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Contemporary Methodology vs Popper's Philosophy of Science

> No assumptions about economic behavior are absolutely true and no theoretical conclusions are valid for all times and places, but would anyone seriously deny that in the matter of techniques and analytical constructs there has been progress in economics?
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> Mark Blaug [1978, p. 3]

> we look upon economic theory as a sequence of conceptual *models* that seek to express in simplified form different aspects of an always more complicated reality....
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> Tjalling Koopmans [1957, p. 142]

> progress in a discipline is better described by a sequence of theories, or models, not by a study of individual theories. A 'research program' is the organizing conception; to describe it is to characterize the various sequences of models that have family resemblance....
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> E. Roy Weintraub [1979, p. 15]

So far we have examined the effects of the hidden agenda on neoclassical theoretical problems and research programs; now we wish to examine its effect on the neoclassical views of methodology. Generally speaking, methodology is rarely discussed in the leading journals. We need to examine this empirical fact before we examine the more subtle questions of 'realism', 'usefulness' and the meaning of a 'sequence of models'.

Methodology and the Hidden Agenda

Methodology is not considered an urgent topic for neoclassical research programs simply because methodology has historically been concerned only with the nature of the items on the hidden agenda. Being concerned with the items on the hidden agenda means that, to the extent that methodologists tend to question the adequacy of various views of the agenda items, the subject of methodology is paradoxically considered either a waste of time or too dangerous to handle. Consequently, novice economists are often advised to steer clear of methodology, as there is no way to establish a career based on methodology. It is claimed that no significant contributions can be made in that area. The question we shall consider is: does this orthodox attitude towards methodology merely reflect a deep-seated insecurity about the adequacy of the hidden agenda? If it does, then there can be no doubt that the advisors are correct!

A 'significant contribution' to neoclassical economics can be made in only two ways. One can either (1) provide a new application of neoclassical theory, or (2) provide a proof of a theoretical proposition which is relevant for applications of neoclassical theory. It is easy to see that with such a limited range of possibilities there is little room for the study of methodology as part of a neoclassical research program.

As long as the domain of methodology is limited to the study of the hidden agenda, the logic of the situation facing an aspiring methodologist is limited. Primarily, given the presumed need to deal with the Problem of Induction and the logical impossibility of providing inductive proofs, the only methodological questions of concern are those relating to acceptable ways of solving the Problem of Conventions. If one can provide a new theory-choice criterion which is in some way superior to previous criteria, then that would be considered a contribution to methodology. But since the purpose of any criterion is to provide a basis for justifying a given theory-choice, the givenness of the theory-choice precludes any methodological contribution. For example, in the methodological debates between the followers of Samuelson and the followers of Friedman's so-called Chicago School, or those between the 'Keynesians' and the 'Monetarists', the appropriate theory-choice criterion is dictated by the opposing theories that are given. Samuelson and the 'Keynesians' urge the dominance of a criterion of 'generality', while Friedman and the 'Monetarists' argue for 'simplicity' or for 'usefulness'.

Many economists consider such debates to be sterile. Although it might appear that questions of methodology matter, they really are not decisive, since side is already committed to its respective theory. Methodology is only an afterthought. Those liberal methodologists who wish to defuse such extremist methodological debates try to confuse the methodological issues. Usually they recommend some *ad hoc* middle ground where both methodological views are represented and thus make methodological questions irrelevant. A recent example is the view expressed by Robert Lucas:

> One of the functions of theoretical economics is to provide fully articulated, artificial economic systems that can serve as laboratories in which policies that would be prohibitively expensive to experiment with in actual economies can be tested out at much lower cost. To serve this function well, it is essential that the artificial 'model' economy be distinguished as sharply as possible in discussion from actual economies. Insofar as there is confusion between statements of opinion as to the way we believe actual economies would react to particular policies and statements of verifiable fact as to how the model will react, the theory is not being effectively used to help us to see which opinions about the behavior of actual economies are accurate and which are not. This is the sense in which insistence on the 'realism' of an economic model subverts its potential usefulness in thinking about reality. Any model that is well enough articulated to give clear answers to the questions we put to it will necessarily be artificial, abstract, patently 'unreal'.
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> At the same time, not all well-articulated models will be equally useful. Though we are interested in models because we believe they may help us to understand matters about which we are currently ignorant, we need to test them as useful imitations of reality by subjecting them to shocks for which we are fairly certain how actual economies, or parts of economies, would react. The more dimensions on which the model mimics the answers actual economies give to simple questions, the more we trust its answers to harder questions. This is the sense in which more 'realism' in a model is clearly preferred to less.
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> On this general view of the nature of economic theory then, a 'theory' is not a collection of assertions about the behavior of the actual economy but rather an explicit set of instructions for building a parallel or analogue system – a mechanical, imitation economy. A 'good' model, from this point of view, will not be exactly more 'real' than a poor one, but will provide better imitations. Of course, what one means by a 'better imitation' will depend on the particular questions to which one wishes answers. [1980, pp. 696-7]

A major factor determining the irrelevancy of contemporary methodology is the lack of a logical consistency of purpose. As we see in the comments of Lucas above, there is a little bit from Instrumentalism (e.g., 'usefulness') and another bit from Conventionalism (e.g., 'better imitation'). Of course, such a mixture is consistent with Instrumentalism. Perhaps that is all that is revealed by the liberal compromise methodologies.

No matter how much methodological discussion appears in neoclassical articles, as long as the theories presented are put beyond question, the methodology provided is irrelevant. But many neoclassical economists who do provide some mention of methodology would imply that methodology potentially matters in their choice of their theories; and this also implies that their theories are not beyond question. Nevertheless, there is little a methodologist can contribute, given the second item on the hidden agenda – the explanatory problem of methodological individualism. As long as psychologistic individualism is considered to be the only acceptable form of individualism for neoclassical economics, the Problem of Induction will not be considered questionable. The key to the apparent irrelevance of methodology is the implicit acceptance of psychologistic individualism.

Methodology and the History of Economic Thought

The area where methodology is supposed to matter most is the study of the history of economic thought. But if methodology (as we are led to believe) is not decisive in the choice of any particular theory, then how can methodology matter in the historical development of our theories? This contradiction is easily handled today. The common view is that the study of the history of thought does not matter either! Nevertheless, let us leave this controversial subject for a while and instead focus on the questions of methodology from the respectful host of the history of thought.

The study of methodology and the study of the history of economic thought go hand in hand. As the views of Koopmans and Weintraub (quoted above) indicate, a common methodological view says that we must see a research program as a ‘sequence of models’. This immediately puts methodology into an historical context. What is probably not often appreciated is that putting methodology into an historical context is just a straightforward application of either Inductivism or Conventionalism.

Two views of the history of economic thought

Many historians of economic thought study methodology under the title of the ‘Growth of Knowledge’ [e.g., Latsis, 1976]. What is presumed by all such perspectives is that there is some sort of continuity. The continuity is established either by a logical relationship to some original theory or theorists or by a family and/or social relationship provided by the continuity of a specific community of scholars. The former view is usually in the tradition of Inductivist histories of science [see Agassi, 1963] and the latter in the tradition of Conventionalist histories of physics [e.g., Kuhn, 1962/70].

In the older, orthodox Inductivist tradition the history of any science is the history of the development of an inductive proof of some

‘scientific law’. According to Inductivism, a ‘scientific law’ is established by the presentation of logically sufficient facts – facts which have been gathered by true scientists. A ‘true scientist’, so the tradition goes, avoids making mistakes by striving to be unbiased and open-minded, that is, by not jumping to conclusions until all the facts have been collected. This takes a great deal of patience and hard work (the similarity to the ‘labor theory of value’ is not accidental). One’s patience and hard work will be rewarded in the end, perhaps by having one’s work included in someone’s history of science! Since the speed and veracity of one’s inductive proof depends so much on the quality of one’s collected facts, the real test of any science is the personal character of the scientists involved. For this reason, inductivist histories of science tended to dwell on the personal qualities of leading scientists.

Agassi [1963] argues that the older historians of particular sciences tended to see what they thought they should see. As he says, they were often unable to ‘avoid being wise after the event’. That is, by taking Inductivism for granted, many historians of science would selectively portray a given scientist as if he were pure in heart and mind and unable to make mistakes. This is because whenever a ‘scientific law’ had been established (i.e., inductively proven), the facts must have been scientifically clean, and that is possible *only* when the scientist is unbiased, open-minded, etc. To those of us in economics these histories of science seem a bit silly, but that is because very few orthodox inductivist histories of economic thought have been written in recent times.

The other approach to writing histories of science is much more common in economics. More and more, the history of economic thought is considered to be the history of an impersonal enterprise. Today one can discuss the ‘marginalist revolution in economic theory’ without going into any detail about the lives of Jevons, Marshall, Walras, or Menger. What is recognized today is that although each of these men contributed to the body of economic thought, their contributions depended on acceptance by other economists. Of course, the idea that anyone’s contribution depends on acceptance by others is the keystone of modern Conventionalism. Where Inductivist scientists strived to provide empirical, objective proofs, Conventionalist scientists provided acceptable arguments and propositions. Whether one’s intended contribution is accepted depends on whether one has satisfied the currently approved criteria of acceptance for one’s evidence and for one’s mode of argument.

There are two essential elements in the Conventionalist view of the history of economic thought. First is the continuity of the enterprise; second is the tentativeness of the certification of one’s contribution. In some sense there was a continuity involved in the Inductivist view of the history of science but it was due to the presumed durability of any alleged inductive proof. The Conventionalist view, which denies the existence of both inductive proofs and absolute truth, takes a broader historical view. Any body of knowledge is treated like a river flowing through time. We can all attempt to pour our contributions into the stream but their significance will be judged downstream.

Implicitly, the continuity of the growth of knowledge would seem to presume that whenever somebody is to have made a contribution, it remains a contribution forever. But this implication of continuity has not always fitted the facts. That is, 'contributiveness' itself must be judged downstream. What may be considered a contribution today might tomorrow be considered an illusion. The resulting tentativeness of the judgement concerning whether one has actually made a contribution leads to a breakdown in the continuity aspect of the history of the enterprise.

The best illustration of the tentativeness of contributions is the history of Samuelson's contribution to demand theory [see Wong, 1978]. In 1938 Samuelson said that he had solved the problem plaguing all psychologistic theories of behavior – namely, that the basis of such explanations of individuals' behavior is not 'operational', that is, is not observable. He offered a new way to explain an individual's demand. Instead of assuming the existence of a psychologically given utility function or preference ordering, we were to assume only that the individual was consistent in his or her choices. Consistent choices meant only that whenever one faced the same price-income situation one would make the same choices (i.e., if one could afford both bundle A and bundle B in two different situations, it would be inconsistent to buy A in one and B in the other). In effect, one was supposed to be a slave to one's past history. On the basis of this postulate of consistency (and a few minor postulates that provide that the consumer does make choices), Samuelson was able to prove what he thought was the essential purpose of the Hicks-Allen [1934] orthodox theory of the consumer – a theory that seemed to require the existence of psychologically given preferences.

Now, the success of Samuelson's research program is widely accepted and even hailed by many as a major contribution to economic knowledge. What is interesting about the history of Samuelson's contribution is that by 1950 *he* readily admitted that a complete version of his demand theory was logically equivalent to the 'ordinal demand theory' which Hicks and Allen had developed. Now, there is an inconsistency here. How can Samuelson's 'operational' theory of demand be both different from and logically equivalent to the Hicks-Allen theory? What appeared as a major contribution in 1938 disappears as a mirage in 1950. Probably more significant, what was hailed as a major breakthrough in economics methodology has disappeared in a puff of philosophical smoke. Such are the ways of Conventionalist histories of economic thought!

Methodology and continuity-based histories

The paradigm of continuity theories of the history of science is, of course, Thomas Kuhn's view, which he presented in his *Structure of Scientific Revolutions*. According to his view, we are to see a steady progress in everyday, 'normal science', with the steady accumulation of solutions to theoretical puzzles. What distinguishes a puzzle from a problem is that a puzzle is approached on the basis that there definitely is a way to solve it – if only we can find it. On the other hand, a problem may not always have a solution, no matter how long we look for one (e.g., the Problem of Induction). No one claims that the solution to the puzzle constitutes absolute proof. Nevertheless, each piece added to the puzzle warrants much the same reward as the discovery of each additional fact leading to an inductive proof.

It might be asked, if Kuhn's book is so concerned with puzzle-solving (i.e., normal science), why is the title concerned with 'revolutions'? The answer is that puzzle-solving is not very progressive and historians are more concerned with significant progress. Historians record the abandonment of one puzzle deemed to be a bit stale and its replacement by a new and more promising puzzle. He calls these puzzle-replacements 'revolutions', since each old puzzle is abandoned only after internal sociological developments within the scientific community. In particular, there are no devastating refutations, as might be suggested by Popper's view, but instead a steady evolution along Darwinian lines. A given puzzle is not abandonded until a 'better' puzzle comes along *and* is accepted.

The question of acceptance brings us right back to the Conventionalist basis of Kuhn's view. Although would-be revolutionaries are stimulated by Kuhn's book, it is really just an effort to explain so-called 'revolutions' away rather than to promote them. A 'revolution' is never a complete break but depends on the acceptance of an on-going community of scientists. The acceptance of a 'revolution' depends on the acceptance of any criteria used to assess the intended 'revolution'.

Methodologists could easily argue that a real revolution would require a revolution in criteria – but on what basis would the new criteria be assessed? Some may argue that such considerations show that Conventionalism is circular, but this is not the point we are making. What we wish to point out is that changes in any social enterprise require the stability of some frame of reference. In order to assess any change in methodological criteria we would still need some fixed basis from whence to assess the changes. We could appeal to some outside authority (such as philosophers of science) but this would only bring into question the basis of their authority. To assess methodology within an enterprise such as neoclassical economics requires the acceptance of neoclassical theory. Given this theory of social change, there could hardly ever be a genuine revolution.

Conventionalism and the 'growth of knowledge'

If it is difficult to specify a revolution within the context of a Conventionalist concept of the history of economic thought, can we at least identify unambiguous signs of 'progress'? If we no longer identify progress with establishing new 'scientific laws', then what is regarded as progress now? Consider Leijonhufvud's comments:

> Traditionally, the history of economic doctrines has for the most part been written as a 'straight' historical narrative – as a chronological story of 'progress' by accumulating analytical improvements in a field of inquiry of more or less stable demarcation and with a largely fixed set of questions.... [1976, p. 67]

The term 'stable demarcation' refers to what we are calling acceptance criteria. In this sense, given a criterion which specifies when a model or theory is 'better', we could simply say that progress is identified with finding a 'better' theory. But this reveals that there still is an element of the Problem of Conventions here, as long as there are judgements to be made about whether progess has been made.

So when Blaug asked, 'Has there been progress in economic theory?' his answer was a clear 'Yes' and his initial specification was a long list of Conventionalist criteria:

> analytical tools have been continuously improved and augmented; empirical data have been increasingly marshaled to verify economic hypotheses; metaeconomic biases have been repeatedly exposed and separated from the core of testable propositions which they enmesh; and the workings of the economic system are better understood than ever before. [1978, p. 7]

In more general terms he says:

> The development of economic thought has not taken the form of a linear progression toward present truths. While it has progressed, many have been the detours imposed by exigencies of time and place.... [p. 8]

Although Conventionalism and its presumption that there are standards of acceptance seems to dominate the historian's view of the methodology of economics, there does not seem to be as much agreement over what constitutes acceptable progress in economics as some historians might like us to think. For example, consider the views expressed at a recent meeting of the History of Economics Society:

> Jaffe expressed the opinion that there is a poverty of helpful economic ideas today and that future historians, though impressed by the technical progress of the discipline, may see a mismatch between means and ends....
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> Bronfenbrenner, on the contrary, said that the last 50 years might be considered a golden age because of technical advances, the shift from statics to dynamics, and the shift from the exclusive emphasis on microeconomics to the inclusion of macro elements. Coase replied that the state of *today's* economics is near disaster, as evidenced by the concentration of interest in microeconomics, the sameness of treatment of all subjects, and the concentration on techniques rather than economic problems....
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> In response to [a] question as to what of present-day economics will be remembered in 50 years, Bronfenbrenner listed (1) imperfect competition; (2) macroeconomics, including the *General Theory*, the new quantity theory, the theory of rational expectations, and the Phillips curve; (3) the rise of mathematical techniques; (4) the input-output table; and (5) growth models.... Coase believes that no book of the present will be remembered....
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> [Dingle, 1980, pp. 18-19]

Conventionalism and the Sequence of Models

The view that a research program in economics should be seen as a sequence of models is an example of the Conventionalist continuity theory of the history of economic thought. Is there anything more that one can infer from such a view? Probably not, since the recognition of a sequence does not imply that each step represents unambiguous progress, although that may be what Koopmans and Weintraub have in mind. Today, few economics writers find it worth while to add some romantic comments about how far we have progressed beyond our primitive forefathers. This is simply because real progess was always the promise of those who believed in inductive sciences and, we might now say, in an inductive learning possibilities curve which reaches the probability of 1.00 in real time. Now, today, we are apparently more modest, as it is agreed that there is always room for improvement. Each subsequent model in the sequence may be *more* realistic but nobody will claim that it is *realistic* – that is, that it is true. Each model may be more useful but, as Lucas said, that depends on what you want to do. Given all this modesty, one might wonder why anyone bothers with neoclassical research programs.

Revealed Methodologies

We have now painted a rather bland picture of contemporary methodology in neoclassical economics. Perhaps we should say that we have constructed a collage. The unifying element is the predominance of Conventionalism which is only lightly colored by its Inductivist origins. Model-building is the primary focus of all recent studies of methodology. And we are led to believe that 'progress' is any movement along some continuum formed by the growing sequence of accepted models. No one model is ever claimed to be true. Successful model-building is only tentative; our final judgement is to be postponed.

So we ask again, why do so many economists strive to contribute to the body of knowledge if their success is to be considered so tentative? Our answer, which we have been developing in the previous chapters, is that although there is much talk that might indicate a belief in the postulates of Conventionalism (namely, since we do not have an *operational* inductive logic, theories are not true or false but only 'better' or 'worse'), the acceptance of Conventionalism is only a short-run measure. When philosophers tell us that we cannot conduct an inductive proof, neoclassical methodologists have interpreted this to mean that we cannot give an inductive proof in our lifetime, and this does not logically preclude an inductive proof in the *very* long run. What contemporary methodologists and historians of economic thought presume is that our short-run tolerance of acceptably false models will be rewarded with the one true model in the long run. Eventually the sequence of models has to lead somewhere. Each model added to the sequence is like one more fact in the process of providing an inductive proof. In effect, neoclassical methodologists accept Conventionalism in the short run but hold out for Inductivism in the long run – perhaps Blaug's methodological view of the history of economics [1978] can be considered the paradigm of this perspective.

Misappropriation of Popper's View of Science

Contrary to our view that contemporary methodology is dominated by Conventionalism, given all the popular references to falsifiability of economic theories some might think that Popper's view of science has been adopted by most methodologists today. For example, consider the

following views:

> Popper, more than any other philosopher of science, has had an enormous influence on modern economics. It is not that many economists read Popper. Instead, they read Friedman but Friedman is simply Popper-with-a-twist applied to economics.... [Blaug, 1978, p. 714]

> I see no reason for denying to the study of the activities and institutions created by scarcity the title of science. It conforms fundamentally to our conception of science in general: that is to say the formation of hypotheses explaining and (possibly) predicting the outcome of the relationships concerned and the testing of such hypotheses by logic and by observation. This process of testing used to be called verification. But, since this way of putting things may involve an overtone of permanence and nonrefutability, it is probably better described, as Karl Popper has taught us, as a search for falsification – those hypotheses which survive the test being regarded as provisionally applicable.... [Robbins, 1981, p. 2]

Judging by Blaug's comments, one gets the impression that Karl Popper's philosophy of science has been adopted by most methodologists in economics. Judging by Robbins' comments, one gets the impression that Popper's role is only that of an elocution instructor. We shall argue here that Robbins' view is a better reflection of the state of affairs. So far, Popper's only real accomplishment in economics is the suppression of any open advocacy of Inductivism. Popper also claims to be opposed to both Conventionalism and Instrumentalism, yet both are openly promoted in mainstream neoclassical economics.

One reason why Popper has not had any significant impact on the nature of neoclassical methodology is that most economists have obtained their view of Popper by way of the writings of one of his students, Imre Lakatos. For many years most philosophers of science considered Popper to be in direct competition with Thomas Kuhn. As we noted above, Kuhn's view of science is quite compatible with that of most methodologists, as both are forms of Conventionalism. Lakatos endeavored to build a bridge between Kuhn and Popper; and to a great extent he has succeeded. But the cost of the reconciliation has been the abandonment of most of the more important aspects of Popper's philosophy of science.

The Foundations of Popper's Methodology

There are two essential and related considerations without which no clear appreciation of Popper's views can be reached. One is Popper's view of Plato's 'Socrates', the other is the observation that Popper has strong ties to what is usually called the Austrian School of economics.

Popper's anti-Justificationism

What makes Popper's view of methodology incompatible with Conventionalism is that he rejects the Problem of Induction [unfortunately, he calls his rejection a 'solution'; 1972, ch. 1]. What makes his view appear to be compatible with Conventionalism is that both deny the logical possibility of inductive proofs.

Popper's rejection of the Problem of Induction is based on a specific view which explicitly separates the process of knowing from the object we call knowledge. That is, for Popper we can examine 'knowledge' without the necessity of examining the 'knower' [1972, ch. 3]. All knowledge, in his view, must include one or more assertions which are of the form of 'strictly universal statements' [1934/59, ch. 3]. It is here that the impossibility of induction plays a crucial role. Where Conventionalism would say that these considerations would deny truth status for anyone's knowledge, Popper does not. For him, one's knowledge may very well be true, even though we cannot prove that it is true such as when it involves unverifiable universal statements.

A corollary of his separation of the question of what is the truth status of one's knowledge from the question of how one knows the truth status of one's knowledge is his separation of epistemology from methodology. Epistemology is about our theories of the nature of knowledge, and methodology is about our theories of learning or of the knowledge acquisition process [Agassi, 1969a]. Popper's epistemological position is that all knowledge is essentially theoretical conjecture [1972, ch. 1]. Any conjecture may be true or false – but even if it is true, there is no way we can ever prove that it is true. That is, even when we allow for specific observations to be considered true, there is no logic which can connect the truth of a finite number of observations to the necessary truth of any needed (strictly) universal statement. However, he observes that positive statements which are true are not completely useless – they can be used in refutations. In his terms, since strictly universal statements logically deny certain specified positive statements (i.e., observations), an observation of an instance of a logically denied statement constitutes a proof of the falsity of one's theory. Furthermore, since all theories involve universal statements, we can learn by proving that our knowledge is false if we continue to allow some observations to be considered true. But this is now a major departure from the traditional belief in what we have called the inductive learning possibilities function. More positive information does not increase the probability of one's model being true. If we are to learn from experience, it can only be

that we learn that some of our theories are false. This, we shall argue, is the essence of Popper's Socratic theory of learning.

Now, for all we know, Socrates may have been a figment of Plato's imagination. There is a considerable difference between the Socrates of the early dialogues and the Socrates of the later dialogues [Popper, 1945/66, pp. 306-13]. In both versions Socrates spends much of his time asking questions. But there is a major difference. In the early dialogues Socrates is the student asking questions in the process of attempting to learn. In the later dialogues he is the teacher attempting to teach by asking critical and revealing questions. Popper identifies with the early Socrates – that is, with Socrates the student.

Socratic learning theory

The best illustration of Socrates the student is to be found in the one dialogue which everyone agrees is fictitious – 'Euthyphro'. Let us examine this dialogue, since it can provide an excellent basis for understanding Popper's theory of learning. The plot of the dialogue is quite simple. Socrates is on his way to the court, where he is to be tried for 'impiety'. Now, Socrates does not understand why he is being charged with impiety – that is to say, given Socrates' understanding of impiety, he does not understand the charges against him. He encounters his former student Euthyphro, who is also going to the same court. Euthyphro's business there is that he has charged his father with impiety for killing a servant.

It is immediately obvious to Socrates that Euthyphro is an expert on the question of the nature of impiety. Surely no man would take his own father to court for impiety unless he was absolutely sure that he understood what piety and impiety were. The dialogue between Socrates and Euthyphro is carefully staged to illustrate the Socratic approach to learning. In this case, Socrates attempts to determine where *his own* understanding of piety and impiety has obviously gone wrong. Cynics might say that Socrates was only using Euthyphro to prepare his own defense, but that misses the point, as Socrates is sure that Euthyphro is correct. So the dialogue consists of Socrates' attempt to reveal his own understanding of piety and impiety so that it can be *critically* examined by the expert.

Socrates puts his understanding of piety and impiety on the table for Euthyphro to examine in the same way that we approach a physician when we have an ailment. Piece by piece, each element in Socrates' understanding is put to the test of Euthyphro's expertise. Every time Socrates puts to Euthyphro the question 'Is this correct?' Socrates' understanding passes the test! In the end, nothing is accomplished, as Euthyphro is unable to help by showing where Socrates has gone wrong.

But it is the supreme test – since if anyone were going to find something wrong with Socrates' understanding of piety and impiety, Euthyphro would.

For our purposes the point of this dialogue is that Socrates does not learn anything. The only thing that Socrates could learn with the help of his friend Euthyphro is that his understanding is faulty – that is, that there is an error in his understanding. For all of his agreement – that is, his verification of each of the elements in Socrates' understanding – Euthyphro is no help. He could only help by finding an error. Even though Socrates tries not to conceal any element in his understanding, the failure to find a flaw still does not prove that Socrates' understanding of piety and impiety is correct. Surely there is an error somewhere because the fact still stands that Socrates is being charged with impiety and Euthyphro is taking his father to court for impiety.

Now Popper's position is that science and the scientist are always in the same predictament as Socrates. We can never prove that our understanding is correct – even when it is. And the only thing we can ever really learn is that our understanding is false – if it actually happens to be false. For this reason, Popper sees science as a *learning* enterprise whose sole objective is to find errors in our understanding. This is why he puts such emphasis on testing, but it must be realized that the only successful test is the refutation of one's theory. This, then, is Popper's Socratic theory of learning: One's understanding is always conjectural but potentially true. The only way one can learn is to find a refutation – to find that one's understanding (i.e., one's theory) is false.

Learning as a process without end

There is a profound perversity in the Socratic learning theory. Given Popper's point that all explanatory theories involve unverifiable universal statements, learning in the more traditional, positive sense (verifying true explanations) is impossible. In this sense, one could never justify one's attempt to learn on the grounds that the ultimate end is possible. If one can never learn the true theory, why bother? This question is the essence of skepticism. But skepticism is merely an indirect expression of a belief in Justificationism – the view that we are not allowed to claim that our theories are true unless we can prove that they are true [Agassi, 1971a]. If one rejects Justificationism, then one is not necessarily led to skepticism. Although we may not be able to prove that our theory is true, it does not mean that our theory is not true. Even though we cannot learn in the more positive sense, we can still learn by correcting our errors. Discovering one's errors is definitely a positive step – as long as one does not reserve the idea of a positive step only for a step leading towards a justification or an inductive proof.

For Popper, science is a social institution that is pointing in the right direction even though it is readily admitted that it never reaches the goal at which we might think it is pointing. This is the same situation as that encountered when discussing Austrian economics. Economists from the Austrian School [see Blaug, 1980, pp. 92-3] do not recommend free-enterprise capitalism because it necessarily leads to Adam Smith's world of long-run equilibrium. On the contrary, as we saw with Hayek, to the extent that reaching any long-run equilibrium requires the acquisition of correct knowledge (or the correct expectations), reaching a long-run equilibrium is never possible. Besides that, what constitutes a long-run equilibrium depends on the exogenous givens, and we all know that they change faster than the process can ever get us to any long-run equilibrium.

If pushed to justify their faith in free-enterprise capitalism, the Austrians *cannot* say, 'We favor capitalism because, by following it, eventually we reach the "best of all possible worlds" – that is, reach the long-run equilibrium where everyone is a maximizer and all resources are optimally allocated.' Instead, their justification must involve only an evaluation of the process at a specific point in real time. The fundamental Austrian position in this regard is that when individuals are free to choose they are able to exploit (and thereby unintentionally to eliminate) errors in resource allocation. Eliminating error in resource allocation is an improvement for society, just as the Smith-Schumpeter view saw attempting to get ahead as leading to improvements in the overall efficiency of the economic production process. However, unlike Smith's classical world, which begins with a long-run equilibrium in order to show how greed can thus be virtuous, the Austrians are satisfied with a short-run view.

If one took a survey among neoclassical economists, one would not find very many believers in Austrian economics, but that may only be because neoclassical economists require justifications based on the properties of the hypothetical long-run equilibrium. One of the major analytical tools used by neoclassical economists is 'comparative statics', which does nothing but compare alternative long-run equilibria that differ only because there is posited a difference in some of the exogenous givens. We can extend this difficulty one more step. As long as neoclassical economists accept only teleological (i.e., goal-directed) justifications, they will never understand Popper's Socratic philosophy of science!

False Problems Raised by Popper

The demarcation problem

Early in Popper's career he tried to impress the leaders of the Logical

Positivist school of analytical philosophy. His method of doing this was to offer challenging solutions to their problems [viz., 1934/59]. They were unimpressed. One of his tactics was to argue that they wanted to solve what he called the 'Demarcation Problem'. According to his story, the Logical Positivists claimed that science was distinguished from philosophy on the basis of the *verifiability* of scientific theories, which entails the view that empirical evidence is significant only when it contributes to verifications. Philosophy, supposedly, was not verifiable. Popper argued that the Logical Positivists had it all wrong: empirical evidence is significant only for refutations, thus if science were to be distinguished, (i.e., 'demarcated'), from philosophy, it would be only in terms of the *falsifiability* of scientific theories. For those of us who have approached methodology from the perspective of economics and without any prior commitment to analytical philosophy, all this seems rather silly. But perhaps we are being too wise after the fact.

If we do not get involved with the older Logical Positivist views of methodology, then the so-called Demarcation Problem is at best uninteresting. Popper misleads us when he seems to be saying merely that our choice is between falsifiable theories and metaphysics [cf. Bartley, 1968]. Metaphysics is a matter of choice and not a matter of logic [Agassi, 1971b]. Some theories which may appear to be tautologies may be transformed into non-tautological statements [Watkins, 1957]. As we have argued before, a circular argument need not be a tautology [Boland, 1974]. Theories which are falsifiable may still be false [Wisdom, 1963].

'Degrees of corroboration'

In another place Popper creates an intellectual fog with his 'degrees of corroboration'. Presumably this is his effort to accommodate some aspects of Conventionalism – namely, the well-established acceptance of degrees of confirmation. In Popper's view [1934/59, ch. 10], a theory is 'corroborated' whenever it passes a test by not being refuted. The greater the likelihood of being refuted, the greater the 'degree of corroboration'. In a sense, corroboration is just a fancy name for unintended confirmation – but this is Popper's point. We do not set out to corroborate a theory; we set out to refute it in order to test our understanding. To placate those who feel uncomfortable about not having a positive reason for testing theory (or their fear of looking for the hole instead of the donut), he offers them an unintended reward for their efforts. But if one really takes the Socratic theory of learning seriously, no such reward is necessary. What is worse, for Popper's purposes, is that it is too easy to incorporate 'degrees of corroboration' as just another (sophisticated) Conventionalist criterion of acceptability.

Theories that are more corroborated are somehow superior to those which are less [see further, Hattiangadi, 1978].

The growth of knowledge

Another unnecessary dispute which Popper flames is the question of what constitutes the growth of knowledge. According to Popper's epistemology, knowledge consists exclusively of theories. Thus if knowledge is to grow, we must be able to compare theories on that basis. So Popper would have us believe that we are better off whenever (1) a new theory can explain everything that any rejected old theory explains, and (2) a new theory explains more and thus is capable of a higher degree of corroboration (because by explaining more it runs a higher risk of being refuted when tested). We are led to believe that when a new theory is offered that is better by these criteria we are supposed to drop the old, inferior theory. But if the old theory has never been refuted, why must it be dropped? The old theory may be true even though the new theory is considered superior by the criterion of the 'degrees of corroboration'. As long as we are comparing unrefuted theories, if they cannot be verified, then we are simply not in any position to choose! If we do, then the dreaded Conventionalism wins.

Friedman and Popper

According to Blaug, 'Friedman is not guilty of "instrumentalism" ' [1978, p. 703] and, as the quotation above indicates, Blaug believes that Friedman's methodology is merely a version of Popper's philosophy of science. It is true, as we have previously argued, that Friedman rejects Conventionalism. However, we have argued that Friedman's alternative is a form of Instrumentalism [Boland, 1979] – and Friedman has stated that we were correct in this characterization of his essay [1978]. Added to this, Friedman claims to be closely aligned with Popper's views [see Boland and Frazer, 1982].

Now, this sets up an interesting triangular situation. Friedman identifies with Instrumentalism and Popper. Blaug identifies Friedman with Popper's views but denies the connection with Instrumentalism. Popper rejects Instrumentalism [1972, ch. 3], yet both Popper and Friedman reject Conventionalism [Boland, 1979]. There is no way all three positions can be correct. Given Friedman's statements to us, Blaug draws the short straw; his view cannot be true. But, given Popper's rejection of Instrumentalism, how can Friedman be correct?

We conjecture that the reason why Friedman thinks that he is in agreement with Popper is that Friedman sees only two options. Either one accepts the dominant Conventionalist view of methodology, or one does not. On this basis, since Popper supposedly rejects Conventionalism, it would seem to follow that as Friedman also rejects Conventionalism, he must agree with Popper's view. We think Friedman's position in this matter is rather weak. Nevertheless, when it comes to practical policy, Popper's 'piecemeal engineering' [1944/61, 1945/66] is difficult to distinguish from Instrumentalism – particularly since Popper seems to dwell on a problem-oriented methodology. By Popper's rules, if one defines one's problem as a purely practical one, then perhaps Instrumentalism is the only way to go.

Conventionalist Pseudo-Popper

According to Blaug:

> To the philosophical question 'How can we acquire apodictic [i.e., logically certain] knowledge of the world when all we can rely on is our own unique experience?' Popper replies that there is no certain empirical knowledge, whether grounded in our own personal experience or in that of mankind in general. And more than that: there is no sure method of guaranteeing that the fallible knowledge we do have of the real world is positively the best we can possess under the circumstances. A study of the philosophy of science can sharpen our appraisal of what constitutes *acceptable* empirical knowledge, but it remains a provisional appraisal nevertheless.
>
> [Blaug, 1980, pp. 27-8, emphasis added]

What is clear from such a comment by a well-meaning methodologist is that Conventionalism lives, no matter what Popper says. Why is acceptability so important? If one agrees with Popper that theories can be true or false and that even when they are true there still is no method to establish their truth, what does it mean for a theory to be 'acceptable'? It matters a great deal for the Conventionalist method of dealing with the Problem of Induction. But if we follow Popper's rejection of the Problem of Induction, why should anyone be concerned with the acceptability of empirical knowledge? Unfortunately, there is no way to answer these questions in a manner that would both satisfy a believer in Conventionalism and still be consistent with Popper's rejection of Conventionalism.

Falsifiability as a Conventionalist criterion

Despite Popper's intentions, his trumpeting of the falsifiability criterion to solve his Demarcation Problem is all too easily incorporated into the list of acceptable Conventionalist criteria. Again and again we have to

point out, no matter how well a theory fares by any Conventionalist criterion (which does not include truth or falsity), there is nothing to connect the success of the theory in those terms with the actual truth or falsity of the theory. So what is accomplished by requiring that all 'scientific' theories be falsifiable? It does preclude tautologies, but despite this criterion's origins, it does not preclude metaphysics [Agassi, 1971b].

The most important assumptions in neoclassical economics, such as the maximization hypothesis or the assumption of the variability of all factors, are unfalsifiable. Although the maximization hypothesis is not a tautology, it is usually unfalsifiable because it is put beyond question [see Boland, 1981b]. Similarly, the most important assumptions in Marxist theory are unfalsifiable. Almost every Marxist model presumes the existence of a class struggle or an exogenously given rate of capitalist accumulation. Neither of these assumptions is ever put to the test. Both are just assumed to be obviously true – just as the neoclassical maximization assumption is considered to be obviously true. If we believed in a Conventionalist implementation of the falsifiability criterion, there would virtually be no acceptable social theory, since all explanatory theories involve at least one key assumption which is put beyond refutation [Agassi, 1965].

Popper and the 'new heterodoxy'

Blaug identifies Popper's philosophy of science as the 'watershed between old and new views of the philosophy of science' [1980, p. 2]. The new view, according to Blaug, is the Conventionalism of Kuhn's or Lakatos' compromised version of Popper's view. How one conceives of a 'watershed' transition from the Conventionalism of the Logical Positivists to the Conventionalism of Kuhn which passes through Popper's anti-Conventionalism is difficult for us to understand.

The 'new heterodoxy' is nothing but the 'old heterodoxy' dressed up in clothes designed by Lakatos. The 'watershed' has yet to be crossed. Nowhere do we find Popper's Socratic view of learning represented in either neoclassical methodology or neoclassical theory. Without any doubt, Socrates did not submit to the conventional wisdom of authorities he faced in the court. Socrates considered his view of his situation to be true even though the votes were not in its favor. To the extent that Blaug's views represent the state of the methodology of mainstream neoclassical economics, Popper's impact on economics may be only cosmetic.

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Putting Popper on the Agenda

> L: Couldn't you have been more original?
> I: No, I didn't have enough time!
>
> Anonymous

> When I was a boy, I had a clock with a pendulum which could be lifted off. I found that the clock went very much faster without the pendulum. If the main purpose of a clock is to go, the clock was the better for losing its pendulum. True, it could no longer tell the time, but that did not matter if one could teach oneself to be indifferent to the passage of time. The linguistic philosophy, which cares only about language, and not about the world, is like the boy who preferred the clock without the pendulum because, although it no longer told the time, it went more easily than before and at a more exhilarating pace.
>
> Ernest Gellner [1959/68, p. 15]

We would be less than fair or honest if we let the issue end in such a state of discordance. We think that if one drops one's interest in the classical Problem of Induction, then Conventionalism ceases to be of any interest whatsoever. No real problem is solved by Conventionalist methodology. That is, there is no reason for our having to choose one theory rather than another other than short-term practical considerations. And, worse, if one turns to consider practical problems, then Conventionalism is not appropriate, but instead Instrumentalism should be the guide. But, far more important, if one is concerned with the 'realism' of economic models, then Conventionalism is totally inappropriate, as it eschews the models' truth or falsity.

The only methodological perspective, other than the impossible Inductivism, which is directly concerned with the realism or unrealism

of economics models is Popper's so-called critical rationalism. Those neoclassical economists who are concerned with the realism of their models might now wish to consider Popper's methodological viewpoint. Let us explore what it would mean for an inclusion of Popper's methodology in the hidden agenda of neoclassical economics and its possible role in neoclassical theory.

Adjusting the Neoclassical Hidden Agenda

Eliminating the first item on the agenda

If one is so inclined, including Popper's methodological perspective is conceptually rather easy. The key to Popper's methodology is the rejection of the Problem of Induction. If we eliminate the need for authoritarianism, then there is no need to solve the classic Problem of Induction. This means that we can also cease taking such things as the inductive learning possibilities function for granted. For example, we might wish to recognize that some observations or additional bits of information actually refute our knowledge rather than increase its probability of being true. Instead we can focus on neoclassical model-building as a systematic attempt to learn by our theoretical mistakes and thereby emphasize the role of criticism and disagreement in the development of neoclassical economics. As long as one's contribution is criticizable, anything should be allowed to be considered. There is no theoretical reason why we should choose between competitors. What is more important, from the perspective of Popper's methodology, is our understanding of the problems that anyone's contribution is intended to solve, as well as the alternative ways the problems may be solved. For the purposes of learning, rather than looking for the one correct solution, it is more important that we continue to look for more and more alternative solutions.

Generalizing the second item on the agenda

Continuing along the lines of considering the implications of any attempt to include Popper's methodology among the items on the hidden agenda, let us now examine the second item, methodological individualism. If we reject the need either to deal with or to solve the Problem of Induction, then there is no need to adopt the extreme form of methodological individualism that is based on an unsupported presumption of psychologism. What this means for methodology is that individuals are not identified with their psychological states. Rather than taking individuals' psychological states as irreducible givens, we can attempt to explain their psychological states. This does not necessarily rule out individualism. Individuals still make all of the decisions. We are concerned here only with the basis of their decision-making. What we will argue below is that a major ingredient in every decision is the theories held to be true by the decision-maker and that in the absence of an inductive logic such theories cannot be reduced to the given nature of the physical world. Why any individual may consider a particular theory to be true may or may not be at issue. It all depends on the problems that the individual is trying to solve.

Dealing with the knowledge basis of decision-making

By following Popper's rejection of the Problem of Induction – and with it, Inductivism and Conventionalism – the door is open for the neoclassical economist to attempt to explain the knowledge basis of decision-making. By dropping the presumption that permits only psychological states and natural givens, the way is clear for the recognition that in order to explain the process of decision-making, the methodology of the decision-maker needs to play an essential role. What a particular decision-maker's methodology actually is depends on the problem-situation facing the decision-maker. To a great extent the methodology of the decision-maker depends on the decision-maker's *theory* of that problem-situation. There is no reason why anyone should expect any decision-maker to hold a true theory of the problem-situation, nor is there any reason why all decision-makers should employ the same methodological perspectives. The focus of these considerations will ultimately be concerned with how individual decision-makers deal with the discovery of evidence that contradicts the theories which they thought were true in the process of making their decisions.

Real-time Individualism in the Short Run

Combining Popper's methodology with neoclassical economics

Discussing arbitrary changes in the research agenda of neoclassical economics is really not very interesting unless we can see how the new agenda affects the nature of any neoclassical theory. The one research topic where Popper's methodology and epistemology can play a dramatic role concerns the appropriate short-run setting for neoclassical economics. As we explained in Chapter 6, the usual treatment of time in neoclassical explanatory models has been quite inadequate. Specifically, the dynamics of the usual neoclassical models based on Inductivism and Conventionalism are exogenous and hence unexplained. We wish to show here that by dropping Inductivism and Conventionalism and instead relying on Popper's views of knowledge and learning, the way is open to the development of real-time explanations in neoclassical theory. To be neoclassical all that is required is that we retain individualism –

that is, the view that only individuals make decisions – as well as rational decision-making. However, it should be stressed that Popper's methodology focuses on rational decision-*making* and not on rational decision-*makers*.

As Hicks [1976, p. 136; 1979] observes, the general problem of explaining change (dynamics) in the context of rational decision-making is that the decision-maker's knowledge (of the givens) is hopelessly static. Although Hicks appreciates the problem, he has missed the source of the difficulty. It is not that our knowledge itself is static, but rather that the traditional *views of knowledge* assert that knowledge is static. We shall argue that there is not necessarily a problem with rational decision-making, except when its logical basis presumes that the individual's knowledge (of the givens), or its acquisition, is exogenously given.

Traditionally we are required to choose between the two views of knowledge that we have identified with the first item on the hidden agenda. On the one hand, there is Inductivism, which asserts that knowledge is only the facts collected up to a certain point in time. On the other hand, there is Conventionalism, which considers knowledge to be only the latest, accepted theory (of the facts) at a certain point in time. Both views make knowledge static because it is exogenously given at any point in time.

To emphasize our viewpoint of knowledge in the short-run setting, let us review the essentials of our discussion in Chapter 1 of these two views of knowledge. What is salient in both of the traditional views or theories of knowledge is that an empirical statement or a theory is considered knowledge only to the extent that it is supported by the facts. These traditional views differ only in regard to what is meant by 'supported by the facts', or what constitutes 'the facts'. With Inductivism, factual support is alleged to be direct and logically complete. However, with Conventionalism, all knowledge can be considered an accepted system of catalogues used to file or 'capture' the available facts and thus knowledge is only 'better' or 'worse' rather than 'true' or 'false'.

As we explained in Chapter 1, both views are based on the common belief in Justificationism, that is, the doctrine that a theory is not true knowledge unless it can be justified (i.e., proven true). A first step toward solving the problem of explaining dynamics in the short run is the recognition that Justificationism is false (not only because it is unjustified itself). We shall argue below that by rejecting Justificationism, that is, by separating the truth status of a statement from the proof or the provability of its truth status, the way is clear to resolving the dilemma discussed in Chapter 6 of having to choose between dynamic explanations and explanations of dynamics.

A basis for an individualist explanation of dynamics

To solve the problem of explaining dynamics we begin (closely following [Boland 1978]) by formulating a new, non-psychologistic, individualist research agenda based on the epistemology of Popper and a modified version of the methodological individualism of Hayek. We shall call this the Popper-Hayek program for explaining any rational dynamic process. For the purpose of discussion let us itemize the essential parts of this proposed agenda.

Anti-Justificationism. First, all knowledge is presumed to be essentially theoretical, hence conjectural; second, it is possibly true, although we cannot prove its truth status [Popper, 1972, ch. 3].

Anti-psychologism. It is presumed that everyone's knowledge is potentially objective [Popper, 1972, ch. 1].

Rational decision-making. It is presumed that what one does at any point in time depends on one's knowledge *at that time* and the logic of the situation in which that knowledge is used [Hayek, 1937/48; Hicks, 1973, 1979].

Situational dynamics. It is presumed that one's behavioral changes can result from changes in one's knowledge as well as from intended or unintended changes in one's situation [Hayek, 1937/48; Shackle, 1972].

It should be pointed out that this approach to solving the problem of explaining dynamics within a short-run individualist framework requires the rejection of Hayek's inductivist epistemology and its replacement with Popper's concept of objective knowledge. The latter requires the rejection of psychologism. The first step is to specify one or more actors, in the past or present, who have been causing or contributing to the change in question, and the *theories they held at the time of their actions*. Next, we must specify the unintended consequences of their actions, entailing conjectures about why their theories were *false*. Note that the falsity of the theories may be unknown to the actors at the time; in fact, it is by means of these unintended consequences that actors in question may learn that their knowledge is false. In short, this framework asserts that economics *in* time is a sequence of unintended consequences of acting on the basis of (unknowingly) false theories [cf. Hicks, 1965, p. 184; 1979]. (Note that this is not instrumentalism, since the truth status may still matter.)

Objective theoretical knowledge

Let us examine the elements of this Popper-Hayek individualist program. Discussing the nature of knowledge is quite difficult because knowledge itself is usually given a rather lofty status. Nevertheless, it cannot be

avoided. We propose to recognize a simple separation between the truth status of someone's knowledge (i.e., whether it is true or false) and the role that knowledge plays in his or her decision-making process – that is, to provide a sufficient and logically consistent explanation of the world he or she faces. Of course, at the very minimum, knowledge must be logically consistent if it is to be able to provide a true explanation of something. This is so even though the logical consistency of any explanation does not imply its truth. Nevertheless, it is the consistency of a decision-maker's knowledge which plays the major role in our explanation of his or her behavior. The truth of his or her knowledge is much more difficult to ascertain. But, more important, the truth of his or her knowledge is not always necessary for a successful action on his or her part. It should be noted that by separating the truth status from the role of knowledge we are not suggesting that theories or knowledge cannot be true; and we are definitely not agreeing with Solow's [1956] view that all theories are false, since that is a self-contradiction. On the contrary, we are asserting that a theory can be true even though its truth status is usually unknown to us.

By saying that knowledge is essentially theoretical we are emphasizing that the truth status of anyone's knowledge is always conjectural (i.e., not completely justified) and that it is potentially objective. By 'potentially objective' we mean only that by its logical nature it is capable of at least being stated in words or in other repeatable forms to the extent that it is the knowledge of the real world [Popper, 1972, pp. 106ff.]. It could be argued that the potential objectivity of any decision-maker's knowledge makes possible a so-called operationally meaningful explanation of his or her behavior.

In our view, since all knowledge is theoretical, anyone's knowledge can be put on the table for everyone to see. The view that knowledge is potentially objective stands in opposition to the implications of the more common view identified above as psychologism. Psychologism presumes that knowing is either a natural given, directly provable by induction, or a psychological process. While the former is precluded by rejecting induction, the latter makes one's knowledge private or subjective [Popper, 1972, pp. 1-7]. A corollary of psychologism is that one can never explain someone else's knowledge in the absence of induction. Either way, the proposed view requires at least a rejection of psychologism.

The common psychologistic view of knowledge may only be saying that one cannot guarantee a *true* explanation of someone else's knowledge. We propose this reading of psychologism to explain why anyone might think that it is impossible to explain someone else's knowledge. If this reading is correct, then psychologism is merely another variant of the Justificationism rejected earlier. In the remainder of this chapter when we speak of someone's knowledge we shall not be referring to his or her inherently private views but rather to his or her explanations or theories of the behavior and nature of the world around him or her.

The role of knowledge

Hayek and others have recognized that the individual decision-maker must have knowledge of the givens or constraints if these are to play an active role in the decision process. If this view is correct, the individual's knowledge must also play an active role in any explanation of his or her behavior. This prescription is not novel. Since late in the nineteenth century most social scientists have adopted a methodology in which the actor is presumed to be 'rational' concerning his or her given situation. This is evident in much of the formal sociology of the late nineteenth century, which often presumes a fixed frame of reference, an 'ideal type', whose behavior is based either on perfect knowledge or on a fool-proof method of acquiring perfect knowledge. In this old methodology the behavior of an actual individual is explained by noting to what extent, or why, his or her behavior is not ideal or perfectly rational.

In ideal-type methodology, one source of an individual's deviance from the ideal stems from the so-called imperfections in his or her knowledge of the givens. The imperfections of one's knowledge might result, as argued in previous chapters, from the fact that in real time an inductively rational acquisition of knowledge is always inadequate. With regard to explaining rational dynamic processes, we may wish to give the imperfections a systematic and prominent role, but this is possible only to the extent that knowledge itself plays a role. Perhaps the only complaint one might have regarding the ideal-type methodology is that it actually neutralizes the role of the actor's acquisition process by presuming that there is some ('scientific') method of acquisition which will always give him or her the true knowledge of the givens. Such a method is essential to the definition of the ideal type. If such a method is presumed to apply, any deviance from the ideal can only result from the actor's 'irrationality'. Note that the use of the Rational Expectations Hypothesis avoids this escape clause by arguing that apparent imperfections are actually quite rational! Except for a few 'a priorists' such as Ludwig von Mises, using the ideal-type methodology usually implies a reliance on inductive logic to provide the rational method of acquisition. With the prior rejection of inductivism, we thus have at least rejected any reliance on ideal-type methodology with regard to the knowledge of the individual decision-maker.

Here we argue that the question of the truth status of an actor's knowledge (i.e., whether it is actually true or actually false) is a separate question from why the actor thinks or believes his or her knowledge is true. In particular, the truth status of any actor's knowledge is usually independent of the method of its acquisition. An actor's theory of something can be true regardless of how he or she came to hold that theory to explain numerous observations; he or she could have dreamt it. Any method of acquisition may succeed or fail. In our view, this separation of status and method is important because the truth status of the actor's knowledge and the method of acquisition play different roles in any ongoing decision process.

Hayek's view of the essential role of knowledge seems to be widely accepted today [e.g., Hirshleifer and Riley, 1979] – but more care needs to be taken to avoid taking Inductivism for granted. For example, Hayek's use of the word 'acquisition' was consistent with an inductivist theory of learning, namely, one in which learning involves collecting facts (e.g., observing 'grey elephants') and then inductively leaping to the conclusion that some general proposition about them is true (e.g., the statament 'All elephants are grey'). Such general propositions or theories are said to have been 'acquired'. We do not wish to limit the concepts of learning or acquiring to exercises in inductive logic, since, as argued above, such learning requires an unreal (infinite) amount of time. The actual (real-time) discovery of refuting evidence that shows one's current theory to be false is also a form of learning. This form of learning (i.e., having one's knowledge refuted) will be most important in our program for explaining dynamic processes in the short run. We shall argue that the status of an actor's knowledge may give a reason for change, but it does not tell us what the change will be. However, knowing the actor's learning methodology may provide a clue to what change he or she may attempt to effect [Boland and Newman, 1979].

An illustration

Let us consider an example from orthodox microeconomics. We traditionally say that the consumers know their preferences and their givens (i.e., what their budgets will be as well as what the 'given' prices will be). We explain their behavior, first, by assuming that their preferences are convex, transitive, etc., and that prices are given and, second, by assuming the consumers buy the 'best' bundles according to their preferences. Now, Hayek argued that the consumers in a competitive market economy cannot always 'know' *a priori* what prices, or availability, will be, or even what their incomes will be the next time they go to the market. In terms of the proposed epistemology, the consumers have theories of what their incomes and the prices will be, although those theories may not be provable by reference to the facts known at any point of time prior to going to the market. Nevertheless, we (and the consumers), on the basis of their theories, logically predict what they will buy. Their theories might be inferred from past experience or deduced from knowledge of some prior institutional controls or from the pronouncements of the local authorities, etc.

Even recognizing that our predictions might be wrong, this illustration has not gone far enough for our purposes because it is still taking psychologism for granted. So we argue that in addition to having theories about what their price-income situations are, the consumers may also have only theories of what their preferences are. Specifically, unless they have tried all conceivable 'bundles', they do not 'know' from experience what their preferences are or will be even if their preferences do not change over time. They may believe the orthodox demand theories, thus may assume that their preferences are 'convex', 'transitive', etc., and may thereby rationally choose their optimum bundles for their expected price-income situation. As long as they are able to buy what they *think* is their respective 'best' bundle, there is no reason for them to change to any other bundle. They would have to be willing to test their theories of their preferences before we could expect them rationally to try other bundles which, on the basis of their current knowledge, they think would be non-optimal.

If our orthodox theory of consumers' behavior is true, then they would find that they are not better off with the test bundles and may return to the predicted optimum. If our theory is not true, they may find that they are better off with their respective test bundles, and hence their prior knowledge about their own preferences will have been revealed to be false. Or they may still not be better off with those particular test bundles.

Consider an alternative situation. It is quite possible for the consumers' preferences to be concave somewhere, yet, for some unknown reason, for them to have picked their 'best' bundles. Most important, if their theories of their own preferences turn out, upon testing, to be wrong and if their preferences do play a significant role in their decision-making, they will at some point be led to change their behavior. Depending on their views concerning facts and knowledge, they may change immediately by buying their own 'better' test bundle if they have found one or may change at some future point when facing new price-income situations. Unless we can say something about the consumers' methodology, logically anything can happen.

In general, if one's theory of the world plays a decisive role but is false, accepting it as true must lead to errors in real time. How one responds to such errors depends on one's view of knowledge and how it is acquired. Furthermore, unless there is only one conceivable methodology, consumers can be clearly distinguished by their respective methodologies as well as by their preferences and their income situations.

Responses to the need for change

Our consideration of the role of knowledge offers two possible reasons for change. First, an actor may change his or her behavior because exogenous changes in the givens can cause the actor's knowledge to be 'out-of-date', i.e., *false*. A typical example of response to this type of false theory is a movement along the demand curve as a consequence of (disequilibrium) price changes. When the consumer learns that the price has gone up he or she adjusts to the new price by buying less.

Secondly, an actor's mistakes which result from acting on the basis of false knowledge, even when the givens have not changed, will directly and endogenously cause changes in the future givens. For example, consider how an imperfectly competitive firm decides on the quantity of its product to supply and its price, given its current financial situation. Let us say that in making a decision it estimates the demand curve incorrectly. Having supplied the wrong quantity, it soon discovers that it put the wrong price on its product – its actual sales do not correspond to the level it expected. This leads to unintended changes in its financial situation. The new givens will affect its future decisions even if it never learns anything about how to estimate future demand curves.

This example is not designed to suggest that an actor's situation changes only as a result of unintended consequences. It is quite conceivable that an actor might change his or her situation deliberately. The producer may decide to invest in new machines in order to reduce production costs or change the nature of his or her product. Such changes in the givens would be *intended* consequences. As long as the givens have changed, deliberately or not, the future behavior of the producer will usually change. New givens require new knowledge of the givens. Since there is no foolproof method of acquiring new knowledge, one's knowledge is very often false [Newman, 1976]. False new knowledge yields new errors and new unintended consequences.

The evidence of errors or mistakes could be considered a criticism of the 'realism' of one's assumptions and would thereby seem to bring about a change in one's theory of the world. However, this depends crucially on one's methodology and view of knowledge. By adopting a Conventionalist view of knowledge, decision-makers might find it possible to deflect such empirical criticism by some form of approximationism [Boland, 1970b]. For example, they might say that the evidence of a counter-example (an error) is not really contrary to their theory of the world, because that theory is probabilistic and thus allows a few counter-examples provided they are not 'too numerous' [cf.

Rotwein, 1959, 1980; Shackle, 1972; Boland, 1977c]. Or it might be said that only when the errors continue to happen will one be pushed to consider changing one's view of the world. (Remember, one must not 'jump to conclusions'.) Thus adherents to Conventionalism may be slow to react to unintended consequences. On the other hand, adherents to Intrumentalism, such as the followers of Friedman's essay on methodology, who knowingly accept false assumptions may never change.

Alternatively, those actors with a 'skepticist' theory of knowledge may always be looking for indications that their knowledge is false and may always be ready to modify it. Their behavior, unlike that of typical adherents to Conventionalism, will appear very erratic and will certainly be more difficult to predict. More might be said about this; for now it is enough merely to conjecture that *the way one responds in a real-time short run to unintended consequences or counter-examples to one's assumptions reveals a great deal about one's theory of knowledge* [Boland and Newman, 1979].

Although we have used examples from microeconomics, these epistemological considerations are equally important to the usual conceptions of macroeconomics. Government policies today are based on the assumption that specific macro theories are true, and estimates are made of parameters of models of these theories, predictions are made, and so on. What would happen if these theories were false [Robinson, 1967]? How do governments respond to counter-examples?

A Neoclassical Program for a Real-time Short Run

Having now presented all the ingredients for a non-psychologistic version of neoclassical economics, let us now present our program for explaining rational decision-making in the short run in a way in which real (i.e., irreversible) time matters. We should also point out at the outset that this research program is not limited to only questions of pure dynamics. As Samuelson pointed out with his famous 'Correspondence Principle' [1947/65], even the explanation of a static situation is not complete until we have also explained the dynamic path which led to the static situation.

We accept Hayek's view that all rational decision-making must depend on knowledge of the givens and any explanation of rational decision-making must include assumptions about the decision-makers' methodology (i.e., about how knowledge is acquired or changed). We argued that this depends on the decision-maker's *theory of knowledge*. Thus, in any explanation of an actor's behavior we must specify the actor's view of the nature of knowledge and how it is acquired or changed. Along the lines of Chapter 10, we should point out that the methodology that is adopted by the actor may depend on the actor's aims. A different aim may require a different methodology. For example, an actor may only have very short-run goals and thus it would be appropriate for us to assume that the actor adheres to an Instrumentalist methodology. Another actor may include long-term learning as a goal; thus this second actor may adhere to either Inductivism or the Popper-Socrates theory of learning.

Despite these examples, traditional explanations involving knowledge or information may be unsatisfactory in any a real-time short run. Similarly, any macroeconomic explanations which presume unanimity concerning the epistemology or methodology may not be considered adequate. Any static concept of the actor's knowledge or its acquisition – that is, a concept for which real time does not matter – renders Hayek's view incapable of explaining economic change. Furthermore, although it is well known that all models require at least one exogenous variable, any view which considers knowledge or its acquisition to be exogenous will not permit an explanation of the endogenous dynamics of a rational decision process.

Our program for explaining short-run dynamics consistent with neoclassical economics is based on a dynamic concept of knowledge where its acquisition is endogenous. In particular, the *process* of acquisition depends on the specific *view of knowledge* held by the actor. Primarily, all decisions are seen to be potentially part of the learning process. Learning, by definition, is irreversible; hence it is always a real-time process. The decision-maker can learn with every decision made. What he or she may learn at least is that his or her theory of the givens is false. *How* he or she responds depends on his or her theory of knowledge. Thus, an essential ingredient of the short-run neoclassical program presented here is the requirement of an explicit conjecture concerning the actor's objective theory of knowledge. Moreover, this solution specifically recognizes that even when facing the same facts (i.e., the same experience), two decision-makers who differ only with respect to their theories of knowledge will generally have different patterns of behavior *over time even in the short run*, patterns that may not be equally predictable.

Rational decision-making does not require rational decision-mak*ers* nor does it require proven true knowledge. It requires only the explicit assumption on the part of the decision-maker that some or all of his or her knowledge is true. Actions based on knowledge that is actually (but unknowingly) false will yield errors or other unintended consequences in the short run. These consequences are not evidence of the actor's so-called irrationality; rather, they are evidence that some of the actor's knowledge is false.

The view that one is irrational if one's knowledge is false presumes that there exists a rational process which yields guaranteed true knowledge. Unfortunately, such a process does not exist, so that the charge of 'irrationality' is misleading and perhaps unfair. Yet the actor's knowledge does play an *essential* role in his or her decision process. Not only is it not logically possible to assure that knowledge is true; it may also actually be false. Thus, our research program includes Popper's epistemological viewpoint which explicitly separates the truth status from the role that knowledge plays in the decision process. Primarily, this permits us to separate the static nature of the truth of knowledge from the dynamic nature of the learning process. However, this separation alone is not enough to solve the problem of explaining endogenous dynamics within an individualist framework. One must also assume that the learning process is not one of the exogenous givens of the explanatory model.

With traditional equilibrium dynamic models, as we explained in Chapter 6, explanations of changes rely on exogenous changes in the givens for the rational decision-maker. Every decision-maker would be expected to respond to the new givens, and the new equilibrium is reached at the point where everyone's behavior is consistent with the new givens. Thus traditionally there are two types of observable change: long-run moving equilibria and short-run movements towards a new equilibrium. In models where real time does not matter these two types of change are indistinguishable by simple observation. By definition, an unambiguous short-run change is identifiable only where there have been no changes in the givens. Long-run equilibrium change can occur only after the givens have changed. Once an equilibrium has been reached, no changes should occur without exogenous changes in the givens.

In Chapter 3 we discussed the limitations of the commonly accepted view that the explanation of macrodynamics should be based on the existence of disequilibria caused by the necessary uncertainties of the individual decision-makers. We argued that recognizing that false knowledge is a reason for change is not enough. A complete explanation of macrodynamics based on knowledge uncertainties must also explain *how* individuals respond to those uncertainties in real time – that is, individuals' views of methodology must be included in the short-run setting. However, in Chapter 4 we argued that current views of the individual's way of dealing with knowledge and learning are based on a false theory of knowledge – the inductive learning possibilities function – and thus can never be the basis of a realistic theory of economic behavior. We might add now that the common view is quite inconsistent with Popper's view of knowledge and learning. For Popper, learning occurs only when additional information forces a change in one's knowledge; thus learning cannot be characterized by the inductive learning function and thus the Rational Expectations Hypothesis is at best irrelevant for questions of endogenous dynamics.

Contrary to the views which base all dynamics on the existence of disequilibria, we argue that the absence of a disequilibrium does not imply the absence of change because any current equilibrium may not be compatible with existing knowledge. Any definition of long-run equilibrium which requires that existing knowledge (or 'expectations') be compatible with the given equilibrium is in effect presuming that there exists a solution to the Problem of Induction since every long-run equilibrium is merely a special short-run equilibrium (e.g., one where all markets are in a short-run equilibrium). Since there is no solution to this problem, knowledge incompatibility is always possible in real time. Depending on the actors' learning methodology, at least one of the givens (viz. their theories of the givens) may change. Such a change, which can be explained in terms of the actors' theories of knowledge, leads to a new disequilibrium. If the actors learn with each decision, their knowledge may always be changing. They will therefore always be in a state of disequilibrium. However, this state can be completely explained if we explain how the actors respond to knowledge incompatibility.

The evidence that one's knowledge is incompatible with the equilibrium values of the givens and the variables is one's unfulfilled expectations. Unfulfilled expectations are interpreted as unintended consequences. This means that in equilibrium models unintended consequences are the motivating reasons for *endogenous* change. Thus, if we are going to explain change in the short-run setting, we must focus on the sources of unintended consequences, namely, the actors' false theories and their methodologies, which together play a primary role in all learning and thus in all dynamic processes.

12

Problem-dependent Methodology

> Do not presume, one of the thieves was damned; do not despair, one of the thieves was saved.
>
> St Augustine

Despite recent comments by methodologists indicating that Popper's philosophy of science is a guiding light for economists, the fact is that neoclassical economics is still founded on a methodology consisting of Conventionalism mixed with bits of overt Instrumentalism and inadvertent Inductivism. Popper's contribution so far has been limited to only an improvement in the methodological jargon. Where Popper sees science as an enterprise built upon systematic criticism, our profession's reliance on Conventionalism to deal with the Problem of Induction has always put a high value on agreement, that is, on having our views accepted by our colleagues. Given that there is no formal inductive logic, everyone seems to think that a theory can be considered successful only if it has been included somewhere in the accepted view of economics.

The opinion that there should be one accepted view is immediately open to question. Yet it is an opinion that is at the core of virtually every methodological dispute. The traditional view is that in order to discover the true nature of the economy we must first have the one correct method for analyzing the economy. As the tradition goes, famous physicists such as Newton or Einstein were successful only because they used the correct 'scientific method'. The companion tradition says that anyone who is not successful must be using an 'unscientific method'.

These traditional views are so well entrenched that it may be difficult for us to convince any reader that there may be something wrong here. Nevertheless, that is our chosen task for this chapter. We shall argue that the traditional view is misleading on two counts. First, it presumes there is only one correct method for all of science; and second, it reflects an even more fundamental item on the hidden agenda of every science that would require 'authoritative support' for anyone's explanation of anything of scientific interest.

Regardless of the wisdom or foolishness of anyone's concern for whether or not economics is a science, there is an overriding concern that whatever the outcome of an examination of our methods of analysis, we should at least agree on some general principles of analysis. The reason is simple. Economics is not a one-person affair these days. Improvements in our understanding of the workings of an economy usually depend on the combined efforts of many individuals. But we must be careful here. No matter how necessary common agreements may be, there are still some dangers of putting too much emphasis on them. (Remember how Hans Christian Andersen demonstrated those dangers in his story of 'The Emperor's New Clothes' in which the common, agreed upon view was definitely wrong.)

The primary reason for putting too much emphasis on common agreement is the frequent plea that the economic problems of society need urgent solutions. Sympathy with this urgency puts the academic economist in an awkward position. On the one hand, if we all could agree on general principles, less time would be wasted in arguing about fundamentals and more time would be available for finding good solutions to our pressing problems. On the other hand, good solutions may require new principles better suited to contemporary conditions. In effect, we always face a choice between immediate returns which may be limited by our current understanding and long-term benefits which follow from a new or improved understanding. The choice is never easy – and we, furthermore, deny the existence of universally acceptable criteria.

The Traditional View of Methods

Is there a method of analysis somewhere which, if we always used it, would ensure that we would never make a mistake? Indeed, it would be nice should there ever be such a method, but unfortunately there is not. We need not despair, though. Popper tells us that we learn by our mistakes; all we can hope is that our mistakes do not cause too much damage. As we discussed in Chapters 1 and 10, there was a time when many people thought there was a foolproof and objective method by which individuals could avoid mistakes by being extemely careful in the collection of 'facts' and, above all, by not passionately 'jumping to conclusions' before all the facts were collected.

Today, being a scientist is not such a personal matter. Rather, it is a matter of being part of a scientific community. Membership in a scientific community is governed by two factors: one's credentials and the acceptance of one's methods. The appropriate credentials are rather obvious – one needs a graduate degree or two. But one's education is not enough unless it involves being trained in the use of the accepted methods. Just what are the appropriate credentials or the accepted methods is not always obvious, since they can vary from one generation to the next or from one discipline to the next.

In many cases it is not easy to tell whether the latest accepted methods are not just the latest fad – but we will leave this critical note for now. It is important to recognize that what may be considered '*the* scientific method' today may tomorrow be considered very inadequate and thus may be replaced by another accepted method. The method supposedly followed in Newton's time would be considered rather silly or naive today by some scientists. Yet no one is willing to dismiss Newton's theories merely because his methods may be a bit suspect today. In retrospect, it would seem that the significance of one's theories may be judged separately from the acceptability of one's methods.

Notwithstanding this historical perspective, every scientific community operates day to day as if there were one and only one acceptable method of analysis. It is this fact that we must face. If you want to play an immediate role in the development of modern economics, you must learn how to use the currently accepted method. Paradoxically, even attempts to change the accepted method must proceed according to the currently accepted method.

Authoritarianism and the Hidden Agenda of Science

Apart from the obvious paradox, the problem of pulling oneself up by one's bootstraps that may trouble anyone who wishes to change the currently accepted method of analysis, there are other problems that should concern us. Although it is difficult for educated people to admit, the reliance on credentials and accepted methods as a means of discriminating significant from insignificant theories carries with it a more serious problem. It is the problem of inadvertently advocating authoritarianism.

The primary item on the hidden agenda (i.e., dealing with the Problem of Induction) is the view that if anyone wishes to be 'scientific', he or she must imitate the methods of physics or some other 'hard' science. It is as if physicists had a monopoly in clear thinking. Nevertheless, we must be careful to avoid overreacting. Economics and most of the natural sciences have many things in common. Logic, mathematics and statistics are the same regardless of where they are used. And many of the

apparent differences turn out, upon close examination, to be merely terminological, reflecting only differences in professional jargon. But there is no reason why physics methodology should carry any authority in economics analysis.

The view that there is one and only one acceptable method of analysis implies that a theory created according to the accepted method has some authority over other possible theories. Despite our years of education, which were supposedly directed at teaching us to think for ourselves, we are supposed to surrender our judgement to the authority of the accepted scientific method or the current scientific community. It is unlikely that we could convince anyone that there is no authority implied by anyone's theory being deemed scientific because we will be asked to specify the authority upon which we have based such a claim. The best we can hope for is that we become aware of the hidden agenda involved in any enterprise. We shall not try to solve the problem of authoritarianism here. Instead, we shall just try to call attention to its role in the hidden agenda peculiar to methodology discussions in economics.

Methodological Agreement

Although there is considerable personal recognition given to individuals in science (for example, Nobel prizes), most of the everyday business of doing economic analysis relies on the cooperation and combined efforts of many people. The publication of articles and books would not be possible without some common intellectual framework, paradigm, or research program. All introductory textbooks are written to introduce students to that which is common to all members of the given scientific community. But apart from giving textbook writers a job, a common agreement is necessary for the coordination of a large community's research efforts. Those familiar with the current research program will know which problems are on the agenda, and most important, which research methods are considered acceptable.

The need for agreement

The necessity of commonly agreed upon research principles is most evident when the scientific community faces problems needing urgent solutions. Many of the current research tools were developed during the urgencies of World War II. Of course, the development of the tools was facilitated by large government grants. But what the grants did was to focus the research and to force a minimum amount of agreement on principles. When there is an agreement over research principles and problems, it is possible for everyone to avoid endless arguments over which problems need solving and which tools should be used. Thus one expects research to be more productive when there is widespread agreement and very little disagreement. But such expectations can be misleading.

The dangers of forced agreement

Very often an argument in favor of the urgency of a problem may be only a disguised attempt to deflect a potential argument over basic principles. For obvious reasons, once one has spent many years of toil obtaining the necessary training in currently accepted research principles, one is not going to welcome a change to new and different techniques. This is very often the reason why methodology itself is not accorded priority on the research agenda, as it tends to focus criticism on currently accepted research principles.

One does not usually have to argue for the urgency of a problem when it is really urgent. Thus it is usually easy to spot such false arguments. Nevertheless, the dangers or costs of misrepresenting the urgency of a problem can be far-reaching. To the extent that any science progresses in Socratic learning terms, by improving its fundamental principles and theories, any diversion of research from fundamental theoretical problems in favor of short-term, immediate, practical problems may lead to extensive long-term costs.

Just as it is a mistake to think that there is one and only one scientific method for all problems and for all time, it is a mistake to think our understanding of the economy today will be adequate for everything in the future. It is thus in the scientific community's interest to allocate some research efforts or funds to the study of basic research methods.

The False Choice Problem

The primary source of disputes over criteria such as simplicity, generality, or falsifiability is the Conventionalist's choice problem itself. It is a false problem. That is to say, nothing much is accomplished by solving that problem. We realize that very many philosophers think it is an important problem, but we shall argue to the contrary. Specifically, we shall argue that when it comes to problems which require a choice between theories, those problems are usually the problems that involve Instrumentalism. But of course, we are going too fast. Let us take things one step at a time. Disputes over the choice of the best methodology (e.g., between Conventionalism, Pragmatism, or Instrumentalism) presuppose that there is one correct, all-purpose methodology. We will argue that this is wrong. The best methodology for today depends on the problems that concern us today. Different problems sometimes require different methods. This leads to a similar problem concerning choice criteria. There need not be an all-purpose criterion.

Is there an all-purpose criterion?

Our purpose here is to argue that there need not be one all-purpose methodology in the usual authoritarian sense, and that, instead, there are many different methodologies, each of which contains prescriptive or proscriptive criteria that are only appropriate for a specific set of problems. Every given methodology has its limitations and may not be appropriate for other problems.

Conventionalism is designed to deal with the shortcomings of our not having a direct solution to the Problem of Induction. Specifically, versions of Conventionalism can be used to provide a philosophical perspective when writing textbooks or when writing about the history of a given science. For example, Samuelson uses his form of Conventionalism to explain the history of Demand Theory. In his view, we can see how Demand Theory has changed over time, each change representing an improvement in generality. In his view the history of Demand Theory has culminated in the 'Generalized Law of Demand', which is a mathematical relationship between the slope of the demand curve and the nature of consumers' preferences [1953]. According to Samuelson's version of Conventionalism, then, the ultimate criterion for choosing among competitors is generality. Another follower of Conventionalism, Mark Blaug, in his history of economic thought utilizes a different criterion. For him progress is seen in terms of improvements in our ability to mathematize economic theories. Thus Samuelson's models are superior to, say, Marshall's because Samuelson's can be represented by mathematical functions, whereas Marshall's view is based on a rejection of mathematical models.

Judging by the current form of published articles [e.g., Lucas, 1980], many economists agree with both Samuelson and Blaug. The prescribed methodological objective of many writers is to increase the generality of economic analysis. Formal mathematics is recognized as the means of providing the most general form of any given theory. Surely there are some limitations to formal mathematical analysis? Open any leading economics journal and you will find rather complicated arguments concerning such questions as the mathematical stability of a given theory, the existence and uniqueness of its solution, its axiomatic basis, etc. It is all too easy to argue that little of the content of such journals has any direct relevance to practical questions of policy.

Few followers of Friedman's Instrumentalism would find anything useful in the leading analytical theory journals. Most of modern eco nomic theory is so general that it is virtually impossible to apply it to practical situations. For example, the ‘Generalized Law of Demand’ basically says anything is possible. The old-fashioned ‘Law of Demand’ said that only downward-sloping demand curves were possible. This is not a trivial matter for those economists interested in making policy judgements based on a calculation of consumer surplus. Such a calculation requires a downward-sloping demand curve.

Generalized economic models have so many variables that it would take forever to collect all the information just to apply them to simple cases. For example, where a first-degree (i.e., linear) demand function between a single good’s price (P) and the quantity demanded (Q) has only two parameters, its slope (b) and its intercept (a), as in the following equation:

$$Q = a + bP \qquad [1]$$

just raising its generality by saying it is a second-degree (i.e., quadratic) demand function between the two same variables adds another parameter (c):

$$Q = a + bP + cP^2 \qquad [2]$$

and increasing the degree increases the number of extra parameters that will have to be measured.

The linear model is very special and very simple. The non-linear model allows for the linear model as a special case (e.g., when $c = 0$) but it also allows for many other cases (i.e., when c is negative and when c is positive). In this sense the non-linear model is more general. But we can see why general models can easily get out of hand. We can allow for more and more types of cases but only by introducing more and more parameters.

These considerations show both sides. We can see why Instrumentalism puts a premium on simplicity rather than generality. And we can see why Conventionalism finds generality superior to simplicity. From the Conventionalist standpoint, increased generality allows for a larger filing cabinet; and the bigger the filing cabinet, the better the theory. For Instrumentalism, the benefits of increased generality may not always justify the extra costs.

Is there an all-purpose methodology?

These considerations also show us why Conventionalism and Instrumentalism can be at such odds whenever anyone thinks there is one and only one correct methodology. Except for very special occasions, both views cannot simultaneously be correct. Instrumentalism’s desire for simplicity is appropriate whenever we are faced with immediate, short-run, practical problems which preclude measuring a large number of parameters. On the other hand, short-run practical success may not be very durable because parameters have a tendency to change quite often. For longer-run problems, perhaps Conventionalism’s generality is more appropriate.

The Fundamental Choice Problem

Once one accepts that these two competing methodologies have their respective places, then one has reached the position where it seems that most of the methodological disputes in economics are rather empty on their own terms. If there is a dispute between adherents of Conventionalism (such as Samuelson, Solow, or Blaug) and adherents of Instrumentalism (such as Friedman and his followers), it is only about specifying what are the most important problems facing economists today.

Objectives come first

Before economists argue about what is the ‘best’ methodology (and note that the use of the term ‘best’ may have already predisposed the argument in favor of Conventionalism), they should reach some agreement about their objectives. If they do not, then their arguments will likely be at cross-purposes. But as we have just warned, one must be careful to avoid posing the choice problem so that only one method can win the debate.

Very often when economists think their methodology is the final word on the one true, all-purpose methodology they tend to search only for those problems that can be solved by their methods. Such an approach is not necessarily wrong, but from the perspective of the study of methodology it can be very misleading. When reading books or articles written by Conventionalist methodologists, one will find that the problems of ‘scientific’ interest are those problems for which one is supposed to choose the ‘best’ alternative theory (or model) from a list of competitors. Conversely, when reading Instrumentalist views of methodology, one will find that the truly scientific problems are those dealing with immediate practical problems and thus one should choose the most ‘useful’ method for dealing with those problems. Of course, the method that is most ‘useful’ is Instrumentalism itself. Thus one can see that in these cases, objectives do not come first. For these writers there is one fundamental choice problem in methodology: the arbitrary prior choice of one’s all-purpose methodology.

Problem-dependent methodology

Once one accepts our argument here that there is no universal, all-purpose methodology, then most discussions of methodology become uninteresting because they are too biased. The celebrated dispute between Friedman and Samuelson is a case in point. Without some way of independently determining what are the really interesting problems, there will never be a way to resolve the dispute.

Instead of an all-purpose methodology there are really many possible methodologies. Each one is appropriate for a limited list of problems. If at present practical problems are most interesting, then Instrumentalism is appropriate. If catalogue choice problems are the most pressing, then perhaps Conventionalism is the appropriate methodology. If learning for learning's sake is an important consideration, then perhaps Popper's methodology, which emphasizes problems, criticism and, above all, disagreement, is a more appropriate perspective.

The Role of Methodology

As a final note, let us point out that throughout this book, we have been stressing a significant role for both the Popper-Socrates theory of learning of Chapter 10 and the related problem-dependent methodology of this chapter in any neoclassical program for explaining individual decision-making. We would welcome critics who may argue that in stressing one view of methodology we are in effect violating our own caution to avoid seeking an all-purpose methodology. We would argue that we are not, for the following reasons. (1) We are *not* arguing that a problem-dependent methodology is the 'best' methodology, but rather, that it is the only available methodology which is consistent with a realistic short-run neoclassical theory – that is, with one in which individuals are assumed to be making decisions in real time. (2) Conversely, not much will be gained by considering our Popper-Hayek program of explanation if one does not wish to consider such avant-garde research topics as real-time dynamic neoclassical models, 'expectational errors' or disequilibrium models of macroeconomics.

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