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Rationality in Psychology and Economics*

The assumption that actors maximize subjective expected utility (economic rationality) supplies only a small part of the premises in economic reasoning, and that often not the essential part. The remainder of the premises are auxiliary empirical assumptions about actors' utilities, beliefs, expectations, and the like. Making these assumptions correctly requires an empirically founded theory of choice that specifies what information decision makers use and how they actually process it. This behavioral empirical base is largely lacking in contemporary economic analysis, and supplying it is essential for enhancing the explanatory and predictive power of economics.

The task I shall undertake here is to compare and contrast the concepts of rationality that are prevalent in psychology and economics, respectively. Economics has almost uniformly treated human behavior as rational. Psychology, on the other hand, has always been concerned with both the irrational and the rational aspects of behavior. In this paper, irrationality will be mentioned only obliquely; my concern is with rationality. Economics sometimes uses the term "irrationality" rather broadly (e.g., Becker 1962) and the term "rationality" correspondingly narrowly, so as to exclude from the domain of the rational many phenomena that psychology would include in it. For my purposes of comparison, I will have to use the broader conception of psychology.

One point should be set immediately outside dispute. Everyone agrees that people have reasons for what they do. They have motivations, and they use reason (well or badly) to respond to these motivations and reach their goals. Even much, or most, of the behavior that is called abnormal involves the exercise of thought and reason. Freud was most insistent that there is method in madness, that neuroses and psychoses were patients' solutions—not very satisfactory solutions in the long run—for the problems that troubled them.

* In writing out my remarks for publication, I have had the benefit, of course, of hearing the observations of my discussants at the Chicago meeting. I shall take the liberty of commenting, from time to time, on their remarks, but I shall try to indicate when I do so, in order to avoid a confusing anachronism.

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I emphasize this point of agreement at the outset—that people have reasons for what they do—because it appears that economics sometimes feels called on to defend the thesis that human beings are rational. Psychology has no quarrel at all with this thesis. If there are differences in viewpoint, they must lie in conceptions of what constitutes rationality, not in the fact of rationality itself.

The judgment that certain behavior is "rational" or "reasonable" can be reached only by viewing the behavior in the context of a set of premises or "givens." These givens include the situation in which the behavior takes place, the goals it is aimed at realizing, and the computational means available for determining how the goals can be attained. In the course of this conference, many participants referred to the context of behavior as its "frame," a label that I will also use from time to time. Notice that the frame must be comprehensive enough to encompass goals, the definition of the situation, and computational resources.

In its treatment of rationality, neoclassical economics differs from the other social sciences in three main respects: (*a*) in its silence about the content of goals and values; (*b*) in its postulating global consistency of behavior; and (*c*) in its postulating "one world"—that behavior is objectively rational in relation to its total environment, including both present and future environment as the actor moves through time.

In contrast, the other social sciences, in their treatment of rationality, (*a*) seek to determine empirically the nature and origins of values and their changes with time and experience; (*b*) seek to determine the processes, individual and social, whereby selected aspects of reality are noticed and postulated as the "givens" (factual bases) for reasoning about action; (*c*) seek to determine the computational strategies that are used in reasoning, so that very limited information-processing capabilities can cope with complex realities; and (*d*) seek to describe and explain the ways in which nonrational processes (e.g., motivations, emotions, and sensory stimuli) influence the focus of attention and the definition of the situation that set the factual givens for the rational processes.

These important differences in the conceptualization of rationality rest on an even more fundamental distinction: in economics, rationality is viewed in terms of the choices it produces; in the other social sciences, it is viewed in terms of the processes it employs (Simon 1976/1982). The rationality of economics is substantive rationality, while the rationality of psychology is procedural rationality.

Substantive and Procedural Rationality

If we accept values as given and consistent, if we postulate an objective description of the world as it really is, and if we assume that the decision maker's computational powers are unlimited, then two important consequences follow. First, we do not need to distinguish between the real world and the decision maker's perception of it: he or she perceives the world as it really is. Second, we can predict the choices that will be made by a rational decision maker entirely from our knowledge of the real world and without a knowledge of the decision maker's perceptions or modes of calculation. (We do, of course, have to know his or her utility function.)

If, on the other hand, we accept the proposition that both the knowledge and the computational power of the decision maker are severely limited, then we must distinguish between the real world and the actor's perception of it and reasoning about it. That is to say, we must construct a theory (and test it empirically) of the processes of decision. Our theory must include not only the reasoning processes but also the processes that generate the actor's subjective representation of the decision problem, his or her frame (Simon 1978/1982).

The rational person of neoclassical economics always reaches the decision that is objectively, or substantively, best in terms of the given utility function. The rational person of cognitive psychology goes about making his or her decisions in a way that is procedurally reasonable in the light of the available knowledge and means of computation.

Embracing a substantive theory of rationality has had significant consequences for neoclassical economics and especially for its methodology. Until very recently, neoclassical economics has developed no strong empirical methodology for investigating the processes whereby values are formed, for the content of the utility function lies outside its self-defined scope. It has developed no special methodology for investigating how particular aspects of reality, rather than other aspects, come to the decision maker's attention, or for investigating how a representation of the choice situation is formed, or for investigating how reasoning processes are applied to draw out the consequences of such representations.

All these investigations call for empirical inquiry at the micro level—detailed study of decision makers engaged in the task of choice (Simon 1979*b*/1982, 1982). They are not questions that are easily answered by even the most sophisticated econometric analysis of aggregate data. To understand the processes that the economic actor employs in making decisions calls for observing these processes directly while they are going on, either in real world situations or in the laboratory, and/or interrogating the decision maker about beliefs, expectations, and methods of calculation and reasoning.

Securing access in order to observe decision processes in business firms or government organizations is difficult but often quite feasible—there are already a substantial number of successful studies of this kind in the literature. To extrapolate and generalize the findings requires attention to problems of sampling and aggregation; but these problems are surely easier to solve than the problem of going from the micro level of hypothetical "representative firms" or "typical consumers" to the level of markets.

Laboratory experiments on decision processes raise questions of their generalizability to real world situations. Studies that depend on interrogation of one kind or another raise questions of the veridicality of responses. There is no dearth of methodological issues but no reason to suppose that these issues are any more intractable than those encountered in standard econometric practice. But since experimental economics is well represented at this conference, I will say no more about the methodological issues it faces. They are best discussed in the context of concrete examples, a number of which will be provided here.

To move from substantive to procedural rationality requires a major extension of the empirical foundations of economics. It is not enough to add theoretical postulates about the shape of the utility function, or about the way in which actors form expectations about the future, or about their attention or inattention to particular environmental variables. These are assumptions about matters of fact, and the whole ethos of science requires such assumptions to be supported by publicly repeatable observations that are obtained and analyzed objectively.

In the following sections of this paper, I should like to illustrate, with concrete examples, the difficulties that contemporary neoclassical economics faces on a number of fronts owing to the insufficiency of its empirical foundations. These examples will also suggest the directions in which empirical work needs to go. My topics will include the shape and dimensions of the utility function, the role of attentional processes, the formation of expectations, and the sources of the empirical parameters and models that characterize cost and supply functions.

In all these examples we will see that the conclusions that are reached by neoclassical reasoning depend very much on the "auxiliary" factual assumptions that have to be made to define the situation and very little on the assumptions of substantive rationality—in particular, the utility-maximization assumptions. Indeed, in many cases, provided that the factual assumptions are retained, the conclusions reached within the utility-maximization framework could be reached as readily from much weaker assumptions of "reasonableness" in behavior (Becker 1962). Almost all the action, all the ability to reach nontrivial conclusions, comes from the factual assumptions and very little from the assumptions of optimization. Hence it becomes critically important to submit the factual assumptions to careful empirical test.

Empirical Basis for the Utility Function

Contemporary neoclassical economics provides no theoretical basis for specifying the shape and content of the utility function, and this gap is very inadequately filled by empirical research using econometric techniques. The gap is important because many conclusions that have been drawn in the literature about the way in which the economy operates depend on assumptions about consumers' utility functions.

To illustrate this claim, I will take two examples from the work of one of my discussants, Gary Becker, at the risk, of course, of introducing circularity by discussing a discussant. Both examples are drawn from Becker's well-known *A Treatise on the Family* (1981).

The Opportunities of Children

My account of the first example is not original but follows an enlightening analysis that has been carried out recently by Arthur Goldberger (in press) of Becker's (1981, chs. 6, 7) theory of the opportunities of children and intergenerational mobility. On page 116 of his book, Becker follows a mathematical demonstration with the interpretation: ''If parents correctly anticipate their children's luck and endowment, an increase in either would not add an equal amount to the income of children.'' This interpretation is later (pp. 125–26) used to question whether public compensatory education programs will achieve their goal since parents whose children participate in these programs simply reallocate elsewhere resources they would otherwise have invested in these children.

I will return in a moment to the empirical evidence for the conclusion and its application. First, however, I would like to report Goldberger's analysis of the underlying argument that leads up to it.

Goldberger shows that Becker's conclusion follows from specific assumptions that parents' utility grows positively with parent's consumption and child's income and that the child's income is an additive function of parents' investment and child's luck. If the latter function is multiplicative instead of additive, the conclusion does not follow. Moreover, the whole derivation employs a homothetic utility function. No empirical support is provided for these assumptions.

But Milton Friedman (1953) would tell us that we should concentrate our efforts on testing the conclusions, not the assumptions, and this is what Becker does. Three pieces of evidence are cited, one relating to compensatory education and two to other public ''compensatory'' programs. On education, Becker quotes Arthur Jensen's ''famous and controversial essay'' (the characterization is Becker's) to the effect that ''compensatory education has been tried and it apparently has failed'' (p. 125). The essay quoted is, indeed, controversial; others have subsequently reached quite different evaluations of Headstart and other compensatory education programs. Moreover, Jensen himself attributes their failure—if there was one—to causes very different from the utility functions of parents. To choose among Jensen's explanation, Becker's, and the thousand others that could be dreamed up, it would surely be important to find out directly whether families whose children participated in such programs diverted their money, nurturance, or attention away from those children. That would provide a relatively direct test of the nature of their preferences (although very far from a test either that they possessed consistent utility functions or that they were maximizing anything).

Becker's other two pieces of evidence are more to the point. It does appear that public health programs cause people to devote less of their private budgets to health matters (the programs are presumably intended to do this) and that food supplements to pregnant women are to some extent diverted by reallocation of private budgets. To that extent we can conclude that some substitutions of the sort that utility theory predicts do actually take place. But of course as Becker has pointed out elsewhere (Becker 1962), price elasticity of demand is not a very strong test of utility maximization.

What one sees in this example are matters of substantial practical as well as theoretical importance disposed of on the basis of unsupported theoretical assumptions and scanty evidence about the conclusions. Economics is too important, intellectually as well as practically, to be treated with this kind of casual empiricism.

What one also sees in this example is that the conclusions depend primarily not on the assumption of optimization but on the (untested) auxiliary assumption that the interaction of luck and endowment is additive rather than multiplicative. Utility maximization is neither a necessary nor a sufficient condition for compensatory behavior.

Finally, in this example one begins to understand the real decision making when one undertakes (as in the public health and food supplement research) to gather direct evidence about behavior through field studies or field experiments.

Labor Force Participation of Women

Let me now turn to a second example from Becker's (1981) book. On pages 245–56 he gives us his interpretation of the evolution of the American family since World War II. The salient fact around which the analysis revolves is that there has been a steady rise in the labor force participation of married women, including those with small children. At the outset (p. 245), Becker tells us that he believes "that the major cause of these changes is the growth in the earning power of women as the American economy developed." This credo is then buttressed by the empirical observation that the weekly earnings of employed women grew substantially during this period. This, he observes, implies an increase in the opportunity cost of staying in the home and also raises the relative cost of children, thereby reducing the demand for children.

However, the question is never raised as to (*a*) whether the increase in real earnings of women was more rapid than the increase in real earnings of men during this same period or (*b*) whether the increase in women's weekly earnings might not have been in some measure due to an increase in average hours worked—itself a form rather than a consequence of greater labor force participation.

Moreover, Becker places the whole weight of explanation on an unexplained shift in the demand curve for women's labor. No account is given of why this event should have taken place at this particular moment in American history or whether it was a sudden shock or a continuing development. Nor is any evidence provided (except the circular evidence that women moved into the labor force) that the event in fact took place. In particular, the possibility is not explored that a shift in the utility function of women caused a shift in the supply of women in the labor market in the face of a highly elastic demand curve and generally rising productivity in the economy.

So in this example as in the previous one, the action comes, not from the assumption of utility maximization, but from factual assumptions about the shifting or stability of particular supply and demand curves. The true explanation will be obtained not by raising the sophistication of the economic reasoning but only by painstaking examination of occupations in manufacturing and service industries and an even more difficult empirical examination of changes in women's attitudes about where they prefer to work. Utility maximization is neither a necessary nor a sufficient condition for the conclusion that was reached. The action comes from the empirical assumptions, including assumptions about how people view their world.

Attention and Representation

In a substantive theory of rationality there is no place for a variable like focus of attention. But in a procedural theory, it may be very important to know under what circumstances certain aspects of reality will be heeded and others ignored. I wish now to present two examples of situations in which focus of attention is a major determinant of behavior. The first rests on very strong empirical evidence; the second is more speculative, but I will try to make it plausible.

The Purchase of Flood Insurance

Kunreuther et al. (1978) have studied decisions of property owners whether to purchase insurance against flood damage. Neoclassical theory would predict that an owner would buy insurance if the expected reimbursable damage from floods was greater than the premium. The actual data are in egregious conflict with this claim. Instead it appears that insurance is purchased mainly by persons who have experienced damaging floods or who are acquainted with persons who have had such experiences, more or less independently of the cost/benefit ratio of the purchaser.

If we wish to understand the insurance-buying behavior, then we must determine, as Kunreuther and his colleagues did, the circumstances that attract the attention of a property owner to this decision alternative. Utility maximization is neither a necessary nor a sufficient condition for deducing who will buy insurance. The process of deciding—in this case, the process that puts the item on the decision agenda—is the important thing.

Voting Behavior

Voting behavior provides a more complex example of the role of attention in behavior. Both before and since Marx, it has been widely believed that voters respond, at least to an important extent, to their economic interests. Let us assume that is so. A substantial number of empirical studies have shown correlations between economic conditions and votes in American elections (Simon 1985). But such studies use a great variety of independent variables as measures of voters' perceptions of the economic consequences of their choices. (See, e.g., Hibbs 1982; and Weatherford 1983.) Some investigators have tried to measure the economic well-being of voters at the time of the election as compared with their well-being at some previous time. Others have measured the state of the economy—the level of the GNP, say, or of employment. Which of these (or what other measure) is the true measure of economic advantage? Quite different predictions can be made if different measures are chosen.

Consider the situation of a voter at the time of the 1984 presidential election who wished to maximize his or her economic well-being. Which of the following facts about the economy should influence the vote? (1) Real incomes of a majority have increased over the past 4 years but at less than the historical rate of a couple of decades earlier. (2) Dispersion of incomes has increased. (3) The rate of inflation has declined dramatically. (4) The rate of interest remains high compared with the historical past. (5) The national debt and deficit have increased dramatically. (6) The balance of trade has "worsened" dramatically. (7) Farm foreclosures have increased substantially. (8) Unemployment has decreased recently but is higher than it was 4 years previously. If we throw noneconomic considerations into the voter's utility function, we may add such facts as, The armament situation has changed in complex ways, et cetera, et cetera, moving into race tensions and equity to minorities, energy, the environment, creationism, abortion, and what not.

To predict how a voter, even a voter motivated solely by concern for his or her economic well-being, will vote requires much more than assuming utility maximization. A voter who attends to the rate of inflation may behave quite differently from a voter who attends to the federal deficit. Moreover, in order to predict where a voter's attention will focus, we may need to know his or her economic beliefs. A monetarist may consider different facts to be salient than the facts to which a Keynesian will attend. In any model of voting behavior that has any prospect of predicting behavior, almost all the action will lie in these auxiliary assumptions about attention and belief that define the decision maker's frame.

Expectations

Neoclassical theory, either with or without the assumption of rational expectations, cannot explain the phenomenon of the business cycle. In previous papers (e.g., Simon 1984) I have shown that auxiliary assumptions, which in this case amount to departures from objective rationality, must be annexed to the neoclassical model before a business cycle can be made to appear. I will repeat the argument here very briefly.

If we examine Keynes's reasoning in *The General Theory of Employment, Interest and Money* (1936), we see that, at most points, it fits perfectly the neoclassical mold of substantive rationality. Auxiliary assumptions (it does not matter whether we view them as "irrationalities" or simply as expectations) are introduced, however, at points that are critical to the explanation of the business cycle. One of these auxiliary assumptions is the postulate that labor suffers from the money illusion—that unions cannot distinguish between changes in real and money wages, respectively. The addition of this postulate is sufficient to produce underemployment stagnation in the Keynesian theory.

Remarkably enough, when we move from Keynes to the other end of the spectrum of economic theories—for example, to Lucas's (1981) rational expectations models—the same picture presents itself. The business cycle in these models derives, not from the assumptions of rationality, but from the appearance of money illusion in an erstwhile Eden. Only, in Lucas's theory, the illusion is suffered by businessmen, who cannot distinguish between general price movements and price changes in their industry, instead of by workers.

The action in business-cycle theories appears to reside not in the rationality assumptions but in auxiliary assumptions about the processes that people use to form expectations about future events. A theory of procedural rationality would have to employ empirical research to investigate these expectation-forming processes. Again, the assumption of utility or profit maximization provides neither a necessary nor a sufficient condition for the existence of business cycles.

We already have experience in using direct methods to learn how people form expectations about the future. Direct inquiries into people's expectations about business conditions, pioneered by George Katona (1951) at the University of Michigan, have for many years supplied inputs to econometric models of the economy. Studies have been made of planning methods and investment decision processes of business firms (for examples, see Eliasson 1976; and Bromiley 1986) that give us considerable information about what the forecasting firms do and whether and how forecasting enters into the process of choosing investments. This kind of information, which could easily be multiplied, provides us with powerful means for testing the rational expectations theory or other theories about how expectations are formed. There is no need to fall back on casual empiricism or on dubious indirect inferences from econometric data.

Other Empirical Parameters

My two final examples of the role of facts in economic reasoning are a little different from the previous ones. There are important cases in which not only are assumptions of substantive rationality insufficient to account for the observed phenomena but also parsimonious alternative explanations can be provided with only a minimal reference to rationality. The two examples I will discuss are the distribution of business firm sizes and the magnitudes of executive salaries.

Distribution of Business Firm Sizes

There is no unequivocal neoclassical theory of the distribution of business firm sizes. The traditional theory of the determinants of firm size was expounded by Jacob Viner (1932) and, by some kind of intellectual Gresham's Law, has survived to this day in elementary economics textbooks and books on intermediate price theory. It postulates a family of U-shaped short-run cost curves; a U-shaped long-run cost curve that is the envelope of the short-run curves; and a firm whose size corresponds to the scale of minimum cost on the long-run curve.

There are innumerable difficulties with this account. The most serious is that empirical studies very often show cost curves to be J shaped, without a recognizable minimum, rather than U shaped. With a J-shaped curve, that is, decreasing costs at all sizes, there is no upper bound on firm size. Nearly as serious is that the theory says nothing about parameter values of the cost curves, hence nothing about what sizes of firms will actually be observed. In particular, no conclusions can be drawn about the distribution of firm sizes.

If the theory is interpreted to mean that all firms in an industry have identical cost curves (which, in the absence of rents, should be the case), then, in equilibrium, all firms will be the same size. But this prediction is contradicted by the facts as squarely as any prediction could be. The actual distributions are highly skewed, with many small firms and a few that are very large.

If, on the contrary, each firm has its own cost curve, bearing no relation to the curves of the others, then the theory predicts too little: any size distribution whatsoever can be accommodated. But the fact is that the actual distributions are quite regular and similar, approximating the Pareto distribution in the upper tail. The traditional theory is therefore a total failure in predicting actual firm size distributions and should be banished from the textbooks.

But here we face the difficulty that nature abhors a vacuum, that a bad theory is preferred to none. However, in this case, a wholly satisfactory alternative is available. It rests on plausible (although not well-tested) premises, and these premises are even consistent with possible behaviors of reasonable men and women. The key premise (Ijiri and Simon 1977) is that the expected rate of growth of a firm during any period is proportionate to the size it had attained at the beginning of that period—the so-called Gibrat assumption. With this assumption and a little attention to boundary conditions, the Pareto distribution can be deduced as the steady-state equilibrium of the system.

Such tests as have been made of the Gibrat assumption have had generally positive results. But even if we were satisfied that the assumption is empirically valid, we might wish to probe deeper. In a world in which people have reasons for their actions, why would such a relation hold? This is not the place for a full discussion of the matter, and I limit myself to two comments. First, if average rate of return on capital is independent of size of firm (as seems to be true to a first approximation), it is rather easy to think of reasons why access to internal or external capital for expansion should be, on average, roughly commensurate with present size. Second, we really do not need to do this kind of armchair guessing. We can undertake to study business firms to determine how growth comes about. What we should not do is to cling to a theory that predicts very little and that little incorrectly.

Distribution of Executive Salaries

My second example of a failure of classical theory that can be remedied within a framework of procedural rationality is the prediction of the distribution of the salaries of top executives.

Neoclassical theory would, of course, explain the salaries of executives in terms of individual abilities for managerial work. Moreover, a very able executive would presumably have greater productivity when engaged in large affairs, as in managing a large business firm. Hence the combined forces of supply and demand would produce a strong correlation of executive ability with firm size. Rees (1973, p. 201), in his textbook exposition of the theory, puts it this way: "The person who alone among hundreds of competing junior executives in a large corporation eventually rises to the presidency must surely have special qualities that account for this rise, although they differ from the qualities that make a successful scientist or a successful salesperson. In any event, a business that is not a monopoly would soon run at a loss if it selected executives without any regard to those kinds of ability relevant in managing a business well.''

The claim is moderate and plausible. It does not require any assumption of maximization, only the assumption that those who select executives will behave reasonably, by taking ability into account. It leaves open the question of the processes of selection: how ability is judged and how accurately it can be estimated. It also leaves open the question of whether the characteristics that allow a person to compete successfully for advancement are closely correlated with the characteristics that make for effective management. But we will ignore the Peter Principle and other postulates that have been put forward challenging the close relation between managerial ability and selectability.

Difficulty arises, however, when we try to move from these premises to an account of the observed salary distribution function. A number of studies (e.g., Roberts 1959) have shown that the average compensation of top executives increases with the cube root of the size of the firm. I have been told, though I have not seen the data, that this relation continues to hold when executive bonuses and fringe benefits are included in salary. How shall we explain this very regular and persistent distribution?

Neoclassical theory, without strong auxiliary assumptions, is helpless. As Lucas (1978) has pointed out, what are needed are an assumption about the marginal product of managerial work as a function of firm size and managerial ability and an assumption about the distribution of managerial ability measured in the same units as in the previously mentioned function. Since there are no empirical data either on the marginal productivity of managers (except the salary data themselves) or on the distribution of ability, it is easy to manufacture functions that will produce the desired distribution of salaries. However, it should be noted that certain ''obvious'' choices of function will not work—for example, the Gibrat assumption that the marginal productivity of a manager is proportional to the size of the firm. If a function is found that fits the data, it is, of course, not refutable and hence profoundly uninteresting as a theoretical premise.

As an alternative route to an explanation of the observed salary distribution (Simon 1979*a*/1982), we can introduce two factual assumptions that can be verified (and for one of which we already have sufficient empirical data). The first assumption is that business organizations have a pyramidal form and that the number of subordinates reporting directly to an executive does not vary much from one organization to another or from one organizational level to another—that the span of control is relatively constant. This is a well-known fact about business organizations. The second assumption is that, by some generally accepted norm of "fairness," the ratio of the salary of an executive to the salaries of his or her immediate subordinates is a constant over organizations and over levels.

With these two assumptions, it follows immediately that the log of the salary of the top executive will vary linearly with the log of company size; and with reasonable assumptions, fitting observed facts, about the sizes of the two parameters of the theory (the span of control and the "fair" ratio of salaries), the coefficient in this linear relation can be predicted to be in the neighborhood of .3—very close to the observed value.

Of course, we will be more confident that this is the correct explanation when we have more direct evidence that the postulated norm of fairness really exists in peoples' minds. My only claim at the moment is that here we have an explanation that takes process into account, that does not rely on any assumption of utility or profit maximization, and that does describe a realizable decision process that is not dependent on quantities that are unobservable by the actors and by economists studying the phenomena. Of course this explanation does make the correct prediction—quantitatively as well as qualitatively.

Neoclassical economists have raised several objections to the explanation I have just outlined. Rees (1973, p. 201) argues that the theory "is too special, since it applies only to hierarchical organizations." He points out that the salary distributions of such professionals as architects and attorneys, who typically work in small organizations, also are highly skewed. Of course, there is no reason to suppose that salaries in all occupations are fixed in the same way. A procedural theory of rationality would predict that the method of salary determination would depend on what kinds of information were available for assessing worth.

Position in hierarchy provides such information in large organizations. In the case of architectural and legal firms, direct measures are generally available of the magnitude of the revenues associated with an associate's work and the magnitude of the contracts he or she is able to attract. Moreover, the sizes of jobs have a highly skewed distribution for a variety of reasons that we could explore empirically. Hence the fact that the salaries of professionals other than executives of business firms are skewed hardly seems a reason for rejecting the theory of executive compensation I have proposed.

Recently, new information has been gained about bonuses and other nonsalary awards that make up an increasing part of executive compensation. This information shows that bonuses vary strongly with profits and other statistics that might be taken as measures of the effectiveness of executive performance. But such evidence merely shows that companies try to motivate their executives to make profits (often, alas, with excessive attention to short-run profits, as has been pointed out in the literature). It does not show that the incentives bear any close relation to the marginal worth of the executives or even that the fluctuations in profits are the result of the incentive compensation.

The evidence that incentives are offered to executives to increase profits is just what a procedural theory would predict. It would predict also that the rewards would be related to available statistics of company performance, even in the absence of reliable information about the exact relation between performance and managerial behavior. It would make no assumption that profits are maximized since the observable evidence provides no basis for that assumption. It would assume only that executives (and corporate boards) believe that executives can influence profits through their behavior and that a bonus plan would thereby motivate them to try harder.

A procedural theory would assume that people have reasons for what they do when they set executive salaries and that these reasons take account of the highly imperfect and incomplete information available to them. However, it would be essential, in order to predict behavior more precisely, to have good empirical information both about the kinds of information to which the decision makers have ready access and about their beliefs and opinions on the mechanisms of the world on which their decisions operate. The most likely sources of such information are direct studies of the behaviors, values, beliefs, and opinions of the actors.

Summing Up

Between supporters of substantive and procedural theories of rationality there are fundamental differences about what constitutes a principled, parsimonious, scientific theory. We may put the matter in Bayesian terms. Neoclassical economists attach a very large prior probability (.9944?) to the proposition that people have consistent utility functions and in fact maximize utilities in an objective sense. As my examples show, they are prepared to make whatever auxiliary empirical assumptions are necessary in order to preserve the utility-maximization postulate, even when the empirical assumptions are unverified. When verification is demanded, they tend to look for evidence that the theory makes correct predictions and resist advice that they should look instead directly at the decision mechanisms and pro-

Given the magnitude of the Bayesian prior that expresses confidence in the theory and the weakness of the kinds of indirect evidence that are allowed for testing it, neoclassical economics becomes, as has been observed more than once, essentially tautological and irrefutable. Because of its preoccupation with utility maximization, it fails to observe that most of its ''action''—the force of its predictions—derives from the, usually untested, auxiliary assumptions that describe the environment in which decisions are made. The examples show that the important conclusions it draws can usually also be drawn, with the aid of the auxiliary assumptions, from the postulate that people are procedurally rational and without assuming that they maximize utility.

It is too easy, within the neoclassical methodological framework, to save the theory from unpleasant evidence by modifying the auxiliary assumptions and providing a new framework within which the actor ''must have been operating.'' Hence neoclassical theory, as usually applied, is an exceedingly weak theory, as shown by the difficulty of finding sets of facts, actual or hypothetical, that cannot be rationalized and made consistent with it.

Behavioral theories of rationality attach a high prior probability (.9944?) to the assumption that economic actors use the same basic processes in making their decisions as have been observed in other human cognitive activities and that these processes are indeed observable. In situations that are complex and in which information is very incomplete (i.e., virtually all real world situations), the behavioral theories deny that there is any magic for producing behavior even approximating an objective maximization of profits or utilities. They therefore seek to determine what the actual frame of the decision is, how that frame arises from the decision situation, and how, within that frame, reason operates.

In this kind of complexity, there is no single sovereign principle for deductive prediction. The emerging laws of procedural rationality have much more the complexity of molecular biology than the simplicity of classical mechanics. As a consequence, they call for a very high ratio of empirical investigation to theory building. They require painstaking factual study of the decision-making process itself.

What is to be done? What prescription for economic research derives from my analysis?

First, I would recommend that we stop debating whether a theory of substantive rationality and the assumptions of utility maximization provide a sufficient base for explaining and predicting economic behavior. The evidence is overwhelming that they do not.

We already have in psychology a substantial body of empirically tested theory about the processes people actually use to make boundedly rational, or ''reasonable,'' decisions. This body of theory asserts that the processes are sensitive to the complexity of decision-making contexts and to learning processes as well.

The application of this procedural theory of rationality to economics requires extensive empirical research, much of it at micro-micro levels, to determine specifically how process is molded to context in actual economic environments and the consequences of this interaction for the economic outcomes of these processes. Economics without psychological and sociological research to determine the givens of the decision-making situation, the focus of attention, the problem representation, and the processes used to identify alternatives, estimate consequences, and choose among possibilities—such economics is a one-bladed scissors. Let us replace it with an instrument capable of cutting through our ignorance about rational human behavior.

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