Thirty-five years ago, E. C. Harwood, AIER’s founder, 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.”

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

(1) An emphasis on the importance of “firm naming” in economic inquiry. Such naming, although firm in the sense that much terminology in developed fields is firm, is not 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.

(2) An emphasis on the entire pertinent transactional field, rather than on presumed separate and interacting “realities” making up that field. The “reality” of the various aspects and phases of the transaction generally depends 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 have tended to emphasize 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 requires that assertions following therefrom 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 eschew the general procedure of inquiry, which has achieved dominance among academic economists, 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.

To summarize, we long have 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.

ORIGINS AND DEVELOPMENT OF AIER’S PROCEDURES

During the past several 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 forewandering too far in its development.”

The 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.”

Also, an entire chapter (“The Existing Confused Terminology”) of 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 disci-
plines — i.e., the importance of the entire pertinent transac-
tional field — was noted in 1949:

“Economists have been justly criticized for failing to
take into consideration the knowledge about human behav-
ior that has become available through the efforts of scient-
ists in related fields. In recent decades the scientific ad-
ance in biology, psychology, and the other sciences con-
cerned 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.”

On the alleged claim to have achieved certain knowl-
edge, 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 mere imaginations.”

In short, for decades we have argued that the usual pro-
cedures of inquiry used by many economists were out-
moded and inadequate, and that useful solutions to eco-

duction problems are best facilitated by adopting the pro-
cedures used successfully in the physical and physiological
fields.

A BRIEF “GLIMMER OF LIGHT”

In our review 13 years ago of the methodology of econ-

omists, we observed that similarly critical comments about
the usual procedures of inquiry used by economists had been
made in recent presidential addresses to groups of academic
economists, which gave some encouragement that a “glim-
mer of light” was beginning to shine on the profession. We
noted that a number of eminent economists in particular
had expressed concern at the development of elaborate the-
ories from untested or inaccurate assumptions.

Perhaps the most severe criticism in this respect had
been leveled by Wassily Leontief in his 1971 presidential
address to the members of the American Economic Asso-
ciation. He remarked:

“Economics today rides the crest of intellectual respecta-
bility 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
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 pre-
cisely the empirical validity of these assumptions on which
the usefulness of the entire exercise depends.” (p. 2)

Other critics of economists’ procedures of inquiry dur-
ing the 1970’s included P. T. Bauer and A. A. Walters,9 who
wrote:

“The promotion of unwarranted claims reduces the ef-
fectiveness and potentialities of a subject. In recent decades
exaggerated and even extravagant hopes have been enter-
tained 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 pro-
gress 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 even inevitabil-
ity 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 con-
ventional sense of the term, criticism of which would
simply reflect the wisdom of hindsight.” (p. 4)

“ Pronounced differences in the competence and consist-
tency of performance of practitioners are inevitable in
scholarship and science. What seems peculiar to economics
is the frequency of lapses of simple analysis and disregard
of evidence by leading practitioners. Sometimes the lapses
are of a kind which are readily apparent to reflective
laymen who have not studied economics, a situation which
rarely applies in other disciplines.” (p. 6)

“The need for direct observation in economics is under-
lined 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 over-
looked the possibility that it could provide a protective
facade for incompetent or irrelevant analysis.” (p. 16)

Such criticisms ... apply also to large econometric mod-
els of developed economies, the focus of so much effort in
recent years. The size of these latter models inhibits effec-
tive 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 assimili-
ate 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
ture source of such paradoxes. Moreover, misleading re-
sults may be hidden in the equations and remain unrecog-
nized for a long time.” (p. 20)

We viewed such recognition of some of the difficulties
with the procedures commonly used by economists as en-
couraging. However, we also wrote: “Whether or not the


progressive trend noted … will develop further remains to be seen.”

RECENT TRENDS

Despite the efforts of Professor Leontief, Lord Bauer, and others, there is little evidence that many economists since have taken their criticisms seriously. Rather, the recent literature concerned with improving economists’ procedures of inquiry would seem to suggest that the profession is even more distanced from the “real” world than previously. In terms of firm naming, describing relationships within the entire transactional field, and testing conjectures empirically by careful observation, retrogression, not progress, seems to have occurred.

An illustrative example is a recent Richard T. Ely Lecture given by Professor Alan S. Blinder of Princeton University to the members of the American Economic Association.11 Blinder’s address, which purports to call for reduced emphasis on purely theoretical constructs and greater attention to solving “real” rather than imagined problems through empirical testing, actually suggests how little understood even by leaders of the economics profession are the procedures of inquiry described on the first page of this bulletin. The bulk of Blinder’s remarks pertain to the question of unemployment, but a number of passages relate explicitly or implicitly to those procedures and are excerpted below. He writes:

“Every science has its game playing and puzzle solving. It’s harmless, good clean fun, helps sharpen the mind, and occasionally turns up something spectacularly useful. Economics is no exception, nor should it be. But I want to suggest that contemporary academic economists have taken a good thing too far, pushed the game-playing aspects beyond the region of even positive marginal returns, and disengaged themselves from the practical policy concerns that affect the lives of millions…. Didn’t Keynes have a point when he longed for the day when economists would make economics “less scientific,” as is implied above, seems extraordinarily bizarre. How “real” or “unreal” Blinder’s own problem-solving procedure is may be suggested by the following somewhat lengthy sequence. We have intentionally exercised all descriptions of his hypotheses, yet retained the methodologically key words and passages respecting his theory of unemployment. The point may be obvious:

“Another possibility is that…. I could make this explanation sound less like pop sociology and more like modern economics by gussying it up with words like signalling, asymmetric information, and adverse selection. I could even say it with algebra — but not right after dinner…. But there is one big problem…. So let me suggest an alternative hypothesis…. To be more precise, suppose…. Supose further, and this is the critical leap. Direct empirical evidence on this hypothesis is difficult to come by…. So, once again, a thought experiment may help. Suppose…. I suggest that it may really be because…. If workers assume…. I again ask you to introspect. Imagine that…. Each of you can make your own judgment, but this strikes me…. That strikes me as roughly correct…. I find all this a refreshing departure from the scholastic dogma of High Neoclassicism. The Keynesian promised land is not yet in sight; but we may, at long last, be emerging from the arid desert and looking over the Jordan. Let me use … this occasion to peer beyond where we can really see and speculate briefly on the outlines of a model that is both theoretically respectable and can be explained in mixed company without embarrassment…. Consider what might happen…. And so on. (pp. 5-8, emphasis added)

In short, devoid of its ingenious argumentation, this line of reasoning is revealed for what it is: supposition upon supposition upon supposition, all leading to one man’s opinion, which, by implication at least, is only as good as anyone else’s (note italicized passage).

Professor Blinder’s ideas about empirical testing are explicated in the following passages:

“Economics is not an art form. So we must not be content with a coherent and vaguely sensible theory … welcome as that would be. We must give the theory empirical content, test it, and estimate its central parameters.”

“In a sense, macroeconomics has progressed further on the empirical front. The truth of the matter is that empirical Keynesian models equipped with Phillips curves that allow for supply shocks have done rather well lately. Furthermore, the Phillips curve has been one of the strongest links in the empirical chain.”

“What macroeconomics needs next is to give the new generation of Keynesian micro-foundations some empirical teeth. You can think of this as providing theoretical justification for the Phillips curve, if you wish. I prefer to think of it as providing empirical justification for all the theorizing.” (p. 9)

It is difficult to know what the above passages are trying to say. What does it mean to give a theory “empirical content”? — and what is an “empirical model”? If these terms refer to the continuing process of testing conjecture through observation, then why does Professor Blinder not come out and say it? The vagueness of his statements strongly suggests fundamental confusion about what is and is not empirical. A “model,” no matter how sophisticated and ingenious, remains an intellectual construct — a made-up thing — and so is not “empirical,” since that name refers to the results of direct observation. Moreover, his embrace of “Keynesian micro-foundations” sans teeth would seem to imply the precedence of a priori reasoning over observation (the unstated assumption is that there are “teeth” that will fit the Keynesian maw).

A further tendency among economists to try to find uses even for theories that have yielded predictions wildly at odds with observed events may be suggested by recent studies that have explored the influence of chaos on the “behavior” of various economic models. Underlying such studies is the notion that seemingly random fluctuations in some time series that do not “behave” according to theory may actually be experiencing non-random chaotic effects that are the result of deterministic independent variables.

The technical aspects of chaotic movements are beyond the scope of this discussion. However, it is well understood among statisticians that, given a particular set of “reaction functions,” a time sequence eventually may yield wild oscillations.

Of greater methodological interest is the application of chaos effects to economic models. Professors William J. Baumol and Jess Benhabib recently have observed that “Where chaos occurs economic forecasting becomes extremely difficult,” and caution:12 “Chaos theory has … power in providing caveats for both the economic analyst and the policy designer. For example, it warns us that apparently random behavior may not be random at all. It demonstrates
dramatically the dangers of extrapolation and the difficulties that can beset economic forecasting generally.” (p. 80)

Nevertheless, in reviewing the sources of interest in chaos theory, they also seem to suggest that chaos has

The roots of economists’ interest in complex dynamics are to be found in the vast nonmathematical literature on

business cycles, with its large number of models each undertaking to provide a set of conditions sufficient to generate oscillatory behavior in the economy. These models were received enthusiastically and generated many writings by leading economists. Still, before long disappointment seemed to set in and publication slowed. First, it became clear that the behavior of the time path generated by such a linear dynamic system can be extremely sensitive to changes in the values of the parameters, as well as the structure of the model. That made it hard to formulate models (and econometric estimates of their parameter values) that constituted robust and reliable representations of reality. Still, as we will see, this is not really a shortcoming of the models, but a weakness of some of their interpretations.” (pp. 78-9, emphasis added)

The work on chaotic dynamics suggests that disenchantment with earlier dynamic models is perhaps attributable to failure to recognize their most promising role — that of revealing sources of uncertainty, and enriching the list of recognized possible developments.... ” (p. 80)

Again, it is difficult to know how any “enriching the list of recognized possible developments” might proceed. But if economists’ past behavior is any indication, the implied rehabilitation of discredited economic models promised by chaos theory would seem to be an open invitation to even the most wild-eyed fantasy. The problem is that, while “chaos” may follow from mathematical constructs, those underlying constructs may or may not bear any relation to what goes on in the transactional field (the “real” world).

An even more explicit endorsement of a priori thinking is contained in another recent discussion of the difficulties of economic modeling. Professor Dale J. Poirier of the University of Toronto unabashedly endorses Bayesian “subjectivism” as useful method. He writes:13

“I will not dwell on whether prior beliefs exist since I believe they undeniably do. I never cease to be amazed at non-Bayesian who question the existence of subjective prior beliefs out of one side of their mouths, and who out of the other side point out how somebody else’s regression coefficient is of the ‘wrong sign.’... Prior beliefs manifest themselves in all econometric investigations in the form of the elusive ‘wisdom’ that senior masters of our profession bring to bear on empirical investigations. The question of whether the subjectivist approach is legitimate within ‘objective’ science is a play on words.... [I] believe subjective prior beliefs should play a formal role so that it is easier to investigate their impact on the results of the analysis. Bayesians must live with such honesty whereas those who introduce such beliefs informally need not.” (pp. 129-30)

In short, a priori beliefs, which pose a major obstacle to scientific inquiry, in effect are now being celebrated by some as useful for discovering just how erroneous bad science can be. Perhaps the kindest thing that can be said about this perspective is that, given the current abundance of such results, any further effort to produce flawed analyses would seem to be redundant in the extreme.

Whether the examples cited here will constitute a trend in the methodology of economics remains to be seen. However, it seems clear that little genuine progress has been made by economists during the past decade and a half toward developing procedures of inquiry that meet the requirements of modern science.

ENDNOTES
3 On this issue, see our Economic Education Bulletin for January 1985 entitled “How Do You Know That You Know Anything?”

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