THE METHODOLOGY OF ECONOMISTS

A Glimmer of Light

In our June 30, 1975 issue of Research Reports we reprinted and commented on an article entitled "Are Economists Really Useful??" In that article the point was made that many economic forecasts for 1974 were highly inaccurate. One reason for this inaccuracy was the unwarranted convictions of many economists that the severe business contraction was a thing of the past and that they had solved the mystery of how to prevent that unpleasant development. Once that bubble of arrogance was broken by the marked contraction of business activity late in 1974, all could see clearly that such economists had assumed an undeserved image of scientific respectability.

What accounted for the errors of these economists? As long ago as 1934, when American Institute for Economic Research was founded by E. C. Harwood, a few keen observers recognized the folly of supposed solutions to economic problems that were based on outmoded and discredited procedures of inquiry. Colonel Harwood began then to gather together at the Institute a research team to apply to economic problems the procedures that had been used successfully in other fields such as chemistry, physics and physiology. The procedure was called the modern scientific method of inquiry.

Researchers at the Institute have endeavored since its founding to use the scientific method in their work. This work yielded descriptions of some fundamental economic relationships that differed substantially from those of the dominant academic economists (the Keynesians). However, the political appeal of Keynesian (which enfolded into the "new") economics, and the seemingly persuasive theoretical underpinning for that dogma provided by Lord Keynes and his followers, soon gained many converts. The grave danger of this situation was (and remains) that unsound economic practices based on the Keynesian notions could destroy the economic and social order of the United States and other countries, just as the unsound practices of witch doctors and other charlatans often destroyed patients before the modern scientific method of inquiry had been adopted widely in the medical profession.

By the early 1950's researchers at the Institute became concerned that non-Keynesian economics might be relegated to oblivion and that the "new" economics would continue to reign supreme. These researchers redoubled their efforts to expose the "new" economics as without scientific warrant. Moreover, they realized that even if they were unable to discredit the "new" economics, they had to continue application of the modern scientific method of inquiry in order to develop economic descriptions for use after the inevitable day when the "new" economics would be discarded. Therefore, the Institute began to publish articles about the scientific method of inquiry in the course of its development and about its application to economics.

At that time, few others gave attention to the various procedures of inquiry used by economists. The initial apparent success of the "new" economics evidently had lulled to sleep many of those who earlier may have doubted the validity of those notions. Recently, however, some eminent economists have begun to sound the alarm that the Institute has been ringing for more than two decades.

In this bulletin we focus on the procedures of inquiry that some others have applied in economics and not on the conclusions that have arisen therefrom. The results of such economic inquiry are described and analyzed on a regular basis in our weekly Research Reports and monthly Economic Education Bulletins. We are convinced that the widespread understanding and adoption by economists of the scientific method of inquiry offers the best chance that the errors of the past 40 years will not be prolonged nor repeated and that economic progress will be achieved in the future.

Our Work

In the 1950's, E. C. Harwood wrote the following about the relation of economic conclusions to the methods of inquiry used by many economists:

That economists frequently do not agree has become so commonplace that some economists no longer seem to be troubled by the suggestion that such a state of affairs is scandalous. That many economists do agree on certain analyses and conclusions is equally scandalous from the viewpoint of modern science, however, because that agreement rests on methods of inquiry that have been found unreliable and have been discarded by capable scientists. The fact that a few conclusions on which some economists agree do have adequate scientific bases emphasizes by contrast the more fundamental disagreement among economists generally regarding the methods...
of inquiry that can be expected to yield useful results.\(^2\)

During subsequent years, our views about procedures of inquiry were developed and refined. Among the key aspects of our views, as presently developed, are the following:

1. An emphasis on the importance of "firm naming"\(^3\) in economic inquiry. Such naming, although firm in the sense that much terminology in developed fields is firm, is not \textit{final}. As inquiry progresses, improvement in naming is to be expected, just as occurred with the name "atom" in physics. Much of the conventional work in economics is plunged almost immediately into a semantic swamp by the use of naming that is inconsistent, incoherent, and frequently based on ancient epistemological ideas that long have been outmoded.\(^4\)

2. An emphasis on the entire pertinent transactional field, rather than on presumed separate and interacting "reals" making up that field. The "reality" of the various aspects and phases of the transaction generally is dependent on the field itself; for example, borrowing cannot exist without lending, and vice versa. Concentration on too limited an area of transaction is associated with much of the inquiry conducted by conventional economists. For example, Keynesian economists appear to concentrate on the volume of current consumption to the exclusion or minimization of its effects on future production and consumption.

3. An emphasis on the crucial importance of continuous testing of the conjectures (often called hypotheses) that are developed about what happens under specified circumstances by careful observation of available data and events. Conventional economic inquiry often proceeds by the development of elaborate conjectures far in advance of any significant testing of them or of their underlying "assumptions" against observed facts. Conventional economic inquiry relies on logical consistency, initial plausibility, assumed truisms, etc. and has resulted in elaborate "theories" or "models" in which the developers have great confidence. Unfortunately, the predictions based on such models often have been notably inaccurate, as subsequent events demonstrated.

The facts and notions with which we begin an inquiry also may turn out to be inaccurate, which is why assertions always must be carefully qualified. Inquiry, then, neither begins with certainty nor attempts to achieve certainty; rather, the objective is the development of warranted (but not final or certain) assertions adequate to solve the problem at hand.

We also oppose the general procedure of inquiry, often found in economics, in which great emphasis is placed on the deductive and mathematical elaboration of a few "axioms." After the model is developed, supposedly it is to be tested against the facts, but in practice the major emphasis is placed on the "internal" deductive and mathematical elaboration.


\(^4\)On this issue, see the \textit{Economic Education Bulletin} cited in footnote 1.

To summarize, we have long maintained that the "quest for certainty," whether through deduction, revelation, intuition, or any other means, has not been a reliable source of useful solutions to human problems. That policy recommendations based on the results of such procedures often have proved disastrous supports this view.

During the past two decades, our description of the scientific method of inquiry has been modified in our continuing effort to improve this description, and some of the key names used in earlier descriptions have been replaced. That the principal ideas go back many years, however, can be illustrated with a few quotations from some of our early publications:

In 1936 the following was written concerning the importance of not elaborating conjectures far in advance of the facts: "This latest book by Mr. Keynes is an attempt to find his way out of the maze into which his earlier erroneous assumptions drew him. . . .to all economists it should be an object lesson, illustrating the desirability of squaring theory with facts before wandering too far in its development.\(^5\)

The semantic swamp (unclear naming) problem also was described in 1936: "The unfortunate fact is that the general run of academic economists have neither defined their words carefully, nor have they adhered to the careless definitions given. The result is confusion worse confounded.\(^6\) Also, an entire chapter ("The Existing Confused Terminology") of \textit{Useful Economics}, first published in 1956, was devoted to the seriousness of the terminological confusion found in economic inquiry.

The importance to economists of findings in other disciplines, or in other words, the importance of the entire pertinent transactional field, was noted in 1949: "Economists have been justly criticized for failing to take into consideration the knowledge about human behavior that has become available through the efforts of scientists in related fields. In recent decades the scientific advance in biology, psychology, and the other sciences concerned with one or more aspects of man's behavior has provided much new and useful information. Assumptions previously serving as bases for elaborate economic theories can now be tested in order to ascertain whether or not they are sound; and to some extent scientific knowledge, or what John Dewey has aptly called 'warranted assertibility,' can now be substituted for the assumptions of an earlier day.\(^7\)

On the alleged claim to have achieved certain knowledge, in 1955 we made the following statement of one of the primary duties of economists: "To emphasize on every possible occasion that those who claim to have found certainty have been chasing a will-o'-the-wisp that thus far has not been certainly located and identified elsewhere than in men's imaginations.\(^8\)

In short, for decades we have argued in our various publications that the usual procedures of inquiry used by many economists were outmoded and inadequate, and that useful solutions to economic problems are best facilitated by adopting the procedures used so successfully in the physical and the physiological fields. We also have sponsored a program in the education of economic scientists, with emphasis on developing research skills that


\(^7\)\textit{AIER Weekly Bulletin}, August 8, 1949, p. 127.

can be applied usefully to human problems (see the section "The Development of Economic Scientists" in Reconstruction of Economics). We also have emphasized the importance of mathematical competence, yet we have criticized the development of complex mathematical models of economic activities far in advance of the pertinent observations that are required for the development of sound, useful conjectures.

Similar Views of Other Economists

Some highly critical comments about the procedures of inquiry used by economists have been made in recent presidential addresses to groups of professional economists. In his presidential address to the Econometric Society, F.H. Hahn said: "The achievements of economic theory in the last two decades are both impressive and in many ways beautiful. But it cannot be denied that there is something scandalous in the spectacle of so many people refining the analysis of economic states which they give no reason to suppose will ever, or have ever, come about. It probably is also dangerous... It is an unsatisfactory and slightly dishonest state of affairs."9

In his 1971 presidential address to the Royal Economic Society, E.H. Phelps Brown10 concluded: "On the view I have advanced, our own science has hardly yet reached its 17th century. I believe we shall make better progress when we realize how far we still have to go." (p. 10)

Phelps Brown criticized in particular the separation of observation from "theory" and the absence of useful predictions suggested by such theories:

"Models which though complex are built out of a small number of elements that are assumed but not known to be representative of the predominant tendencies of the real world, are no help to those who have to diagnose and prescribe for the actual economy.... It may even be that training in advanced economics is actively unhelpful. I find it is a common experience that when graduates in economics first assume practical responsibilities they have something to unlearn." (p. 2)

"I would say that they [advanced economic theorists] deal with problems of challenging severity, whose solution requires such powers as are given only to high intellectual endowment and intensive application. It is this that gives this kind of work its prestige."

"None the less, I believe that its usefulness is not equal to its distinction: I believe that it is impaired from the first by being built upon assumptions about human behaviour that are plucked from the air... the human propensities and reactions it purports to abstract are not in fact abstracted, that is to say drawn out from observations, but are simply assumed..." (p. 3)

"My argument further calls for some change of esteem. In every science the ascending scale of intellectual status tends to be one of rarification: the more abstract, the more rigorous, the more general, so much the more distinguished. This is natural, because distinction is conferred by rarity, and few of us are capable of soaring into the empyrean of abstraction, whereas there is a saying that no man is so short that his feet do not reach the ground. In economics, at least, those who devote themselves to the direct observation of attitudes and behaviour have commonly been regarded as playing in the 2nd. XI [in American English, the "second team"] I have heard them called hewers of wood and drawers of water.... In the present stage of our science, at least, I believe that this relative valuation should be inverted: we ought to value powers of observation more highly than powers of abstraction, and the insight of the historian more than the rigour of the mathematician." (p. 9)

Perhaps the most severe recent criticism made in a presidential address was that by Wassily Leontief to the members of the American Economic Association.11 He said:

"Economics today rides the crest of intellectual respectability and popular acclaim. The serious attention with which our pronouncements are received by the general public, hard-bitten politicians, and even skeptical businessmen is second only to that which was given to physicists and space experts a few years ago when the round trip to the moon seemed to be our only truly national goal. The flow of learned articles, monographs, and textbooks is swelling like a tidal wave...."

"And yet an uneasy feeling about the present state of our discipline has been growing in some of us who have watched its unprecedented development over the last three decades. This concern seems to be shared even by those who are themselves contributing successfully to the present boom. They play the game with professional skill but have serious doubt about its rules...."

"The uneasiness of which I spoke before is caused...by the palpable inadequacy of the scientific means with which they try to solve [practical problems].... The weak and all too slowly growing empirical foundation clearly cannot support the proliferating superstructure of pure, or should I say, speculative, economic theory." (p. 1)

"In the presentation of a new model, attention nowadays is usually centered on a step-by-step derivation of its formal properties.... By the time it comes to interpretation of the substantive conclusions, the assumptions on which the model has been based are easily forgotten. But it is precisely the empirical validity of these assumptions on which the usefulness of the entire exercise depends." (p. 2)

In addition to such presidential addresses, other recent articles critical of the procedures of inquiry commonly used by economists have been published. P. T. Bauer and A. A. Walters,12 for example, recently wrote the following:

"The promotion of unwarranted claims reduces the effectiveness and potentialities of a subject. In recent decades exaggerated and even extravagant hopes have been entertained of the practical potentialities of economics, from the so-called fine tuning of advanced economies in the short period, or the forecasting of their position and prospects for decades ahead, to its potentialities in promoting the progress of less developed societies by sophisticated planning models. And many economists have readily encouraged these expectations both about the subject as a whole and about certain techniques and methods." (p. 2)

"In macroeconomics perhaps the most revealing lapse is the discussion of the balance of payments without reference to domestic prices, incomes, exchange rates or monetary and fiscal policies. The discussion of the 1940's and 1950's of the dollar shortage and the likelihood or...

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inadequacy of its persistence provides a celebrated example. Many economists, including outstanding price theorists, treated this matter without reference to these variables. The prompt falsification of these predictions by events in the late 1950's did not reflect unsuccessful forecasting in the conventional sense of the term, criticism of which would simply reflect the wisdom of hindsight.” (p. 4)

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The need for direct observation in economics is underlined by the ambiguities of some of the concepts widely used in economics, notably mathematical economics. While certain difficulties of interpreting phenomena also underline the need for direct observation. Preoccupation with mathematical methods, including econometrics, has contributed to the neglect of direct observation.” (p. 13)

“Pigou claimed it as an advantage of the mathematical method that it acted as a barrier to charlatans. He overlooked the possibility that it could provide a protective facade for incompetent or irrelevant analysis.” (p. 16)

“Such criticisms apply also to large econometric models of developed economies, the focus of so much effort in recent years. The size of these latter models inhibits effective criticism. Very few people can test large econometric models such as the FRB-MIT-Penn model of the behaviour of the United States economy, or even the more modest models such as that of the London Business School. Large sums of money are needed for the necessary runs; and in addition much time is required to enable a critic to assimilate all the peculiarities of the model. The sheer size of some of these models makes it very difficult to understand the nature of the system being investigated. Effects may be produced which are inconsistent with common observation or indeed with common sense. But it is difficult to trace the true source of such paradoxes. Moreover, misleading results may be hidden in the equations and remain unrecognized for a long time.” (p. 20)

In an interesting article, S.N.S. Cheung showed that inaccurate notions about presumed relationships between beekeepers and farmers have served as the basis for some policy recommendations about government intervention: . . . it appears evident that some economists have been distilling their policy implications from fables. In a desire to promote government intervention, they have been prone to advance, without the support of careful investigation, the notion of ‘market failure’ . . . Thus to assume the state of the world to be as one sees fit is not even to compare the ideal with the actual but, rather, to compare the ideal with a fable.”

“I have no grounds for criticizing Meade and other economists who follow the Pigovian tradition for their use of the bee example to illustrate a theoretical point: certainly, resource allocation would in general differ from what is observed if the factors were ‘unpaid.’ My main criticism, rather, concerns their approach to economic inquiry in failing to investigate the real-world situation and in arriving at policy implications out of sheer imagination. As a result, their work contributes little to our understanding of the actual economic system.” (p. 13)

**Conclusion**

We have shown that some eminent economists have begun to recognize that some of the difficulties with the discipline called economics are associated with the procedures used by economists in their investigation of economic phenomena. In particular, they have expressed concern at the development of elaborate theories from untested or inaccurate assumptions. Such criticism is encouraging.

However, one of the key aspects of our work on procedures of inquiry is seldom, if ever, mentioned by other economists: the importance of "firm naming." The evidence abundantly indicates the importance of such naming in useful inquiry. Inattention to terminological problems is highly conducive to uncontrolled speculation about relationships and to the spinning of "theory" out of fanciful assumptions. If there is not general agreement as to what is referred to when commonly used names are applied, how can scientific inquiry in a discipline advance?

Whether or not the progressive trend noted in this article will develop further remains to be seen. Substantial hope for progress must await a wider recognition of the procedural problems described in this bulletin. An early sign of such recognition might be revealed in the content of the articles published in the leading economic journals. We hope that the glimmer of light revealed by the comments noted in this bulletin soon becomes a powerful beacon.


14 The recent criticism by eminent economists of conventional procedures of inquiry used by economists has been followed by defenses of those procedures. For example, in his 1974 presidential address to the members of the American Economic Association, Walter W. Heller admitted that some recent criticisms of work in economics were justified, but he concluded: "As economists, we have many sins, none deadly, to confess. But these are far outweighed by the virtues, all quite lively, that we can legitimately profess." ("What's Right with Economics?" _The American Economic Review_, Vol. LXV, March 1975, p. 25.) He praised the field for its "many competent, tough, and rigorously trained minds" who are able to "draw on a hard core of economic theory and methodology, together with a growing body of empirical knowledge, to provide standards for testing the validity (though not necessarily the relevance and reality) of ideas, analysis, and empirical findings" (p. 14).

Many of the criticisms presented in this bulletin follow from the failure of conventional economic notions to pass the test of "relevance and reality." What useful meaning can there be to "validity" if not "relevance and reality"? Heller seems so bogged down in his semantic swamp that he can not rise from it even when writing nontechnically. To illustrate, what could he mean by a "tough mind," and what relevance could that have to a defense of the accuracy of economic descriptions? What is a "hard core" of economic theory? How can there be a "growing body of empirical knowledge" if there is an admission of irrelevancy and unreality? Indeed, what is "knowledge"? We should guess that Heller thinks he knows what "knowledge" is, but his words suggest that his understanding of what constitutes "knowledge" is far different from ours.