

Could There Be a Science of Economics?

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Much scientific thinking and thinking about science involves the assumption that there is a deep and pervasive order to the world that it is the business of science to disclose. A paradigmatic statement of such a view can be found in a widely discussed paper by a prominent economist, Milton Friedman (a paper which will be discussed in more detail shortly):

A fundamental hypothesis of science is that appearances are deceptive and that there is a way of looking at or interpreting or organizing the evidence that will reveal superficially disconnected and diverse phenomena to be manifestations of a more fundamental and relatively simple structure. (1953/1984, 231)

On the other hand, the person sometimes described as the father of modern science, Francis Bacon, wrote:

The human understanding is of its own nature prone to suppose the existence of more order and regularity in the world than it finds. And though there be many things in nature which are singular and unmatched, yet it devises for them conjugates and relatives which do not exist. (1620/1960, 50)

I myself find the empiricism of Bacon's position much more attractive than Friedman's rationalism. Assumptions of the kind Friedman's remark illustrates lead naturally, almost inevitably, to philosophical doctrines such as reductionism (of various kinds) and determinism. In a recent book (Dupré 1993) I have argued at length against various doctrines of these kinds and, more generally, against the assumption of underlying metaphysical order with which they are inextricably connected. In the concluding chapters of that book I suggested that more modest and defensible views of the prevalence of order in the world would have important consequences for our general understanding of science,

and even for the practice of science. More specifically, I proposed that certain scientific projects might be seen to be quite misguided when deterministic or reductionistic assumptions about the domains under investigation were abandoned. And finally, the rejection of such assumptions suggests a much greater role for antecedent value judgments in scientific investigation than is generally allowed.

In the previous work just mentioned, the arguments against reductionism and determinism were developed mainly in relation to a discussion of biological science. Although biological ideas, through their relevance or alleged relevance to conceptions of human nature, have important implications for normative questions, further investigation of the relation of values to science may more readily be pursued in relation to some part of the social or human sciences. A particularly suitable candidate is the science of economics. The complexity of the phenomena investigated by economics is such as to make claims of fundamental lack of order at least superficially plausible; and how, if at all, social or political values relate to economic investigation is a question of obvious importance. This paper provides a sketch of the argument that economics does indeed provide an illustration of the need for normative input in the investigation of a domain with quite limited preexisting order. In the course of developing this thesis I shall touch on various more specific objections to aspects of economic theory. None of these objections are new: they have existed since the beginnings of neoclassical economics in the late nineteenth century, in the writings of Weber, Veblen, Commons, and numerous successors. Where the skepticism developed in the present paper goes beyond this critical tradition is in the suggestion that the failures of economics derive not merely from excessively simplistic assumptions, crude theories of human nature, and so on, but rather from a fundamental mismatch between the kinds of phenomena with which economics is concerned and widely held conceptions of what it is for an investigation of any realm of phenomena to be genuinely scientific. It is this mismatch, I contend, that raises the question whether a *science* of economics is possible.

I should emphasize that I use the word "sketch" advisedly. One part of the picture that is in particular need of filling in is the question of the extent to which there is empirical support, particularly predictive success, for the broad theoretical claims of economics. Though the skeptical position I adopt in the paper does not seem to me unreasonable, clearly it is in need of more detailed investigation. At any rate, one of the aims of the present paper will be to assess the relevance of such further investigation to the general thesis I am developing. Another point that needs to be stressed at the outset is that throughout the paper I am addressing the foundational, abstract, and typically mathematical theories of economics rather than local, generally qualitative knowledge of specific economic systems. I do not know how the actual labor of professional economists is divided between efforts that fall (roughly) into each of these categories. Arguably much of the work of, for example, labor economists, development economists, and even econometricians, has little connection with

this supposedly foundational core. But it is surely the articulation of the formal and abstract core of economic theory that is the most conspicuous and prestigious aspect of the activity of economists.

In the first section of this paper I shall be concerned with economic methodology. In particular I shall criticize a conception of economic methodology that might serve to defuse the worries about the lack of empirical success in economics that have been so widely emphasized by critics of economics. The second section addresses the relevance of questions of underlying order to the prospects for the development of economics, and the third section explores the consequences of skepticism about underlying order for the relation of economic fact to value. I shall argue that the most plausible escape from pressing doubts about the possibility of a scientific study of economic phenomena is to recognize a much greater interpenetration of science and value than is generally recognized.

1. THE METHODOLOGY OF ECONOMICS

One obvious motivation for the question in the title of this paper is the remarkably widespread belief that the study of economics has failed almost totally in at least one of the traditionally central marks of science, prediction. (As Thomas Love Peacock put it: "Premises assumed without evidence, or in spite of it; and conclusions drawn from them so logically, that they must necessarily be erroneous" [cited by Hausman 1984, 1].) Of course it is not that no economist has ever said anything true about any future economic phenomenon, but just that economic theory seems to add little to the ability of a well-informed person in possession of average common sense to be right about such matters.¹ More broadly, this worry reflects a serious doubt as to whether economics is in any sense genuinely an empirical study. It is, at any rate, widely supposed that if economics were to be judged solely on the basis of the extent to which it has succeeded in making predictions about the future, then it would be seen to be a dismal failure. This empirical failure is all the more remarkable given the extent to which economics continues to be regarded as a model for the social sciences. To give just one example, Alexander Rosenberg, in a paper otherwise highly critical of the development of microeconomic theory, nevertheless describes this theory as "the most impressive edifice in social science yet erected" (1979, 47). Apparently some other grounds of scientific excellence than predictive success are being supposed. My first aim in this paper is to consider the plausibility of the claim that any such grounds could compensate for the empirical failure just noted.

One very influential economist, on the other hand, apparently sees no serious weakness in the predictive achievements of economics. Milton Friedman, in an extremely influential article (1953/1984), argues that economics should be judged, not as is often supposed, by the truth of its premises, but rather by the empirical success of its predictions. And it is clear enough that Friedman thinks that this is a test that economics will readily pass. Nevertheless,

it is striking how little he says in this article to justify complacency about the predictive powers of economics. Friedman compares, for example, the hypothesis that businessmen act as if they aimed to maximize profits with the hypothesis that expert billiard players act as if they were able to make all the appropriate mathematical calculations of the trajectory of a billiard ball. The value of this latter hypothesis is sufficiently demonstrated by the successful shots of the expert billiard player, and the psychology of neither the billiard-player nor the businessman is of any relevance to the evaluation of either hypothesis. But remarkably, rather than offer any empirical evidence that, just as expert billiard players generally make their shots, businessmen, whatever they may be intending, do in fact maximize profits, Friedman offers nothing but a broken-backed a priori argument for this conclusion. "[U]nless the behavior of businessmen in some way or other approximated behavior consistent with the maximization of returns," he writes, "it seems unlikely that they would remain in business for long" (223). But the implicit argument here has no force unless it is assumed that the competitors of the non-profit-maximizing businessman are profit maximizers, which is blatantly question-begging. If no businesses, or very few businesses, are profit-maximizers, there may be very little tendency for non-profit-maximizing firms to go out of business. Perhaps survival in business is largely a matter of luck, or of access to corrupt government officials. Distinguishing between these and many other possible hypotheses would require empirical evidence of a kind that Friedman neither offers nor, apparently, sees the need for.

Another example Friedman offers is the hypothesis that a sudden increase in the money supply will produce a substantial increase in prices (216). Although the "evidence is dramatic" for this hypothesis Friedman admits regretfully that the issue remains highly contentious, an observation that might reasonably lead the external observer of economics to wonder whether this paradigm of empirical support could be quite that dramatic. It appears that in the end Friedman wants to appeal not to "any textbook list of instances in which the hypothesis has failed to be contradicted" (224), but rather to "experience from countless applications of the hypothesis to specific problems and the repeated failure of its implications to be contradicted. This evidence is extremely hard to document; it is scattered in numerous memorandums, articles, and monographs . . ." (223–24). In contemporary philosophy of science, where the constant apparent confirmation of the basic assumptions of a research program, or a paradigm during normal science, etc., has become a commonplace, this appeal is unlikely to impress. Friedman's paradigmatic though pre-Kuhnian description of a Kuhnian paradigm becomes even more striking when we read that the ability to apply economic models in the right way requires considerable professional judgment. This "is something that cannot be taught; it can be learned, but only by experience and exposure in the 'right' scientific atmosphere, not by rote. It is at this point that the 'amateur' is separated from the 'professional' in all sciences and the thin line is drawn which distinguishes the 'crackpot' from the scientist" (226). Very likely, this

is the way that all sciences work. But the fact that economists know how to make their models work is even less impressive in the face of such a frank statement of the judgment that awaits those who, for whatever reason, fail to do so. In short, Friedman's manner of endorsement of the empirical success of economics tends to argue rather for the opposite conclusion.

An obvious reply to the complaint that economics has had little predictive success would be to suggest that prediction was unnecessary for the scientific status of a discipline, provided only that it be sufficiently successful at explanation. One example that might naturally be offered in support of this suggestion is that of evolutionary biology. While it is sometimes argued that evolutionary theory does have *some* predictive successes, for example with regard to (previously unexplored) aspects of the distribution and geographical relations of similar species, it is for its explanatory achievements that the theory of evolution is so widely admired. Two additional features of evolutionary theory contribute to the lack of concern about predictive deficiencies. First, it is widely perceived that there are no serious rivals to evolutionary theory as explanations of the origin and diversity of life. Thus there is some plausibility to the claim that evolution is not merely *an* explanation of these phenomena, but the only possible such explanation. In view of the complexity of the patterns of similarity between both living and fossil organisms, and the consistency of this pattern with the hypothesis of relation by descent, this claim is not implausible. I shall return to the relevance of plausible alternatives later.

The second point is of much closer relevance to traditional defenses of economics. It is that the constituent processes from which full and interesting evolutionary explanations are constructed are *empirically confirmed* beyond any reasonable doubt. To the extent that this is so, it is surely legitimate to accept logical elaborations of the consequences of the iteration of such processes as empirically supported explanations of observable phenomena. It is presumably in the interests of such an explanatory strategy that Darwin devotes so much space at the beginning of *The Origin of Species* to a discussion of the thoroughly empirical and moderately predictive art of animal breeding. More recently, it explains the importance of the occasional studies, such as that of the evolution of industrial melanism in the Peppered Moth (Kettlewell 1973), that really do appear to document in detail processes of natural selection in the wild. Just as such cases establish the reality of the basic mechanisms of evolution, so it is assumed that the basic postulates of economics—self-interest,² profit-seeking, etc.—are established by introspection or common experience. Another way of stating the point is that some, at least, of the scientific credentials attributed to evolutionary theory derive from the perception that it constitutes a successful application of what Mill described, for the case of economics, as the method *a priori*.

The method *a priori*, or the deductive method, sees economics as concerned with working out the consequences of basic postulates for which some strong independent warrant can be provided.³ This is in sharp contrast with the hypothetico-deductive method, according to which the truth of the

consequences thus deduced would provide the warrant for believing the basic postulates. Thus Lionel Robbins, a prominent advocate of the former, *a priori*, perspective, wrote, concerning such postulates: "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" (1935/1984, 119). But although these postulates are taken to be true, they are taken to be true only *ceteris paribus*. And it is this proviso that enables the deductive method to accommodate the falsity of most actual economic conclusions. In the real world, the factors distinguished by the basic postulates must always interact with a variety of interfering factors, a circumstance that makes it unlikely that any conclusion drawn from them will coincide with observed reality (as noted in the satirical remark of Peacock, cited above). For Mill this is precisely the reason why we must insist on the appropriateness of the *a priori* method rather than the *a posteriori* (inductive) method:

It is vain to hope that truth can be arrived at, either in Political Economy or in any other department of the social science, while we look at the facts in the concrete, clothed in all the complexity with which nature has surrounded them, and endeavour to elicit a general law by a process of induction from a comparison of details: there remains no other method than the *a priori* one, or that of "abstract speculation." (Mill 1836/1984, 59)

More recently Hausman (1992), while in many ways sympathetic to the deductive method, diverges from it in one crucial respect. For Mill, predictive failures will be important in helping to identify the forces interfering with the action of those presupposed by economic theory. But if this is taken as a methodological prescription, predictive failures can never serve to cast doubt on the basic assumptions from which they are derived. Hausman considers this to be an intolerable departure from empiricist standards. Although he agrees that it is very unlikely that predictive failure will call basic assumptions into question, this is not because alternative explanations of their failure will almost always be more plausible. Thus though Hausman considers it important to remove this suggestion of dogmatism from the theoretical account of economic methodology, in practice he describes something very close to the methodology adumbrated by Mill and Robbins.

A key question to which we are led is what kind of support should be offered for these effectively unassailable basic postulates. The most basic of such postulates are those that define the behavior of (economically) rational actors and profit maximizing firms.⁴ Here I shall focus only on the former. Economic rationality consists in ranking all the options available to the agent and always choosing the most highly ranked available option. These rankings must at least be complete (they must assign a position in the ranking to all options) and transitive (if *A* is preferred to *B* and *B* to *C*, *A* must be preferred to *C*). In addition the economic theory of consumer behavior requires a set of more restrictive assumptions about the kinds of options that are the subjects of people's rankings, a set of assumptions Hausman refers to as

"consumerism" (1992, 30). Roughly, what this amounts to is that people should have an insatiable desire for commodities and an absolute lack of concern for the material well-being of anyone else.

It is somewhat surprising in view of the preceding discussion of methodology that these basic postulates, far from being self-evident or plainly true in the light of common experience, are quite plainly false. Certainly no one but the wholly destitute has in fact considered all the bundles of commodities available to them, let alone ranked them. There is no reason why anyone should have a definite preference between, say, a box of shredded wheat and a pound of apples and a box of Wheaties and a pound of pears. Still less need it be possible to rank a dining table against a two-week vacation in Crete, or an elephant against three camels.⁵ Consumerism is, if possible, more obviously false, even in the present post-Reaganite world. And even the somewhat more plausible assumption that the most favored bundle of commodities available will be selected is massively constrained by information deficits.⁶

What then should we make of these basic postulates given that they are neither true nor, a fortiori, self-evidently true? The standard answer to this question is that they are true *ceteris paribus*, or that they describe what would happen in the absence of the various acknowledged interfering factors.⁷ In support of this maneuver, we may begin by noting that *all* laws must be qualified to some extent by *ceteris paribus* conditions. Even such nomological paradigms as Newton's laws of motion are true only in the absence of interfering forces (for instance, electro-magnetic) (see Cartwright 1983). At the other extreme, not everything that could be said to be true, *ceteris paribus*, should count as a law. It is true, *ceteris paribus*, that strong earthquakes cause houses to fall down, provided that the *ceteris paribus* conditions include such things as that the frame was not bolted to the foundation, the walls were not reinforced, etc. Without careful specification of what it is for other things to be equal, we could claim a *ceteris paribus* law whenever anything has even a rarely exercised capacity to bring about some effect. Presumably the answer to this must be that *ceteris paribus* clauses involve what is usually, or typically, true, or at least what is typically approximately true. But this immediately seems too strong for the putative economic laws that we have been considering. For if the postulates of economic theory were typically true, or even typically approximately true, then we should surely expect that the consequences deduced from them would be typically (approximately) true. Thus if it is indeed the case that economic predictions are generally of little value, and this is because there are generally interfering factors, then the *ceteris paribus* clause is certainly not typically true.

No doubt the preceding argument has moved a bit too fast. One avenue that remains open is to stress the abstraction rather than the a priority in Mill's method. One line of thought that can be found in Mill is that good methodology consists in investigating significant forces in isolation from all interfering factors.⁸ When we have a good understanding of the behavior of all the main forces we can try to see how to put them all together. Thus we

need only assume, in the present case, that the economic postulates identify significant factors in the determination of social outcomes. Given the acknowledged presence of other significant forces, strict falsity both of the postulates and of deductions from them is only to be expected. This seems to me to be the strongest interpretation of economic methodology with a fighting chance of defensibility. Some important consequences of adopting this view should be noted, however. First, it would render very questionable any attempt to base policy decisions on economic theory alone. Second, it would even more clearly preclude any attempt to derive normative conclusions from economic foundations. And third, as is a major theme of Hausman (1992), giving equal status to social forces distinct from those studied by economists would be strongly opposed to the general ideology of the discipline. But rather than pursue any of these issues, I want to raise some skeptical questions as to whether even this view of economics as the production of (false) abstract models holds out much hope of being a genuine source of insight into social reality.

2. THE POSSIBILITY OF THE IMPOSSIBILITY OF ECONOMICS

Let us suppose that economic theory does consist of abstract models in the sense indicated at the end of the previous section. In what way might such models be true or useful? It will be helpful here to consider a distinction discussed by Nancy Cartwright (1990, chap. 5) between abstraction and idealization. In an idealized model, causal variables are not ignored, but rather set to convenient values. Since important variables are all included, it is supposed that idealized models are approximately true. Abstract models, on the other hand, describe only the effects of selected causal influences. The point emphasized in the preceding section, that economic models are not expected to be even approximately true, and Hausman's idea that even the *ceteris paribus* laws in economics are vague as to what exactly has to be equal make it clear that economic laws must be abstract rather than idealized. This leads us to the question: What are the criteria of, if not truth, then rightness or legitimacy for such abstract models?

A simple answer would be that abstract models say merely that if there were objects subject to only the forces defined in the model, then from the definition of those forces it follows that the objects would behave in the ways specified by the model. This has momentary appeal in view of the idea that forces must ultimately be defined in terms of the kind of response they produce in appropriate objects to which they are applied. But this reply is plainly too easy to help with our question. On this criterion, any model is legitimate if it is coherent. It cannot, therefore, address the issue of which models have some relevance to the real world. It is tempting, then, to resort to counterfactual claims about real objects. Thus, for example, we might interpret models of consumer behavior as asserting that if people were motivated solely by self-interest, had perfect information, complete transitive preferences, etc., they

would. . . . To which a natural response would be, Why should we care about the hypothetical behavior of people so different from ourselves?

This entire discussion raises important philosophical difficulties that I shall, however, note only to pass over. It is questionable whether self-interest, for example, can coherently be thought of as one force competing with others in the manner of the combining or opposing forces of classical mechanics. (Though perhaps with self-interest in the guise of greed such a vision is implicit in certain, generally religious, models of moral deliberation.) However, assuming for present purposes that some adequate psychological theory can be provided to make sense of the economic model, let me consider further the proposed counterfactual. The response to the objection at the end of the last paragraph must be, presumably, that these people are not different from ourselves with regard to their dispositional structure. The claim is about how *we* would behave subject to only the forces specified. And this is not merely to serve as a definition of those forces (unless it is to be subject to the objection of triviality), but to say something contingent about our causal or dispositional structure. But even if we pass over all the notorious general difficulties with the interpretation of counterfactuals, it is still extremely doubtful whether such counterfactual claims have any relevance to real people in the real world.

We should begin with the fairly obvious point that the forces central to economic models, notably self-interest, must at least be seen as important determinants of human behavior. It is unimaginable, for example, that anyone should be much interested in a theory of some social phenomenon that treated human behavior solely insofar as it derived from embarrassment, or pique. With this assumption in mind, the question also arises, under what circumstances is self-interest a crucial or even decisive determinant of human behavior? Is the centrality of self-interest a universal feature of human nature, or something true only in a fairly specific set of social contexts?⁹ Apparently training in economics has some tendency to elicit the kind of behavior assumed in economic models,¹⁰ which suggests that such behavior might be very sensitive to details of the social environment. McCloskey (1990, 140) makes some scathing remarks about the prevalence of unrestrained self-interest among his fellow-economists.¹¹ Regardless of the answer to this question, even assuming that self-interest is a sufficiently crucial determining factor of human behavior to justify the development of models of its consequences, what does the counterfactual tell us about human behavior in contexts in which other important factors are also acting such as altruism, force of habit, deference to local custom, systematic "irrationality" of the kind increasingly familiar to psychologists,¹² etc.

I suggest that if there is any sense to be made of the question what is the causal force of self-interest abstracted from such other factors relevant to the determination of economic behavior, it can only be because it is possible, at least in principle, to combine the causal influences of all such forces so as to produce a resultant that fully determines, if not the relevant behavior, at least the probability of its occurrence. It is not merely that without such a possibility the abstract model can have no practical significance, but rather that this is a

condition of the intelligibility of the abstract model. One way to make this point is to note that abstraction is often thought of as involving the *subtraction* of causal factors other than those under consideration (see Cartwright 1990, 197). But this metaphor only makes sense if there is some totality (the resultant of the various factors) from which this subtraction can begin, and if this really is a *sum* of the various causal factors involved.

I see no reason why these conditions should be met. The assumption that there must be some such resultant of the causal influences on social, or specifically economic, phenomena is an expression of the commitment to determinism, or at least the probabilistic successors to determinism that still insist on the existence of the complete causal truth about any sequence of events. I cannot address the difficulties with these views in any detail here.¹³ However, in the absence of a metaphysical argument for the necessity of determinism, the general failure to provide empirically adequate models of social phenomena is, *ipso facto*, a failure to provide evidence for the truth of determinism. Evidence for determinism, I suggest, is hard to come by.

It may be easier to see what is at stake here in the context of a concrete example. I do not at all mean to deny that there are economic causes. So, for instance, a reduction in the price of a commodity will generally cause an increase in the quantity of that commodity demanded unless some other causal factor counteracts that causal tendency. Does this mean that there is some objectively real relation between price and quantity demanded of the sort expressed in a demand curve? Not necessarily. What an objectively real demand curve appears to add to the mere recognition of the causal capacity of the reduction in price is the expectation that the relation between price and quantity demanded should be quantitatively stable.¹⁴ Thus it implies that if, on two different occasions, the price falls from P_1 to P_2 , the quantity demanded should, on both occasions, rise from Q_1 to Q_2 . Of course, this is not really what anyone expects. Tastes may have changed, substitutes or complements become more or less available, etc. So, as usual, this implication will be qualified as true only *ceteris paribus*. This, in turn, implies that if the change in quantity demanded on the second occasion is not the same as on the first, there must be some explanation of this fact—some other factor must not have been equal. But only a commitment to determinism makes it seem necessary that there be such an explanation. I see no reason why demand curves should not evolve in erratic and unpredictable ways over history so that quantitative relations between price and quantity would have very little inductive value for predicting the future. In short, the failure of economics to predict future phenomena might reflect not merely the inability of economists to take account of the numerous factors impinging on economic reality, but rather the inherent unpredictability of the phenomena concerned. And if this were the case, finally, it would be impossible even to make sense of the counterfactuals that were supposed to underwrite the abstract economic models.

There are two kinds of response to the recognition of this possibility short of abandoning entirely the traditional quantitative methodology of economics:

empirical optimism and metaphysics. By the latter, I mean that we might offer a metaphysical argument that there must be stable economic relations regardless of our inability to discover them. I have argued at length against this kind of metaphysical prejudice elsewhere (1993), and shall not discuss it further here. (Somewhat relatedly, we might suppose in the way suggested by the quote from Friedman at the beginning of this paper that the assumption of the existence of underlying order is a precondition of scientific research. I shall explain below why I think that, even in a merely methodological sense, such a presupposition is unacceptable.) In accordance with the former strategy, we may certainly continue to hope that empirical evidence will eventually sustain the stability of quantitative economic relations, though I suggest that this amounts to little more than blind faith. No doubt it may in the end turn out that, perhaps with the incorporation of specific further factors and barring gross and identifiable interventions, economic self-interest really does come close to providing an empirically adequate explanation or prediction of economic phenomena. But given the ease with which significant interfering factors can be imagined, this seems something of a forlorn hope.

It will be recalled that in offering evolutionary biology as an area of science the legitimacy of which does not seem seriously threatened by lack of predictive potential, I mentioned that the absence of serious alternatives to the theory of evolution as an explanation of the origin and diversity of life was important to its credibility. Could the same be said of economics? Surely not. As I have indicated, it seems hard to deny that the forces assumed by economic theory act, for example, on the prices of and demand for commodities. So any plausible account of the determination of these parameters will have to leave room for these causal influences. But how large a part of the story these forces will provide is another matter. In the short run, demand and supply may seldom be in equilibrium even if eventually producers will react to increasing inventories or shortages. These responses may seldom keep pace with endogenously determined changes in demand. Perhaps prices are largely determined by rule of thumb pricing techniques (cost + $X\%$, for example), imitation, or guesswork, and demand fluctuates continuously with changes in fashion, advertising strategies, and so on. And most importantly unless determinism is assumed, there need be no stable and consistent way in which these various forces combine.

An even more significant employment of the parallel between economics and evolutionary theory is the following. The TINA¹⁵ defense of evolutionary theory is persuasive when it is deployed on behalf of broad qualitative accounts of aspects of the history of life. It is much less plausible if it is taken to legitimate detailed mathematical models of particular evolutionary phenomena. Indeed I have argued elsewhere (1993, chap. 6) that population genetics, supposedly a source of detailed mathematical models of evolutionary changes in the genetic constitution of populations of organisms, is an extremely dubious enterprise. Similarly, insofar as economics restricts itself to identifying causal influences on economic phenomena, and deploying these in narrative accounts

of economic phenomena, it should be seen as constituting a generally salutary contribution to general historiography. But the mismatch between the apparent precision and scope of theoretical economic models and any reasonable assessment of the importance of the processes they describe to real economic history makes most of economic theory an unpromising candidate for epistemologically respectable science.

3. SCIENCE AND VALUES

In a suggestive article, Mary Hesse (1978) raises the question whether the criteria for scientificity generally associated with the natural sciences, especially the ability of science to facilitate prediction and control, might not be explicitly rejected by (parts of) the social sciences in favor of some more obviously value-driven goal. My suggestion that an economic theory capable of more than a quite limited degree of prediction and control might be precluded by the nature of economic, or more broadly social phenomena, provides a compelling ground for considering carefully the possibility Hesse explores.

The starting point for developing this idea is to note that given the view of a largely qualitative economics applied to a partially indeterministic domain of phenomena that I have advocated, there is no reason to think that there is any uniquely empirically best economic theory. This contrasts sharply with one of the best known metaphors for modern science, mechanism. There is one unique best theory of the workings of a machine, the theory that correctly explains how it works.¹⁶ This is a consequence of the fact that it is (generally) entirely clear what is the behavior of a machine that calls for explanation, namely, the behavior which the machine was designed to exhibit. This is easily seen by thinking of cases where there is some departure from the normal use of the machine. Just as it is generally irrelevant to the theory of bicycles what color they are, so when the bicycle is raised off the ground and used as an exercising device it becomes irrelevant what is the condition of its tires. In more extreme cases, say the use of the bicycle as a structural element in a postmodern building, color might become the most salient feature.

The machine model often seems to be exactly what underlies pictures of the economy, and hence of the nature of economics. The economy is a machine that produces commodities. The parts of the machine are firms, consumers, and governments. Taxes or government regulations, we are often encouraged to believe, create friction and slow down the production of commodities. Ideally if we can engender enough greed to fuel the machine, and minimize all possible sources of friction, the machine will run at its best. My point here is not so much to criticize this picture—though certainly there is much to criticize—but to point to its optionality. If we reject the idea that this picture is simply descriptive—that's just what economies are for, like it or not—it is natural to see it as implicitly supporting the normative assumption that maximally efficient commodity production is what economics should be intended to promote. But if that is a correct interpretation, then surely we should object at once that it

is far from obvious that this is a goal—still less *the* goal—that we should aim to promote. For some historical political economists, perhaps even economists such as Jevons at the beginning of neoclassical economics, this normative connection was explicit, with the economy as a generator of utility yoked to a theory of the good as utility-maximization. Nowadays, however, with economics concerned with utility solely in the sense of moving individuals on to ordinally higher indifference curves; with comparisons between social states generally limited to the identification of Pareto-improvements; and with moral philosophers, if not economists, widely skeptical even about genuinely substantive utilitarianism, there is no convincing possibility of providing a normative justification of the traditional economic machine. It is of course possible to undercut the need for such a justification by insisting that the machine is inevitable: that is just how things are. (This is the metaphysical version of the TINA defense.) And though such natural necessity is precisely what is often implied by the hard-headed advocates of positive economics, this is a metaphysical bullet, I have argued, that we have no reason to bite.

The myth of positive economics, an economics forced on us simply by the way things are, serves to deflect debate from the really important question, which is what goals we would like the economy to serve. Consider, for instance, the possibility of an economics premised on the assumption that the function of an economy was to maximize, and distribute as equally as possible, standard of living. Needless to say, a very substantial part of this discipline would be involved with the philosophical question of what constitutes standard of living, though as Sen (1987) has made very clear, we can be sure that this is only tangentially related either to any standard account of utility or to the accumulation of wealth. It is of course possible that the best way to do such an economics would be to graft the normative conclusions on to very much the traditional economic account of the market economy, in the manner of much contemporary welfare economics. But it seems unlikely that things would be that simple. Once we recognize the possibility that an economy can be highly productive and growing fast in current economic terms, and yet thoroughly sick in the normatively significant sense of its failure to generate high standards of living for many of its citizens, we are likely to direct our attention to somewhat different measures of economic performance and individual motivations. Recalling the observation that training in economics has some tendency to generate economically “rational” behavior, it seems likely that an economy substantially under the control of economists will promote such behavior in many different ways among all or most segments of the community. I suggest that the only remedy for this situation is to recognize that the normative part of economics, far from being hitched on to the back of a putatively positive economics, must be seen as fundamental to, and epistemologically prior to, any acceptable science of economics.

The preceding discussion should, finally, answer the objection to the indeterministic metaphysical grounding of my claims suggested by Friedman’s remark about the deceptiveness of appearance through which science aims to

see, the insistence that methodologically it is sheer defeatism not to assume that there is some complete, empirically adequate, theory of economics. My reply is that there are great costs to error on either side of this question. Pursuing the current, empirically generally failing, theory of economics merely on the grounds that we must hold out the hope for a physics-like, mechanistic, and empirically sufficient theory of economics may very well stand in the way of seeing the need for an admittedly partial, indeterministic, qualitative, etc. account of economics, but one driven by truly worthwhile social or political goals.

4. CONCLUSION

With a reminder that this paper is to be understood as provisional and in some respects sketchy, let me nonetheless venture some conclusions. I can best do so by returning to the question that provided the title of this paper. Though my tone has sometimes verged on the polemical, it is clear that my answer to that question is a qualified, Very probably yes. First, my skepticism has been directed to theoretical, especially mathematical constructs in economics. I do not deny that there may well be much useful, if generally loose, causal knowledge to be had in economics, though perhaps a good deal of this will be at a level accessible to no more than sophisticated common sense, and suspicions about the inductive projectibility of such causal knowledge will often be appropriate. Ultimately, my affirmative answer here may depend on some major revisions in what we take to be legitimate goals of science. Second, my skepticism about the one true economic story opens up the possibility that there might be numerous approaches to the investigation of economic phenomena, each capable of delivering partial, but nonetheless useful, insights into economic reality. And third, and most importantly, the preceding section suggests the possibility of a science of economics of real value to humanity, something which has only intermittently been true of the history of that science and seems decreasingly true of it in recent years.¹⁷ But the precondition of this, I have argued, is the recognition that fundamental value judgments must be made at the very beginning of the project. They cannot just be tacked on at the end.¹⁸

NOTES

1. One economist (McCloskey 1990) seems to argue on a priori grounds that economists could not make predictions: the windfall profits that would accrue to one able to make economic predictions would motivate further economic actions that would soon falsify such predictions. Certainly economic predictions would often be an important causal influence on the phenomena they attempted to predict, though this seems only to show a further reason why prediction would be difficult, rather than that it would be impossible.

2. Strictly speaking I should not say "self-interest" but merely economic "rationality." Formally, the latter concept allows that I might prefer the well-being of others in certain respects to my own. However, I prefer to use the term "self-interest" for two reasons. First, the well-being of others can only be of concern to me through some psychological state of mine such as satisfaction, pleasure, etc. The economically "rational" man cannot aim for the good of another simply for its own sake or for moral reasons. Second, notwithstanding the

possibility of giving an extremely abstract interpretation of the formalism, a great deal of the rhetoric of economics clearly embraces a quite explicit commitment to a quite narrow sense of self-interest.

3. It is presumably in opposition to such a conception of economics that Friedman's insistence on the primacy of predictive success, discussed above, is largely motivated. No doubt this goes some way to explain some of the apparent anti-realist excesses of Friedman's position with regard to economic theory, excesses that could have been greatly vitiated by a sharper distinction between the fundamental postulates of a theory and the merely auxiliary assumptions at which his skepticism is primarily addressed. This issue is, however, largely irrelevant to my present concerns with Friedman's ideas.

4. A useful and fairly detailed discussion of the basic premises of microeconomics is provided by Hausman (1992, chaps. 1–3). My very cursory discussion leaves out some of the more technical postulates, notably the continuity of ranking and the diminishing marginal rates of substitution, for which see Hausman, *op. cit.*

5. I say *nothing* about so-called revealed preferences, since these are of no possible philosophical interest unless they do indeed reveal preferences, which is the point at issue. If they may nevertheless be of some relevance to economics, so much the worse for economics.

6. One can, of course, include the cost of information about the availability of a bundle of goods in the cost of that bundle, though this would undermine some of the main attractions of the whole theory by making the cost of a bundle of goods largely inscrutable and widely variable from person to person.

7. Hausman (1992) goes a step further and notes that these must be *vague* *ceteris paribus* conditions. We do not typically know exactly what are the various possible sources of interference. The presence of such vague *ceteris paribus* conditions defines what Hausman calls inexact laws.

8. Consideration of Mill's account of the method of difference in *A System of Logic* (1875) suggests a more empiricist interpretation of his methodology than I have here allowed. Combining his remarks on economics with his general theory of inductive reasoning in *A System of Logic* suggests a more complex and subtle methodology. Though my treatment of Mill here is therefore somewhat unfair, further exploration of this idea would be a project for another paper.

9. Economists often note that their stories apply only to conditions of scarcity. Here I have in mind no such conditions internal to the structure of economic theory, but rather suggestions such as that the behavior of 'economic man' might be uniquely characteristic of people living in market, capitalist societies, as proposed by Marx in his well-known criticisms of Smith and Ricardo in the *1844 Manuscripts* (1844/1978).

10. See Marwell and Ames 1981, cited in Hausman 1992, p. 218.

11. In accordance with his salutary emphasis on the rhetorical dimension of economics, McCloskey notes that "[t]he economist who relishes the telling of a story of greed is advocating it, whatever he may say about 'is' and 'ought'" (1990, 141).

12. See, for example, Tversky and Kahneman 1981.

13. Determinism specifically, and causal completeness generally, are criticized at length in Dupré 1993, pt. 3.

14. One economist who was apparently highly skeptical of the legitimacy of assuming inductive stability of economic parameters was John Maynard Keynes. (See the letters to Roy Harrod reprinted in Hausman 1984, 300–302.)

15. An acronym of Margaret Thatcher's famous attempt to justify a particularly unattractive economic policy, "There Is No Alternative."

16. Though for the most complicated machines it may be appropriate to approach them in various different ways, as suggested by Dennett's well-known distinctions between the physical, design, and intentional stances that can be taken towards some machines, most notably computers (Dennett 1987). Because even computers are highly deterministic we are inclined to see the physical stance as basic. Perhaps this will be less compelling for the

machines of the future, in which case the traditional associations of mechanism will have become misleading even for the understanding of machines.

17. A few proposals have been put forward for economic projects radically different from traditional neoclassical economics, especially by feminist critics of economists. Work in progress by Julie Nelson (1993), for instance, suggests that we should replace a model of economics as primarily concerned with choice with one centered on the idea of provisioning. A very important and sustained critique of a variety of fundamental economic concepts, especially those connected with the measurement and comparison of national incomes, together with some proposals for doing things differently, is provided by Waring (1988).

18. Helpful comments on an earlier draft by Regenia Gagnier and Debra Satz have led to numerous improvements in this paper.

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