Clarifying Popper

Bruce J. Caldwell

Journal of Economic Literature, Vol. 29, No. 1. (Mar., 1991), pp. 1-33.

Journal of Economic Literature
Vol. XXIX (March 1991), pp. 1–33

Clarifying Popper*

By BRUCE J. CALDWELL
University of North Carolina at Greensboro

Introduction

WHEN ECONOMISTS WRITE about the methodology of their subject, they often cite philosophical authorities to buttress their arguments. Among the most popular of these authorities is the philosopher of science Sir Karl Popper. There is some irony in this: Popper has often railed against arguments from authority! Be that as it may, the list of economists whose explicitly methodological writings reveal a Popperian influence includes G. C. Archibald, Jack Birner, Mark Blaug, Lawrence Boland, Wade Hands, Friedrich Hayek, T. W. Hutchison, Joop Klant, Kurt Klappholz, Spiro Latsis, Stanley Wong, and the present author. The list of economists whose general outlook has been influenced by Popper's work would of course be much longer.

If one sticks to the methodological writings, one finds a variety of interpretations of Popper. There are differences of opinion about the significance of his philosophy, which is understandable. But there are also disagreements about its content. The latter conflicts usually arise because different authors make reference to different parts of Popper's work. The goal of this paper is to clarify Popper's thought and to offer an assessment of its importance for economics. Three areas of the philosopher's work will be critically examined: his writings on falsificationism and demarcation, on situational analysis, and on critical rationalism. In each case, Popper's contribution will be reviewed and then related to the relevant literature within economics.

Summarizing Popper's contribution is no small task. He has been writing prolifically since the 1920s; a bibliography compiled in 1974 runs 86 pages. Attempts to reconstruct Popper's thought are further complicated by the presence of time lags between the writing and publication of many of his key works. His most famous book is *The Logic of Scientific Discovery*. First published in German in 1934, it was not translated into English until 1959. The translation includes copious footnotes which had been added in the intervening years. A lengthy *Postscript* to the *Logic* was written in the 1950s, prior to the publication of the English translation. Though the galley proofs of this manuscript circulated for

* This is a revised version of a paper first presented at the History of Economics Society meetings in Toronto in May 1988. Thoughtful comments were received from participants in seminars at Washington University in St. Louis, Duke University, and the Istituto di Bergamo, Italy. I am deeply grateful to the late Bill Bartley, Mark Blaug, Larry Boland, Bob Coats, Neil de Marchi, Wade Hands, Dan Hausman, Ian Jarvie, Uskali Mäki, Andrea Salanti, and three anonymous referees for their careful reading and criticisms of earlier drafts. I doubt any of them would agree with everything in the new version. In any case they are absolved of responsibility for the errors that remain.
This paper is dedicated to the memory of Bill Bartley, who died in February 1990 at the age of 53.

decades among Popper's disciples, the three volumes of the *Postscript* were not published until the early 1980s, and the final version contains new material. Sorting out "what Popper thought when" is a daunting job.

Given the difficulty of the task at hand, it is appropriate to impose some limitations. There will be no attempt to provide a chronological depiction of the development of Popper's thought. Those interested in such an account might profitably consult Popper's (1974) "Intellectual Autobiography." In addition, this paper focuses rather narrowly on Popper's methodology of science. I have in general resisted the urge to explore the epistemological or metaphysical foundations of Popper's ideas. When these areas are mentioned my treatment of them will be cursory, and interested readers will be pointed toward additional sources in the literature.

The paper can be read on at least three levels. It is first and foremost an introduction to those writings of Popper that are of most importance to economists. But it is also a critical assessment of the existing methodological literature on Popper within economics. Finally, I advocate a particular interpretation of Popper's work, one that clears up certain ambiguities that can be found both in Popper and within the methodological literature.

Popper on Demarcation and Falsificationism[1]

Statement of Popper's Position

1. Science presents us with the clearest case of the systematic growth of knowledge. In order to examine the growth of scientific knowledge, we must be able to distinguish science from nonscience, we need a *demarcation criterion.*

2. Popper's criterion of demarcation is *falsifiability.* "[A] statement (a theory, a conjecture) has the status of belonging to the empirical sciences if and only if it is falsifiable" (Popper 1983, xix).

3. Why falsifiability as the demarcation criterion? To answer this, let us review Popper's attack on the logical positivist position.

Writing in the late 1920s and early 1930s, the logical positivists of the Vienna Circle initially chose *verifiability* as their *criterion of cognitive significance.* A synthetic statement was considered cognitively significant (or cognitively meaningful) if it were capable, at least in principle, of complete verification by observational evidence.

Popper shared the logical positivist concern with demarcation, but he did not accept the meaningful-meaningless dichotomy. His demarcation criterion separates the scientific from the nonscientific, or metaphysical. (Thus for Popper some statements can be nonscientific but still meaningful.)

Popper's critique of verifiability involved consideration of both the *logical status of statements* and the *logic of scientific explanation.* Popper noted that a certain type of statement, affirmative existential statements, could be verified but not falsified. An example is the sentence, "Unicorns exist." The sentence can be verified by finding a unicorn. But it cannot be falsified, even if it is false: The failure to find a unicorn does not establish that none exists. If verifiability is used as the demarcation criterion, the statement "unicorns exist" would have to be considered a part of science.

Another problem arises with affirmative statements of universal form, such as "All swans are white." One can falsify

[1] Popper does not like to use the word "falsificationism" in referring to his views on the methodology of science (1983, p. xxxi). The term is frequently encountered in the critical literature, however, and its usage is standard among economic methodologists.

the statement by discovering, for example, a black swan. But the statement is not verifiable: Even if all swans observed up until now have been white, one can never know whether an as yet unobserved black swan exists somewhere. The most important instances of statements of universal form in science are sentences expressing universal laws. This is where the logic of scientific explanation fits in. Both Popper and his opponents agreed that all legitimate scientific explanations follow one of the covering law models of explanation. According to the deductive-nomological covering law model, an explanation is considered scientific if an explanandum (a sentence describing a condition to be explained) can be deduced from an explanans (which includes sentences describing initial conditions and at least one universal law). But because they are universal in form, statements of universal laws are unverifiable. If the logical positivist criterion of verifiability rather than Popper's falsifiability is used, explanations employing universal laws would have to be judged as nonscientific.

Popper had established that falsifiability was preferable to verifiability as a demarcation criterion. The debate did not end there, however. The next generation of positivists, the logical empiricists, also rejected verifiability, but following Rudolf Carnap (1936–37) they turned to confirmability as an alternative demarcation criterion. This led to, in no particular order, the paradoxes of confirmation, problems with defining a law of science, the riddles of induction, and the Popper-Carnap debates on the (in)adequacy of an inductive logic (Frederick Suppe 1977; Caldwell 1982). Though these topics will not be treated here, they provide the philosophical foundations for alternative interpretations of the theory of probability, which have consequences for certain recent debates in econometrics (Colin Howson and Peter Urbach 1989).

4. *Falsifiability is a logical affair.* A statement is falsifiable "if and only if there exists at least one potential falsifier—at least one possible basic statement that conflicts with it logically" (Popper 1983, p. xx).

5. Though potential falsifiability is a logical affair, there are several reasons why, as a practical matter, the actual falsification of a scientific theory is always difficult. First, according to the Duhem-Quine thesis, in any test situation a number of auxiliary hypotheses are tested along with the hypothesis on which attention is focused. If a test result is negative, one of the auxiliary hypotheses may be false rather than the hypothesis under test. Next, in a typical test situation scientists accept the *empirical basis* (a number of statements describing "the facts") as given. But the empirical basis is itself conventionally accepted and thus subject to revision. Finally, because the empirical basis is *theory impregnated* (the "facts" one sees depend upon the theories one holds), its content will vary as theories change (Popper 1983, pp. xxii–xxiv).

6. Let us adopt the following terminology in referring to the testing of scientific theories:

(a) Though potentially falsifiable, a theory may be currently untestable.

(b) If a theory is potentially falsifiable, currently testable, and has been tested, then there are two possibilities:

(i) If a test result is positive, the theory is *corroborated.* (We avoid using the term *verified* because of the connotations it was given by the positivists.) Scientists often accept a theory that is repeatedly corroborated as provisionally true. Corroboration does not mean *proven true;* Popper's *fallibilism* prohibits us from claiming that we have discovered the truth. (Fallibilism states that we can never demonstrate that we have discovered the truth, even when we have: All knowledge is conjectural.) Nor should even a consistently corroborated theory be viewed as *highly probable* or even as *more probable*. This was the point of Popper's critique of inductive logic. It is a radical implication: Even perfect corroboration carries no evidential weight.

(ii) If a test result is negative, the theory is *refuted or falsified*. Just as corroboration does not prove a theory true, refutation does not prove it false. Nor need we reject a refuted theory: The decision to reject or accept is always a separate matter. (Thomas Kuhn 1970 has noted, for example, that one might not wish to reject a refuted theory if there were no alternative theory to replace it. Popper 1983, p. xxiv, cites Kuhn's example approvingly.)

7. Falsifications are always more interesting than corroborations, for they force scientists to reexamine the theory and test situation to see what went wrong. Such critical reexamination offers the best hope that false theories will be eliminated from science. Unfortunately, given the difficulty of interpreting test results, it is always possible for scientists to protect a favored theory by blaming a refutation on something else. Popper's response is to lay down certain methodological prescriptions against what he terms *conventionalist stratagems* or *immunizing stratagems*, whose sole purpose is to protect theories from being refuted. Popper also uses the phrase *ad hoc theory adjustment* to refer to any change in a theory designed solely to save it from a refutation.

8. Some of Popper's rules for avoiding such immunizing maneuvers have been culled from his work by Mark Blaug (1980a, p. 19). Scientific theories are bold conjectures. The best theories are those that forbid much, for they can be the most severely tested. The best tests are intersubjective and repeatable. Refutations should be taken seriously. Even in situations where clean tests are difficult, scientists should specify in advance what sorts of results would lead them to abandon their theories. Auxiliary hypotheses should be added as little as possible, and only when their addition increases the degree of a theory's falsifiability. Scientists should adopt a critical attitude in which they attempt to seek refutations rather than confirmations, even of their own theories. Thus Popper's rules are aimed at both the *actions* and the *attitudes* of scientists.

9. Popper's position is not itself falsifiable, nor is it intended to be. It is a metaphysical doctrine, which he also characterizes as "partly even a normative proposal" concerning how to investigate the world (1983, p. xxv).

10. The fact that Popper's position is not falsifiable implies that it cannot be refuted by the history of science. Popper's methodology is *prescriptive;* it cannot be refuted for being *descriptively inadequate*. Indeed, in cases in which falsificationism is not followed by scientists, the correct Popperian response is to urge scientists to *try harder* rather than to contemplate abandoning falsificationism.

11. Because of his fallibilism, Popper cannot guarantee that by following his method scientists will be led to the discovery of truth. He once hoped to demonstrate that, by following the tenets of falsificationism, scientists would at least be led to produce theories of greater *verisimilitude* (that is, theories with greater truth content and less falsity content). But technical problems in defining verisimilitude caused him later to abandon even this goal. Popper's philosophy is best viewed as a bold conjecture that may help us, if we are lucky, to eliminate error.

Proponents of Falsificationism in Economics

Popper's most important contribution within the dismal science is doubtless the influence he has had on those economists who, though not specialists in methodology or the philosophy of science, discovered some insight in a casual reading of his books which they then applied in their own work. What practicing economists might have taken away from Popper is as varied as their possible readings of him. Some might have learned that it is good to try to test one's theories as severely as possible; others to be suspicious of theories that repeatedly manage to survive by having many small ad hoc adjustments made in their initial conditions. Some may have learned to welcome falsifications, which are more interesting than confirmations because they lead one to reexamine one's hypotheses. Others might have gained a new, somewhat schizophrenic mindset, in which one tries to keep one's mind open to new ideas while simultaneously submitting those ideas to intense critical scrutiny. Finally, some may not have read Popper at all, but simply been brought up short when, perhaps in a seminar or at a conference session, they first encountered the ultimate Popperian question: What evidence would cause you to give up your hypothesis? If one fancies oneself an empirical scientist but has never thought of the question before, it can have a devastating effect.

Whatever influence Popper may have had on the practice of economics, there is no easy way to measure it. It might be possible to gather anecdotal evidence, as Arjo Klamer (1984) has done in his oral history "conversations." A less direct but probably more revealing approach might be to examine how economists talk about their empirical results, especially anomalous ones, in their articles. In any case, the emphasis here will be on Popper's influence on and relevance for the field of economic methodology, which is far easier to document and to assess.

Among economic methodologists, the major proponents of falsificationism in economics are T. W. Hutchison, Johannes Klant, and Mark Blaug: In *The Significance and Basic Postulates of Economic Theory* (1960), originally published in 1938, Hutchison was the first to introduce testability as a criterion for distinguishing between science and "pseudo-science." In their critical summaries of the methodological literature, both Blaug (1980a) and Klant (1984) employ Popperian criteria to assess the cogency of various methodological positions.

For example, all three are critical of Ludwig von Mises' infallibilist claim that the axioms of economics (he called it "the science of human action," or praxeology) though untestable are known a priori to be true. From a Popperian perspective, Mises' claim to certain knowledge, his infallibilism, is dogmatic. Additionally, if the axioms of praxeology are truly unfalsifiable, then the Misesian system does not qualify as a science (Blaug 1980a, pp. 91–93; Hutchison 1956, pp. 482–83; 1981, ch. 7; 1988, p. 176; Klant 1984, pp. 71–82). Classical Marxism is also accused of infallibilism, but rather than being viewed as unfalsifiable it is characterized as having been falsified (Blaug 1980b; Hutchison 1981, p. 18). "Equilibrium" theories are found wanting because the assumption of perfect foresight empties them of empirical content (Hutchison 1960, ch. 4; 1977, ch. 4). American institutionalists, whose theories are "all too easy to verify and virtually impossible to falsify," are also criticized (Blaug 1980a, p. 127). Much of mainstream economics (of the Marshallian partial equilibrium type) is viewed as scientific, but there is still too much use made of immunizing stratagems. "Innocuous falsificationism" is practiced, which is "like playing tennis with the net down" (Blaug 1980a, pp. 128, 256). Though acknowledging the difficulty of obtaining unambiguous falsifications in a social science like economics, we are told that the proper response is to try harder (Blaug 1980a, ch. 15).

Friedrich Hayek might also be considered a Popperian, though his case is more controversial. The relationship between the ideas of Hayek and Popper is exceedingly complex. Hutchison (1981, forthcoming) argues that Popper's influence on Hayek dates back to the 1930s. Caldwell (forthcoming a, b) denies that there is any good evidence of Popperian influence on Hayek until the 1950s, after which Hayek's admiring citations of Popper's work become numerous. But even then, Hayek mixes his praise for Popper with an insistence on the complexity of economic phenomena, a situation that makes it all but impossible to obtain clear-cut falsifications in the dismal science. Thus Hayek, like Blaug, finds it difficult to falsify economic theories. But rather than to enjoin economists to try harder, Hayek concludes that predictions of patterns are usually all that is possible in economics, and that if progress is to take place "we must also push forward into fields where, as we advance, the degree of falsifiability necessarily decreases. This is the price we have to pay for an advance into the field of complex phenomena" (Hayek 1967, p. 29). Hayek's acceptance of potential falsifiability as a demarcation criterion and of the deductive-nomological model of scientific explanation establishes his Popperian credentials. Furthermore, Hayek states that he has long been a Popperian (Walter Weimer and David Palermo 1982, p. 323). But his qualifications concerning the usefulness of falsificationism as a practical methodological position for economics make any simple labeling scheme inappropriate.

The Critique of Falsificationism in Economics

It is only fair to alert the reader that I have been a frequent and persistent critic of falsificationism for nearly ten years.

The critique of falsificationism in my book *Beyond Positivism* (1982) was part of a larger project. I used the term *positivism* loosely to refer to a number of doctrines within twentieth century philosophy, including logical positivism, logical empiricism, operationalism, and falsificationism. My reading of developments within contemporary philosophy of science indicated that these doctrines had been severely and effectively criticized, if not repudiated. While part of the book was devoted to enumerating the arguments against the "positivist" doctrines, another task was to identify the implications of these developments for economic methodology.

It seemed to me that if falsificationism had been effectively criticized within philosophy, then the falsificationist objections to praxeology, institutionalism, Marxism, and other programs lost much of their force. A methodological double standard also sometimes appeared to exist. Heterodox research programs were usually dismissed for being either unscientific or falsified. But mainstream economists, who also often failed to follow the prescriptions of falsificationism, were usually let off the hook with the reprimand to "try harder." Furthermore, I felt that falsificationism was too *easy* on the opponents of mainstream economics because it allowed their theories to escape scrutiny. Once it had been shown that such theories were either falsified or not scientific, there was nothing more to be said. But many of the proponents of these alternatives rejected Popper's demarcation criterion (Mises 1966; Martin Hollis and Edward J. Nell 1975). As such, these heterodox economists could dismiss the arguments of falsificationists as easily as falsificationist critics had dismissed their theories. Instead of increasing the amount of scrutiny theories received, the adoption of falsificationism in economics seemed to result in a diminution of critical dialogue across paradigms.

I pursued these and similar lines of attack in my book and in a number of articles (1981, 1984, 1985, 1986). I now believe that at least one of the arguments I made, if not wrong, is seriously incomplete. My mistake was a common one; let me quickly summarize it here so that others may avoid it.

My error was to claim that falsificationism is an inappropriate methodology for economics *because* most economic theories cannot be conclusively falsified. To buttress the claim, I noted numerous obstacles to getting clean tests of theories in economics: initial conditions that are large in number, subject to change, and some of which are not independently observable; the absence of general laws; the use of models in the place of theories; and so on (Caldwell 1984). I discovered later that similar reasoning produced the same conclusion when a group of economists at the LSE in the early 1960s attempted to apply Popper's ideas to the practice of economics; Neil de Marchi (1988) recounts this fascinating episode. One of the LSE participants was G. C. Archibald; his statement of the problem and of his response to it in his paper, "Refutation or Comparison?" (1966), is both simple and eloquent.

> My starting point is Popper's Demarcation Rule. . . . that a statement is to be regarded as scientific if it is refutable, and his associated injunction: try to refute. My problem is due to the circumstance that much activity in economics is devoted to empirical work with hypotheses that do not satisfy Popper's falsifiability criterion: should I judge that the hypotheses, and the associated empirical work, fall short of some ideal scientific standard, or should I judge that the methodology calls for what is impossible, and that it should therefore be altered? My own judgment is that many of the irrefutable hypotheses in economics are important, that they are incurably irrefutable for good and fundamental reasons, and that the activity of comparing them with observation is useful (too practically useful to be acceptably called metaphysics), so that it is the demarcation rule that should give way. (1966, p. 279)

As Blaug (1984) and Daniel Hausman (1985) soon pointed out, Popper anticipated this objection. Because of the Duhem-Quine thesis and related problems, *every* science encounters difficulty in coming up with clean refutations. The Popperian response is to acknowledge the difficulty, to insist that tests be taken seriously anyway, and most important of all, *to insist that, whenever a refutation is encountered, any proposed theory modification be free of the taint of immunizing stratagems*. This is why Popper's discussion of ad hoc theory changes and immunizing stratagems is so crucial to his methodology: Because we cannot get clean tests of our hypotheses, we must at least be sure that they are not further protected by adjustments designed solely to save them from falsification.

Thus it is not an effective argument against falsificationism to simply point out that it is difficult to get clean tests of hypotheses, that decisive refutations are rare. That problem always exists. The argument must be against Popper's insistence that *nevertheless* refutations should be taken seriously, and that when one occurs, certain theory adjustments are forbidden. If Popper's advice concerning immunizing stratagems is viewed as the target, then at least three objections can be raised. To keep them straight, let us call these the Philosopher's Objection, the Historian's Objection, and the Economic Methodologist's Objection.

1. The Philosopher's Objection. This

argument begins by noting that Popper never makes it clear *why*, if test results are always so ambiguous, scientists should adopt his prescription to avoid ad hoc theory adjustments. "If we are lucky," such a procedure may lead us to eliminate false theories. But the phrase "if we are lucky" is not just verbiage; we can be unlucky, too. And if we are unlucky, following Popper's stringent prohibitions could easily lead us to reject true theories. After all, it is really very simple to produce a refutation of a theory, especially in sciences like economics which study (to use Hayek's phrase) complex phenomena. If Popper's prescriptions were always followed, it would not be long before many of the sciences found their theories falsified.

Another set of problems has its origin in Popper's vehement anti-inductivism. For Popper, even repeated confirmations carry no evidential weight. As a result, one is not entitled to make assertions like "this hypothesis is more (or less) certain than its rival," or "this theory is well supported by the available evidence," or "this belief is warranted, or justified, or well founded." The empircal basis can be used only to refute theories, not to support them. Matters would be somewhat improved had Popper's attempt to develop a theory of verisimilitude been successful: At least then we could assert that the more highly corroborated theory was the preferred one. But in its absence, all one can say is that all knowledge is conjectural.

The Philosopher's Objection states that the Popperian program is inadequate as a methodology and philosophy of science. On the methodological level, scientists who follow Popper's prescriptions will sometimes be led to make bad decisions. On the epistemological level, Popper's philosophical position rules out any discussion of how evidence supports theories, which is one of the most important questions facing the philosophy of science. Finally, Popper has offered no good reasons for following his sometimes counterintuitive proposals; he (consistently!) provides no justification for his nonjustificationist philosophy (Hausman 1988; Hands forthcoming a).

Additional philosophical arguments have been raised against Popper's position (Robert Nola 1987; Anthony O'Hear 1980). Popperians have generally chosen one of two lines of defense: either to deny that the justification of Popper's methodology is necessary, or to attempt to develop some kind of an alternative to the theory of verisimilitude (William W. Bartley 1984; John W. N. Watkins 1984). Because most discussions of these issues take place at the epistemological rather than the methodological level, we will not pursue them further here.

2. The Historian's Objection. Another way to attack falsificationism is to ask its proponents to provide examples of its successful application within a science. After all, science is typically distinguished from other forms of inquiry both by its methods, and by the progress in knowledge that those methods have produced. If falsificationism is truly the appropriate methodology for all sciences to follow, examples of its successful use should be readily identifiable.

Popper responds directly if a little ambiguously to the Historian's Objection in the Introduction to *Realism and the Aim of Science* (1983). He points out first that, because it is a prescriptive doctrine, it is a mistake to try to "test" his methodology against the history of science. Nonetheless, he goes on to list 20 examples of refutations from the history of (natural) science, and concludes that ". . . *I doubt whether there exists any theory of science which can throw so much light on the history of science as the theory of refutation followed by revolutionary and yet conservative reconstruction*"

(1983, p. xxxi, emphasis in the original). Thus Popper feels that it is possible, at least in natural science, to provide examples to answer the Historian's Objection. But he denies that he is under any obligation to do so.

Because none of Popper's examples involves the social sciences, the Historian's Objection is obviously available to opponents of falsificationism in economics. But to be complete, the argument would also need to rebut Popper's claim that he is under no obligation to provide examples. One way to do this is to argue that any methodology, to be judged acceptable, must be able to make sense of at least some instances of scientific progress within a discipline. Another way of putting this is to say that prescriptive methodologies must also be, in some sense, descriptively adequate.

Unfortunately, this is not an easy point to establish. It turns out that "testing" a methodology against the history of a discipline is, if anything, more difficult than testing a theory against an empirical basis. Let us take economics as our example. At any given point in time, economists will differ over which historical episodes provide examples of scientific progress. (Was the Keynesian revolution progressive, or simply a mistake?) Matters do not improve with the passage of time, because estimates of the importance of particular theory changes continue to evolve. (Was the marginal revolution an essential transformation leading to the development of modern economics, or is Philip Mirowski, 1989, correct to characterize it as a mimetic mistake brought on by economists' "physics envy"?) These problems are equivalent to saying that historians often have difficulty agreeing on what constitutes the "empirical basis" against which a methodology is to be tested. But a fundamental problem would remain even if economists could achieve a stable consensus on their reading of the history of their discipline. Simply put, there are no brute, atomic "historical facts" against which to test our methodologies. History is theory impregnated, just like any empirical basis is: What we include within the category of "history" depends on prior theories of what is deserving of attention, and *that* is influenced by a discipline's prior methodological commitments. These problems are vexing ones, not just for economists, but for philosophers and historians of science in general (John Losee 1987).

Now despite these problems, advocates of falsificationism in economics have generally tried to respond to the Historian's Objection; they have been inclined to assert that falsificationism *could be and has been practiced* in the dismal science. This is perhaps not surprising when we remember that two of the principal proponents of falsificationism, Hutchison and Blaug, are historians of thought. For intellectual historians, the ability of a methodology to make sense of the past history of their discipline is understandably an important and desirable characteristic.

When it comes to specific examples, however, neither Hutchison nor Blaug has been able to pinpoint paradigmatic episodes of falsificationist practice. Hutchison's examples (the refutations of Malthusian population theory and of certain unqualified versions of Keynesian and monetarist macroeconomics) involve instances in which, usually after fairly long periods of time, it became evident that a theory's predictions did not come to pass (Hutchison 1988, p. 178). These examples do not accord well with the falsificationist image of a theory being subjected to a decisive refuting test. And because modified versions of all three theories still persist, the examples could equally easily be used to argue that ad hoc theory adjustment is alive and well in economics. In developing his examples, Blaug has tended to move away from Popper and into the camp of the erstwhile Popperian, Imre Lakatos. The general movement toward Lakatos within economics occurred in the mid-1970s, and came in part as a reaction against the growing influence of the work of Thomas Kuhn among historians of economic thought. It is worthwhile briefly to recount the episode.

An Aside: Kuhn and Lakatos

In the late 1960s and early 1970s, Thomas Kuhn's *The Structure of Scientific Revolutions* swept into the history of thought community in economics with a roar, as it had in so many other disciplines. Its first effect was to challenge the way the history of science was written. It was wrong, Kuhn asserted, to portray the history of a science as consisting of the gradual but progressive accumulation of true knowledge. For Kuhn, all sciences develop fitfully. There are, of course, periods of normal science during which scientists follow well-established procedures and aim at predictable results. But inevitably this activity leads to the discovery of anomalies. Eventually the piling up of anomalies brings on a period of revolutionary science, when old methods are questioned and new areas of investigation emerge. Often this results in a paradigm switch, the transformation of the science. The proper way to chronicle the development of a science, then, is to recognize this pattern of paradigm shifts, of alternating periods of normal and revolutionary science.

In constructing his general theory of how sciences develop, Kuhn also criticized the prescriptivist models favored by philosophers of science. It was not simply that such models were of little use for reconstructing the history of science. The problem was a deeper one: The prescriptions concerning legitimate scientific explanations and behavior found in the models of philosophers often directly contradicted what had actually happened in many "successful" sciences. For Kuhn's purposes as an historian of science, these prescriptivist models were misleading and should be abandoned. Though his major opponents were the logical empiricists, Kuhn's criticisms obviously challenged all prescriptivist approaches, including Popper's. In a sense, what Kuhn was calling for in the writing of the history of science was a movement away from the heavy emphasis on prescription favored by philosophers and a movement toward the ostensibly descriptive tools used by sociologists of science. Because most historians view their task as explanatory rather than prescriptive, it was not surprising that Kuhn's argument soon gained popularity among historians of economic thought.

Enter Imre Lakatos, a student of Popper's, who argued that such a choice was not necessary: his Methodology of Scientific Research Programs (MSRP) purportedly provided a conception of science that was both prescriptively robust and descriptively adequate (Imre Lakatos 1970). For Lakatos, scientific disciplines are comprised of one or more research programs, which are series of theories evolving through time. In describing these programs Lakatos employs the metaphors of "hard cores" and "protective belts." The hard core of a program consists of its fundamental assumptions. The assumptions are usually irrefutable, but are not subject to question by those who work within the program. All testing takes place in the protective belt, where the empirical implications of the program are teased out, confront the data and each other, and are gradually modified and refined. This process of testing and modification of the protective belt takes place over a long period of time, and typically involves a series of "problemshifts."

Lakatos injects prescriptive content into his MSRP by providing criteria for evaluating problemshifts as being either progressive or degenerating. A program is judged to be progressive if each new theory, each problemshift, generates some novel, unanticipated facts, some of which are corroborated. Programs that fail to meet these requirements are considered degenerating. There is no "instant rationality" when it comes to judging research programs as progressive or degenerating. The assessment can only be made after a long period of time, because programs can start out progressive, stagnate for a while, then become progressive again. As such, Lakatos offers no decision rule stating when it is rational to abandon a degenerating program.

Lakatos' vision of science differs from Popper's on a number of key points. Though Popper admits that science contains metaphysical elements, he does not give them the prominent place accorded by Lakatos with his concept of the hard core. Lakatos also deemphasizes the importance of refutation. When testing takes place within the protective belt, scientists are usually trying to support rather than to refute their theories. Refutations are also less decisive for Lakatos; what matters is whether a series of theories is capable of predicting unexpected facts. Finally, Lakatos differs by claiming that his MSRP provides an accurate portrayal of scientific activity. It can be tested against the history of science, or be used to "rationally reconstruct" the best gambits of scientific activity.

Like Kuhn before him, Lakatos' ideas were introduced into economics primarily by historians of thought. The occasion of their introduction was a conference sponsored by the Latsis Foundation, held at Nafplion, Greece, in 1974. Two conference volumes followed, one dealing with the Lakatosian framework as applied in the physical sciences, the other applying it within economics (Howson 1976; Latsis 1976). In many ways the volume on economics initiated the modern period in economic methodology. For the 20 preceding years, most discussions of methodology had been assessments of Milton Friedman's famous 1953 essay. The directions taken in the Latsis volume were radically different. There was little discussion of Friedman; the philosophy of science was directly consulted, but it went far beyond the usual obligatory nod to logical empiricist orthodoxy; and there was much closer attention paid to the development of specific research programs in economics. One of the most significant essays in the collection is Blaug's (1976) "Kuhn versus Lakatos *or* Paradigms versus Research Programmes in the History of Economics." In a section entitled "From Popper to Kuhn to Lakatos," Blaug is quite clear about why Lakatos' approach should be viewed as superior to those of Kuhn and Popper.

> As I read him, Lakatos is as much appalled by Kuhn's lapses into relativism as he is by Popper's ahistorical if not antihistorical standpoint. The result is a compromise between the 'aggressive methodology' of Popper and the 'defensive methodology' of Kuhn, which however stays within the Popperian camp. Lakatos is 'softer' on science than Popper but a great deal 'harder' than Kuhn and he is more inclined to criticise bad science with the aid of good methodology than to temper methodological speculations by an appeal to scientific practice. (1976, p. 155)

As an historian of thought, Blaug was bound to find Popper's "ahistorical if not antihistorical standpoint" profoundly unsatisfying. Yet as a methodologist, the prospect of a retreat into Kuhnian relativism was equally repugnant. Lakatos provided a golden mean: a prescriptivist methodology that was tough enough to challenge the unscientific or degenerate program but sufficiently flexible to permit the "rational reconstruction" of the best of science. To be sure, Lakatos' (1971) highly obscure description of how one might test one's methodology against a history that invariably must be written from the viewpoint of a methodology was troubling, as was his suggestion that in any rational reconstruction, the actual course of history would be provided in the footnotes. Nonetheless, it seemed clear that Lakatos was worth a try.

In the years since Nafplion, Blaug has become the chief exponent of the Lakatosian program in economic methodology. In a recent paper he uses a somewhat modified Lakatosian framework to reconstruct rationally the Keynesian revolution (1991). He also helped to direct a second Latsis Foundation symposium, this one in Capri in 1989. The goal of the conference was to see how much of mainstream and heterodox economics could be given a Lakatosian reconstruction. In his introduction to the conference volume, Blaug's co-editor Neil de Marchi (de Marchi forthcoming) argues that though economists think that their practice is consistent with the description of science found in the MSRP, it is less clear that the MSRP sheds much light on their actual practice. De Marchi concludes that economists would do better to focus on the entirety of Lakatos' works, and especially Lakatos' treatise on mathematical reasoning entitled *Proofs and Refutations* (1976), rather than on the MSRP.

De Marchi is not the first to raise questions about the relevance of the MSRP for economics. Hands has been a frequent critic, and the exchanges he has had with Blaug provide considerable insight into the strengths and weaknesses of the Lakatosian framework (Hands 1985a, 1990, forthcoming a; Blaug 1990, 1991).

Though of independent interest, debates over the importance or relevance of the MSRP need not concern us here. It is clear that economists are generally attracted to Lakatos' program. His proposal that scientific programs have hard cores which though untestable are taken as given, his view that testing often aims at confirming rather than falsifying theories, and his claim that methodologies should be descriptively adequate all make sense to economists when they think about their discipline. But as has been argued persuasively by Hands (forthcoming a) and Jeremy Shearmur (forthcoming), these ideas of Lakatos which economists find so appealing *are precisely those points where he differs most from Popper*. As such, even were economists to judge the Lakatosian program to be a viable one, the assessment would be due in large part to the *distance* between the frameworks of Popper and Lakatos. In an evaluation of Popper we may safely ignore the Lakatosian development.

3. The Economic Methodologist's Objection. Because stating the final objection to falsificationism will lead us directly into the next section, a brief review of the argument thus far may be useful. Popper's ideas have been actively discussed and debated within the methodological literature. Both proponents and critics recognize that it is difficult to get clean tests of hypotheses in economics. Advocates urge economists to take Popper's injunctions against immunizing stratagems seriously, and critics argue that it would be a mistake to do so. The Philosopher's Objection states that following falsificationism will sometimes lead scientists to make bad decisions, and that Popper has provided no rationale for his program. The Popperian response comes at the level of epistemology, where the debate continues. The Historian's Objection demands that methodology be descriptive as well as prescriptive, and that examples of beneficial falsificationist practice be provided. Popper's counterclaim is that prescriptive methodologies need not be descriptive. Up un-

til now, Popperian advocates within economics have not explicitly availed themselves of this out. But neither have they come up with convincing examples of falsificationist successes. And at least in the case of Blaug, Popper's vision gradually gave way to the more congenial framework of Lakatos for describing the history of the discipline.

Though many will find the Philosopher's and Historian's Objections to be sufficient grounds for rejecting falsificationism as an adequate methodology for economics, it is just as clear that a convinced falsificationist could maintain that his position has not been defeated. But rather than judge the debate a draw, we turn now to a third argument against falsificationism.

The Economic Methodologist's Objection states that within economics there are often good reasons for ignoring Popper's prohibitions concerning immunizing stratagems. Even stronger, I will claim in the next section that *the actual methodology followed in much of economics may best be described as one in which a particular immunizing stratagem is elevated, and for good reasons, to the status of an inviolable methodological principle*. Finally, even the most convinced of Popperians will be hard pressed to deny that I have indeed provided "good reasons," because the source of those reasons is none other than Popper himself, in his writings on *the logic of the situation!*

Popper on Situational Logic

Given that Popper's writings on situational logic are less well known among economists than are his falsificationist doctrines, and given that they are spread out in a variety of places, it is appropriate briefly to identify where they can be found.

Three early sources of Popper's work on situational logic are *The Poverty of Historicism* (1957), *The Open Society and Its Enemies* (1963), and "Prediction and Prophecy in the Social Sciences" (1965a), all of which were originally published in the 1940s. Much of this early work is not very good. The *Poverty*, which Popper dubbed one of his "stodgiest pieces of writing" (1974, p. 90), is poorly organized. Popper explains why in his autobiography: After the first ten sections were written, his organizational plan broke down when, "without any plan and against all plans," section 10 was ultimately expanded to become *The Open Society* (1974, p. 90). It may also have been that frequent rewriting (perhaps to please Hayek, the person responsible for the *Poverty* being published) of what was once a tightly organized paper finally caused its continuity to disintegrate. Access to the early version of the *Poverty* (it was first delivered in 1935) would allow a test of this conjecture.

Two more recent and complete statements of the method of situational analysis are "The Rationality Principle" (1985) and "The Logic of the Social Sciences" (1976). The former is from a radio address and was originally published in French in 1967. It was translated into English so that it could be included in a collection of Popper's works (David Miller 1985), a collection on which Popper apparently bestowed his blessings. The English version of "The Rationality Principle" thus represents Popper's most recent formulation of situational analysis.

Statement of Popper's Position

1. It is not the task of the social sciences to prophesy the future course of human history, as the historicist believes. Rather, "the *main task of the theoretical social sciences . . . is to trace the unintended social repercussions of intentional human actions*" (1965a, p. 342, emphasis in the original; cf. 1963, ch. 14). The ap-

propriate method for doing this is called the *method of situational logic* or *situational analysis*.

2. This method is *individualistic*, but it is not *psychologistic*. Psychologism is the doctrine that "all laws of social life must be ultimately reducible to the psychological laws of 'human nature', . . ." (1963, p. 89). Instead of making reference to psychological states, "we *replace* concrete psychological experiences (or desires, hopes, tendencies) by abstract and typical situational elements, such as 'aims' and 'knowledge' " (1985, p. 359, emphasis in the original).

3. Situational analysis is a generalization of the method of economic analysis (1963, p. 97; cf. Popper's autobiography, 1974, p. 93). It may also be the *sole* method of explanation in the social sciences. Popper appears to embrace that position when he claims "that only in this way can we explain and understand what happens in society: social events" (1985, p. 358).

4. There are similarities between explanations in the natural and social sciences.[2] Recall that in a scientific explanation an explanandum is deduced from an explanans. In many explanations in the natural sciences, the explanans consists of sentences describing typical initial conditions and one or more universal laws. In social science explanations, states of knowledge or aims would be typical initial conditions. In place of the universal law is the *rationality principle*. This principle states that "the various persons or agents involved act *adequately, or appropriately;* that is to say, in accordance with the situation" (1985, p. 359, emphasis in the original).

5. When viewed as a universal law, the rationality principle is *false:* Agents do not always act appropriately. Popper's example is "a flustered driver, desperately trying to park his car when there is no parking space to be found . . ." (1985, p. 361).

6. Even though the rationality principle is an empirical conjecture that turns out to be false, it "does not play the role of an empirical explanatory theory, of a testable hypothesis" (1985, p. 360). The assumption that agents act appropriately is never rejected. It is considered a kind of "zero principle," a starting point for the analysis. When a theory in which it is employed is falsified, Popper argues "that it is sound methodological policy to decide not to make the rationality principle accountable but the rest of the theory; that is, the model" (1985, p. 362).

7. Popper justifies this approach as follows:

> The main argument in favor of this policy is that our model is far more interesting and informative, and far better testable, then the principle of the adequacy of our actions. We do not learn much in learning that this is not strictly true: we know this already. Moreover, in spite of being false, it is as a rule sufficiently near to the truth. . . . Another point is this: the attempt to replace the rationality principle by another one seems to lead to complete arbitrariness in our model building. And we must not forget that we can test a theory only as a whole, and that the test consists in finding the better of two competing theories which may have much in common; and most of them have the rationality principle in common. (1985, p. 362)

[2] Sometimes Popper expresses this point more strongly: "In this section I am going to propose a doctrine of the unity of method; that is to say, the view that all theoretical or generalizing sciences make use of the same method, whether they are natural sciences or social sciences" (1957, p. 130). A few pages later, though, Popper acknowledges that the use within the social sciences of "what may be called the method of logical or rational reconstruction, or perhaps the 'zero method' " accounts for "perhaps *the most important difference*" from the methods of the natural sciences (1957, p. 141, emphasis in the original).

If the "unity of science" thesis is to make any sense, I think it is best to interpret it as stating that all scientific explanations share the same structure. This also seems to be the position taken by Popper (1957, sect. 28), and accordingly it is the interpretation presented in the text. Hands (forthcoming b) argues that adherence to a covering-law model of scientific explanation has been *the* constant in Popper's position.

Two Problems with Popper's Exposition of Situational Analysis

Before going any further, two weaknesses in Popper's presentation require attention. The first is a certain vagueness in Popper's explication of how a situational analysis should be undertaken. The second is Popper's apparent belief that situational logic is the only proper method to follow in the social sciences.

1. Popper is not always clear about how one would do a situational analysis. Fortunately, a better statement of how to apply the doctrine has been provided in two papers by the Popperian philosopher Noretta Koertge (1975, 1979). Koertge first provides the following informal model of Popper's account of situational explanations (1975, p. 440).

Description of situation:	Agent A is in situation C.
Analysis of situation:	In situations like C, the appropriate thing to do is X.
Rationality principle:	Agents always act appropriately to their situation.
Explanandum:	Therefore, A did X.

In instances in which the explanandum does not obtain, Popper's methodological advice is for social scientists to revise their model of the agent's situation rather than to reject the rationality principle. Koertge's next contribution is to clarify how the process of revision takes place. She shows which parts of the model are most likely to be modified, noting that revisions can actually increase the empirical content of situational explanations if they build on "supplementary theories of error, decision making and belief formation" (1975, p. 447).

2. It is not clear whether Popper actually adheres to the claim that situational analysis is the only appropriate method for the social sciences. After all, the statement appears only once in print, and Popper does not emphasize it. Does the claim make any sense?

Situational logic is a powerful method which has been applied fruitfully to a host of social science problems, from explicating the notion of social class, to reconstructing the problem-situation facing Paul Samuelson when he developed the theory of revealed preference, to elucidating the causes of the perennial conflict between adults and teenagers (Ian C. Jarvie 1972; Stanley Wong 1978). More speculatively, one might imagine the rationality principle as playing the role of a central organizing metaphor for a variety of social sciences. Jon Elster's (1988) recent outline of the scope and nature of rational choice explanations demonstrates the wide variety of interpretations that may be given to the notion of rational choice. Subject areas within the social sciences might be delimited according to their use of particular variants of the principle. Some would postulate a specific variant of rationality, as most theories in economics do. Others might attempt to challenge or replace specific aspects of the principle, as for example the proponents of prospect theory have recommended (Daniel Kahneman and Amos Tversky 1979). Still others might examine and try to explain behavior that refused to yield to rational choice explanations, as Elster (1989) does in his analysis of the limitations of the framework for explaining the emergence of social norms. Finally, the scope of situational analyses is not necessarily restricted to the social sciences. If one leaves off the rationality principle, a study of the logic of a situation can also provide an explanation of certain natural science phenomena, especially those dealing with evolutionary processes (Günter Wächtershäuser 1987).

But to claim that situational analysis is the *only* legitimate method in the social sciences seems wrong for a number of reasons. First, as Elster (1989) shows, not all social science phenomena are explicable within a rational choice framework. Next, there is in fact a wide array of alternatives to choose from within the social sciences, from functionalist to behaviorist to hermeneutic explanatory frameworks (Christopher Lloyd 1986; Alexander Rosenberg 1988). Third, at least some philosophers question whether "folk-psychological" explanations (explanations in terms of desires and beliefs) should be considered causal. These critics raise doubts as to whether explanations that give the reasons for actions, as belief-desire explanations seem to, can also give the causes for actions. If they do not, then social science explanations that employ situational logic are not causal scientific explanations at all (Rosenberg 1988). The final reason to refrain from claiming that situational analysis is the only legitimate method for the social sciences is, paradoxically, a Popperian one. I will argue presently that, if one takes Popper's falsificationism seriously, a conflict arises between that doctrine and situational analysis. Depending on how one defines the rationality principle, situational analysis results in social science explanations that are either metaphysical, or ad hoc, or inconsistent with explanations in the natural sciences. Given this weakness, it seems reasonable to remain open to the possibility that someday we may discover a better means of explaining social phenomena.

The argument is of importance within economics, because many mainstream economists appear to believe that *only* theories that employ the rationality principle are acceptable. Lawrence Boland (1982) gets at this point when he talks darkly about the "hidden agenda" which is followed by both mainstream and "avant-garde" groups within economics. Though he describes the agenda in terms different from those employed here (his emphasis is on methodological individualism), one item on the agenda is that all legitimate economic theories include a rationality postulate. Hands provides a clear statement of the problem in his paper, "Ad Hocness in Economics and the Popperian Tradition" (1988). Hands argues that the derisive term *ad hoc* is used differently by different groups. When philosophers or economic methodologists in the Popperian tradition employ the phrase, they are describing a theory adjustment that is designed to protect a theory from a refutation. In contrast, many economists label a theory *ad hoc* if it fails to employ some form of the maximization hypothesis. To the extent that the maximization hypotheses economists invoke (e.g., consumers maximize utility, firms maximize expected profits) are simply specific variants of the more general rationality principle, the directive is equivalent to the claim that situational analysis is the only permissible method for economics.

The tendency for economists to believe that the only legitimate theories are those that use the maximization hypothesis has led some methodologists to be suspicious of situational analysis. This appears to be the case for Blaug, who notes that what Popper advises

> comes very close to the attitude of all orthodox economists, to whom any economic problem is only a challenge to reinterpret the rationality principle, say, by reinterpreting the meaning of the constraints facing individuals, so as to produce a solution in terms of constrained maximizing behavior. [Situational analysis] is very permissive of economic practice as interpreted in the orthodox manner. . . . (1985, p. 287)

If these methodologists are right in their characterization of the way mainstream economists view the use of the rationality postulate, then the position taken in this paper is at odds with the mainstream position. Though I regard situational logic as an important tool of social analysis, there is no claim that it is the *only* method of analysis. Furthermore, theories that fail to employ a rationality principle are not judged deficient solely on those grounds.

To conclude: two improvements on Popper's statement of situational logic are suggested. The first recommendation is to replace Popper's statement of how the method of situational logic works with the cleaner version provided by Noretta Koertge. The second concerns Popper's (possible) prescriptive claim that situational analysis is the only appropriate method in the social sciences. The claim is amended to read that situational analysis is a powerful and fruitful method for social and other sciences, but that it need not be considered the only viable method.

Does Economics Follow the Method of Situational Analysis?

The questions of whether and, if so, to what extent economics follows the method of situational analysis turn out to be difficult to answer. To address them competently would require that we have on hand a widely agreed on description of what constitutes normal scientific practice in economics. Such a description is not now available.

It is perhaps enough to note that at least some economic theories can be reconstructed as following the method of situational analysis. Hands argues that situational analysis is the method of standard textbook microeconomic theory:

> Economists specify the situation of the agent (individual or firm) usually in terms of the preferences and/or technology and the relevant constraints (prices, income, factor constraints, etc.). Included in the description of the situation is some "motivating" consideration (maximizing utility, maximizing profit, etc.). The second step is to deduce the appropriate behavior of the agent given the situation specified (buy more, buy less, increase production, decrease production, etc.). This second step is what constitutes most of economic *theory*, the formal deduction (usually mathematical) of the "appropriate" behavior in a particular "situation." Finally, if the economist's task is to explain an observed action, the rationality principle is activated to connect the analysis of the situation with the action to be explained. (Hands forthcoming, a)

One can also reasonably argue that certain modifications of standard theory are consistent with the precepts of situational analysis. In initial formulations of the theory, it is assumed that all agents have perfect information, that transactions are costless, that agents have an unlimited computational ability, and so on. By altering each of these assumptions, one obtains any of a number of extensions of or alternatives to the standard account: decision making under risk, exchange under conditions of positive transactions costs, analysis of problems associated with asymmetric information, the satisficing model, and so on.

Finally, even some economic analyses that appear to deviate substantially from standard microeconomic theory may nonetheless be characterized as following the method of situational logic. Jack Birner (1990) argues that Carl Menger's methodological position anticipated Koertge's version of situational analysis. More provocatively, Elster (1985) claims that Karl Marx employed a variant of rational choice explanation in his work.

Though each of these examples could be challenged, taken together they suggest that at least some areas within economics can be accurately described as following the method of situational logic.

Three Interpretations of the Rationality Principle

Despite the similarities between the method of situational analysis and the methodological approach followed by many economists, relatively few economists have cited Popper's work on situational logic. The principal exception is Spiro Latsis, whom we met before as the editor of the 1976 volume on Lakatos. In an early and seminal paper, Latsis (1972) combined Popper, Lakatos, and economics in a straightforward way: The (Popperian) rationality principle constitutes a major part of the (Lakatosian) hard core of the neoclassical (he called it *situational determinist*) research program. Later Latsis (1983) criticized Popper for vagueness in his formulation of the rationality principle in terms similar to objections soon to be raised. His interpretation of the meaning of the principle differs, however, as does his proposed solution to the dilemma, which draws on Popper's ontology concerning the relationship between mental states and behavior.

The next economist to focus attention on Popper's situational logic was Hands (1985b). Hands found it strange that economists should cite Popper's writings on falsificationism while ignoring his work on the rationality principle. This odd asymmetry led him to posit the existence of two Poppers within the economic methodology literature: the relatively well-known Popper$_n$ (*n* for "natural science") and the relatively obscure Popper$_s$ (*s* for "social science" or "situational logic"). Hands raises a number of "interesting questions" concerning the two Poppers.

> The possibility of Popper having a nonfalsificationist view of economic method raises a number of interesting questions. Exactly what is the relationship between economics and situational analysis? Will a detailed study of situational analysis provide additional insights into the methodological questions of economics which would be unavailable through falsificationist spectacles? What questions does such a potential dualism raise regarding methodological monism, the view ostensibly supported by Popper, that the method of social science and the method of natural science should not differ in significant ways? And finally, what does Popper really advise about practicing the science of economics? (Hands 1985b, p. 84)

The most important question raised by Hands is whether the two methodological positions advocated by Popper, falsificationism and situational logic, are mutually inconsistent. An analysis of the question requires that we be very clear about what interpretation is given to the rationality principle. Unfortunately, if we examine Popper's recent writings on the subject, and especially his paper "The Rationality Principle" (1985), we find him to be almost perversely obscure on this crucial point.

The rationality principle states that agents act appropriately to their situation. It would seem that there are at least three ways to interpret this statement. Each has a different implication, both for the logical status of the statement and for the conflict between falsificationism and situational analysis.

1. One way to interpret it is to say that all agents act appropriately to the situation *as they see it*. And indeed, in the last paragraph of his article Popper embraces such an interpretation: The rationality principle "assumes no more than the adequacy of our actions to our problems as we see them" (1985, p. 365). As Popper acknowledges, this is a minimal notion of rationality. It assumes only that an agent has certain beliefs and goals, and that he acts in an attempt to reach those goals. The agent's beliefs may be incomplete, or inconsistent, or erroneous, and his goals may be unreachable. As long as he acts in accordance with his beliefs, however, such an agent would still be viewed as rational. This type of "subjective rationality" does little more than to posit purposeful, goal-directed behavior. It has echoes in the Austrian economist Ludwig von Mises' definition of rationality as purposeful behavior, which he dubbed "the fundamental axiom of human action" (Mises 1966).

It is no easy matter to discover the logical status of the rationality principle when it is interpreted in this way. It has the form of a universal statement, so it is not verifiable. But it also appears to be unfalsifiable, because it is difficult to imagine a basic statement that would falsify it. Popper seems to take this position when he states that the rationality principle "does not play the role of an empirical explanatory theory, of a testable hypothesis" (1985, p. 360), or later on the same page when he says it "is not empirically refutable." On the other hand, Popper goes to some length in the article to deny that the rationality principle is known a priori to be true. Though he names no names, Popper here is directly opposing Mises' claim that the fundamental axiom is both a priori true *and* empirically meaningful.

Perhaps the best way to characterize the postulate is to borrow a phrase from John W. N. Watkins (1958) and call it a bit of "confirmable and influential metaphysics." It is *confirmable* by introspection, and it is confirmed by the apparent goal-directed behavior of other agents. And because the rationality principle on this interpretation is unfalsifiable, theories that employ it must be viewed as *metaphysical:* They fall on the nonscience side of the demarcation criterion. Given this interpretation, to the extent that economics or other disciplines employed situational analyses, they would not be considered sciences.

2. A second way of interpreting the rationality principle is to view it as the equivalent of a universal law within the social sciences. Popper again provides evidence that this is the way he views the principle. He begins his article by comparing explanations in the natural and social sciences, noting the "importance" of their similarities (1985, p. 358). He focuses on the use of universal laws that "animate" models in the natural sciences. A rhetorical question follows: "Now if situational analysis presents us with a model, the question arises: What corresponds here to Newton's universal laws of motion which, as we have said, 'animate' the model of the solar system?" (1985, pp. 358–59). His answer is "that we need, in order to 'animate' it, no more than the assumption that the various persons or agents act *adequately, or appropriately* . . ."(1985, p. 359). Thus it seems clear that the rationality principle plays the same role in social science explanations that universal laws play in natural science explanations.

But a problem arises. If we interpret the rationality principle as a statement of a universal law within the social sciences, it cannot be unfalsifiable (as we found it to be under the first interpretation). Simply put, if the law is *unfalsifiable,* it is wrong to call a theory in which it is used a *science,* because sciences are required to employ falsifiable laws. If situational analysis explanations are scientific rather than metaphysical, the rationality principle must be falsifiable.

Now if the rationality principle is falsifiable, has it been tested, and if so, what have been the results? Popper's answer here seems plain. He uses the example of a flustered driver, desperately seeking a nonexistent parking place, to buttress his statement that "the rationality principle seems to me to be clearly false—even in its weakest zero formulation which may be put like this: 'Agents always act in a manner appropriate to the situation in which they find themselves' " (1985, pp. 360–61). Later he states that there are "good reasons to believe that the rationality principle, even in my minimum formulation, is actually false, though a good approximation to the truth" (1985, p. 362).

This brings us to a dilemma. If one accepts the tenets of falsificationism, the social sciences are legitimate sciences, because they employ a falsifiable (and false) law. But if Popper's advice to always retain the rationality principle is followed, then the social sciences must also be viewed as ad hoc. The dilemma can be reconstructed as follows.

(a) Popper maintains that the structure of scientific explanation in both the natural and the social sciences follows the same pattern. In both cases, an explanandum is deduced from an explanans, which contains sentences describing initial conditions and at least one universal law.

(b) Popper asserts that ". . . the explanans ought to be true although it will not, in general, be known to be true; in any case, it must not be known to be false . . ." (1983, p. 132).

(c) When theory revision is called for, the prime directive of Popper the falsificationist is to avoid the use of immunizing stratagems, which are ad hoc adjustments of a theory undertaken to protect it from refutation.

(d) The universal law used in the social sciences, the rationality principle, is false. (This violates condition b.)

(e) Yet Popper the situational analyst insists that the universal law of the social sciences, though false, should never be rejected. Instead, the theory in which it is used should be adjusted until the agent's actions can be shown to follow from the logic of the situation. (This violates condition c.)

Situational logic violates the prohibition against using false statements within the explanans, then compounds the problem by failing to revise the theory according to the canons of proper scientific procedure. From a falsificationist's point of view, *situational logic employs a false law, then justifies the procedure by elevating an immunizing stratagem to the status of an immutable methodological principle.*

3. But there is a third alternative. For the rationality principle may be interpreted as being *neither* an unfalsifiable universal statement *nor* a falsifiable (and falsified) statement of a universal law. For it need not be considered a *statement* at all, but rather a *methodological principle* that is retained because it has shown to be particularly fruitful in the past. And yet again, there is some support for such a reading in Popper. He calls it, after all, a "zero principle," and refers to it as "a consequence of the methodological postulate that we should pack or cram our whole theoretical effort, our whole explanatory theory, into an analysis of the *situation:* into the *model*" (1985, p. 359, emphasis in the original). But two pages later Popper contrasts the rationality principle interpreted as "a methodological principle" with a second interpretation of it as "an empirical conjecture," and claims that "this second case is precisely the one that corresponds to my own view of the status of the rationality principle" (1985, p. 361).

There are a number of reasons why Popper might reject this third alternative. Perhaps most important, it flies in the face of his anti-inductivism: To retain a principle because it has shown itself to be useful in the past is to provide an inductivist argument on its behalf.[3] Accepting the third interpretation would also require that the unity of science thesis be dropped. Finally, it requires the further admission that the best method currently available in the social sciences violates the prohibition against the use of immunizing stratagems, which plays such a key role in Popper's falsificationist philosophy.

Are Situational Analysis and Falsificationism Incompatible?

Which of the three interpretations should we choose? Popper's own writings

[3] I thank Andrea Salanti for this observation.

are of absolutely no use here; his article obfuscates all of the important questions. There are advantages to vagueness, of course, especially under the circumstances: It permits Popper to avoid confronting the tension between falsificationism and situational analysis.[4] But the tension cannot be sidestepped. If the former doctrine is accepted, then situational explanations (supposedly the best form of explanation available in the social sciences) are either metaphysical, or ad hoc, or completely different in kind from those in the natural sciences. What are we to make of this dilemma? Is Popper caught in a hopeless inconsistency?

There are a number of ways one might respond to the dilemma. The easiest is simply to accept one of the positions and to reject the other. Most economists would presumably accept situational analysis and reject falsificationism. The former doctrine at least accords with some economic practice, whereas the latter insists that that same practice is unscientific or ad hoc. On the other hand, those who believe that the social sciences are not true sciences might be more likely to choose falsificationism over situational analysis.

Another alternative is first to portray Popper's writings on situational analysis as misguided or naive, then to refashion them into a more acceptable position. This is the route chosen by Blaug:

> Those like myself who claim that modern economists largely subscribe to Popperian falsificationism have a little difficulty here that they have not squarely faced up to.
>
> My own resolution of the clash between P_s and P_n is to throw doubt on Popper's views on social science. The fact is that Popper knew little about social science and even less about economics. (1985, p. 287)[5]

Blaug prefers a Lakatosian solution to the dilemma, one that is reminiscent of Latsis' (1972) gambit: The rationality principle is part of the hard core of the neoclassical research program, and our assessment of the progress of the program (and with it, of the prospects for situational analysis) is an ongoing endeavor.

Still another option is to reject Popper's thought altogether as hopelessly confused.

My own solution to the dilemma again follows a route laid out by Popper, this time in his writings on critical rationalism. The solution involves positing an alternative and broader conception of ac-

[4] Popper's vagueness on the status of the rationality assumption may produce an uncomfortable sense of déjà vu among economists. His evasiveness is mirrored in the multifarious formulations economists have provided of the rationality assumption, and accounts as well for the wide variety of (often ingenious, though not always mutually consistent) defenses of the assumption in the literature. These issues are more thoroughly discussed in Caldwell (1983).

[5] There is some justification for Blaug's complaint. Popper only infrequently provided examples of economic reasoning, and when he did, they usually were somewhat naive (e.g., Popper 1957, p. 62; 1965a, p. 343).

On the other hand, Blaug's comments elicited the following response from Popperian readers like W. W. Bartley and I. C. Jarvie in their correspondence with me.

Jarvie: "I find the speculation about how much economics Popper knew distasteful because elitist. Popper is one of the quickest and fastest studies I have ever come across. If he was in the company of economists of the calibre of Menger or Hayek for more than a few hours in his lifetime, I can guarantee that he got the hang of economics, at least in its basics. I also think that he read lots of economics, although I have no doubt that he's read virtually nothing in the field since the early 1950's." (Letter of 5 July 1988)

Bartley: ". . . Popper was steeped in economics: he read Böhm-Bawerk, Menger, Mises, and Wieser in his father's library in the late 'teens and early 'twenties. He also knew Keynes' work in probability theory, and the analysis of economics of the peace; he was a friend of Menger, Jr., and a leading member of his seminar. His favourite uncle, Walter Schiff was an undersecretary in the Ministry of Finance, and also a Professor of Economics and Statistics in the University of Vienna, and had been a member of the Menger and Böhm-Bawerk seminars. . . . To be sure, Popper has never much *liked* economics: he thought it degenerate compared to physics and biology. . . ." (letter of 13 June 1988) ceptable scientific practice, one that would allow the use of *both* falsificationism and situational logic, each within the contexts in which it is most appropriate. I call this my own solution simply because Popper has never acknowledged that a tension exists between falsificationism and situational logic, and has never portrayed critical rationalism as providing a resolution of the conflict.

Popper on Critical Rationalism

Statement of Popper's Position

1. The Preface (1983) to the three-volume *Postscript to the Logic of Scientific Discovery* carries the provocative title, "On the Nonexistence of Scientific Method." There is less to the title than may first appear: What Popper denies is that there is a foolproof method for the discovery of scientific theories or that there is a method for verifying the truth of scientific hypotheses. This is not a new position but simply a restatement of fallibilism.

2. Later in the Preface Popper describes what really constitutes "the so-called method of science." His emphasis is on criticism.

> The only things which partners in an argument must share are the wish to know, and the readiness to learn from the other fellow, by severely criticizing his views—in the strongest possible version that can be given to his views—and hearing what he has to say in reply.
>
> I believe that *the so-called method of science consists in this kind of criticism.* Scientific theories are distinguished from myths merely in being criticizable, and in being open to modifications in the light of criticism. (1983, p. 7, emphasis in the original)

3. The central role of criticism is also emphasized in the "Introduction" to the *Postscript:* "It so happens that the real linchpin of my thought is fallibilism and the critical approach . . ." (1983, p. xxxv).

4. The links between fallibilism and the critical approach are clarified in chapter 2 of the *Postscript:*

(a) Popper agrees with William W. Bartley's (1982a, 1984) formulation of his contribution, namely, that Popper gives a negative answer to the problem of *justification,* and replaces it with the new problem of *criticism.*

(b) The problem of justification can be posed as follows: Is it possible to justify our beliefs rationally? Popper's answer is that it is *not* possible: "my solution of the central problem of justification—as it has always been understood—is as *unambiguously negative* as that of any irrationalist or sceptic" (1983, p. 19, emphasis in the original). This is a direct result of his fallibilism: We cannot know when we have found the truth, even when we have. All knowledge is conjectural. Thus Popper shares with "irrationalists and sceptics" the belief that a *criterion of truth* is not possible (1983, p. xix).

(c) But Popper is not a skeptic: He believes in a *theory of truth* (the correspondence theory) and further, that the *search for truth* is important as a regulative principle for scientists.

> My position is this. I assert that the search for truth—or for a true theory which can solve our problem—is all-important: *all rational criticism is criticism of the claim of a theory to be true, and to be able to solve the problem which it was designed to solve.* (1983, p. 24, emphasis in the original)
>
> . . . .
>
> . . . in replacing the problem of justification by the problem of criticism we need give up neither the classical theory of truth as correspondence with the facts nor the acceptance of truth as one of our standards of criticism. (Other standards are relevance to our problems, and explanatory power.)
>
> Thus although I hold that more often than not we fail to find the truth, and do not know even when we have found it, I retain the classical idea of absolute or objective truth as a *regulative idea;* that is to say, *as a standard of which we may fall short.* (1983, p. 26, emphasis in the original)

(d) Though stopping short of a full endorsement of Bartley's formulation, Popper praises it as "most illuminating" and acknowledges that it clarifies much of his thought (1983, p. 27).

5. Many conjectures about the physical universe as well as many philosophical theories are irrefutable, hence metaphysical. Popper's emphasis on criticism has implications for the assessment of metaphysical arguments. In the penultimate section of *Quantum Theory and the Schism in Physics* (1982), which constitutes the final volume of the *Postscript,* Popper acknowledges that many of the conjectures about the structure of the physical universe advanced in the three volumes are straightforwardly metaphysical. These conjectures are all elements of his metaphysical research program; they are designed to guide research; and he thinks that they may well be true. What are we to make of the fact that they are irrefutable? Popper poses and answers the question.

> But if my dream is metaphysical, what is the use of it? Is there anything in it beyond, perhaps, an emotional satisfaction? Is it not utterly different from a scientific hypothesis—one in which we are mainly interested *because of its implicit claim to be considered, tentatively, as true?*
>
> I no longer think, as I once did, that there is a difference between science and metaphysics regarding this most important point. I look upon a metaphysical theory as similar to a scientific one. It is vaguer, no doubt, and inferior in many respects; and its irrefutability, or lack of testability, is its greatest vice. But, *as long as a metaphysical theory can be rationally criticized,* I should be inclined to take seriously its implicit claim to be considered, tentatively, as true. (1982, p. 199, emphasis in the original)

6. On the following page Popper provides a general description of the process of criticism. Note that refutability is only one, albeit a very important, criterion of critical inquiry.

> Any critical discussion of it will consist, in the main, in considering how well it solves its problems; how much better it does so than various competing theories; whether it does not create greater difficulties than those which it sets out to dispell; whether the solution is simple; how fruitful it is in suggesting new problems and new solutions; and whether we cannot, perhaps, refute it by empirical tests.
>
> This last method of discussing a theory is not, of course, applicable if the theory is metaphysical. But the other methods may well be applicable. This is why rational or critical discussion of some metaphysical theories is possible. (1982, p. 200; cf. 1965b, p. 199)

7. Popper deals harshly with theories that are *not* criticizable; that do not try to solve problems; that do not provoke rational criticism. He calls such theories "valueless" and "worthless," and states that scientists would be "well justified in dismissing them" (1982, pp. 199, 211; 1983, pp. 189–93).

8. In addition to criticizable *theories,* Popper endorses a critical *attitude:* "What distinguishes the attitude of rationality is simply openness to criticism" (1983, p. 27). Popper opposes any attempts to shield theories from criticism, as well as any attitude that does not give prominence to the critical scrutiny of ideas.

9. In his 1976 paper on the rationality principle, Popper acknowledges that the principle is false. He notes that it is often approximately true, which he considers to be a point in its favor. But the major argument in its favor is that it is *criticizable.* As he puts it: "Above all, however, situational analysis is rational, empirically criticizable, and capable of improvement" (1976, p. 103).

10. Evolutionary epistemology provides the epistemological foundations for critical rationalism. This doctrine emphasizes the similarities between the growth of animal (including human) knowledge and the evolution of species. Bold conjectures are analogous to blind variations (mutations) in nature; the process of criti-

cism is analogous to the process of natural selection. Evolutionary epistemology provides an empirical basis for epistemology (in processes found in nature) as well as an argument for realism (the survival of both ideas and organisms depends on their fit within their environment, and the assumption of an existing environment is consistent with realism). The goal of the evolutionary epistemologist is to create an "ecology of rationality" in which the optimal amount of critical discourse is able to flourish (Bartley 1984, appendix 2; Radnitzky and Bartley 1987).

The Reception of Critical Rationalism in Economics

There have been relatively few comments from economists on Popper's critical rationalism. James Wible (1982) uses the distinction between justificationist and nonjustificationist metatheories of science to classify a number of contributions to the methodological literature in economics. Hands (1985b, p. 96) mentions that "Popper's few suggestions about the dynamics of metaphysical research programs" may have some relevance for economics, but does not pursue the point. And very recently Shearmur (forthcoming) examines some of Popper's writings on metaphysics in the course of contrasting Popper and Lakatos' approach to methodology and historiography.

There are two important exceptions. The first is a remarkable but little read paper by an economist (Kurt Klappholz) and a Popperian philosopher (J. Agassi) entitled "Methodological Prescriptions in Economics" (1959).[6] Though nominally a review of two long-forgotten books on methodology by Sidney Schoeffler and Andreas Papandreou, the authors also discuss the views of Hutchison, Friedman, and Lionel Robbins. Two passages from the article, one from the introduction and one from the conclusion, demonstrate the authors' adherence to critical rationalism.

> Our view . . . is that there is only one generally applicable methodological rule, and that is the exhortation to be critical and always ready to subject one's hypotheses to critical scrutiny. (p. 60)
>
> . . . .
>
> Above all, we contend, that it is important to guard against the illusion that there can exist in any science methodological rules the mere adoption of which will hasten its progress, although it is true that certain methodological dogmas, such as the dogma that only theories pertaining to measurement are significant, or the dogma of inductivism, may certainly retard the progress of science. All one can do is to argue critically about scientific problems. (p. 74)

The other exception is Lawrence Boland, who studied under Agassi. While Boland's positive contributions to methodology are uniquely his own, his critical work is clearly within the critical rationalist tradition. For many years Boland has attacked the view that falsificationism provides a set of criteria for choosing the best theory. Popper's true message, which Boland argues is anti-justificationist and anti-inductivist, has been obscured by devotees of a "Conventionalist Pseudo-Popper" (Boland 1982, p. 172). The Popper Boland prefers is a follower of Socrates, who teaches by asking critical and revealing questions. Boland endorses methodological pluralism, in which "instead of an all-purpose methodology there are really many possible methodologies. Each one is appropriate for a limited list of problems" (Boland 1982, p. 196). Given these views, it is no wonder that Boland has been critical

[6] It is difficult to trace the origins of views, even when those views are one's own. I first read the Klappholz and Agassi article about 15 years ago when I was doing my dissertation. When I reread it recently I realized that this book review had a major influence on my subsequent methodological thinking.

of the standard interpretations of Popper that exist in the literature.

Critical Rationalism as a Solution to the Conflict between Popper$_n$ and Popper$_s$

It seems to me that Popper's writings on critical rationalism provide a way out of the dilemma posed by the conflict between falsificationism and situational logic. The dilemma is caused by the emphasis on empirical testing within falsificationism. Popper the falsificationist is most concerned with the *differences* among theories. He uses falsifiability as a criterion to demarcate between the metaphysical and the scientific. He further differentiates between empirically corroborated theories (those that have repeatedly survived severe attempts at falsification) and theories that survive only because of ad hoc adjustments. The problem, of course, is that if Popper's falsificationist criteria are consistently applied, many scientific theories end up looking pretty bad. In particular, the decision never to reject the rationality hypothesis by situational analysts (a group that would seem to include economists) would lead us to judge their theories to be, at a minimum, hopelessly ad hoc.

Popper the critical rationalist is most concerned with the *similarities* among theories. All theories are attempts to solve problems, and should be criticized according to how well they solve them. The goal of analysis is to subject theories to an optimal amount of criticism. But most important of all, *the level of criticism will depend on the problem to be solved and the nature of the material under investigation.* Empirical criteria are the strongest and whenever possible they should be used. Furthermore, it is generally the case that immunizing stratagems should be avoided. But at least in the special case of situational analyses, one is able to *criticize more severely and obtain more fruitful criticisms* if one blames the model rather than the rationality principle whenever a falsification occurs. Similar reasoning directs us to inquire about the criticizability of even irrefutable metaphysical theories. Under falsificationism, the goal was demarcation. Under critical rationalism, the goal is to keep the critical process going, to build an ecology of critical inquiry, an environment in which the optimal amount of criticism is able to flourish. The process ends only when a theory is totally uncriticizable, or when its proponents adopt a noncritical attitude.

Critical rationalism is a problem-solving approach which itself appears to resolve a problem within Popper's philosophy of science, the tension between situational analysis and falsificationism. Critical rationalism states that sometimes it is appropriate to evaluate a theory using the strict empirical criteria of falsificationism. But at other times, especially within the social sciences, one is better able to criticize a theory by applying the canons of situational logic. And there are still other circumstances, particularly when metaphysical theories are considered, when other routes to criticism are preferable. Which methods of criticism are most appropriate cannot be specified in advance: That will depend on the subject matter and the problem to be solved. But one can say that within the ecology of rationality envisioned by the evolutionary epistemologist, the goal is to subject all theories to the optimal amount of criticism.

Criticisms of Critical Rationalism

There are two general objections to applying critical rationalism within economics.

The first is that critical rationalism focuses too much attention on the question of theory appraisal. As a result, it is not very helpful for the resolution of other problems facing economic methodology.

For example, critical rationalism provides few categories for analyzing the structure of economic theories, a task that is currently being undertaken (albeit in very different ways) by members of the "structuralist program" and by the scientific realist Uskali Mäki (Hands 1985c; Mäki 1989, 1990). Nor does it provide any guidance to those who wish to initiate a radical critique and restructuring of economic theory (Mirowski 1986, 1989). More generally, because it focuses on appraisal, critical rationalism is most useful when the discussion concerns the critique of *existing* and *fairly well established* research programs. It is least appropriate when new, alternative approaches are being tried out. For such programs, the emphasis on criticism must be tempered by an encouragement of novelty.

It would not be difficult to modify critical rationalism to deal with this criticism. A sort of infant industry protectionist scheme for new theories could easily be appended, one reminiscent of Lakatos' insistence that criticism should not be allowed to kill new theories too quickly.

The second objection is that critical rationalism does not have enough content. As Hands recently put it:

> The real problem for critical rationalism is not that one can say very much against it, but rather than one cannot say very much with it. Critical rationalism is a view which seems to be palatable by virtue of its blandness, the epistemological analog of the ethical mandate to "live the good life." (forthcoming a)

According to this objection, it is not enough to say that theories must be criticizable and that scientists must exhibit a critical attitude. At a minimum, criteria must be provided so that one can tell when such conditions are being met.

For example, the invocation to hold a critical attitude certainly seems reasonable. But do we really want to use it to rule out a theory? After all, many good theories are defended dogmatically. Furthermore, it is often hard to judge who is being dogmatic in a debate, besides one's opponents. How does one identify what constitutes a genuine critical attitude, versus, say, a curmudgeonly one?

When it comes to criticizability, similar problems appear. Aren't all theories criticizable? After all, even an uncriticizable theory can always be criticized, on the grounds that it is uncriticizable! And just how is criticizability to be identified, anyway?

Perhaps most important, are there any criteria by which the *effectiveness* of a criticism might be assessed? How does one decide what constitutes an "optimal" amount of criticism, or when a criticism has been successful? Clearly not all criticism should carry the same weight. Otherwise, a proof of logical inconsistency would have to be considered as on equal footing with the following "criticism" provided by one of the referees:

> I do not like thee Doctor Fell
> The reason why I cannot tell
> But this I know, and I know it well
> I do not like thee Dotor Fell.

A critical rationalist might try to sidestep this argument by stating that it misconstrues his position. For example, though Popper frequently emphasizes the importance of holding a critical attitude, he also states that "what we call 'scientific objectivity' is not a product of the individual scientist's impartiality, but a product of the social or public character of the scientific method; and the individual scientist's impartiality is, so far as it exists, not the source but rather the result of this socially or institutionally organized objectivity of science" (1963, p. 220). A critical rationalist might also note that his views do have *some* content: It would allow one to reject, for example, the Misesian methodological position as an infallibilist, justificationist one. Finally, in at least one formulation of critical rationalism (Bartley's Comprehensively Critical Rationalism), the fact that a program cannot be criticized does not count as a criticism of it, but rather as a reason not to consider it further (Radnitzky and Bartley 1987, part II).

But given Popper's epistemological views, the consistent critical rationalist must ultimately oppose the argument. Critical rationalism assumes that one cannot know now what the knowledge of the future will look like, and that there is no infallible method for discovering the truth. The best one can do is continually to be critical, to keep an open mind, and to let the nature of the problems one faces dictate the methods of criticism one employs. To attempt to specify criteria of assessment any further starts one down the slippery slope to justificationism.

As a practical matter, I think that the following response to those who believe that economic methodology must specify concrete appraisal criteria is a suitably modest one. Throughout much of the twentieth century, philosophers of science have been preoccupied with discovering universally applicable criteria for demarcating science from nonscience (criteria of demarcation) and good theories from poor ones (criteria of theory appraisal). Despite strenuous efforts, these searches have borne precious little fruit. This does not mean that the search should be abandoned. Furthermore, if and when a solution is discovered, we will use it. But until that time, as an interim position, let us agree to support some variant of critical rationalism.[7]

Conclusions and Conjectures

Summary

We are now in a position to summarize the argument. Falsificationism is the aspect of Popper's thought that is most well known among economists. When interpreted loosely, falsificationism contains a useful and beneficial set of general guidelines for scientific practice. Both proponents and critics of falsificationism concur that it is difficult to obtain clean tests of hypotheses. Those who interpret Popper strictly insist on enforcing his prohibitions against the use of immunizing stratagems, which are ad hoc theory adjustments designed to save theories from refutations. Critics are wary of Popper's injunctions against the use of immunizing stratagems. They point out that their use would quickly lead to the falsification of most of what is generally considered to be science, and that attempts to avoid this outcome would have to be considered ad hoc if Popper's standards are upheld. Critics insist that any proposed methodology be supported by good reasons for using it, and further that it be able to make sense of at least some of the history of science.

Falsificationism runs into additional problems within the social sciences. Popper himself posits the method of situational analysis as the best (and possibly only) method available in the social sciences, and claims further that situational analysis is simply a generalization of the methodology of economics. But situational analysis appears to be incompatible with falsificationism. Depending on how one defines the rationality principle, if one accepts the tenets of falsificationism, then economics and the other social sciences are either not sciences, or ad hoc, or follow a method radically different

[7] Again, the reader should be warned that I am not a neutral observer. I have advocated a position called *critical pluralism* on a number of occasions (Caldwell 1985, 1986, 1988). There are some important differences between my approach and Popper's position. I am much more interested in developing a coherent methodological position for economics, for example, so have been less concerned with strictly epistemological matters. Critical pluralism deemphasizes demarcation and encourages novelty. It encourages new programs, looking ever forward to the day when they can be subjected to critical scrutiny.

from the method alleged to be followed in all the sciences.

Rather than choose between falsificationism and situational analysis, I proposed that Popper's writings on critical rationalism permit an escape from the dilemma. If one is a critical rationalist, one is less interested in such questions as how to demarcate science from nonscience or how to justify one theory as better than another. Instead, the emphasis is on criticism. The type of criticism that one should employ cannot be specified prior to the statement of a problem to be solved; criticism is always problem specific. Within the social sciences, it turns out that the decision to retain the rationality principle is often a very effective way to develop and criticize theories. This does not mean that it is the only acceptable method. A critical rationalist would also encourage alternative approaches, those that try to improve on the rationality assumption or that attempt to explain social phenomena without recourse to individual, maximizing behavior.

Some may think it is inconsistent to endorse at one and the same time programs that use the rationality hypothesis and investigations whose ultimate goal is to supplant that hypothesis. But the appearance of inconsistency vanishes when one redefines one's goal as *the provision within economics of an environment in which the optimal amount of criticism is able to flourish*. If one believes that only theories that employ the rationality principle are legitimate, a number of alternative research programs are automatically ruled out. If one believes that any theory using the empirically false hypothesis of rationality should be rejected (as some critics of neoclassicism might claim), then one misses the richness of criticism that is applied by economists daily to the wide varieties of economic theories they analyze. By encouraging the mutual interaction of both sorts of programs, and by submitting the claims of each to critical scrutiny, we come closer to achieving the kind of creative and critical environment which might well be labeled "an ecology of rationality."

Should Falsificationism Be Banished from Economics?

Some may wonder why, in reaching the conclusions above, there was any effort to retain elements of Popper's falsificationism. If falsificationism is so alien to the practice of economists, why not reject it altogether? In particular, what does it mean to say that falsificationism, when loosely interpreted, can provide a useful set of guidelines for scientific practice?

I think that the best answer might be that falsificationism captures a recognizable *part* of scientific activity, even within economics. But it is only a part. To see this, let us contrast falsificationism with another methodological position within economics, the one made famous by Milton Friedman (1953). Friedman's position is generally though not universally portrayed in the methodological literature as a variant of *instrumentalism*, the view that theories are instruments that are assessed according to how well they enable scientists to accomplish some specified goal. In its simplest form, Friedman's position is that the realism of a theory's assumptions does not matter in its appraisal. What counts is a theory's predictive adequacy and, secondarily, its simplicity.[8]

[8] The secondary literature on Friedman's position is enormous. Methodologists have concerned themselves with what Friedman *said;* what he may have *meant;* how best to *reconstruct* his position so that it makes sense, given categories within the philosophy of science; the *origins* of his ideas; their *internal consistency;* how they might be *applied;* and their *consistency with Friedman's practice*. Elements of most of these interpretive approaches can be found in a forthcoming symposium on Friedman's method-

Both a falsificationist and Friedman would demand that theories be confronted in some way by the data. But their emphases are very different. The falsificationist wants to overturn the theory, to do it in: This is why he is always trying to think of new ways to test it, and is most impressed by theories that are severely testable. When might an economist be most likely to exhibit such behavior? Obviously, falsificationism is an ideal methodological position for a critic, for someone who dislikes the theory under scrutiny. But it can also be embraced by someone who truly does not know which of a number of theories is the right one, one who is trying to find out which theory is true. Finally, the procedure might be used if a scientist is trying to remain objective about his own theory: What are the strongest objections I can raise against this theory of mine? What tests am I overlooking? What are its weaknesses? Have I really tried to falsify it? What test results would cause me to reject my theory?

Friedman gets at different themes. The confrontation with the data is still important, for ultimately scientific theories must make sense of the data. But Friedman is much less concerned with which theory is *true*. His concern, rather, is with which theory *works best*, given some problem. Usually the problem for economists is to forecast the future, hence the importance of predictive adequacy, which is usually measured by how well the theory has performed in the past. In diametric opposition to falsificationism, then, high corroboration not only carries evidential weight for Friedman, it is something like an ultimate desideratum. Furthermore, simplicity counts because simple theories are the easiest to apply. When are economists most likely to follow Friedman's prescriptions? Simply put, Friedman's instrumentalist approach is most likely to dominate when knowledge is incomplete but some sort of policy decision must be made.

Each position has dangers associated with it. Friedman's views can all too easily lead one to be complacent about one's theories: If what works is all that matters, why concern oneself with the truth of one's theories? Sometimes following such advice does little harm. For example, Okun's law is a useful empirical generalization that can be profitably used even if we do not know why it holds. But the instrumentalist position hardly qualifies as a description of the totality of science, and sometimes it could be a prescription for disaster. Indeed, one way to bolster one's own confidence in the applicability of a highly corroborated theory is to inquire about its truth: Why is it highly corroborated? Furthermore, when a theory that once worked well suddenly begins to fail, it is appropriate to try to find out why it is failing, and inquiring about its truth is a direct way to do so.

Falsificationism is anything but complacent. Always seeking out new ways to prove a theory wrong, it is not content with even high corroboration. Its weakness is that, in its zeal to avoid complacency, it lays out criteria that even true theories may not be able to satisfy, given all of the problems that are associated with hypothesis testing. There is the danger, too, that falsificationism might be applied selectively to defeat one's opponents. This is why its best use is probably as a check on one's own enthusiasm, rather than as a weapon for the elimination of one's rivals' theories.

Both Friedman and the falsificationist

---

ology (Dan Hammond forthcoming); the definitive work on the origins of his ideas and their consistency with Friedman's practice is Hirsch and de Marchi (1990). Given the variety of possible interpretations of the phrase "Friedman's methodological position," it is important to emphasize that when I use the phrase in the text I am referring exclusively to the instrumentalist interpretation of his 1953 article.

tell a part of the story of science. But there are other parts of the story that neither one captures at all. For example, many theorists engage in activity that might better be reconstructed by using the writings of the early Lakatos on mathematical reasoning. Nor is a simple synthesis of the competing methodologies possible, because they often give contradictory advice. Friedman would differ with a falsificationist on what weight high corroboration should carry, and both would doubtless disdain mathematical exercises that do not contain empirical implications.

Science is rich in diversity. So far no single, well-specified methodology has come even close to capturing it in its entirety. Again, this is not to deny that someday such an account may become available. But it is manifestly not on hand today, and to ignore this involves at best wishful thinking, and at worst a disregard for the facts. This is why something like critical rationalism makes so much sense, at least as an interim position. By rejecting the notion that one can narrow the range of acceptable criteria of appraisal before a problem-situation is stated, it highlights the richness both of science and of the numerous methods available for the appraisal of scientific theories.

Popper's Popularity

A great many professional philosophers are critical of Popper's philosophy of science, and Popper's followers have offered a variety of explanations for why this might be so (Jarvie 1982; Bartley 1982b). Why then is Popper so popular outside of philosophy, and especially in economics? One might note certain sociological factors at work: Popper taught at the London School of Economics for a number of years, and this brought some economists into contact with his ideas. Ideological factors also cannot be discounted: Popper's critique of Marxism was bound to find a sympathetic audience, at least among mainstream economists.

Though initially tempting, neither of these explanations is sufficient to explain Popper's popularity. As de Marchi notes, it was Popper's writings rather than his personal contacts that accounted for his influence at the LSE: "As to contact between the LSE economists and Popper, there was none, because they smoked and he had an aversion to smoke" (de Marchi 1988, p. 33). The ideological explanation is unconvincing because relatively few economists are aware of Popper's opposition to Marxism, and in any case his views on the subject are separable from his more widely cited writings on scientific method.

I think that two other factors are more important in accounting for Popper's popularity outside of philosophy. The first is that he writes well, in a direct and engaging style. This is a trait that he consciously cultivated, and insisted that his pupils also develop (Bartley 1982b). It is a rare characteristic among academic philosophers (and academics generally). By making his work accessible, Popper gained a wider audience. The other factor is that Popper, unlike many philosophers, deals with issues that go to the heart of the problems that actually trouble practicing scientists (economists among them). How do we know if something is science? How are competing theories to be evaluated? On what grounds can we rule out certain strange theories that do not seem to be very scientific to us? How can we improve our own objectivity? Why is the rationality principle so important within the social sciences? How can its use be defended? Popper offers what appear to be simple and direct answers to these questions. We have seen that, even at the methodological level, his answers have not always been as simple and direct as they first may appear. We may even ultimately conclude that his answers have not been satisfactory. But we must credit him with asking the right questions.

REFERENCES

ARCHIBALD, G. S. "Refutation or Comparison?" *British J. of the Philosophy of Science*, 1966, *17*, pp. 279–96.

BARTLEY, WILLIAM W. III. "The Philosophy of Karl Popper: Part III. Rationality, Criticism, and Logic," *Philosophia*, 1982a, *11*, pp. 121–221.

______. "A Popperian Harvest," in *In pursuit of truth*. Ed: PAUL LEVINSON. Atlantic Highlands, NJ: Humanities Press, 1982b, pp. 249–89.

______. *The retreat to commitment*. 2nd ed., revised and enlarged. La Salle, IL: Open Court, [1962] 1984.

BIRNER, JACK. "A Roundabout Solution to a Fundamental Problem in Menger's Methodology and Beyond," *Hist. Polit. Econ.*, 1990, *22* (Special issue).

BLAUG, MARK. "Kuhn versus Lakatos *or* Paradigms versus Research Programmes in the History of Economics," in *Method and appraisal in economics*. Ed.: SPIRO LATSIS. Cambridge: Cambridge U. Press, 1976, pp. 149–80.

______. *The methodology of economics: or How economists explain*. Cambridge: Cambridge U. Press, 1980a.

______. *A methodological appraisal of Marxian economics*. Amsterdam: North-Holland, 1980b.

______. "Comment on Hutchison: Our Methodological Crisis," in *Economics in disarray*. Eds.: PETER WILES AND GUY ROUTH. Oxford: Basil Blackwell, 1984, pp. 30–36.

______. "Karl Popper and Economic Methodology: A New Look: Comment [on Hands]," *Econ. Philos.*, Oct. 1985, *1*(2), pp. 296–88.

______. "Reply to Hands," *Rev. Polit. Econ.*, 1990, *2*(1), pp. 102–04.

______. "Second Thoughts on the Keynesian Revolution," *Hist. Polit. Econ.*, forthcoming 1991.

BOLAND, LAWRENCE A. *The foundations of economic method*. London: Allen & Unwin, 1982.

CALDWELL, BRUCE J. "Book Review: Blaug's *The methodology of economics*," *Southern Econ. J.*, July 1981, *48*(1), pp. 242–45.

______. *Beyond positivism: Economic methodology in the twentieth century*. London: Allen & Unwin, 1982.

______. "The Neoclassical Maximization Hypothesis: Comment," *Amer. Econ. Rev.*, Sept. 1983, *73*(4), pp. 824–27.

______. "Some Problems with Falsificationism in Economics," *Philosophy of the Social Sciences*, Dec. 1984, *14*(4), pp. 489–95.

______. "Some Reflections on *Beyond Positivism*," *J. Econ. Issues*, Mar. 1985, *19*(1), pp. 187–94.

______. "Towards a Broader Conception of Criticism," *Hist. Polit. Econ.*, Winter 1986, *18*(4), pp. 675–81.

______. "The Case for Pluralism," in DE MARCHI, 1988, pp. 231–44.

______. "Hayek the Falsificationist? A Refutation," *Research in the History of Economic Thought and Methodology, 10*, forthcoming a.

______. "Reply to Hutchison," *Research in the History of Economic Thought and Methodology, 10*, forthcoming b.

CARNAP, RUDOLF. "Testability and Meaning," *Philosophy of Science*, 1936, *3*, pp. 420–68, 1937, *4*, pp. 1–40.

ELSTER, JON. *Making sense of Marx*. Chicago: Cambridge U. Press, 1985.

______. "The Nature and Scope of Rational-Choice Explanation," in *Science in reflection. 110, Boston studies in the philosophy of science*. Ed.: EDNA ULLMANN-MARGALIT. Dordrecht, Holland: D. Reidel, 1988, pp. 51–65.

______. "Social Norms and Economic Theory," *J. Econ. Perspectives*, Fall 1989, *3*(4), pp. 99–117.

FRIEDMAN, MILTON. "The Methodology of Positive Economics" in *Essays in positive economics*. Chicago: U. of Chicago Press, 1953, pp. 3–43.

HAMMOND, DANIEL, ed. "Realism, Instrumentalism, and Friedman's Methodology," *Research in the History of Economic Thought and Methodology*, forthcoming.

HANDS, D. WADE. "Second Thoughts on Lakatos," *Hist. Polit. Econ.*, Spring 1985a, *17*(1), pp. 1–16.

______. "Karl Popper and Economic Methodology: A New Look," *Econ. Philos.*, Apr. 1985b, *1*(1), pp. 83–99.

______. "The Structuralist View of Economic Theories: The Case of General Equilibrium in Particular," *Econ. Philos.*, Oct. 1985c, *1*(2), pp. 303–35.

______. "Ad Hocness in Economics and the Popperian Tradition," in DE MARCHI, 1988, pp. 121–37.

______. "Second Thoughts on 'Second Thoughts': Reconsidering the Lakatosian Progress of *The General Theory*," *Rev. Polit. Econ.*, 1990, *2*(1), pp. 69–81.

______. "Falsification, Situational Analysis and Scientific Research Programs: The Popperian Tradition in Economic Methodology," in *Methodological reflection in economics*. Ed.: NEIL DE MARCHI. Boston: Kluwer, forthcoming a.

______. "Popper, the Rationality Principle, and Economic Explanation," in *Economics, culture and education: Essays in Honour of Mark Blaug*. Ed.: G. K. SHAW. Cheltenham: Edward Elgar, forthcoming b.

HAUSMAN, DANIEL. "Is Falsificationism Unpractised or Unpractisable?" *Philosophy of the Social Sciences*, 1985, *15*, pp. 313–19.

______. "An Appraisal of Popperian Economic Methodology," in DE MARCHI, 1988, pp. 65–85.

HAYEK, FRIEDRICH A. "The Theory of Complex Phenomena," in *Studies in philosophy, politics and economics*. Chicago: U. of Chicago Press, [1964] 1967, pp. 22–42.

HIRSCH, ABRAHAM AND DE MARCHI, NEIL. *Milton Friedman: Economics in theory and practice*. Ann Arbor: U. of Michigan Press, 1990.

HOLLIS, MARTIN AND NELL, EDWARD J. *Rational economic man: A philosophical critique of neo-classi-*

cal economics. Cambridge: Cambridge U. Press, 1975.

HOWSON, COLIN, ed. *Method and appraisal in the physical sciences: The critical background to modern science, 1800–1905*. Cambridge: Cambridge U. Press, 1976.

HOWSON, COLIN AND URBACH, PETER. *Scientific reasoning: The Bayesian approach*. La Salle, IL: Open Court, 1989.

HUTCHISON, T. W. "Professor Machlup on Verification in Economics," *Southern Econ. J.*, Apr. 1956, *22*(4), pp. 476–83.

______. *The significance and basic postulates of economic theory*. NY: Augustus Kelley, [1938] 1960.

______. *Knowledge and ignorance in economics*. Chicago: U. of Chicago Press, 1977.

______. *The politics and philosophy of economics: Marxians, Keynesians, and Austrians*. Oxford: Basil Blackwell, 1981.

______. "The Case for Falsification," in DE MARCHI, 1988, pp. 169–81.

______. "Hayek and 'Modern Austrian' Methodology: Comment on a Non-Refuting Refutation," *Research in the History of Economic Thought and Methodology, 10*, forthcoming.

JARVIE, IAN. *Concepts and society*. London: Routledge & Kegan Paul, 1972.

______. "Popper on the Difference between the Natural and the Social Sciences," in *In pursuit of truth*. Ed.: PAUL LEVINSON. Atlantic Highlands, NJ: Humanities Press, 1982, pp. 83–107.

KAHNEMAN, DANIEL AND TVERSKY, AMOS. "Prospect Theory: An Analysis of Decision Under Risk," *Econometrica,* Mar. 1979, *47*(2), pp. 263–91.

KLAMER, ARJO. *Conversations with economists*. Totowa, NJ: Rowman & Allanheld, 1984.

KLANT, JOHANNES J. *The rules of the game: The logical structure of economic theories*. Translated by INA SWART. Cambridge: Cambridge U. Press, 1984.

KLAPPHOLZ, KURT AND AGASSI, J. "Methodological Prescriptions in Economics," *Economica, N.S.*, Feb. 1959, *26*(101), pp. 60–74.

KOERTGE, NORETTA. "Popper's Metaphysical Research Program for the Human Sciences," *Inquiry*, Winter 1975, *18*(4), pp. 437–62.

______. "The Methodological Status of Popper's Rationality Principle," *Theory and Decision*, 1979, *10*(1–4), pp. 83–95.

KUHN, THOMAS. *The structure of scientific revolutions*. 2nd enlarged ed. Chicago: U. of Chicago Press, [1962] 1970.

LAKATOS, IMRE. "Falsification and the Methodology of Scientific Research Programmes," in *Criticism and the growth of knowledge*. Eds: IMRE LAKATOS AND ALAN MUSGRAVE. Cambridge: Cambridge U. Press, 1970, pp. 91–196.

______. "History of Science and Its Rational Reconstruction," in *PSA 1970: In memory of Rudolf Carnap. 10, Boston studies in the philosophy of science*. Eds.: ROGER BUCK AND ROBERT COHEN. Dordrecht, Holland: D. Reidel, 1971, pp. 91–139.

______. *Proofs and refutations: The logic of mathematical discovery*. Eds.: JOHN WORRALL AND ELIE ZAHAR. Cambridge: Cambridge U. Press, 1976.

LATSIS, SPIRO. "Situational Determinism in Economics," *British J. for the Philosophy of Science*, 1972, *23*, pp. 207–45.

______, ed. *Method and appraisal in economics*. Cambridge: Cambridge U. Press, 1976.

______. "The Role and Status of the Rationality Principle in the Social Sciences," in *Epistemology, methodology and the social sciences. 71, Boston studies in the philosophy of science*. Eds.: ROBERT COHEN AND MARX WARTOFSKY. Dordrecht, Holland: D. Reidel, 1983, pp. 123–51.

LLOYD, CHRISTOPHER. *Explanation in social history*. Oxford: Basil Blackwell, 1986.

LOSEE, JOHN. *Philosophy of science and historical enquiry*. Oxford: Clarendon Press, 1987.

MÄKI, USKALI. "On the Problem of Realism in Economics," *Ricerche Econ.* Jan./June 1989, *43*(1,2), pp. 176–98.

______. "Mengerian Economics in Realist Perspective," *Hist. Polit. Econ.*, 1990, *22*(Special Issue).

DE MARCHI, NEIL. "Popper and the LSE Economists," in DE MARCHI, 1988, pp. 139–66.

______, ed. *The Popperian legacy in economics*. Cambridge: Cambridge U. Press, 1988.

______. "Introduction: Using MSRP," in *Appraising modern economics: Studies in the methodology of scientific research programmes*. Eds.: MARK BLAUG AND NEIL DE MARCHI. Cheltenham: Edward Elgar, forthcoming.

MILLER, DAVID, ed. *Popper selections*. Princeton: Princeton U. Press, 1985.

MIROWSKI, PHILIP, ed. *The reconstruction of economic theory*. Boston: Kluwer, 1986.

______. *More heat than light*. Cambridge: Cambridge U. Press, 1989.

VON MISES, LUDWIG. *Human action: A treatise on economics*. 3rd revised ed. Chicago: Henry Regnery, [1949] 1966.

NOLA, ROBERT. "The Status of Popper's Theory of Scientific Method," *British J. for the Philosophy of Science*, 1987, *38*, pp. 441–80.

O'HEAR, ANTHONY. *Karl Popper*. London: Routledge & Kegan Paul, 1980.

POPPER, KARL. *The poverty of historicism*. Boston: Beacon Press, [1944–45] 1957.

______. *The logic of scientific discovery*. New York: Harper & Row, 1959.

______. *The open society and its enemies*. Princeton: Princeton U. Press, [1945] 1963.

______. "Prediction and Prophecy in the Social Sciences," in *Conjectures and refutations*. 2nd ed. NY: Harper & Row, [1948] 1965a, pp. 336–46.

______. "On the Status of Science and of Metaphysics," in *Conjectures and refutations*. 2nd ed. NY: Harper & Row, [1958] 1965b, pp. 184–200.

______. "Intellectual Autobiography," in *The philosophy of Karl Popper*. Ed.: PAUL SCHILPP. Lasalle, IL: Open Court, 1974, *1*, pp. 3–181.

______. "The Logic of the Social Sciences," in *The positivist dispute in German sociology*. Eds.: THEODORE ADORNO ET AL. NY: Harper & Row, 1976, pp. 87–104.

______. *Quantum theory and the schism in physics*.

The postscript to the logic of scientific discovery, 3. Ed.: W. W. BARTLEY III. Totowa, NJ: Rowman & Littlefield, 1982.

———. *Realism and the aim of science. The postscript to the logic of scientific discovery, 1.* Ed: W. W. BARTLEY III. Totowa, NJ: Rowman & Littlefield, 1983.

———. "The Rationality Principle," in *Popper selections.* Ed.: DAVID MILLER. Princeton: Princeton U. Press, 1985, pp. 357–65.

RADNITZKY, GERARD AND BARTLEY, W. W. III, eds. *Evolutionary epistemology, rationality, and the sociology of knowledge.* La Salle, IL: Open Court, 1987.

ROSENBERG, ALEXANDER. *Philosophy of social science.* Boulder, CO: Westview Press, 1988.

SHEARMUR, JEREMY. "Theoretical Problem-Solving in Economics," in *Appraising modern economics: Studies in the methodology of scientific research programmes.* Eds.: MARK BLAUG AND NEIL DE MARCHI. Cheltenham: Edward Elgar, forthcoming.

SUPPE, FREDERICK. *The structure of scientific theories.* 2nd ed. Urbana, IL: U. of Illinois Press, 1977.

WÄCHTERSHÄUSER, GÜNTER. "Light and Life: On the Nutritional Origins of Sensory Perception," in *Evolutionary epistemology, rationality, and the sociology of knowledge.* Eds.: GERARD RADNITZKY AND W. W. BARTLEY III. La Salle, IL: Open Court, 1987, pp. 121–38.

WATKINS, JOHN W. N. "Confirmable and Influential Metaphysics," *Mind,* July 1958, *67*(267), pp. 344–65.

———. *Science and scepticism.* Princeton: Princeton U. Press, 1984.

WEIMER, WALTER AND PALERMO, DAVID, eds. *Cognition and the symbolic processes,* Vol. 2. Hillsdale, NJ: Erlbaum Associates, 1982.

WIBLE, JAMES. "Friedman's Positive Economics and Philosophy of Science," *Southern Econ. J.,* Oct. 1982, *49*(2), pp. 350–60.

WONG, STANLEY. *The foundations of Paul Samuelson's revealed preference theory: A study by the method of rational reconstruction.* London: Routledge, 1978.