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Economic Methodology in a Nutshell

Daniel M. Hausman

The literature on economic methodology is concerned mainly with questions of theory confirmation or disconfirmation or empirical theory choice. The central question is usually, “How one can tell whether a particular bit of economics is good science?” Economists would like methodologists to provide the algorithm for doing good economic science—and they want the algorithm to vindicate their own practice and to reveal the foolishness of those who do economics differently. For example, Milton Friedman (1953) tells economists that good theories are those that provide correct and useful predictions, while Paul Samuelson (1947, 1963) tells economists to formulate theories with “operational” concepts that are, ideally, logically equivalent to their descriptive consequences.

In my view, not only are these (and most other) specific views on theory appraisal mistaken, but the concern with problems of empirical appraisal is exaggerated, for there are also interesting methodological questions to consider—both normative and descriptive—concerning the structure, strategy, goals and heuristics of various economic theories. For example, few writers on economic methodology recognize that the activities of formulating economic models and investigating their implications are a sort of conceptual exploration. Instead, most mistakenly regard these activities as offering empirical hypotheses and assess them in terms of some philosophical model of confirmation or falsification.

As a tendentious survey of standard methodological literature, this essay will, however, share its preoccupation with the empirical appraisal of theory, microeconomic theory in particular. I shall in particular discuss four approaches to praising or damning microeconomic theory that have dominated methodological discussions.

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They might be called the deductivist, the positivist or Popperian, the predictionist, and the eclectic. Or to assign representative or striking figures to positions, these are John Stuart Mill's, Mark Blaug's, Milton Friedman's, and Donald McCloskey's views. I shall sketch and assess each position and defend aspects of the deductivist and eclectic views. Along the way I shall have something to say not only about how to do economics, but also about how to philosophize about economic methodology.

Deductivism

John Stuart Mill was both a Ricardian economist and a staunch empiricist, yet his economics seems not to measure up to empiricist standards for knowledge. After all, the implications of Ricardian economics appeared to be disconfirmed (de Marchi, 1970); for example, the share of national income paid as rent did not increase. How could Mill reconcile his confidence in Ricardian economics and his empiricism?

In Mill's view (1836, 1843, bk. 6), a complex subject matter like political economy can only be studied scientifically by means of the deductive method. Since so many causal factors influence economic phenomena, and experimentation is generally not possible, there is no way to employ the methods of induction directly. The only solution is first inductively to establish basic psychological or technical laws—such as “people seek more wealth,” or the law of diminishing returns—and then to deduce their economic implications given specifications of relevant circumstances. Empirical confirmation or verification has an important role in determining whether the deductively derived conclusions are applicable, in checking the correctness of the deductions and in determining whether significant causal factors have been left out, but such testing does not bear on one's commitment to the basic “laws.” They have already been established by introspection or experimentation. Political economy is in this regard similar to the science of tides, which applies independently established laws.

Mill believed that these established premises state accurately how specific causal factors operate. They are obviously not universal laws; for example, everyone does not always seek more wealth. These basic generalizations are instead statements of tendencies. Since these tendencies are subject to various “disturbances” or “interfering causes,” which cannot all be specified in advance, vague *ceteris paribus* (other things being equal) clauses that allow for these disturbances will be unavoidable in formulating them. Economics explores the consequences of these established, but inexact, premises. Since much is left out of the theory, these consequences will not always obtain.

In Mill's view economics is a science, for economists do know the basic causes of economic phenomena. But it is an inexact science, for there are myriad interferences or disturbing causes. Mill's views are almost the opposite of Milton Friedman's, for Mill holds that the confidence of economists in the science of political economy is based on direct and rather casual confirmation of its assumptions, not on serious tests

of their implications. Not only were Mill's views adopted by followers such as Cairnes (1888) and early neoclassical methodologists such as John Neville Keynes (1890), but if one updates the language and the economic theory, one has the view to which, I suggest, most orthodox economists (regardless of what they may say in methodological discussion) still subscribe (see also Stewart, 1979).

The transition from classical to neoclassical economics brought not only changes in economic theory, but methodological changes as well, for neoclassical theory focuses much more on individual preferences and decision making than did classical economics. Despite this difference, which was much emphasized by authors such as Frank Knight (1935, 1940), Ludwig von Mises (1981), and Lionel Robbins (1935), early neoclassical economists agreed with Mill that the basic premises of economics are well-justified, and that empirical failures do not cast them into doubt. In defending this view, Lionel Robbins (1935, p. 121) explicitly notes his intellectual debts to Mill, and, by exaggerating the obviousness of the basic assumptions of neoclassical microeconomics, he provides a particularly persuasive formulation of what is essentially Mill's view (pp. 78–9):

The propositions of economic theory, like all scientific theory, are obviously deductions from a series of postulates. . . . The main postulate of the theory of value is the fact that individuals can arrange their preferences in an order, and in fact do so. The main postulate of the theory of production is the fact that there are [sic] more than one factor of production. The main postulate of the theory of dynamics is the fact that we are not certain regarding future scarcities. These are not postulates the existence of whose counterpart in reality admits of extensive dispute once their nature is fully realised. We do not need controlled experiments to establish their validity: they are so much the stuff of our everyday experience that they have only to be stated to be recognized as obvious.

Although Robbins overstates his case, I think that he is basically right.

Positivist or Popperian Views

To anyone familiar with the methodological literature of the last half-century, such a complacent view of the deductive method must seem perverse. For the theme which has dominated this period is that claims that are hedged with qualifications and *ceteris paribus* clauses are untestable and uninformative. What Mill or Robbins called "tendencies" or "inexact laws" are qualified claims such as, "In the absence of disturbances or interferences, people prefer more wealth," or "*Ceteris paribus*, returns to variable inputs will diminish." Since the content of the *ceteris paribus* clause is not fully specified, it seems that these statements are unfalsifiable and lack definite empirical

meaning. Either things are as claimed by the tendency, or there is some disturbance. No outcomes are prohibited, and new evidence never requires economists to alter their beliefs about the basic tendencies.

Fifty years ago, under the influence of logical positivism, Terence Hutchison made essentially just this charge. The statements of “pure theory” in economics are empty definitional or logical truths, he argued, and even applied claims are so hedged that they lack content (1938, esp. ch. 2). Hutchison insisted that economists should start behaving like responsible empirical scientists. Thus, under the guiding star of logical positivism, and later on of Karl Popper (1959), began the first and only major change in economists’ official position on the appraisal of microeconomics.

As Hutchison himself partly recognized (1938, ch. 2), this critique can be answered from within the Millian tradition. For one need not regard or employ *ceteris paribus* clauses as blanket excuses (Hausman, 1981a, ch. 7; 1981b). *Ceteris paribus* clauses are part of almost all of science. Rather than condemning them all, one needs to distinguish when one may legitimately employ them and to recognize that rough generalizations can have worth and content despite their vagueness and imprecision. I learned something useful when I was taught that aspirin cures headaches, even though (alas!) this generalization is not a universal law.¹

Hutchison’s attack was still disquieting. Did neoclassical microeconomic theory measure up to the standards for science defended by contemporary (positivist) philosophers of science? Those who first rose to answer Hutchison’s challenge, such as Frank Knight (1940), may have aggravated rather than allayed this disquiet, for Knight explicitly repudiated the empiricist or positivist philosophy of science upon which Hutchison’s challenge relied. Knight accuses the positivists of overlooking the complexity and uncertainty of testing in all sciences (1940, p. 153) and argues at length that positivist views of science are particularly inappropriate to economics, which, like all sciences of human action, must concern itself with reasons, motives, values and errors, not just causes and regularities. Younger and less philosophically ambitious economists might well have wondered whether there was any way to respond to Hutchison without thus repudiating up-to-date philosophy of science. Indeed, in his review, Knight worries about the pernicious effect Hutchison’s book may have on the young (1940, pp. 151, 152). Positivistic recastings of economic theory, such as Samuelson’s (1947) “operationalism” and particularly his revealed preference theory, which appeared to provide a behaviorist reduction of talk about preference and utility to observable claims about actions, were beginning to appear. Were they the way of the future? Did logical positivism make traditional neoclassical theory untenable?

Indeed, similar challenges to contemporary economic practice continue in works such as Mark Blaug’s *The Methodology of Economics* (1980), which argue that neoclassical economics does not meet Popperian or positivist standards for science. Could the

¹I owe this example to Sidney Morgenbesser. Although rough generalizations have statistical implications, they are also not well construed as statistical laws.

profession's high regard for microeconomic theory be squared with the demand that good science be well-confirmed by empirical data?²

After World War II, qualms about the empirical standing of microeconomic theory grew, when economists such as Richard Lester attempted to test fundamental propositions of the neoclassical theory of the firm (1946, 1947). Lester's tests, which consisted of surveys sent to various businesses, were not well-designed. But they attracted attention and provoked fierce responses (especially Machlup, 1946, 1947; Stigler, 1947), partly because everybody knew that Lester was right about one thing: Firms do not behave precisely as marginal productivity theory maintains they do. Indeed one of Lester's sharpest critics, Fritz Machlup, conceded (1956, p. 488), "But we would certainly not find that all of the businessmen do so [maximize profits] all of the time. Hence, the assumption of consistently profit-maximizing conduct is contrary to fact." But does it not follow immediately that neoclassical theory makes false statements and is thus on positivist and Popperian standards inadequate?

Although some, such as Knight and the Austrians, were prepared to deny that the standards of the natural sciences apply to economics, most tried to show that economics satisfies all reasonable demands that one may make of a science. Fritz Machlup's essays (1955, 1960) give some idea of such attempts. Machlup argues that microeconomic theory is compatible with later and more sophisticated logical positivist (or "logical empiricist") accounts of the nature of science, which considerably loosen the connection that is required between theory and observation. Machlup argues that both instrumentalists and defenders of "partial interpretation" views recognize that one need not be concerned about the truth of a theoretical claim such as profit maximization. But the philosophers who defended instrumentalism and "partial interpretation" views were concerned to show how theories that make claims about unobservable entities and properties and thus cannot be directly empirically tested might nevertheless be meaningful and indirectly testable. They never suggested that one should ignore the falsity of a claim—such as "all firms attempt to maximize profits"—on the grounds that such a claim is "theoretical."

Predictionism

The most influential way of reconciling economics and up-to-date philosophy of science was, however, not Machlup's, but Milton Friedman's. In his famous essay, "The Methodology of Positive Economics" (1953), Friedman offered the apparent

²Given positivist or Popperian standards, the answer is "no." But no science meets these unreasonable standards. In contrast to Samuelson (1963), the early positivists, and (to a lesser extent) Popper, neither the truth nor falsity of theoretical claims can be inferred directly from observation reports. Contrary to Popper's views, intelligent testing requires (inductive) knowledge of how well supported statements are by evidence. Given how poorly supported are the various auxiliary statements needed to derive predictions from economic theories, it is usually not sensible or responsible to follow Blaug's Popperian advice and to regard predictive failures as falsifying economic theories. For a more comprehensive and accurate critique of Popperian economic methodology than is possible here see my 1988(a).

way out of the empirical difficulties raised by Lester and others that has proven most popular with economists. It is that apparent way out, not the possible intricacies of Friedman's views, with which I shall be concerned. Although Friedman does not refer to contemporary philosophy of science, he too attempts to show that economics satisfies sophisticated positivist standards.

After distinguishing between positive and normative economics, Friedman begins by asserting that the goals of a positive science are predictive, not at all explanatory (1953, p. 7). Economists seek significant and usable predictions, not understanding or explanation. The view that science, or at least economic science, aims only at prediction is a contentious one, and one for which Friedman offers no argument. It might reasonably be challenged. But in holding this instrumentalist view of the goals of science, Friedman is in good philosophical company and not obviously mistaken (see Morgenbesser, 1969). Since Friedman's methodological views are untenable even if one grants his claim that the goals of economics are exclusively predictive, let us not contest it here.

In Friedman's usage, any implication of a theory whose truth is not yet known counts as a prediction of a theory, even if it is not concerned with the future. Since the goals of science are exclusively predictive, a theory which enables one to make reliable predictions is a good theory. In case of a tie on the criterion of predictive success, simpler theories or theories of wider scope (that apply to a wider range of phenomena) are to be preferred (p. 10).

Friedman stresses that there is no other test of a theory in terms of whether its "assumptions" are "unrealistic" (p. 14). When Friedman speaks of the "assumptions" of a theory, he includes both fundamental assertions (such as the claim that consumers are utility maximizers) and additional premises needed in particular applications (for example, the claim that different brands of cigarettes are perfect substitutes for one another). Although Friedman equivocates with the term "unrealistic," usually he means (as he must if he is to respond to Lester's challenge) that an assumption is unrealistic if it is not true, perhaps not even approximately true, of the phenomena to which the theory is applied.

Friedman can then argue that researchers such as Lester mistakenly attempt to assess the "assumptions" of neoclassical theory instead of its predictions. In dismissing any assessment of assumptions, Friedman is also responding to a critical tradition which extends back to the German Historical School via American Institutionalists, such as Veblen. This critical tradition questions the worth of abstract theorizing and objects to the purportedly unreasonably unrealistic assumptions of neoclassical theory. Friedman apparently enables one to reject all such criticism as fundamentally confused.

But Lester's case cannot be dismissed so easily, for Lester apparently showed that neoclassical theory makes false predictions concerning, for instance, the results of his surveys. The distinction between assumptions and implications is, indeed, a shallow one that rests on nothing but the particular formulation of a theory. Assumptions trivially imply themselves, and theories can be reformulated with different sets of assumptions that have the same implications. Unrealistic assumptions (in the sense of

false assumptions) will always result in false predictions, except, perhaps, in the case of assumptions concerning unobservables.

Friedman notices the problem (pp. 26–7) and responds to it by insisting that all that matters is how well a theory predicts the phenomena in which economists are (at least on the particular occasion) interested (pp. 20, 27–28). This odd instrumentalism suggests that falsity of assumptions or of predictions is unimportant unless it detracts from a theory's performance in predicting the phenomena in which one is interested. A theory of the distribution of leaves on trees that states that it is as if leaves had the ability to move instantaneously from branch to branch is thus regarded by Friedman as perfectly "plausible" (p. 20), although of narrower scope than accepted theory. If a theory predicts accurately what one wants to know, it is a good theory, otherwise it is not.

When Friedman says that it is as if leaves move or as if expert billiard players solve complicated equations (p. 21), he means that attributing movement to leaves or calculating power to billiard players leads to correct predictions concerning the phenomena in which one is interested. And a theory which accomplishes this is a good theory, for a "theory is to be judged by its predictive power for the class of phenomena which it is intended to explain" (p. 8). Friedman is not just saying that if a theory "works," then one should use it, but that all one wants of science are theories that work for particular purposes. The realism of the assumptions of microeconomics or the truth of its uninteresting or irrelevant implications is unimportant, except insofar as either restricts the theory's scope. Since economists are not interested in what business people say, but in the consequences of what they do, Lester's surveys are irrelevant.

Yet even if one fully grants Friedman's view of the goals of science, one should still be concerned about the realism of assumptions. For there is no good way to know what to try when a prediction fails or whether to employ a theory in a new application without judging one's assumptions. Without assessments of realism (approximate truth) of assumptions, the process of theory modification would be hopelessly inefficient and the application of theories to new circumstances nothing but arbitrary guesswork. The point is simple: if one wants to use a machine in a new application or to build a new machine out of its components or to diagnose a malfunction, it helps to know something about the reliability of the components of which it is made. Even if all one wants of theories are valid predictions concerning particular phenomena, one needs to judge whether the needed assumptions are reasonable approximations, and one thus needs to be concerned about incorrect predictions, no matter how irrelevant.

I have dwelled on Friedman's views because of their influence and because they illustrate a paradox. Friedman's confidence in "the maximization-of-returns hypothesis" and in neoclassical theory in general purportedly rests entirely on "the repeated failure of its implications to be contradicted" (p. 22; but see pp. 26–30 on indirect testing). On this, Friedman is at one with Popperian methodologists such as Blaug. But the implications of neoclassical theory have certainly been contradicted on many occasions. This would be so even if the theory lived up to its highest praises. All it

takes is some disturbance, such as a change in tastes, a new invention or a real or imagined invasion from Mars.³ Does any economist really accept neoclassical theory on the basis of “the repeated failure of its implications to be contradicted”? Is this not rather a doctrine piously enunciated in the presence of philosophers or of their economist fellow travellers and conveniently forgotten when there is serious work to do?

Those who have noticed that economists do not practice what they preach have most often attacked the practice. Instead of attempting to discover what methodology neoclassical economists actually practice and to think seriously about how that methodology might be justified, these critics (with some notable exceptions, such as Simon, 1976) have usually relied on indefensible philosophical theories of science to support broad condemnations (for example, Blaug, 1980). There is, of course, nothing mistaken about judging the work of economists—normative concerns are central to economic methodology. But most of these judgments have relied on unreasonable standards that were supposedly vindicated by up-to-date philosophical insight. Philosophers have, however, little to offer by way of informative well-supported systematic theories of the scientific enterprise and that little does not lend itself to mechanical application.

Eclecticism

Many have by now recognized that there are few good philosophical authorities on matters of theory assessment. Although there is still a great deal to be learned from the judicious study of contemporary philosophy of science, those interested in economic methodology must use their own judgment and their knowledge of the practice of economists to formulate and to defend rational standards for the practice of economics. The situation of a methodologist concerned about understanding and improving economic practice is similar to that of an economist concerned with understanding and improving business practice. Although both may find some of the practices they study mistaken or irrational, both had better show some sense and caution in applying general theories and had better understand thoroughly the actual problems and procedures of the object they study.

Attempts to carry out such a delicate task have varied. Alexander Rosenberg's *Microeconomic Laws: A Philosophical Analysis* (1976) is something of a watershed. In the

³Objections that readers have voiced to these examples instructively support my point. One objected that neoclassical theory obviously allows for “shocks.” But, unless it does so by means of a not-fully-specified *ceteris paribus* clause, there will still be refutations of the kind cited. And if not-fully-specified *ceteris paribus* clauses are permitted, the “repeated failure of its implications to be contradicted” is a cheap triumph. Another reader objected that better examples are those in which the assumptions involved in the particular application of the theory are satisfied. I agree, but this is certainly not a line that Friedman or others who rest everything on the success of predictions can follow. For we are not supposed to pay any attention to whether the assumptions are satisfied—that is, to whether the assumptions are “realistic” for the situation at hand. There are examples in which predictive failures are more puzzling and disturbing than in the cases cited in the text. Consider the fact that even in inflationary circumstances many firms evaluate their inventories on a first in, first out basis or the fact that shares in closed-end mutual funds sometimes sell for less than the value of the assets of the funds (Stiglitz, 1982).

decade since publishing this book Rosenberg's own views have shifted drastically; he denied at one point (1983) that economics is an empirical science at all. But in publishing his first book, and especially in his discussion there of particular aspects of economics, such as the relations between micro- and macroeconomics or the sense in which explanations in economics involve both reasons and causes (chapter 5), Rosenberg is responsible for a growing literature on economic methodology by philosophers of science. This literature is distinctive in its attention to the details of methodological practice and in its cautious use of philosophical models of science.

Among economists the best-known authors in this more eclectic and empirical vein are probably Bruce Caldwell (1982) with his "methodological pluralism" and Donald McCloskey (1985) with his "rhetoric of economics." I do not yet find Caldwell's methodological pluralism to be a clear philosophical position. Sometimes it seems to be intended as the thesis that different economic methodologies must be assessed entirely in their own terms and that no more than internal coherence is to be demanded. But I think that Caldwell should be interpreted more charitably, not as abandoning the normative tasks of economic methodology, but as recognizing that they cannot come first. Since philosophers of science have no gospel for scientific practice, economic methodologists have no prepared sermons. Cast among the heathen, bereft of revealed truth, methodologists must face the bewildering task of attempting to understand and to assess the practices and products of economists. Before judging competing methodological views, one must make a serious attempt to understand and to appreciate them.

Donald McCloskey with his "rhetoric of economics" (1985) also points out that systematic philosophy provides no well-justified code of scientific practice. He proposes that the tools of classical rhetoric and literary criticism are better suited to understanding what economists do. Thus, for example, in discussing a couple of pages from Samuelson's *Foundations of Economic Analysis*, McCloskey (1985, pp. 70–72) finds that Samuelson uses a variety of "rhetorical devices:" analogy, appeals to authority, relaxation of assumptions, and hypothetical "toy" economies. Whether any of these may also be construed as good arguments that ought rationally to persuade the reader to accept Samuelson's conclusions is not McCloskey's concern, because he is skeptical about whether there are any detailed standards for what counts as a good argument in economics apart from whatever in fact persuades economists. Like Rosenberg and others, McCloskey encourages careful study of economic argumentation, and in his striking discussions of works by John Muth (Ch. 6) and Robert Fogel (Ch. 7) he provides memorable models of such study.

But McCloskey offers little solid argument for employing his favored literary tools, and he has a hard time explaining how his proposed successor to economic methodology is supposed to retain any normative role. And the normative role of methodology is unavoidable; whether methodological rules are garnered from imitation, methodological asides, or systematic methodological treatises, there is no doing economics without some standards or norms. Furthermore, if economics is to make any rational claim to guide policy, these standards or norms cannot be arbitrary.

The current literature on theory appraisal in economics and on economic methodology in general is quite eclectic, and I find this development healthy. One

finds work as diverse as Neil deMarchi's and Abraham Hirsch's (1986) analysis of how Friedman employs monetary history to argue for his monetary theory, Cristina Bicchieri's (forthcoming) treatment of the epistemological complications of the rational expectations hypothesis, Philip Mirowski's (forthcoming) detailed account of the analogy between classical physics and neoclassical economic theory, or Alan Nelson's (1986) argument that microeconomics is a theory of individual choice. One moral of the past decade of philosophy of science is that the most interesting and substantive methodological work will usually turn on the details of the particular discipline discussed. A dispassionate look at recent methodological studies of economics strongly supports this view.

Conclusions

Methodological writing is pouring out at an increasing rate. Over the past decade there have been scores of books, hundreds of articles, and even a new journal, *Economics and Philosophy*. This literature is still preoccupied with problems of theory appraisal, although other questions are attracting growing attention. All of the main streams discussed above are represented.

So, first, one still finds positivist or Popperian complaints that neoclassical economists refuse to put microeconomic theory to the test or to heed its disconfirmation. Many of these are from an institutionalist perspective (Eichner, 1983; Samuels, 1980). Since the models of science upon which these criticisms are based are unacceptable, I am skeptical about the value of these criticisms.

Second, one finds more refutations or rehabilitations of Milton Friedman. It will be a step forward when economists come to regard Friedman's essay only as an historically interesting document.

Third, one finds applications of current trends in philosophy of science—especially work by Thomas Kuhn (1970), Imre Lakatos (1970), and Paul Feyerabend (1975). This literature is almost as disappointing as is positivist or Popperian grumbling or rehashing Friedman. Apart from philosophical difficulties with their views, Kuhn, Lakatos, and Feyerabend have been hard to apply, for they are evasive on questions of theory appraisal, which still interest most of those writing on economic methodology. The most valuable work here (such as E. Roy Weintraub's 1985 Lakatosian account of the structure and history of general equilibrium theorizing) has little to say about issues of appraisal (see also Latsis, 1976). There is also a separate technical literature on econometric methods that overlaps too infrequently with the methodological mainstream.⁴

⁴Although less familiar, there have also been a number of attempted applications of the views of Joseph Sneed (1971) and Wolfgang Stegmüller (1979). Many of these are to be found in Stegmüller, Balzer and Spohn (1982). For further references and well-taken criticism, see Hands (1985). There is also a good deal of methodological discussion written from a specifically "Austrian" perspective. Much of this is concerned with the interpretation of the views of major Austrian figures such as Mises and Hayek and with the defense of views of theory appraisal in economics similar to those of Mill and of Robbins. Caldwell (1982) provides a good discussion of the methodological views of the Austrians. For a fairly recent collection of essays, see Dolan (1976).

Not surprisingly, I think that the best way forward concerning both theory appraisal and economic methodology more generally is the fourth (eclectic) way, the path I have taken: to focus on the methodology economists practice, making use of whatever tools philosophers of science have had to offer that appear to be well-made and apt for the job (Hausman, 1981a, ch. 12). Although methodologists may find much to criticize, they had better begin by understanding as thoroughly as they can how economists go about their business and why they do what they do. The Popperian/positivist and predictionist interludes in economic methodology have been largely unenlightening. With some restatement and toning down of the overly optimistic conviction that economics starts with the central truths concerning its domain, I think that Mill's views still stand.

The most promising and interesting methodological issues to tackle now are not directly concerned with theory appraisal. The role and significance of general equilibrium theory are still not entirely clear. The implications of rational expectations for the objectivity and logic of economics remain to be explored. The notion of rationality in strategic and uncertain circumstances presents difficult open questions. In tackling problems such as these, I look forward to profitable collaboration between economists and philosophers.

■ *Many of the details in this paper are drawn from earlier works, like Hausman (1984, 1986). For a more extensive exposition and references, see Hausman (1988b). For more detailed bibliography, see Caldwell (1984) or Hausman (1984). I would like to thank Bruce Caldwell, Wade Hands, Catherine Kautsky, Michael McPherson, and the editors of this journal for their generous help with this essay.*

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