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# ECONOMICS AS A “VALUE-FREE” SCIENCE \*

BY ROBERT L. HEILBRONER

Is economics a science? Partly that depends on how we choose to define the word. Does science mean a search for “repeatable patterns of dependence” among variables, the definition suggested by Ernest Nagel?<sup>1</sup> This nicely fits the current fashion for functional models in economics, but omits large areas of economic scrutiny, including economic history or economic taxonomy (comparative economic systems). Do we mean by science a reliance on the experimental method?<sup>2</sup> This throws into limbo certain central ideas of economics, such as value or utility, for which no experiments seem to be possible. Do we mean only the acceptance of a common paradigm, as suggested by Kuhn?<sup>3</sup> This then presents us with the problem of which economic paradigm to choose among a number of competing claimants: neoclassicism, institutionalism, Marxism.

I do not propose to explore here the question of which definition of science best applies to economics. Rather, my concern will be the relevance for economics of an idea that runs through *all* the ideas of science—the conviction that science must be “value-free.” By this I mean that all scientists agree that their work should be carried on in a manner quite independent of the biases and hopes, not to mention the willful interference, of the scientist. In a word, science exists to explain or clarify things that exist independently of the values of the observer. It is the study of what “is,” not of what “ought to be.”

We shall have occasion later to glance at the purity with which

<sup>1</sup> Ernest Nagel, *The Structure of Science* (1961), p. 4.

<sup>2</sup> P. W. Bridgman, *The Way Things Are* (1959), p. 130.

<sup>3</sup> Thomas S. Kuhn, *The Structure of Scientific Revolutions* (1962, 1970).

science keeps its vows. But I think it fair to state that the vow itself constitutes an ideal to which almost all economists gladly and wholeheartedly subscribe. However they may define their task, nearly all would include “value-neutrality” as a necessary condition for the performance of those tasks in a “scientific” manner.

It is this central contention that I wish to challenge here. I will deny that the vital element of economic analysis (and I will define in a moment what I mean by this “vital element”) can ever be wholly devoid of considerations of a normative or judgmental kind. To put it more strongly, I will try to show that the economic investigator is in a fundamentally different relationship *vis-à-vis* his subject from that of the natural scientist, so that advocacy or value-laden interpretation becomes an inescapable part of social inquiry—indeed, a desirable part.

That, however, is not all I wish to argue. For having sought to demonstrate that economics is not and should not be value-free, I will then turn around and insist that it should nonetheless retain as an objective the methods of science. The resolution of this seeming conflict will constitute the second objective of my paper.

Let me begin with the simpler part of my task, which is to argue that the work of the economist is laden with value judgments. Perhaps the best way to do so is to observe an economist at work. Let us say that he is collecting certain data—say the size distribution of corporations or the movement of prices. This is assuredly a procedure as objective and value-free as that of the natural scientist collecting data on the sizes of natural objects or the movements of the planets. (There are, to be sure, value considerations hidden in his *choice* of research object, but we will let that problem rest for the moment.) Our economist may then relate his first set of data to a second set—say the profit rates of corporations ranked by size, or the quantities of goods ex-

changed at various prices. Here, too, he breaches no rules of value-neutrality, assuming of course that he does not winnow his facts or doctor his observations, and that he avoids falsely imputing causal relationships to his resulting correlations.

Is not such work quite as value-free as that of the natural scientist who performs similar observations or correlations on the objects of the physical universe? Indeed it is. Furthermore, these findings of the economist may be of the utmost importance. But what he has performed up to this point is not yet economic *analysis*, or at least not that “vital element” of analysis to which I earlier called attention. Thus far he has only performed the task of an economic statistician. If economic analysis stopped at this point, the basic contention of my paper would be false.

But an *economist*—not an economic statistician—does not stop here. Indeed, his task now begins—the task of ascribing meaning to the data and the relationships that he has so painstakingly acquired. This meaning takes the form of efforts to “explain,” postdictively or predictively, how and why the social organism displays the objective characteristics he has unearthed. And here is where value judgments inevitably insinuate themselves into his work.

Consider a very simple case. In every elementary textbook on economics (including my own), we find a standard example of economic analysis in the discussion of the social result of imposing a price ceiling below the “equilibrium” price for a commodity, say, a rent ceiling on apartments. At the below-equilibrium price, we are told, there will be more would-be buyers (renters) “in the market” than before the ceiling was imposed. The result is the classic instance of a “shortage”—that is, a situation in which the quantity of a commodity demanded at a given price exceeds that which is offered at that price.

Now, is this not also a “value-free” finding, as removed from the wishes or biases of the economist as the finding of a natural scientist that a compass needle swings when a magnet is placed near it? Has not the unduly depressed price of the commodity

“attracted” buyers in the first case, in the same way that the force field of the magnet has “attracted” the needle in the second? The question brings us to the critical parting of the ways between value-free natural science and value-laden social science. But the answer is not as simple as it might first appear, so I shall take some pains to spell it out carefully.

As perhaps you have anticipated, there is one very easy mode of demonstrating the value-laden content of economic analysis as contrasted with that of the natural scientist. It is that economists do not remain content with a simple observation (presumably derived by empirical techniques) that there co-exist a rent ceiling and a large number of disgruntled apartment-seekers. Invariably they go on to *prescribe* social remedies for this situation, usually remedies that fall back on the workings of the market system. “Thus,” writes Paul Samuelson, “France had practically no residential construction from 1914 to 1948 because of rent controls. If new construction had been subject to such controls after World War II, the vigorous boom in French residential building since 1950 would never have taken place. . . .” He concludes: “To protect the poor from being gouged by landlords, maximal rentals are often fixed by law. These fiats may do short-run good, but they also do long-run harm.”<sup>4</sup>

It is not difficult to spot the value judgments latent in this example of economic analysis. There is a silent acquiescence in the propriety of the market as the mechanism for allocating apartments to would-be renters, rather than government allocations, or other means. There is also the assumption that the “long-run harm” cannot be overcome by non-market means, e.g., the provision of additional dwelling space by state construction. Now, Samuelson may have sound philosophical grounds for preferring the market means of allocation to non-market means, and he may be correct in his contention that the market will ultimately provide more housing than will a program of government construction. But it is quite clear that neither his preference nor his

<sup>4</sup> Paul A. Samuelson, *Economics*, 8th edn., p. 372.

policy judgment follow as “value-free” conclusions from the raw data of ceiling prices and disgruntled apartment-seekers.

Since I have already declared that I do not believe that economists should aim at value-free analysis, it is not my intent to chastise Samuelson for introducing what are clearly value-laden statements into his text. (I *am* concerned about his failure to alert his readers to his value assumptions, but that is another matter—to which I will return later.) Therefore I will not further pursue the easy course of calling to attention other such institutional biases that affect the manner in which economists consciously or unconsciously move from initially neutral facts to ultimately loaded conclusions. Instead I shall set forth a more intricate and abstract, but I think more fundamental, argument. This is the argument that the inherent and inescapable value-content of economic analysis lies in the fact that the “behavior” of objects of social analysis is *not* like the behavior of the needle of the compass. In the difference between the two meanings of the word “behavior” lie the roots of the value problem for social science.

Of course we all know that human beings do not behave like so many iron filings or compass needles. Yet, when we inquire into the reasons for, or the nature of, the difference, the answer is not immediately apparent. Take the scientist who has observed the effect of a magnet on a compass a hundred times, and the economist who has observed the effects of lower prices on expenditures a hundred times. Assume that all the treacherous problems of extraneous influences are eliminated—that *ceteris paribus* truly prevails. In what way is the economist prevented from describing the behavior of his social universe by “laws” that are just as objective as those of the natural scientist?

The answer is obvious, but its implications may not be. The difference is that the objects observed by the social scientist all possess an attribute that is lacking in the objects of the natural universe. This is the attribute of consciousness—of cognition, of “calculation,” of volition. Individuals and social organizations

*do* often behave in ways that are as regular as those of the objects of physics and chemistry—if they did not, society would have long ago disintegrated. Yet, even in the most routine human actions there resides an element of latent willfulness that is lacking from even the most spectacular processes of nature. Indeed, one of the decisive attributes that distinguishes the social world from the physical is that social events are not merely interactions of forces, but contests of wills.

Thus behavior has both a purposiveness and a capriciousness that makes prediction infinitely more difficult than for the natural scientist. It is for these reasons that our efforts to predict economic behavior—however accurate in the “normal” case—suddenly become inaccurate when behavior changes its purpose or displays its caprices. The record of prediction with regard to stock market fluctuations, foreign exchange rates, price levels, or even the growth rate of vast aggregates like GNP, is all evidence of this “distressing” unreliability of behavioral regularity.

But what is the relevance of this unreliability to the problem of the value judgments concealed in economic analysis? The relevance lies in the central role played by behavior (and by the prediction of behavior) in the progress from value-free facts to value-laden conclusions. *Without assumptions about behavior, no conclusions whatsoever can be drawn from any set of social facts.* The problem, then, becomes one of discovering the value-component which is intrinsically part of our behavioral assumptions.

But why “intrinsically?” The answer is a curious one. If the economist hews to a strictly empirical description of behavior, given its latent unpredictability, he retains his value-neutrality, *but at the cost of any usable theory.* To put it differently, if the economist wishes to move from economic statistics to economic analysis, he must go beyond “observations” into “assumptions” with regard to behavior, and it is at this juncture that value judgment enters the picture. For when we examine the analytical work of economists, we do not find that their behavioral

propositions are carefully framed to reflect the fundamental uncertainty that beclouds all behavioral "laws." Instead, we discover that economic behavior is almost universally described in precisely the "magnetic" fashion of the needle and the compass. The ruling "law" of behavior which is assumed to apply to consumers, workers, and businessmen alike is that they seek to "maximize"—consumers maximize their "utilities," workers their incomes, businessmen their profits.

Do they? The question is embarrassing on at least two counts. The first is that we have a great deal of difficulty in specifying exactly what kind of behavior we mean by "maximizing." For example, how shall we specify the behavior of a corporation which seeks to "maximize" its profits, presumably for a very long period of time, with respect to its price policies, its labor policies, its governmental relations, etc.? <sup>5</sup>

Second, there is the awkward probability that whatever behavior presumably "maximizes" utility or profits in one period is not likely to be that which maximizes in another. Lowered rents will not attract renters, as a magnet attracts a needle, if the renters expect the rent ceilings to be still *lower* in the future. So, too, we must take into account changes in the state of mind of the economic actors over history. However consumers may have behaved in the days of the Industrial Revolution when they sought to maximize their utilities, it is surely not the way they behave in the days of the Advertising Age; nor do the entrepreneurs of the New Industrial State, wrestling with the difficulties of maxi-

<sup>5</sup> A problem into which we cannot enter here, but which warrants passing mention, is the similarly empty content of "maximization" with regard to consumers. Of all the terms of economics, none is so cavalierly dealt with as "utility," which is presumably the *summum bonum* of individual economic behavior. Samuelson disposes of it in one sentence: "As a consumer you will buy a good because you feel it will give you satisfaction or utility" (*op. cit.*, p. 410). Yet, in a famous earlier work, *The Foundations of Economic Analysis* (1965, pp. 90–91), Samuelson warned against definitions of utility that are "consistent with all conceivable behavior, while refutable by none!" For a discussion of the difficulties of giving operational content to the "maximizing" behavior of corporations, see Marris and Wood, *The Corporate Economy* (1971), pp. xviii–xx, and A. Lowe, *On Economic Knowledge* (1965), pp. 34–35, 47–48.



mization of which I just spoke, resemble the entrepreneurs of Dickensian England, counting up each day's receipts.

Thus the claim to a knowledge of economic "laws" requires a degree of "insight" wholly different from that required to enunciate natural laws. The natural scientist does not care about how his needle feels about magnetism, but the social scientist *has to know* how his buyers and sellers feel about the "attraction" of prices if his analysis is to be grounded on anything other than guesswork or blind faith.

This crucial aspect in the meaning of social behavior infuses economic analysis with values in two ways. The first has to do with the fact that economists arbitrarily apply to economic reasoning "laws" that they know to be at best partial descriptions of reality and at worst outright mis-descriptions of it. This is surely an attitude at variance with the willingness of the scientist to abandon a hypothesis when it no longer conforms with observations.

Why do economists persist in their *mumpsimus*—a term Joan Robinson has unearthed (no doubt from English crossword puzzles) that means "persistence in a belief one knows to be mistaken?" The answer is, I believe, embarrassingly simple. It is that economists must have *some* kind of behavioral assumptions to make their theories "work." Lacking any better generalization, economists have retained the convenient assumption of maximization because it serves this purpose—even if the resulting theory often works very badly as a predictive instrument.

A second reason for the retention of the assumption of maximization introduces the problem of value-judgment from a different perspective. It is that maximization, for all its vagueness and error, generally accords with the prevailing orientation of most economists that "more is better." The idea of maximization thereby gives a certain "scientific" authority to textbook statements that the consumer who climbs to the peak of his indifference map is more "satisfied" than one who camps out, like a vagabond, on some lower contour, or that an economy with a high

growth rate is “better off” than one with a lower rate. In a word, maximization becomes a prescription for conduct. Since we are all now acutely aware that more is not necessarily better, I will not belabor the value implications of this belief, other than to equate it with a latter-day version of Benthamism, in which push-pin, poetry and pollution are all the same, so long as they get counted in the Gross National Product.

The charge that economics is deeply immersed in value orientations is not a new one, and I shall not spend more time in seeking to prove the point. Indeed, many readers may have wondered why I did not make a much more immediate attack. This is to point out that the value-judgments of economics can be discerned at a simpler level than the one to which I have paid attention—to wit, the ideological biases exemplified in my discussion of rent ceilings. There is an obvious political bias observable in the choice of research tasks arrogated to itself by the profession—the doubter may wish to compare the contents of *The American Economic Review* with that of the *Review of Radical Political Economics*. There is the general failure on the part of economists to recognize that the essential terms of their vocabulary—labor, capital, interest, even wealth—are all historical concepts fraught with socio-political implications.

If I have not chosen this road, it is not because it is not relevant to the topic (indeed, I will return to it later), but because it has been well covered by others.<sup>6</sup> My purpose, therefore, was to call

<sup>6</sup> The literature of “ideological” criticism is substantial. Let me cite here only a few examples. The founding father is no doubt Marx, especially the three volumes of *Theories of Surplus Value*. There is the well-known critique by Myrdal, *The Political Element in the Development of Economic Thought* (first published in 1928). For more recent statements see W. Leontieff, *The American Economic Review*, March, 1961; Benjamin Ward, *What’s Wrong with Economics?* (1972); Assar Lindbeck, *The Political Economy of the New Left* (1971), and my review of it in *Political Science Review*, September, 1972; and numerous essays in the *Review of Radical Political Economics*, especially Vol. 3, No. 2 (July, 1971), entitled “Special Issue on Radical Paradigms in Economics.”

attention to a less well-explored aspect of the problem lodged in the interstices of economic analysis itself, rather than in the underlying premises of economic thought.

But all this is, in a sense, preamble to the more difficult task that I set myself at the outset. This is to question the legitimacy of the idea of "value neutrality" as an ideal for economics, and at the same time to defend the idea of "science" as an appropriate ideal for economics. The task sounds like a contradiction in terms, so I shall proceed with care, trying to specify with precision what I believe are the elements at stake.

The first problem with "value freedom" concerns the psychological or sociological relationship between the observer and the thing observed. Presumably the scientist approaches his research object in a frame of mind that is without conscious prejudice—fearlessly open to an acceptance of results, however unexpected or unwelcome these may be.

This attribute of scientific inquiry has come under sharp attack in the natural sciences. The work of both Polanyi and Kuhn has made it abundantly clear that scientists do not in fact behave with indifference to their observed results, but struggle desperately to fit "anomalies" into preconceived patterns or paradigms, explaining away or simply ignoring results that fly in the face of prevailing expectations.<sup>7</sup> If this is the case with the natural sciences, it is far more so with the social sciences. Within the field of economics many instances can be cited to demonstrate the absence of that scientific detachment that supposedly characterizes the scientist at work. Let me only mention in passing the long intellectual struggle against Keynesianism and in more recent days the equal unwillingness to abandon the Keynesian notion that inflation was incompatible with substantial unemployment. Or I might call attention to the unwillingness of economists to admit the phenomenon of imperialism as a proper subject for economic investigation, or their dogged adherence to

<sup>7</sup> Michael Polanyi, *Personal Knowledge* (1958); Kuhn, *op. cit.*

a benign theory of international trade in the face of disquieting evidence that trade has failed to benefit the poorer lands.

As in my previous discussion involving Paul Samuelson's unwitting use of value criteria, my purpose is not to scold economists for their lack of objectivity. It is rather to point to the cause for this universally observed state of affairs. This cause lies in the fact that the process of social investigation inescapably embroils the investigator in his subject in a way that is different from that of the natural scientist. For the latter, the discovery of an anomaly may constitute a blow to his intellectual "security," perhaps even to his psychological "integrity." *But it does not threaten his moral position as a member of a social order.*

On the contrary, the discovery of unexpected results in the social universe almost invariably threatens or confirms the legitimacy of the social system of which the social investigator is unavoidably a part. Indeed, at the risk of making an assertion that verges on a confession, I would venture the statement that every social scientist approaches his task with a wish, conscious or unconscious, to demonstrate the workability or unworkability of the social order he is investigating. It is not a matter of indifference to the neoclassicist or to the Marxist whether his data fit the hypothesis he is testing, and each struggles mightily to explain away, to minimize, or to reject results that go counter to his initial beliefs.

Moreover, this extreme vulnerability to value judgments is not a sign of deficiency in the social investigator. On the contrary, he belongs to a certain order, has a place in it, benefits or loses from it, and sees his future bound up with its success or failure. In the face of this inescapable existential fact, an attitude of total "impartiality" to the universe of social events is psychologically unnatural, and more likely than not leads to a position of moral hypocrisy. It is not one of their flaws, but one of their claims to greatness as economists that Smith, Ricardo, Mill, Marx, Marshall, and Keynes were explicit in their use of facts and theories as instruments of advocacy. Smith's great model of the economic

system was written not merely to “analyze” late eighteenth-century England, but to plead for a policy of “perfect liberty” and to assail the policies of mercantilism. Ricardo used his theory as the underpinnings of his attack against the Corn Laws. Mill’s *Principles* advocated a stationary state and income redistribution. Marx espoused social revolution, based on his economic model of the “immanent” tendencies of capitalism. Marshall was a partisan of cautious and careful social change, the rationale for which was spelled out in his *Principles*. Keynes sought the social control over investment, for reasons that the *General Theory* made clear.

These “policy” prescriptions were not afterthoughts. On the contrary, they were an inextricable part of the great contributions of these economists to social understanding. Yet in every case, they rested on value-laden assumptions. The most obvious of these, to which I have referred in passing but have purposely not discussed in this paper, lay in their beliefs in the propriety or impropriety of the *class relations* of the societies they analyzed. Take away the sociological or institutional parameters from the thought of the classical economists (or from Marshall and Keynes) and there is nothing in their systems that could not have led them to conclusions similar to those of Marx. But—and this is the element I have chosen to highlight—there is also in every instance the assumption that maximization is the behavioral force that makes the social universe move. Take away maximization, and the conclusions of Marx can be rather easily made to conform with the mild policy prescriptions for the stationary state proposed by John Stuart Mill.<sup>8</sup>

<sup>8</sup>Lowe (*op. cit.*) maintains that short-run maximizing behavior—i.e., behavior that seeks to maximize receipts and minimize expenditures within very short time horizons—may well have been a reasonably accurate generalization with respect to behavior in the early days of industrial capitalism, but that the rise of consumer affluence and corporate oligopoly have progressively enlarged the area of “discretionary” activity, and thus also enlarged the degree of unpredictability that afflicts statements about behavior. This introduces an historical element into the problem, whose implications I shall not pursue further in this paper. Lowe maintains that economic theory (including its inescapable behavioral sup-

Thus value judgments, partly of a sociological kind, partly with respect to behavior, have infused economics from its earliest statements to its latest and most sophisticated representations. And indeed, insofar as economic analysis is concerned with social change, in which the fortunes of men (including the analyst) must be affected, how could it be otherwise?

But this leads me to my final contention—that despite its immersion in values, norms and advocacy, economics should nonetheless attempt to embrace “scientific” canons of procedure. How is it possible to reconcile such seemingly contradictory positions?

The reconciliation involves as its first step a return to our earlier dichotomy between economic statistics and economic analysis. So far as the former is concerned, there is little to trouble us. Precisely the same standards and precisely the same pitfalls confront the economic statistician as the biologist or the physicist. Both must struggle against the inhibitions imposed by the reigning paradigm, first in their choice of research objects, and second in their treatment of research results. Both confront, albeit in somewhat different ways, the problem of the interaction of the observer with the things he observes. It is not here that the problem lies.

The question is, rather, how the economic analyst, whose analysis *must* include normative elements, can aspire to the position of the scientist. Here, at this critical last juncture, I must first state with all the force at my command that I do not believe that the economist has the right, in the name of value-advocacy, to tamper with data, to promote or promulgate policy recommen-

positions) was empirically defensible in the early to mid-nineteenth century, but has become progressively less so in the early to mid-twentieth century. His treatise *On Economic Knowledge* is essentially an effort to circumvent this problem by urging that modern economic theory cease its efforts to create “predictive” theory (which depends on reliable behavioral laws), turning instead toward an “instrumental” approach which seeks to specify a range of behavioral responses and patterns that are compatible with the attainment of a postulated “goal.” I return briefly to this redefinition of the task of economics in footnote 9.

dations without supporting evidence, or to pass off his value-laden conclusions as possessing “scientific” validity. Indeed, one of my objections to much of contemporary economics is that it lends a gloss of such “objective” validity to conclusions that in fact only follow from arbitrary and value-laden assumptions—I refer, for example, to the use of neoclassical economics to “disprove” the usefulness of minimum wage laws, etc. Of course minimum wage laws *may* bring consequences other than those desired by their sponsors. But I hope that my labors in analyzing the dubious nature of the usual assumptions about economic behavior now make it possible for me to state that no economic predictions or prescriptions that rest on these assumptions can lay claim to any “scientific” validity.

How then can the economist possibly aspire to the standards of a social *scientist*? The answer does not lie in efforts to produce behavioral “laws” that will be the counterpart of the laws of nature—that is a chimerical task.<sup>9</sup> The answer lies rather in his efforts to duplicate the methods, not the models, of the natural sciences.

What are these methods? They are to be found, above all, in the openness of the procedures by which science goes about its task, exposing itself to informed criticism at every stage of its inquiry, engaging in painful self-scrutiny with regard to its premises, experiments, reasoning, conclusions. Revelation, “truths beyond question,” unstated premises, missing links in the chain of deduction may all be found in “scientific” analysis, but they are

<sup>9</sup> As Lowe has argued (*op. cit.*), it well may be that the problem for economic analysis lies in specifying the behavior that is required to attain certain postulated goals or targets. His suggestion thus changes the paradigm of economics from that of a “positive” science, predicting future states on the basis of “laws” of behavior, to that of a means-ends science, investigating the various behavioral paths by which a society may attain a goal. This does not, of course, make economics “value-free,” since the selection of alternative routes will inevitably reflect the preconceptions of the investigator. It does, however, free economics from the particular value-orientations implicit in the laws of behavior which it now takes as “constants” of the social universe.

by common consent its weakness to which criticism is rightly directed.

This element of science can be transposed in its entirety to economic analysis. Like the natural scientist, the economist (or for that matter, any social scientist) is expected to keep his journal, recording as best he can his starting points, his successive steps, his final conclusions. He records, with all the honesty and fidelity of which he is capable, not only his data and his processes of reasoning, but his initial commitments, hopes, and disappointments. Since economists perform few experiments that can be rerun in a laboratory, his results cannot be so easily falsified as those of the natural scientist, but they can be equally subject to scrutiny and criticism in the forum of expert opinion.

Thus when I urge the abandonment of the idea of a “value-free” economics, I do not thereby seek to abandon the idea of an economics committed to scientific standards. Rather, I want economics to make a virtue of necessity, exposing for all the world to see the indispensable and fructifying value-grounds from which it begins its inquiries so that these inquiries may be fully exposed to—and not falsely shielded from—the public examination that is the true strength of science.

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