly by the profession, but it remains the case that extremal methods do generate useful theorems about economic behavior. Our criticism lies elsewhere.

A likely place to start is with the tenacity with which economists cling to the assumption of maximizing behavior. Ideally, the maximizing behavior ought to apply to an independently defined variable, but in economics the variables are sometimes conveniently redefined so that they fit the behavioral assumption. Recall the careless tautological use of utility, long-run profits, and average costs. Suppose a firm suddenly decides to make a large contribution to the local United Fund rather than increase dividends to its stockholders. The economist retains his profit-maximizing assumption by arguing that community opinion is a relevant constraint on long-run profits; the contribution is viewed as the purchase of the productive factor "good will." The only trouble with this ad hoc addition is that, done too often, it leaves the theory of the firm in a shambles. A theory that can rationalize all possible courses of events *ex post* is no theory. Or witness the many empirical cost studies that find that a large number of industries exhibit constant returns to scale over a wide range of output. Given the way costs are defined, the very process of competition is guaranteed to lead to an equalization of long-run average cost at various outputs. Capital market revaluations level error and ingenuity. As a result, many conclusions about potential entry are erroneous. Incidentally, much of this can be cleared up by a historical feel for the particular industry.

Finally, observe the latest tautology on the market. Friction and inertia in physics become ignorance and habit in economics. Everyday consumers can be observed buying identical goods at stores charging higher prices than their competitors. This is

interpreted, not as a lapse in maximizing behavior, but as the recognition of search costs. Habitual buying may be entirely rational when the costs of overcoming the ignorance of alternatives are considered. This makes sense and has led to a number of interesting hypotheses about shopping behavior and queue formation, but pushed too far, the notion of transaction costs can become tautological. Thus, everyone is at all times in perfect equilibrium because otherwise he would adjust his consumption position. Again, a theory that cannot rule out some behavior patterns as irrational or noneconomic is no theory. Tautology has a role—science must get its definitions straight before it can proceed—but no analytic role. Admittedly, the sins described above should for the most part be laid at the door of careless practitioners, but the fact that they arise frequently may suggest the need to reexamine the basic paradigm.

Even among the esteemed of the discipline, the traditional paradigm has a way of preshaping the analytical response to added dimensions in economic problems. Uncertainty renders the concept of a utility maximum or a profit maximum meaningless. The paradigm is saved by a slight modification of the behavioral assumption. Consumers maximize expected utility and firms maximize an entrepreneurial utility function containing the mean and standard deviation of profits. Only at the periphery of economics do risk and uncertainty suggest alternative behavioral assumptions—such as satisficing. "Slack" has never played a prominent role in economic theory. Indeed, the process of economic competition weeds out slackers, laggards, and nonmaximizers. It can be argued, however, that in open economic systems characterized by uncertainty and evolution, satisficing is quite sufficient for surviving the rigors of competition. The sterility of general equilibrium theory and modern welfare economics stems from the inapplicability of extremal methods to complex systems.

This is not an argument against mathematical economics as such, however. The problem lies not so much with the abuse of extremal methods as in not appreciating the limitations of their applicability. As Hayek argues, so enamored of the method are economists that they miss *the* economic problem. He is quoted in length here.

> What is the problem we wish to solve when we try to construct a rational economic order? On certain familiar assumptions the answer is simple enough. *If* we possess all the relevant information, *if* we can start out from a given system of preferences and *if* we command complete know-

ledge of available means, the problem which remains is purely one of logic. That is, the answer to the question of what is the best use of the available means is implicit in our assumptions. The conditions which the solution of this optimum problem must satisfy have been fully worked out and can be stated best in mathematical form: put at their briefest, they are that the marginal rates of substitution between any two commodities or factors must be the same in all their different uses.

This, however, is emphatically *not* the economic problem which society faces. And the economic calculus which we have developed to solve this logical problem, though an important step toward the solution of the economic problem of society, does not yet provide an answer to it. The reason for this is that the "data" from which the economic calculus starts are never for the whole society "given" to a single mind which could work out the implications and can never be so given.

The peculiar character of the problem of a rational economic order is determined precisely by the fact that the knowledge of the circumstances of which we must make use never exists in concentrated or integrated form but solely as the dispersed bits of incomplete and frequently contradictory knowledge which all the separate individuals possess. The economic problem of society is thus not merely a problem of how to allocate "given" resources—if "given" is taken to mean given to a single mind which deliberately solves the problem set by these "data". It is rather a problem of how to secure the best use of resources known to any of the members of society, for ends whose relative importance only these individuals know. Or, to put it briefly, it is a problem of the utilization of knowledge which is not given to anyone in its totality.

This character of the fundamental problem has, I am afraid, been obscured rather than illuminated by many of the recent refinements of economic theory, particularly by many of the uses made of mathematics. . . . It seems to me that many of the current disputes with regard to both economic theory and economic policy have their common origin in a misconception about the nature of the economic problem of society. This misconception in turn is due to an erroneous transfer to social phenomena of the habits of thought we have developed in dealing with the phenomena of nature.[15]

In essence, neoclassical analysis represents the logical derivation of a set of equilibrium conditions that must obtain when maximizing behavior is assumed. The focus is on the set of conditions and not the economic process leading to equilibrium. Economists are in the habit of viewing equilibrium, whether static or dynamic, as the end point of their investigations beyond which there is little interest. But the solution (equilibrium) is already implicit in the maximization assumption and in no way depends on an exploration of economic processes. Economists err, and err seriously, when they then elevate the equilibrium conditions to a normative standard for judging observed market behavior, routinely condemning advertising, product differentiation, and price discrimination.

The market is rarely, if ever, in equilibrium in just the way conceived by neoclassical analysis. Two sets of factors prevent that: ignorance, and shifts in underlying tastes and technology. To some extent, neoclassical theory has addressed the latter. Equilibrium as a moving target in static analysis becomes the equilibrium time path in dynamic analysis. However, market processes for reducing ignorance—ignorance of what products customers want, what styles and quality they desire; ignorance of the best production and distribution techniques available; ignorance of the best input prices or the most reliable suppliers; and ignorance of future conditions—have never been systematically incorporated into the neoclassical paradigm. Once it is appreciated that the real economic problem is the coordination of the bits and pieces of knowledge held by different participants in the market process, the roles of advertising, product differentiation, and price undercutting become clear. To overcome ignorance about what potential customers might want, a firm offers a full product line of differing qualities and styles. Some lines will prosper and be expanded; others will fail and be withdrawn. Product differentiation is at once both a method of discovery and a means of adapting to the mosaic of consumer tastes. A concern for economic process puts observed economic behavior in a very different light from when it is judged by those conditions holding when the market is at rest.[16]

Reduction and Isolation

Finally, we turn to the methods of abstraction in physics—reduction and isolation. This approach of analytical dissection, so successful when applied to reversible mechanical processes in closed systems, may not be appropriate for dealing with complex open systems. Economic relationships are not additive in the sense

in which mechanical ones are. Witness the many examples of the fallacy of composition against which we must caution economics students.

Economic relationships are not easily isolated from the host of economic factors that impinge upon them within an economic system. Careless dissection of the *body economicus* gives rise to pseudoproblems and artificial constructs having little analytical meaning and no empirical counterpart. Economists are wont to conceive of the economic system as divisible into distinct markets for separate goods, to divide all economic variables into supply or demand factors, and to draw a sharp distinction between allocative and distributional questions. In the context of an economic system, however, these convenient pigeon holes can be misleading. One need not be Galbraithian to suppose, for example, that tastes are endogenous to the economic system. Consider these fairly typical examples of shop talk: "Assume the cross elasticities are zero," i.e., assume away any possible interaction with adjacent markets. "For your growth model assume fixed proportions, homogenous production functions, and unitary income elasticities for all goods"; i.e., aggregate all consumption and all production so you can neatly solve for a well-behaved steady-state equilibrium. "Let the market solve the allocation problem, then compensate the losers or the poor as the case may be"; i.e., forget the repercussions of redistribution on allocation. As if the question stopped there and the economist had nothing further to say about the eventual qualitative evolution of the system from the point of the policy change. Again, this is loose talk within the context of an economic system or in terms of policy guidelines.

Economists also have a penchant for the assumption of competition, by which is meant that they are free to take input or product prices or both as datum. The assumption obviates the very conditions that the *process of competition* tends to bring about. The assumption does, however, allow an economic process to be pressed into the mold of a mechanical analogue.

There is a further problem here. And again we turn to insights provided by Professor Hayek.[17] The reductionist tradition may serve the natural sciences well because they deal with observable (objective) phenomena. By contrast, social phenomena are inherently subjective. Men act according to their *perception* of relevant data. Subjective evaluation of external stimuli, though unobservable and hence nonquantifiable, are part and parcel of the phenomena economists wish to explain. To formulate functional relationships among variables representing "hard" economic data and refer to

them as economic theory is to commit an unscientific error. Economic analysis cannot be based on such a "slip between lip and cup," for to be meaningful it must be able to explain the qualitative aspects of economic behavior, processes, and evolution. The view from physics is on this score dangerously misleading.

These are not merely obtuse arguments about the need for greater realism in economic theory. Realism is a treacherous criterion for judging abstractions as analytical assumptions *ex ante*. But granting that, the widely held view that the adequacy of an assumption depends on the fruitfulness of the theory in terms of perdictive power need not be raised to a dogma for rejecting any and all questions about *a particular method of abstraction.*

Conclusion

Economics has borrowed from classical mechanics a paradigm for abstracting from the richness, diversity, and intricacies of economic systems. The method has proven highly successful, particularly for the analysis of relatively simple economic relationships. This predictive success tends to justify this procedure for abstraction. Economists should not become so wedded to this paradigm, however, or so enamored with its success, as to allow it to delimit the range of questions economists can legitimately address.

There is no wish to "throw out the baby with the bath water" or to gainsay the very real success of the neoclassical paradigm in economics. It is a magnificent edifice, certainly one of the crowning intellectual achievements in all of the social sciences. What is being suggested is that economics might profitably explore an alternative paradigm, one that starts with complex economic systems as given and seeks to explain qualitative economic change and evolution. If the growing dissatisfaction among economists does lead to a Kuhnian paradigm shift, it seems likely that the critical insights of the Austrian school will play a major role.

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1. Robbins, *An Essay on the Nature and Significance of Economic Science* (London, 1932); Hutchison, *The Significance and Basic Postulates of Economic Theory* (London, 1938; New York, 1960); Friedman, "The Methodology of Positive Economics," in *Essays in Positive Economics* (Chicago, 1953), chap. 1.

2. See Sherman R. Krupp, ed., *The Structure of Economic Science* (Englewood Cliffs, N. J., 1966); Sidney Hook, ed., *Human Values and Economic Policy* (New York, 1967), especially part 2.

3. Hayek, *The Counter-Revolution in Science* (Glencoe, 1952), and *Individualism and the Economic Order* (Chicago, 1948), chaps. 1-6; Mises, *Human Action* (New Haven, 1963), *Theory and History* (New Haven, 1957), *The Ultimate Foundations of Economic Science* (Princeton, 1962), and *Epistemological Problems of Economics* (Princeton, 1960). See also Laurence S. Moss, ed., *The Economics of Ludwig von Mises* (Kansas City, 1976); Edwin G. Dolan, "Austrian Economics as Extraordinary Science," in *The Foundations of Modern Austrian Economics*, ed. Dolan (Kansas City, 1976).

4. Dolan, p. 7.

5. The current theoretical shambles of "macro" analysis and the demise of "fine tuning" as the guiding principle behind government macroeconomic policy have led a number of neoclassical economists to share this view. See, for example, Edmund S. Phelps, et al., *Micro-economic Foundations of Employment and Inflation* (New York, 1970); Gary Becker, *Economic Theory* (New York, 1971), especially the introduction.

6. Hutchison, xxi.

7. To get a flavor of the defense of neoclassicism, see Friedman; F. Machlup, "The Problem of Verification in Economics," *Southern Economic Journal*, July 1955, and "Professor Samuelson on Theory and Realism: Comment," *American Economic Review*, March 1971; E. Nagel, "Assumptions in Economic Theory," *American Economic Review*, May 1963; K. Popper, *The Logic of Scientific Discovery* (London, 1968); P. Samuelson, "Problems of Methodology—Discussion," *American Economic Review*, May 1963, "Theory and Realism: A Reply," *American Economic Review*, September 1964, and "Reply," *American Economic Review*, December 1965.

8. Hayek, *Counter-Revolution in Science; Law, Legislation, and Liberty*, vol. 1 (Chicago, 1973).

9. See the introduction of M. J. P. Magill's *On a General Economic Theory of Motion* (New York, 1970) for the comments of various economists on the applicability of the methods of physics to economic phenomena. Magill is, himself, positively poetic about the view from physics. Also see Frank H. Knight, "Statics and Dynamics: Some Queries Regarding the Mechanical Analogy in Economics," in *On the History and Method of Economics* (Chicago, 1956).

10. See, e.g., Ludwig von Bertalanffy, *General Systems Theory* (New York, 1968).

11. Kuenne, *The Theory of General Economic Equilibrium* (New York, 1963), p. 17.

12. Cited in Nicholas Georgescu-Roegen, *The Entropy Law and the Economic Process* (Cambridge, 1971), p. 319.

13. John S. Chipman, "The Nature and Meaning of Equilibrium in Economic Theory," in *Price Theory*, ed. Harry Townsend (Baltimore, 1971), p. 341.

14. Sir John Hicks, *Capital and Growth* (New York, 1972), p. 16.

15. Hayek, *Individualism and the Economic Order*, pp. 77-78.

16. See the outstanding contribution to this analysis by Israel M. Kirzner, *Competition and Entrepreneurship* (Chicago, 1973).

17. Hayek, *Counter-Revolution in Science*, chaps. 2-4, and "The Primacy of the Abstract," in *Beyond Reductionism*, ed. A. Koestler and J. R. Smythies (Boston, 1969).