Robert L. Heilbroner *

ON THE LIMITS

OF ECONOMIC PREDICTION

The uses of economics are many, as are also the methodologies by which its various purposes can be defended or elucidated.[1] In this paper I shall confine my attention, however, to but one of its many aims—the predictive intent that constitutes a distinguishing characteristic of so much economic work. Lest I be accused of claiming that prediction is an integral part of *all* economics, let me explicitly point out that the discipline has taxonomic, structural, purely formal, normative, and still other aspects, none of which involves prediction, and all of which have their useful roles to play. Nonetheless, I do not think that most economists would disagree with my contention that a predictive purpose lies at the heart of much of economic science today, in particular when that science is used on behalf of policy.

* The author is Professor of Economics at the Graduate Faculty of the New School for Social Research. He wishes to thank Adolph Lowe and Fritz Machlup for their criticism and assistance.

[1] There is a considerable literature on the matter. Let me mention the standard references [2, 4, 12] and call attention to these works in particular [5, 8, 9, 10].

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The question I then wish to pursue is whether we can discern—admittedly in a general rather than a particularistic way—any inherent boundaries or limits to this predictive capability. It may be objected at the outset that in putting the matter in this way I have already begged the crucial question by assuming that economics *has* a predictive capability. Since the bulk of this paper will be devoted to establishing the reasons for believing that this capability is severely bounded, perhaps I had best begin by discussing how economic science can lay claim to the very possibility of prediction in the first place.

I

As with all the sciences, economics asserts its capabilities for prediction on one of two grounds. The first we may call *correlational.* Correlational prediction establishes that certain relationships have been regularly observed in the past, and predicts that they will again be observed in the future *without positing any causal explanations of the observed relationship.* For example, Friedman observes that individuals are "extraordinarily stubborn about the real amount of money they want to hold." He suggests that "Part of the explanation is the currency held by business enterprises. I do not know what the rest of the explanation is." He predicts that if the amount of nominal money in the community is increased, "people can and *will* [my emphasis] try to reduce their cash balances and the process of trying... will bid up the prices of all sorts of goods and services." [1, pp. 10, 11, 12]

In some realms of pure science, where the notion of causality disappears into a mathematical cloud, it may be that correlational prediction is an ultimate terminus for inquiry. This is a matter of current controversy, but it is not relevant to our concerns. For in the applied natural sciences, or in the sphere of social science, correlational forecasts must be taken as no more than a make-shift substitute for a more "solid" basis for prediction: the construction of a model in which a special kind of *functional relationship* is established among the variables in the process.

This special relationship consists in identifying the interaction of the variables as specific instances of a general case, or if we will, as instances of the workings of a "law." For example,

if we are asked to predict whether a certain profit-seeking firm will hire another worker, and if we know that the marginal cost of that worker will be greater than the marginal revenue attributable to him, few economists would hesitate to predict that the worker will not be hired. Why? Because the prediction follows logically as a conclusion from the "law-like" premise about the short-run maximizing behavior of entrepreneurs.

In view of the higher status of this second kind of prediction, I will concern myself in the remainder of this article exclusively with problems that affect what we may call predictive model-building, or more conveniently, predictive theorizing. Perhaps I should add to my opening caveat that not all theorizing need be predictive in purpose, and that not all prediction—vide correlational analysis—need be based on theoretical models. But economics certainly abounds with models constructed with predictive intent, and it is the properties of these models that we must now examine.

All predictive models, economic or other, rest ultimately on two preconditions. The first is the ability to conceive fruitful categories of generalization with which to bring intellectual order into the world of raw data. This obvious precondition is more important than might at first appear, for it determines what it is that we wish to predict. That problem will not concern us until our conclusion: instead let us note that to be useful for a predictive model this concept—or construct-forming activity—must be accompanied by the *practical possibility of fleshing out the construct with data.* The theory of income determination, for example, awaited both the conceptual formulation of such relevant categories as consumption, investment, etc., and the establishment of the means of compiling the data in question. In much the same way, predictive models of the natural world depend not only on the gradual evolution of the appropriate "paradigms," but on the subsequent development of the data required to make the new constructs statistically operational. [6, chps 7-9]

The second aspect of the predictive model-building process is no less essential than the first. It is the ability to formulate so-called "higher-level" hypotheses capable of embracing the data that have been compiled as potential "evidence" under the guidance of the initial constructs. The success of the predictions

that are thereafter made by deductive logic depend, therefore, on the extent to which the overarching hypothesis succeeds in summarizing the "repeatable patterns of dependence" [11, p. 4] of the events themselves. In a word, the model will predict successfully, provided that its higher-level hypotheses are "right."

I do not think there is much disagreement as to the general nature of predictive theorizing that I have just described. Now I wish, however, to proceed to my central theme, which is to inquire into the limitations of this process when it is applied to the specific universe of events denoted as "economic." More precisely I wish to show (1) that there are indeed limits to the predictive capability of economic model-building, imposed both by the nature of the data and by difficulties in framing hypotheses capable of "anticipating" events in the real world; and (2) that the nature of these limits is very different in the short run and in the long. At the conclusion I will venture a word as to the handicaps that this predictive limitation imposes on economic science.

II

At first glance, economics seems unusually well-adapted to the predictive tasks it undertakes because it builds its predictive theories on two well-defined hypotheses, both of which give every promise of yielding reliable results. The first of these concerns the physical nature of the production process with which economics is largely (although not exclusively) concerned. By the physical nature of the production process, I mean the "engineering" sequence of inputs and outputs, or the technical combination or coordination of different kinds of inputs to achieve a desired output. The act of predictive theorizing begins with the premise that these physical requirements of production can be described in functional terms that will enable us to know *how changes in inputs will affect outputs.* These functional relationships may be very complex, but it is assumed that they will not be arbitrary or unknowable. If we are to forecast the economic process, there must be no "surprises" in the production process. The production function need not be linear or smooth, but it cannot change without warning into a step function, or

display sudden discontinuities, without making the act of prediction *ipso facto* impossible.

We shall revert shortly to the plausibility of this essential precondition for economic prediction. But first we must consider a second and no less necessary condition. This is the assumption that we can make reliable statements of a functional kind concerning the *behavioral response to economic stimuli.* These responses need not necessarily be "maximizing" (or even acquisitive), for perfectly adequate hypotheses concerning behavior can be based on the assumption of homeostatic responses (as in some aspects of traditional societies), or potlatch behavior, or whatever. The only requirement, clearly similar to that applicable to the production process, is that whatever the behavioral response, it must be related in a determinable manner to the stimuli that produce it. We must know, before the fact, how human actors will react to economic incentives or sanctions if we are to foretell the movements of the economic system.

I do not think there can be much question that the *possibility* of predictive theorizing in economics rests on these two premises. More to the point, there is not much question, either, that the two premises are "validated" in the real world. For it is not merely the possibility of economic theory, but the continuity of industrial market societies that rests on these basic assumptions. If most production functions were in actuality irregular or discontinuous, the smallest changes in the organization of the productive process could lead to a breakdown of the economic system. In the same way, if behavioral reactions were generally unreliable and "lawless," the web of interactions that binds together the market mechanism would have broken down long ago. Thus in the very persistence of the economic system we can find strong common-sense support for—although not, of course, logical proof of the validity of the two hypothetical pillars of economic prediction.

The failure of the real world to falsify our basic hypotheses must be qualified, however, by the time span and degree of fineness of our predictions. It is one thing to "predict" the continuity of the system, based on our suppositions of regular production and behavior functions, and another thing to use these presumed functions to predict precise rates of growth,

levels of employment, frequency and amplitude of cyclical deviations, etc. It is clear enough from experience that our capability for precise prediction is, to say the least, somewhat less than perfect. The question must then be asked as to whether this failure to produce finely timed and exactly quantified predictions is due to a failure to allow for extraneous factors (comparable to gusts of wind that might disturb a projectile's flight path), or whether it reflects deeper-rooter shortcomings in the data-gathering or hypothesis-making constituents of the theorizing process.

It will be useful if we begin to examine this critical question by dividing the aims of predictive theory into a short-run and a long-run aspect. By the short-run, we mean a period of time during which the fundamental constituents of the two functions do not change. In the case of the production function this requires that we hold technology "constant" and focus our attention on changes in the input mix and in the input-output relationship within a "given" level of technique for the economy as a whole.[2] With behavior, we also take as invariant the fundamental determinants or the underlying motivation, assuming that the maximizing or homeostatic or other drives continue to hold sway during the period in question. In a word, we confine our attention to movements along, rather than of, the production and behavior curves.

Turning now to the production function, and asking what obstacles it poses to "perfect" prediction, we immediately encounter the problem of the adequacy of the data from which our higher-level hypotheses must be formed. To predict the effect of every possible small change in inputs on outputs, even assuming that techniques are unaltered, we would have to know the slope and shape of isoquants for every important commodity, which is to say the physical, chemical and engineering requirements for an enormous array of products and services.

Here we enter a world in which there exists a vast literature in the abstract and a very small one in the concrete. Although

[2] A "given" level of technique is not an easy concept to specify. We will use it in the sense that no changes in the production process affecting the main items of output during the period in question would require the replacement of large quantities of capital or the retraining of substantial numbers of the labor force.

we have a clear understanding of the notions of marginal elasticities of substitutions, variable returns to scale, increasing and diminishing returns, etc., the clear fact is that we do not know the shape of the actual production functions for most commodities, as is demonstrated all too plainly in our inability to construct a dynamic input-output matrix.

Thus there are obvious limitations on our ability to make fine predictions arising out of our sheer ignorance of the nature of the production process. Yet, even if it takes a heroic act of faith, it is not inherently implausible that such knowledge could be attained. There are, as far as we know, physical and chemical properties of materials that determine the proportions in which they can be combined, and given the constraint of an unchanged technology, there are also limits established by mechanics as to the minimum amounts of labor energy of different kinds that are required to work a given process. Thus is seems within the bounds of plausibility that it would be possible to construct a set of production functions that would mirror with a fair degree of accuracy the actual production possibilities open to society in the short-run; or to put it differently, we would expect the degree of precision of our predictions to increase concommitantly with our grasp of the available facts.

Quite a different situation faces us, as Lowe emphasizes [8, pp. 34-39], when we look into the requirements for a reliable behavior function for the short-run. For here, the matter to be predicted is a psychological rather than a physical reaction. In order to predict the behavior of economic actors we must know (1) whether a given stimulus—a price rise, a change in income, a government directive, etc.—will give rise to "positive" or "negative" behavior, that is, to buying or selling, to investing or disinvesting, to compliance or disobedience, and (2) we must also be able to estimate how "intense" will be the response called forth by the stimulus.

It is obvious that the compilation of the necessary data to support our behavioral hypotheses is intrinsically much more difficult than is the case with the world of production. Yet if we again make a heroic effort of belief, it seems possible to venture an affirmative estimate as regards at least one of the wanted sets of behavioral data, that of the intensity of reactions to given stimuli. Assuming that we know the direction

of response, it is plausible to expect that a study of sufficient quantities of data could yield fairly reliable patterns of elasticities of substitution among commodities, or of the marginal propensities of different groups to consume or invest, etc. If we had such data—and there seems no inherent reason why we could not steadily improve our knowledge of such behavioral traits—we could then predict, for example, by how much our purchases of x would rise (or fall) if the price of x or y were to change by such-and-such a percentage, of by how much a change in the rate of interest would induce or restrict the flow of investment.

Hence, whereas there is a practical limitation to the capability of short-term prediction in the collection of data (no doubt much more difficult in the case of behavior than in the case of production), here too we find no inherent limit to the erection of usable higher-level hypotheses. Moreover, economic predictions can be very useful even if they are not wholly accurate. I do not have to be able to foretell the degree of change within a fraction of one percent to render a useful service in predicting that the effect of an increase in government spending is likely to be a rise in the volume of output. Or to put the case even more generously, it may be enough that I can make negative predictions, such as that the effect of a rise in interest rates is very unlikely to bring about an investment boom.

The problem of short-run prediction takes on a graver aspect, however, when we turn to the remaining component of behavioral regularity on which a reliable model would have to be based. This is the question of whether a given stimulus will induce "positive" or "negative" behavior on the part of the actor. As is well known, a price rise, interpreted as a precursor to further price rises, will induce additional rather than decreased buying. A penalty for, say, hoarding, read as the sign of worse to come, may bring about a rush to hoard, etc.

This indeterminacy in the "direction" of economic response is more or less confined, it should be emphasized, to critical moments or turning points in the economic process—indeed, the presence of "perverse" behavior may be a major factor in bringing about such turning points. But perverse reactions, although few enough not to endanger the market process during periods of normality, bulk very large when the validity of

predictive theory is at stake. For the purpose of prediction, as we have already said, is hardly to confirm that the normal processes will continue, but to alert us to the moments when it will not. And it is, of course, just at these points that the labile behavior patterns of the economic actors undermine the very possibility of prediction itself. If, for example, I know that a rise in the rate of government spending *may* affect expectations adversely, then I cannot predict whether more spending will be accompanied by a larger or smaller volume of output. It follows as well that this uncertainty as to behavior makes it impossible even to make negative predictions, for I can no longer be sure that a rise in interest rates—interpreted as a harbinger of a still tighter monetary policy to come—will not induce corporations to increase their borrowings or their capital expenditures before the government "cracks down."

Is the problem of the potential perversity of behavior also a matter that can be repaired by the accumulation of sufficient data? The question ultimately resolves to untestable beliefs in determinism or free will. But even if we take an extreme determinist point of view, it is clear that the problem of amassing the relevant data is qualitatively different in this case from the previous one. The difference is metaphorically suggested by the contrast between a rheostat and a switch. When we are seeking to predict the intensity of behavioral response, we are taking for granted the direction of the flow of current and confining ourselves to estimating its strength and the strength of the resistances it meets. When we are seeking to predict the direction of response we need to know whether we will encounter a critical threshhold of response at which the current reverses itself. Although we could probably learn a good deal about the nature of the environmental conditions that are propitious for "perverse" reactions, it seems probable that there will remain an inner core of the decision-making process that will be for all intents and purposes beyond any possible information retrieval system. The difficulties of forecasting movements in the stock or commodity markets, or the reactions of businessmen to monetary or fiscal policy are thus grounded at a deeper level than the difficulties of forecasting the extent of the response, once we know what its "sign" will be.

III

Thus, in the short run, we encounter a fundamental and impassable limit to the powers of predictive theorizing in the interpretational (or expectational) strand of economic behavior. The practical importance of this limit obviously depends on the frequency and pervasiveness of "perverse" interpretations, a matter to which we will refer subsequently. First, however, let us turn to the problem of the limits of predictive theorizing in the long run, for here we find an instructive and surprising change. The possibility of long-run prediction, like prediction in the short run, is also based on the possibility of discovering regular patterns of material and behavioral functions, but now we find that the breakdown in the predictive possibilities lies not in the realm of behavior but in that of technics and engineering.

How can behavior, the quicksand for short-run analysis, become a bedrock for hypotheses from which we can deduce conclusions about the long run? The answer is that we are now concerned with periods longer than those that will be affected by the indeterminacy of reactions that troubled us in the short run. Indeed, it is characteristic of the perturbations of behavior that are so disastrous for short-run prediction that they are self-limiting or of brief duration. The reason for this is clear. Perverse market behavior, being nonself-equilibrating, is exceedingly difficult to sustain for extended periods of time. Although such behavior may be rationally justified and therefore self-reinforcing for short periods (when buyers, expecting higher prices, increase their rate of purchasing and find that prices are in fact higher), the pursuit of these perverse patterns must sooner or later lead to a breakdown of the market mechanism. At this juncture, when expectations must be newly formulated, we typically find a resumption of normal marketing behavior. In the meantime, the path of the system will have "jumped," which will have upset short-run predictions, but when the regularity of the behavioral element resumes, the possibilities for prediction will be restored.

A second and no less important reason for the long-run stability of behavior is that the underlying drives or mixture of drives on which it is based—acquisitiveness, homeostasis,

obedience, etc.—are cultural manifestations that change only very gradually and over long periods of time. Basically the functional behavior-links between economic stimulus and response express the "habits," customs, traditions and usages of societies, and display all the inertia characteristic of social institutions. Thus, whereas we cannot prophesy whether behavior will be normal or perverse in any particular instance, it is very safe to prophesy that the patterns associated with normality will tend to predominate over the long run, resisting even revolutions in their viscosity.

Curiously, it is now the other attribute of the behavior function—the intensity rather than the direction of reaction—that augurs difficulties for long-run prediction. Acquisitiveness may remain, but tastes change. The question that must be faced for long-run behavior functions is whether we can hazard, no matter how great our intellectual heroism, informed generalizations as to the "drift" of tastes or as to the effect of new commodities on the general shape of economic behavior.

This problem might in the end prove fatal for the hopes of describing behavioral reactions in terms that would continue to be confirmed over several decades or generations, were it not for the presence of an even larger and more intransigent issue. This is the fact that the long-run production functions of the economy are as awkward or impossible to predict "in principle" as those of behavioral responses in the short run. It is one thing to pretend that we can imagine the slopes of existing production functions so that we can construct a model of an economy adjusting itself to these relationships in response to changes in the environment, but it is another to think that we can say anything about the nature of long-run shifts in these production functions. All that we know from experience is that the production possibilities of industrial society display rapid, and seemingly unpredictable, changes that are the exact opposite of the sluggish consistency of the functions representing long-run behavior. It may be that some day we will discover laws of scientific evolution, but until that day the advance of the scientific and technological frontiers takes place in a manner that is beyond our capacity to foresee, with the result that it is still totally impossible to establish long-run production functions for

any of the major commodities of society today, much less to predict what may be the major commodities of society tomorrow. [3]

IV

Thus we discover that there are inherent limitations to the power of predictive theorizing in economics—limitations that are rooted in the nature of the real world and that are, therefore, beyond the power of remedy by improvements in economic technique. In the short run, economic prediction is limited by the residue of behavioral indeterminacy that escapes scientific scrutiny, either directly or indirectly. To be sure, to the extent that the external world can be "regularized" so that expectations are steady and the occasions for perverse behavior accordingly reduced, the performance of economics as a short-run predictive science will be enhanced. Such is the thrust of Lowe's Political Economics, whose purpose is to restore the reliability of predictive theory by making behavior the direct object of manipulative policy, rather than taking the behavioral function as one of the "givens" in the economic process. [8, part IV; 9] Short of a wholly controlled world, however, there must remain an element of behavioral uncertainty that restricts the reach of even the most highly informed prediction.

In the long run there is also a limit to the reach of economic prediction, revealed not so much in an inability to forecast the movements of prices, etc., as in an inability to predict the secular evolution of economic systems, capitalist or any other. The failure of all the great models of economic evolution, from Smith to Marx, has always been attributable primarily to an inability to foresee the nature of technological change, or the results, behavioral as well as material, that a rapidly evolving technology would bring to economic society. To the extent that the evolution of science and technology remain fundamentally unpredictable, higher-level hypotheses concerning the physical processes of production are ruled out, and with them, the chance for models that will accurately display the changing structure of society over long periods of time.

A final word. To establish that there are intrinsic limits to

economic prediction is not to declare that it is therefore a useless activity. The discovery of functional relationships is still the most powerful lever we have for the control of our destinies in the short run. These functional relationships are as yet far from fully explored, so that the predictive possibilities *within* the limits imposed by behavioral and technological uncertainty are by no means wholly exploited.

As to the long run, the situation is less clear. In the short run we are concerned with predicting fluctuations; in the long run with predicting trends. Insofar as these trends depend on the elusive element of technological development, they seem inherently further form our grasp than the vagaries of the short period. The evolutionary models of the classicists, Marx included, appear in retrospect to owe their impressive dynamics to behavioral and technical assumptions of a fixity that would no longer be admissible. The temptation, therefore, is to avoid entirely the risky, and necessarily uncertain, enterprise of seeking to establish long-term paths of socio-economic development. Indeed, if there is any single besetting sin of contemporary economic investigation, it is its studious avoidance of any "historic" perspective on the problems it investigates.

Thus we are caught in a dilemma: on the one hand, there is the clear inability to erect higher-level hypotheses that will successfully cover the technological evolution of the system; on the other hand there is the peculiar inutility of a social science that is unable to consider long-term evolutionary forces and processures. The solution, insofar as I can see one, is perhaps easier to describe than to carry out. In part it suggests the deliberate search for new constructs that may open up previously overlooked functional relationships and thereby somewhat extend our predictive reach. In particular one wonders whether the introduction of class relationships and behavior patterns might not bring to attention functional relationships that escape notice in a model built solely on individual behavioral reactions.[3] In another part, the existing predictive limitations might be overcome by the investigation of long-run trends of technological

[3] For an interesting effort to provide such new constructs, both having to do with the development of technology and with class behavior over the long run, see Paolo Leon [7].

evolution and of patterns of technological interaction with economic and social phenomena—matters that have barely been looked into as yet. Until such new constructs or functional relationships are developed, however, present-day economic science will predict the long term future at its peril—and will suffer the consequences for its inability to do so.

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