An Assessment of the Scientific Standing of Economics

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In his paper on the "Methodology of Positive Economics", Milton Friedman warned his readers that, "more than other scientists, social scientists need to be self-conscious about their methodology." (1953, p. 34). But until quite recently, he seems either to have spoken to deaf ears or, more plausibly, to have been so successful in promoting his own views on methodology as to lead economists to be complacent about the many problems which plague their discipline. Many current textbooks, for example the one by Eugene Silberberg, present economics as a science attaining the falsificationist standards once set down by Karl Popper, despite much evidence to the contrary. Indeed, as Douglas W. Hands (1985) has recently shown, even Sir Karl did not intend economics to be subjected to such severe standards.

Though a "swansong" for positivism was written by philosophers of science in the 1970s, most economists still perfunctorily adhere to an operationalist approach to economic behaviour. According to revealed preference theory, economists have supposed that one can analyze the economy solely at the level of prices and quantities exchanged, without delving into the actual nature of utility-maximization or any other psychological mechanism which might give rise to market exchange in the first place. Recently, a number of publications have begun to expose this lag (Boland 1982, Caldwell 1982, Wiseman 1983), but we have yet to witness, to the best of my knowledge, even mild retractions by the leaders of the field, notably Friedman and Paul Samuelson. The abandonment of revealed preference theory, however, need not render economics any the less scientific. If anything, economists can find comfort in the demise of the positivist program in the philosophy of science, particularly the irrevocable blurring of the theory-observation distinction. The standards to which they have long aspired are perhaps more in reach than ever before. I will suggest here, albeit in a somewhat sketchy way, that the path to redemption lies partly in restoring Carl Hempel's work on explanation and recognizing that the nearest kin to economics is history and not physics, as Smith, Mill, and Friedman, to mention only the most distinguished, have maintained.

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Much of the debate over the scientific reputation of economics centers around the possibility of putting the propositions of neoclassical theory to test, for whatever can be salvaged in post-positivist methodology, the aim of subjecting theoretical claims to empirical tests seems to have survived more or less intact. But this just raises more worries for the economist since, as Mark Blaug points out, so little of the theory is directly testible, particularly the core of microeconomics. (1980, Ch. 4). There are also few if any principles which bridge observational terms to theoretical ones, either directly or indirectly. In fact, as Blaug notes, economists have been more preoccupied with constructing, as elegantly as possible, versions of the theory of general equilibrium and for this reason have often lost sight of guiding their theory towards empirically testable propositions. There is little question that economists have tended to devise theories which are relatively immune to empirical refutation, contra Popper, stemming very much from what Axel Leijonhufvud (1973) has identified as ceremonial worship of mathematics. In this respect, then, there is a considerable lag between the prescriptions of methodologists and current economic theory.

According to Alexander Rosenberg's 1983 paper, "If Economics Isn't Science, What Is It?," neoclassical economics fails even to rank as an empirical science; rather it is like Euclidean geometry which was mistakenly regarded as the "science of space" until the advent of the theory of general relativity. As both Blaug and Rosenberg point out, it is very difficult to imagine what would lead neo-classical economists to reject their theory. But Rosenberg is then using faulty logic to assert that, for this reason, "economics is not empirical science at all" (1983, p. 303).[1] What he has identified is a sociological feature of the economics profession, not an epistemological crevice in the subject itself. Economics may not have been developed with the overriding goal of rendering it more applicable to the real world, but it nonetheless has empirical content to a degree never ascribed to Euclidean geometry (even by J. S. Mill). Perhaps a better comparison for economics might be to Ptolemaic astronomy which, though relatively immune from falsification, was still more than a branch of mathematics. The analogy is also borne out by the striking gap between the mechanism purported to give rise to the phenomena, and the actual techniques used to "save" them. Rosenberg exposes the faulty aspects of his analogy by suggesting that there is no external theory, comparable to the role physics played in favoring non-Euclidean geometry, which would prescribe one economic theory rather than another (1983, p. 311). And yet, psychological research might someday come to bear on the theory of the formation of expectations, one of the major sources for dispute amongst macroeconomists.

It is counterproductive at this point, particularly with no clear alternative, to dismiss neoclassical economics either as mathematical game-playing (Rosenberg 1983) or hired prize-fighting for capitalism (Hollis and Nell 1975). Even Rosenberg, amidst all of his criticisms, recognizes that current economic theory yields insight. Moreover, economic theory is not totally immune to refutation. Such events as the Great Depression of the early 1930's or the failure of the Bretton Woods agreement on exchange rates, have led economists to revise central portions of their theory, or at the very least, enabled the flock to see the wisdom in reviving previously neglected ideas. Though Blaug and Rosenberg fail to imagine what might override neoclassical theory, this is hardly a reason for ruling out such a possibility altogether. Economics has the makings of an empirical science, but needs careful grooming and the setting of more realistic goals. The task at hand is to sort out the more value-laden from the less value-laden, while always keeping in mind that economics, unlike stellar astronomy or high energy physics, is not conducted in a vacuum.

A more modest, less critical appraisal of neoclassical economics can be found in Allan Gibbard and Hal Varian's paper on "Economic Models." In several places they refer to the remarkable fecundity of economic theory and to the many rigorous demonstrations which have been devised from what were once vague generalizations. But they also set economics apart from the harder sciences. "Much of economic theorising consists not of an overt search for economic laws, nor of forming explicit hypotheses about situations and testing them, but of investigating economic models" (1978, p. 676) of varying degrees of empirical accuracy and robustness. They cautiously avoid passing judgment on the scientific merits of the discipline. Indeed, the impression their paper gives is that economists operate quite differently from physical scientists. If this is true, then perhaps the theory of the firm or the theory of the consumer, which Gibbard and Varian consider to be composed solely of descriptive models, need to be renamed. They are not theories in the same sense as the theory of electricity and magnetism or the theory of evolution. Textbooks, such as the one by Varian himself, need to be revised so as to make explicit the fact that economics, as they depict it, is comprised of "structured stories" rather than a systematic body of laws.

Much of what Gibbard and Varian suggest about economics sounds similar to the way in which historians proceed. They too seek structured stories, which are more or less faithful to the facts, and which often result in caricatures or distortions of some aspect of the past in order to make sense of the phenomena.[2] And their subject matter is in the final analysis the same as economics, to wit a causal theory of human action more or less detached from material forces. But as Carl Hempel and his followers have long argued, simply because historians rarely undertake an overt search for laws, or test hypotheses explicitly, does not make their activity any the less scientific. Their attempts to explain the past invoke the covering law model, or approximations thereof, in much the same way as a natural scientist. Needless to say, this glosses over a considerable amount of debate on the subject (see, for example, Gardiner 1974). It also puts aside recent attacks on the model (Cummins 1983, Cartwright 1983) which may carry the day. But insofar as the model captures even a part of the structure of scientific explanation (for no model is fully representative), history shares an essential feature with the other sciences.

True, historians do not usually pose the question Gibbard and Varian maintain is central to economic modelling, namely "what would happen if such and such were the case?" Historians have traditionally been less conjectural and more faithful to fulfilling the initial tasks of chronicling the past. But insightful historical analysis often stems from counterfactual queries, formulated in precisely the same manner as the question Gibbard and Varian identify as central to economic modelling. By manipulating the various factors in their story, they are able to discern in part what might have been a necessary cause, and what merely sufficient. Rather than resolve the debate over the scientific status of economics, or merely put it aside, the position advanced in "Economic Models," given its many striking parallels to historical inquiry, brings the debate full circle, back to the methodological monism once promoted by Hempel. Whether or not economists are explicit about their use of corroborated generalizations or tendencies is simply not at issue.

Another way in which the perspective Gibbard and Varian adopt brings economics closer to history is by tolerating rather than criticizing a lack of objectivity in economic theorising. According to them, "the difference between applying a model as an approximation of reality and applying it as a caricature lies in the intentions of the investigator" (p. 677). Whereas a model for the purposes of approximation strives more or less to be faithful to the real world, a caricature involves deliberate distortion to highlight one or a few aspects of economic activity. In short, by creating caricatures, the economist has put himself on par with the painter who chooses water colors rather than oils. If Gibbard and Varian are correct, the first to devise a model in economics injects a personal element into the very content which is then never fully extricated. Likewise in history, it is commonplace that the stylistic dispositions of the author, however sincere her intentions to capture what actually transpired, pervade every paragraph.

Economic models, Gibbard and Varian maintain, are often not revised so as to incorporate a greater degree of veracity (p. 673). Rather economists tend to start anew, not refine or polish the work of others. This is particularly the case in macroeconomics, where the assumptions are so highly stylized that there is little room for adjustment, even in light of empirical findings. True, there are many versions of the Keynesian or Classical models of aggregate supply and demand, and one can usually identify them as such because of the recurrence of a few salient features. Nevertheless, each author commonly sets out the model from scratch, with little concern for arriving at consensus over nomenclature.

Why, then, does economics lay claim to being an objective science? By the very principles economists themselves set down, it should be the problems at hand which dictate the most efficient means to arrive at a clear understanding of the chosen object of inquiry, and not the mere whim of the economist. If the economist chooses to highlight a particular feature of the reality he has parcelled off, then presumably it is because such a caricature is necessary to make the requisite insights. Under this interpretation, drawing caricatures is not only part of the logic of discovery, but also of the logic of justification. Insofar as this distinction is more difficult to discern in economic modelling, the approach of Gibbard and Varian once again suggests that economics is more analogous to history, where the two logics seem intimately connected, than to the natural sciences.

As an example of an economic caricature, Gibbard and Varian cite Samuelson's famous consumption-loan model. It is true that the tale Samuelson tells is highly stylized (people's lives are divided into two or three periods and are taken to be identical) and that he takes the liberty of suggesting a real-world commodity for his single good, namely chocolate (which miraculously never sticks to one's fingers). Contemporary economists often imitate this style, perhaps with the intention of adding flavor (no pun intended) to what is often in fact a very dry subject. And there is no doubt that Samuelson's paper (1966) is quite impressive for its versatility, in that it sheds light on such diverse topics, abstractly construed, as the time-path of interest rates and the problem of intergenerational bequests.

Any attempt, however, to apply directly his conclusions to say a system of social security would, I think, be disasterous, for the simple reason that his fairy story falls far short of capturing the complexities of the real world. In the real world, there are governments to enforce intergenerational exchange, there is more than chocolate to consume, and people are not pure egoists, as Samuelson assumes. Clearly, to assess something as important as a social security program, comparative empirical studies would be better suited to the task. This criticism is not intended to detract from Samuelson's paper, which as a piece of pure theorising is remarkably incisive and which succeeds very well in capturing the logic of social security systems. But his model could not incorporate, as is the case in the physical sciences, "friction" coefficients or other parameters which would enable the social engineer to approximate the actual problem at hand. Moreover, given the current limitations of econometrics, Gibbard and Varian are too generous in claiming that "when a model is applied to a situation, we can ask how close to the truth its statements are." (p. 668). As Clark Glymour recently noted, "statistical tests don't inform us as to whether or not a model is approximately true. They don't permit us to compare false models to determine which is closer to the truth." (Glymour 1985, p. 293).

Gibbard and Varian are on firmer ground when they remark that economic models, both theoretical and applied, are mainly useful for chanelling one's thoughts towards a specific problem, for giving one a feel for the underlying structure of a situation, rather than mimicing reality directly. Indeed, at one point in the paper the authors make the somewhat suprising claim that "the only statements of most applied models in economics that are true exactly are truths with no empirical content, such as definitions and mathematical truths." (p. 669). Samuelson's consumption-loan model is a case in point. Another reason why economists might persist with stylized models, Gibbard and Varian suggest, is to identify the very problems which call for a solution: "perhaps it is initially unclear what is to be explained and a model provides a means of formulation." (p. 669). This seems doubtful to me. For there is little or no ambiguity, as far as I can tell, what are the phenomena that still await explanation by the economist. Putting aside normative questions, economists are more than challenged simply to explain the level and motion of prices (including wages and interest-rates), the level and motion of inflation and unemployment, and the level and motion of production. Perhaps they have yet to rise to the challenge, but the sort of instrumentalism which both Friedman and Gibbard and Varian endorse tends to ignore the pressing demands of these real-world problems for which economists are often held accountable.

Historically, it is true that economists were not always concerned

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with explaining such things as large-scale unemployment, just as biologists did not recognize extinction as a phenomenon to explain until about 1800 (with the discovery of large mammalian fossils), though marine fossils had been identified as such since antiquity. But this in turn suggests that in economics (and history), the facts to be explained are not generated by the theory in the same way as in the physical sciences. If unemployment or cartels are not prevalent at the time, that is one less problem about which to be concerned. Insofar as the subject matter of economics is bounded by historical periods, economic theories, like history, must be rewritten with every generation. Economists can learn from the past, but strictly speaking, the economy never repeats itself to the extent found in the physical world. Even the business cycles noted by economists (and historians) have defied reduction to causal laws. For these and other reasons, Friedman's call for sound predictions as the goal of economic inquiry is quite misleading.[3] The economist can no more predict the future than the historian.

Yet another point of similarity stems from the realization that the phenomena of economics, the motion of prices and quantities exchanged, are of much the same order of veracity as historical events. There are, of course, some who maintain that the past is something we construct entirely, or that we can at best identify what transpired, not why (see, for example, Collingwood 1956). But insofar as we witness events, for example, the Iranian revolution or the Geneva peace talks, which will inevitably make their way into history, and recognize them as the result of specific deliberations, I think such sceptical claims can eventually be put to rest. Likewise, we daily observe the motion of prices. True, these are not quite the uniform equilibrium prices about which economists theorize. Such prices result from the aggregation of individual demand and supply curves. But then no single person has a full exposure to an historical event. When it comes time to reconstruct one such occurrence, the historian will attempt to synthesize numerous different perspectives. In this respect, we may have much the same sort of first-hand acquaintance with market-clearing prices as we do with historical events. Moreover, the width of such events may be quite analogous. Although historical and economic events in actuality have the same degree of exactitude as in say astronomy, when it comes to modelling these events, and more specifically, to specifying the ways in which a given model would match the real world, the events often become much thicker than mere points in time (see Walsh 1967, p. 142).

In the 1860s and 1870s there was a movement to adopt a historical perspective in economics, led by Gustav Schmoller in Germany and Cliffe Leslie and J. K. Ingram in Britain. The idea that history itself might be a science, however, was not entertained. My point is somewhat different. If economists wish to be credited as full-fledged scientists (and it was this intention which motivated the move to mathematize in the first place), their best tactic is not to follow the positivist's route, at least as it has been interpreted by Friedman, or to continue to emulate classical mechanics. Rather, the question of the scientific standing of their discipline will stand and fall with that of history. Both, it appears, have a fairly similar grip on the external world and rely upon the sort of situational analysis expounded by Hempel and Popper. Moreover, while economics may offer a greater scope for theorizing, in the final analysis it still has much the same goal as history: to explain social events as the result of human agency.

Notes

[1]Rosenberg informs me that he has changed his position somewhat. A new statement will appear in Ethics, Vol. 96 (1986).

[2]Passmore (1958, p. 105), for example, argues that historical accounts are in many respects like models in applied science.

[3]To be fair to Friedman, he incorporates past events in his definition of prediction. As long as the theory can anticipate or make sense of a some hitherto unknown event (past or future), it is doing its job (1953, p. 23). Nevertheless, he does not restrict himself solely to retrodiction.

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