

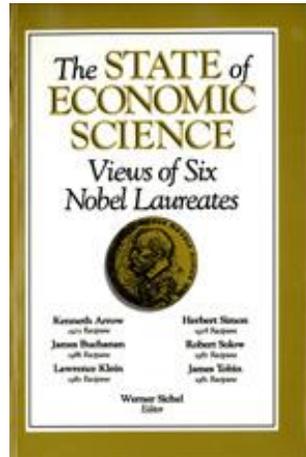
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## [The State of Economic Science]

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Professor Simon holds A.B. and Ph.D. degrees from the University of Chicago. He has been awarded more than a dozen honorary degrees from U.S. and foreign institutions including the University of Chicago, Yale University, Columbia University, the University of Michigan, the University of Pittsburgh, McGill University in Canada, Lund University in Sweden, and Erasmus University in The Netherlands. Before joining Carnegie Institute of Technology in 1949, Professor Simon served as Director of Administrative Measurement Studies at the University of California, Berkeley for three years and as a professor of political science at Illinois Institute of Technology for seven years. At Carnegie-Mellon he has served as Professor of Administration, head of the Department of Industrial Management, Professor of Administration and Psychology, and Associate Dean of the Graduate School in Industrial Administration.

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Professor Simon is the author of about 25 books and monographs and over 600 articles in scholarly journals and books. His books include: *Administrative Behavior*, *Human Problem Solving*, *The New Science of Management Decision*, *The Sciences of the Artificial*, *Models of Thought*, *Models of Bounded Rationality*, *Reason in Human Affairs*, *Protocol Analysis*, and *Scientific Discovery: Computational Exploration of the Creative Process*.



**Dr. Herbert A. Simon**

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of Computer Science and Psychology  
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The title of this volume of essays is “The State of Economic Science,” but I should not like to limit myself to just describing where economics is now: I would like also to say something, however conjectural, about the future. Since, in contrast to Larry Klein, forecasting is not my professional metier, perhaps I can avoid some of the dangers of amateurism by putting more emphasis on the prescriptive than on the predictive; by taking high moral ground and telling economics what it should be doing that it is not now doing.

**Consensus and Dissention Among Economists**

With this agenda, my emphasis will be on methodological issues: on what kinds of research economists should be doing and what tools they will need for doing it. But my methodological prescriptions need to derive from a view of the substance of economics and its problems, and should rest on an assessment of where economic science stands today. Let me begin with that.

***Public Disagreement Among Economists***

I must first explain why economists are seen by the general public to be constantly disagreeing with one another. Of course that is not

wholly true with respect to my distinguished colleagues in this volume. I have observed that Ken Arrow, Larry Klein, Jim Tobin, and Bob Solow often agree even on public policy issues—they may more often disagree with Jim Buchanan. But if Milton Friedman, or George Stigler, or Ted Schultz—to mention just a few equally notable luminaries—had happened to be present in this year's sample, the disagreement/agreement ratio would have increased quite sharply. And we could perhaps liven up the discussion even further by adding Bob Lucas or Sam Bowles. I have not even begun to exhaust the spectrum of policy views we can find within the economics profession.

Although you may have seen relatively little disagreement among the authors in this volume, the public is not mistaken in its perception that economists disagree frequently and vociferously. The fact of the matter is that, no matter what our views on matters of public policy, we can almost certainly find a relatively prominent economist to defend them; and this democratic indiscipline of the profession is well known to all readers of the daily press. We have Keynesian economics, monetarism, supply-side economics, rational expectations, and budget balancing, not to mention free trade, protection for infant industries, and proponents of the income tax, the single tax, the sales tax, and almost no taxes at all.

When national economic problems are under discussion, economists disagree as to whether to give priority to inflation, the public debt, employment, taxes, the trade balance, or the rate of interest. Clearly, these priorities have much to say about what policies economists will advocate. During the late seventies, there was a massive shift in primary concern from employment to inflation, but the shift was not unanimous and left the profession in as much disarray as before—or more. Likewise, it is easy to start a lively argument about whether taxes should be raised in order to stem the rise in the national debt, or whether the burden of the national debt on the federal budget should be relieved by lowering interest rates.

And finally, economists disagree about their forecasts for the economy over the coming months or years. We can almost always find optimists, pessimists, and flat-planers, although their relative numbers vary from

time to time. Nor do these proportions much correlate with what the economy actually does. Economic forecasting is in low repute, often also among economists, who can even provide theoretical reasons why it should be impossible to predict the course of the business cycle or the stock market. The last recession saw a high casualty rate among economic forecasting units attached to business firms—at a time of budget crunch, they were seen as dispensable.

With this cacophony of economic voices, it is not surprising that the status of economics as a science is sometimes challenged by a bemused public. Isn't a science supposed to provide accurate predictions about its phenomena? Is economics therefore not a science? We had best reserve our judgment a bit until I get on with my story. We are aware that meteorologists also disagree widely in their forecasts, but we would not doubt that meteorology is a science. For meteorologists have built a model of the atmospheric processes that is consistent with the basic, well-supported, laws of physics. Possessing such a model does not guarantee an ability to predict the future of the weather with any great accuracy.

In recent years, there has even been a very exciting development in the field of mathematics that suggests that certain complex systems (systems of nonlinear equations) may behave in a way that makes them inherently unpredictable. Such systems are called "chaotic," and their central characteristic is that a tiny displacement of their initial conditions sometimes causes them to go off on completely divergent paths. We don't know whether the atmosphere is actually a chaotic system, but if it is, the wingbeats of a butterfly in Singapore may cause a tornado in Chicago.

We also don't know whether the economy is a chaotic system. There are some statistical tests that can be applied to time series—the money supply, or stock prices, or other economic series—to distinguish between simple randomness and the kind of chaos and unpredictability I am describing here. Some attempts have been made recently by economists to test whether various economic time series are chaotic, but the results so far are inconclusive. (The September 1987 issue of the *Journal of Economic Behavior and Organization*, Volume 8, Number

3, which is devoted to the subject of chaos in dynamic economic systems, will provide the reader with a good introduction to the topic.) I mention chaos to indicate that we should be wary of using prediction as a test of science, and especially of whether economics is a science, for an understanding of mechanisms does not guarantee predictability.

### *Private Agreement Among Economists*

If it is well known that economists disagree, it is less well known to the general public, although perhaps not among economists, that there is a very high level of *agreement* among them. I will try to explain the apparent paradox and show why this is so. In spite of the disagreements I have enumerated, economists subscribe, nearly to a man and woman, to a common central core of theory, and above all, to a way of reasoning about economic questions. Before I state what the common core is, let me give an example of the kind of agreement that this communality produces.

Suppose that during a period of gasoline shortage, the government issues ration tickets to the poor to guarantee them an adequate supply of gasoline at a moderate price. Should the law be written so that the poor may sell their ration tickets to others if they would prefer the money to the gasoline, or should such exchanges be prohibited by law?

When this question was presented to a national sample of lay persons, about 80 percent replied that the exchange should be prohibited—that the poor should be able to use the tickets only to purchase gasoline. When the same question was presented to a national sample of economists, virtually all responded that the exchange should be allowed.<sup>1</sup>

The reason economists are able to agree on this conclusion is that they apply to the problem a common set of assumptions and inference procedures. All (or nearly all) economists assume that every person is a utility maximizer: (1) possesses a consistent, comprehensive utility function that orders all the possible alternatives of choice, and (2) selects among alternatives so as to maximize subjective expected utility. “Subjective expected” means that the decisionmaker assigns (sub-

jectively) probabilities to all possible outcomes of each available alternative, and averages the utilities of these outcomes, using the subjective probabilities as weights. Economists (nearly all economists) claim that people do behave so as to maximize utility, and that they *ought* to behave in this way, which they regard as uniquely rational.

Now in the rationing case, if the potential seller and buyer both agree on a price for the tickets, this must mean—using the standard economic reasoning—that each one thinks that his or her utility will be increased by the exchange. Hence, they will make the exchange if they are permitted to, and they should be permitted to because then everyone will be better off. When certain other conditions are also satisfied, the exchange is said to produce a *Pareto optimum*, a situation that could not be altered in any way to produce an increase in utility for *both* participants. Economists also refer to solutions of decision problems that are Pareto optimal as producing an *efficient allocation of resources*.

My account of the core of standard economic theory is, of course, something of an oversimplification. Many of the conclusions, particularly the normative ones, depend on the economic system being in a stable state of competitive equilibrium, and only under these conditions can a Pareto optimum guarantee an efficient allocation of resources. My simplified version, however, gives a clear enough picture of the nature of contemporary economic reasoning that is subscribed to by almost every school of thought.

Even Marx, in *Das Kapital* subscribes to this theory and this way of reasoning. So does Keynes; for the *General Theory* uses, on every page, standard economic reasoning. So do the rational expectationists, like Lucas and Sargent, and the monetarists, like Friedman. So do my predecessors in this volume. The allegiance of economists of all sects and descriptions to subjective expected utility maximization is hardly less unanimous than the allegiance of physicists to Newton's Three Laws of Motion in their relativised form.

### ***Disagreement From Consensus***

Now I appear to have hooked myself on a serious dilemma. If economists agree in the way I have just outlined, how can they disagree

in the ways I described earlier? They can disagree because, in order to apply the standard core theory to real-world problems, that theory has to be augmented by additional assumptions, especially assumptions about what information people have, and how they deal with uncertainty and with their own limited capacities for computation. Economists disagree about these auxiliary assumptions; and as a result of the disagreement, reach very different conclusions even though applying identical modes of reasoning. That is not surprising: conclusions depend on premises. Using identical laws of logic, we will reach quite different conclusions about the visibility of swans in dim light from the premise “All swans are white” than from “All swans are black.”

Let me provide an example of the sources of disagreement in economics from a central topic of macroeconomics: business cycle theory. The theory of static equilibrium under perfect competition developed by Arrow, Debreu, and others proves that, under equilibrium conditions, all resources will be fully employed: labor, capital, and so on. However, most (not all) economists concede that the real world is plagued by a fluctuating level of employment of both labor and production facilities, with periods, from time to time, of substantial unemployment. How can we, within the framework of the accepted theory and method of reasoning, reconcile general equilibrium theory with the fact of unemployment and underemployment of resources. We can do it by making one small change in the assumptions of rationality—by introducing one small grain of irrationality or nonrationality into behavior, and allowing that grain to grow into the pearl of unemployment of resources.

Keynes uses several mechanisms to produce this result; I will refer to just one of them. Starting with a standard classical model, he assumes that labor is not always quite rational, specifically, that in bargaining for wages, labor sometimes confuses the money wage, in current prices, with the real wage, adjusted for price changes. Labor is just a little stupid, and this stupidity draws the system away from full-employment equilibrium into a situation in which unemployment can appear and persist.

Lucas, a rational expectationist usually regarded as standing at the opposite end of the policy spectrum from Keynes, also admits that there

is sometimes unemployment in the real world. He too starts out with a model of perfect rationality, which of course leads to full-employment equilibrium. He also introduces a grain of sand to displace that equilibrium and produce unemployment. His grain of sand consists in assuming that businessmen are not always quite rational, specifically, that they sometimes stupidly mistake a shift in the general price level for a change in relative prices in their own industry. This mistake (from lack of knowledge or ignorance in calculation) draws the system away from full-employment equilibrium into a state where there is unemployment.

So both Keynes and Lucas depart from the assumptions of perfect rationality in order to account for a fact of the real world, the fact of periodic unemployment. Both introduce a grain of irrationality in the form of a confusion between monetary and real prices—what economists usually call “money illusion.” In the case of Keynes, the liberal, the money illusion is suffered by labor; in the case of Lucas, the conservative, it is suffered by businessmen. Presumably we know our friends better than we know strangers! Not only do they introduce irrationality in different ways, but they draw different conclusions, as a consequence, as to what policies should be followed to deal with the resulting unemployment.

In understanding the disagreement between Lucas and Keynes, it is important to remember where it comes from. It does not come from the central theory of rational economic behavior, on which they both agree, but from the auxiliary assumptions—the grains of sand—on which they disagree. That might lead us to ask where these auxiliary assumptions come from. They are empirical assumptions, assumptions about how particular groups of human beings behave in the real world, about what they know and don’t know and how they use that knowledge or are affected by that ignorance. But you will search the pages of Keynes and Lucas vainly if you want to know the evidence from which these crucial premises are drawn. They are presented simply as “obvious” facts, as something “everyone knows.” A dangerous procedure for a science that purports to give an account of the real world!

It is not only in business cycle theory that the auxiliary assumptions do all the work of deriving the conclusions. Here is an example drawn

from microeconomics. In recent years, we have seen a kind of economic imperialism in which microeconomists seek to demonstrate that they can use the standard theory to explain not only strictly economic phenomena but the phenomena of politics, the family, and indeed of all of our social institutions as well. A vigorous and well-known leader of this intellectual imperialism is Gary Becker, of the University of Chicago; and James Buchanan also belongs to this school of thought, especially as it applies to political institutions.

Let us see how Becker, in his book on the family, deals with the observed fact that the employment of women has increased rapidly in recent years. Becker explains that this is due to the fact that the demand curve for women employees has shifted upward during this same time—more women will be employed at any wage, and hence wages will rise as more women are attracted from the home to the labor market. Becker cites no particular evidence for the shift in the demand curve, and indeed, it is not clear that during the period he is concerned with, the wages of women increased much more than the wages of men.

Moreover, there are alternative explanations we could consider. Perhaps the utility function of women has shifted, for example, as a consequence of consciousness raising, so that they prefer outside work to homework more than they did before. Or perhaps the productivity of homework has increased with the availability of household appliances, while the demand for it is relatively inelastic. All of these shifts would cause an increase in the number of women employed outside the home, and we are far from exhausting the list of possible explanations.

Again we see that the conclusion (which matches an observed fact) is not a consequence of the theory of rationality. The real work is done by the auxiliary assumptions, and without additional empirical evidence (which Becker does not bother to provide), we do not know which of the possible auxiliary assumptions is actually satisfied, and hence is causing the phenomenon. Without taking seriously the task of verifying the auxiliary assumptions empirically, we can find an “explanation” for any fact; our theory becomes tautological and vacuous. As Poincare once remarked about another theory: “An explanation was necessary, and was forthcoming; they always are; hypotheses are what we lack the least.”

In economics, hypotheses are certainly what we lack the least. Auxiliary hypotheses, frequently manufactured in the armchair, are in good supply. What is lacking is empirical evidence to test them. Why don't economists devote themselves more vigorously to testing their auxiliary hypotheses? I can only conjecture (the question is one that should also be settled by facts, historical and sociological facts about the economics profession).

First, there is a tradition of a priorism and deductivism in economics, which goes back at least to Adam Smith (although Smith was not reluctant to take facts where he could find them). It is more fun, and easier, to play with theory, especially mathematical theory, than to grub for facts. As long as the tradition persists in the profession, editors will accept analyses based on postulated facts or facts tested by casual empiricism.

Second, a few philosophers of economics, for example Boland, think that the standard theory is true by definition, and not empirically refutable. Friedman holds a related position, arguing that economic theory can only be tested by assessing the empirical correctness of its "predictions," and not by testing the correctness of its "assumptions." I have not the time here to argue the fallacies in these positions, which have been pointed out by numerous economists on other occasions. I mention these erroneous beliefs simply to account for the relative indifference of economists to empirical evidence.

Third, economists of course do carry out a very considerable amount of empirical work to test their theories, but they employ only a narrow range of methodologies in doing so. As a result, they spend much of their time viewing the world through the wrong end of a telescope, with correspondingly foggy results. By "the wrong end of a telescope" I mean that economists usually test their theories with the help of rather aggregated data about the economy which have mainly been gathered for practical purposes unconnected with economic theory and whose concepts and units bear only a faint resemblance to the concepts to which they are matched in the theory.

Robert Eisner has described these difficulties eloquently in his recent presidential address to the American Economic Association, which

appears as the lead article in the March 1989 *American Economic Review*, 79:1-14. His summary (p. 11) displays the seriousness of his concerns:

I have . . . noted some pitfalls in using current and past variables in analyses that depend critically upon expectations of the future. I have warned of confusion in employing narrowly defined measures of income and product in evaluating flows and trends in comprehensive earnings and output. I have argued that particularly large dangers abound in basing policy on conventional measures of private and public saving, investment and capital. I have suggested that usual estimates of some of the critical behavioral relations of macroeconomics may be suspect because of a failure to match theoretical constructs with appropriate empirical counterparts.

Very generally, I conclude, it is important in economics—as elsewhere—to know what we are talking about.

A very sophisticated body of econometric theory and methodology has been created since World War II to deal with the problem of carrying out economic analyses with data that are too aggregated, measure the wrong concepts, are too infrequently collected, and are exceedingly noisy. The experience of the past 50 years has shown that these sophisticated econometric tools do not compensate for the poor quality of the data to which they are applied.

Other kinds of data can of course be obtained; data that tell us something about the expectations, preferences, and choices of economic actors at micro levels. Opinion and attribute studies can be conducted (and of course have been). Actual decisionmaking processes can be observed in business organizations (and of course have been). But polling data and “case studies” of individual behavior are mistrusted in orthodox economic methodology. The use of these kinds of data to discover the realities of economic life and to provide an empirical basis for the auxiliary assumptions we need will require a revolution in methodological beliefs in economics, and a revolution in the training of economists, who are not now trained to use these techniques.

## Validity of the Standard Theory

If casual empiricism and the neglect of available sources of empirical evidence are a major scandal in contemporary economics, equally scandalous is the retention of beliefs, central to the standard theory, that are empirically false. For when investigators have begun to examine, in field and laboratory, the central assumptions of the standard theory—not just the auxiliary assumptions this time, but the very core of the theory—it has turned out that these assumptions do not fit the facts.

For example, research in the 1950s, reported in *The Behavioral Theory of the Firm* and elsewhere, shows that business firms are not the simple profit-maximizing entities that the standard theory assumes them to be. They sometimes settle for “satisfactory” profits. They operate with slack that is only reduced under the pressure of hard times. They suffer from internal power struggles. They simply do not behave as required to meet the conditions of competitive equilibrium with efficient use of resources.

Second, an increasing number of studies of experimental markets, operated under the controlled conditions of the laboratory, have shown that it is easy to construct market situations where behavior will depart widely from the predictions of the standard theory. Vernon Smith, Charles Plott, and others have shown that behavior tends to settle toward the predicted market equilibrium in simple markets, but that it is not at all difficult to produce booms and busts even under conditions where participants could easily compute the correct, rational behavior that would produce stability and equilibrium.

Third, a long series of studies by Allais, Kahneman, Tversky, and others have shown serious departures from utility-maximizing rationality when choices are made under conditions of uncertainty. Respondents, for example, opt much less frequently for a surgical operation where there is a 20 percent chance of dying than for an otherwise identically described operation where there is an 80 percent chance of living.

The sum and substance of these findings is that people just do not maximize utility. They do not have consistent utility functions (Allais). They do not reason correctly and consistently about probabilities and

risks (Kahneman and Tversky). They use rules of thumb (heuristics) to simplify choice (Cyert and March). They look for satisfactory courses of action, they satisfice, instead of optimizing (Simon).

Nature is supposed to abhor vacuums, and the demonstration that a theory is empirically false, as the standard theory of economic choice most certainly is, appears to leave a large vacuum. Is there anything to fill it? Indeed there is. Over the past 30 years, cognitive psychology has developed a large body of empirically tested theory of decision-making and problemsolving. So far, economists have mostly ignored it.

The theory of decisionmaking to be found in psychology today substitutes satisficing for optimizing. It gives central position to the limits of human ability to deal with real-world complexity, and to the methods of radical approximation that are used to remove this mismatch of adaptive power to problem difficulty. Enlarging the scope of economic decision theory, it does not assume that the alternatives of choice are given *a priori*, but shows how they can be generated by selective (heuristic) search in problem spaces. It is beginning to investigate the processes that focus human attention on certain problems so that others are ignored, and the processes of representing or framing problems in terms of their most important dimensions, omitting those that are less important.

The research that has produced the new theory has also produced new methodologies that are relevant for economic research. Methods have been devised to use computers to simulate complex human thought processes and thereby to model problemsolving and decisionmaking situations of real-world complexity. The theory and practice of taking thinking-aloud protocols from human subjects while they are thinking and solving problems has been developed and explored.

Of course these developments do not mean that the problems of economics have been solved and that economists need merely to replace their standard theory with the new theory and apparatus. Certainly few of the economic implications of the new theory have been worked out. But a vacuum does not exist, and would not exist if the standard theory, or its invalid components, were simply abandoned. There is a firm foundation on which economic research can proceed.

## What is to be Done?

I have described what I believe to be the serious problems facing economics and a path toward their solution. Both the auxiliary assumptions and the central postulates of classical economics must be revised on the basis of sound empirical evidence. To get the proper evidence, economists must receive new kinds of research training, much of it borrowed from cognitive psychology and organization theory. Doctoral candidates must learn how to obtain data about beliefs, attitudes, and expectations. They must learn how to study decisionmaking processes inside and outside business organizations. They must learn how to analyze and interpret the verbal reports of human subjects in both laboratory and field situations. They must learn how to conduct laboratory experiments.

Increasing numbers of mainline economists are beginning to perceive and accept the need for these reforms. I have already mentioned some of them, and I could add many names to the list. But there is a long way to go.

The inability of economics today to play the policy role to which it aspires is a major source of pressure toward reform. The hubris of the 1950s, when many economists thought we knew how to fine tune the economy, has disappeared in the face of stagflation and the ideological struggles between Keynesians and rational expectationists. Nevertheless, the reform will not be rapid.

As it progresses, however, the alteration of economics toward a responsible empiricism will gradually remove the present scandals in economic science: not just the public scandal of disarray in the policy arena, but the private scandal, known best to economists themselves, of a science that nearly renounced the practice of looking hard at the empirical evidence that was supposed to provide its foundations.

The late Tjalling Koopmans, a distinguished econometrician and a Nobel Laureate in economics, warned, in a celebrated book review against “measurement without theory.” The remarks of Robert Eisner, quoted earlier, echo the same concern. The concern for economics that I have emphasized in my remarks is the complement of that one—theory

without measurement. What distinguishes science from every other form of human intellectual activity is that it disciplines speculation with facts. Theory and data are the two blades of the scissors. But the metaphor is not quite right, for the blades are not symmetric. When theories and facts are in conflict, the theories must yield. Economics has strayed from that simple principle, and it must return to it.

### NOTE

1. I have only a newspaper reference to the original survey, which was conducted in Canada. I have repeated the finding by polling several audiences at my talks, but I must confess that when I tried to replicate the result during my talk in Kalamazoo, a majority of the noneconomists present voted like economists. Perhaps most of my audience had been taking economics courses from a very persuasive faculty.