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Positivist Philosophy of Science and the Methodology of Economics

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Positivism and the methodology of economics were first joined in T. W. Hutchison's 1939 classic, *The Significance and Basic Postulates of Economic Theory*. The relationship has lasted forty years, bringing plaudits from some camps, criticism from others. At various times economics has been praised for being a positivist social science, disparaged for not; urged to become one, and denigrated for trying. What seems clear is that many commentators implicitly disagree about the meaning of *positivism*. No one has bothered to ask exactly what it is.

This article addresses the question by tracing the evolution of twentieth-century positivist thought within the philosophy of science, beginning with the logical positivism of the Vienna Circle. Primary attention is given to the mature positivist analyses which emerged in the writings of Carl Hempel, Ernest Nagel, A. J. Ayer, Richard Braithwaite, and Rudolf Carnap in the 1940s and 1950s. It will also briefly show how a knowledge of the philosophy of science can aid in understanding and analyzing the pronouncements of economic methodologists.[1]

By the 1950s, positivist analysis dominated not only the philosophy of science, but also the methodologies of most of the natural and social sciences. To be sure, positivism has always had its critics: Karl Popper and Herbert Marcuse within philosophy;[2] Austrian, institutionalist, and neo-

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Marxist analysts within economics. But to most observers, positivism appeared to be a rigorously constructed and consistent body of beliefs capable of providing a firm and coherent epistemological basis for scientific methodology. In the ensuing years, however, it has been subjected to increasing criticism within the philosophy of science. The attacks have been sufficiently robust to cause many contemporary analysts to turn to alternative approaches. These paradigms, associated with such names as Thomas Kuhn, Paul K. Feyerabend, Imre Lakatos, Stephen Toulmin, Peter Achinstein, and many others, will not be covered here; suffice it to say that their impact on economic methodology is growing with every journal publication in the field.[3] The attacks against positivism which have caused this reconstruction of the philosophy of science, and the implications for economics, are documented in a later section. Suggestions for further work in economic methodology also are included. Conclusions about the implications of these developments are presented in the last section.

The Development of Positivism

Much of the modern philosophy of science simply elaborates upon various empiricist positions, most especially the radical empiricist stance known as positivism. Although it originated in the nineteenth century,[4] only its twentieth-century forms will be treated here, since they are most relevant to recent economic methodology. We will first discuss the logical positivists of the Vienna Circle.

The Vienna Circle began in the early 1920s as Thursday evening discussions among members of various disciplines at the University of Vienna. Moritz Schlick is generally considered the founder of the club; other significant participants included Herbert Feigl, Otto Neurath, Hans Hahn, Friedrich Waismann, and, later, Rudolf Carnap. In 1929, members of the circle issued a pamphlet proclaiming the goals of the new movement, and in the 1930s a journal and several monographs were published. The deaths of Hahn and Schlick and the coming of World War II caused the circle to splinter and disintegrate, so as a unified movement logical positivism ended in the early 1940s. Its influence, however, was to be felt for years to come, both in the philosophy of science and in various disciplines.

The logical positivists believed they had discovered the true task of philosophy: to analyze knowledge statements with the aim of making them clear and unambiguous. The new philosophy would demonstrate the meaninglessness of all metaphysics and, more constructively, provide a foundation for empirical science. As philosopher Abraham Kaplan succinctly expressed it, the form of all philosophy would be logical analysis, and its subject matter the empirical or positive sciences, hence the label logical positivism.[5]

It was asserted that only meaningful statements were to be given scientific consideration and accorded the status of knowledge claims. Meaningfulness, or cognitive significance, was strictly defined as being attributable only to those statements which are either analytic (tautologies or self-contradictions) or synthetic (factual statements which may be tested against evidence).[6] Using this criterion, metaphysical statements are neither analytic nor subject to empirical test, so they must be deemed meaningless, although not necessarily false.

The next task before the logical positivists was to offer some objective criterion for distinguishing between analytic, synthetic, and meaningless statements. The analytic-synthetic distinction seemed to pose no difficulties; the problem lay in separating legitimate synthetic statements from metaphysical assertions. An early solution was dubbed the verifiability principle: A statement has meaning only to the extent that it is verifiable.[7] This implies testability, since one must be able to test whether a synthetic assertion is true. Hempel observed that the testability criterion of the most conservative and dogmatic logical positivists was quite strict: A sentence had empirical meaning only if it was capable, at least in principle, of complete verification by observational evidence, and such evidence was restricted to what could be observed by the speaker and his fellow beings during their lifetimes.[8] The criterion was modified considerably as time progressed, but a heavy reliance on observational evidence persists.

The insistence on the primacy of physical data had a number of implications, the most important of which concerned the status of theoretical terms. No one had ever observed atoms or magnetic fields; were statements positing their existence to be considered nonsense expressions? A predecessor of the logical positivists, Ernst Mach, answered in the affirmative;[9] the American physicist Percy Bridgman shared that view. Bridgman developed operationalism, which asserts that the definition of any concept in science is nothing more than the set of measurement operations which can be performed on it. If one adopts an operational approach, then one must dismiss as meaningless any concept (such as a theoretical entity) which cannot be defined as a set of operations.[10] Some logical positivists were as unabashedly phenomenalist as Mach and Bridgman; even Carnap was in his earliest writings. He later revised his position to state that theoretical terms gain partial meaningfulness to the extent that they can be partially interpreted into an observation language,[11] and elaborations of this view came to dominate later positivist thought.

The stress on observability also led the logical positivists to a belief in the methodological unity of all scientific endeavor. It was held that the social no less than the natural sciences are concerned with observable phenomena; accordingly, approaches to the social disciplines which explain social phenomena by relying on, say, subconscious motivations or introspective states of mind can be accused of metaphysical speculation. This view is concisely summarized by Ayer: "The scale and diversity of the phenomena with which the social sciences dealt made them less successful in establishing scientific laws, but this was a difficulty of practice, not of principle: they too were concerned in the end with physical events."[12]

The logical positivists of the Vienna circle were often fanatical in expressing the belief that their approach constituted the sole and ultimate end of philosophical analysis. From the mid-1930s through the mid-1950s, a more sophisticated positivist stance emerged, one less dogmatically empiricist than logical positivism. The names usually associated with this later movement are Ayer, Braithwaite, Carnap, Hempel, and Nagel, although this list is not exhaustive. Many problems were examined by positivist philosophers of science during this period, but three are relevant to our discussion: (1) the search for a criterion of cognitive significance; (2) the status, structure, and function of theories and theoretical terms; and (3) the nature of scientific explanation. Although these areas are clearly mutually dependent, they are separated here for purposes of exposition.

The basis for the positivist position was that only analytic and synthetic statements have cognitive significance, and that a nonanalytic statement is meaningful only if it can be subjected to empirical tests. Making the testability criterion concrete became a major problem for later positivists, however, and no formulation of it has survived unscathed. The early positivists suggested verifiability, but by the mid-1930s it was evident that that criterion was unnecessarily strict. Verifiability rules out as meaningless those statements of universal form (for example, all ravens are black) which are often used in the specification of general scientific laws. Such statements are not conclusively verifiable because one exception could falsify them, and no number of confirming instances can guarantee that such an exception will never be found.

A criterion of meaning which excludes general laws from the universe of cognitively significant hypotheses is clearly incompatible with a philosophical position which desires to analyze the statements of science; this was Popper's point when he wrote that "positivists, in their anxiety to annihilate metaphysics, annihilate natural science along with it."[13] It was Popper who suggested that the falsifiability of a proposition rather than its verifiability be the "criterion of demarcation" for distinguishing scientific from nonscientific statements.[14] This criterion has the advantage of admitting statements of universal form as cognitively significant; it fails, however, to accept affirmative existential hypotheses as meaningful. As Ayer wrote: "One cannot say that there are abominable snowmen, for this cannot be falsified; the fact that one had failed to find any would not prove conclusively that none existed."[15]

Other attempts to construct a criterion of demarcation include Ayer's notion of "weak verifiability," which was ultimately rejected because it was too lax, and Carnap's suggestion that an empiricist language be developed whose very structure rules out the formation of nonmeaningful statements. Translatability into the empiricist language would then serve as the criterion of cognitive significance. Although formally pleasing, this approach fails because the construction of a workable empiricist language has proven impossible thus far.[16]

Most mature positivists eventually settled on the notion of confirmability as the criterion of meaningfulness. This is a diluted standard, requiring only that statements must *to some degree* be confirmed or disconfirmed by evidence. Carnap was one originator of the approach, and much of his later work in inductive inference was concerned with making concrete the notion of the degrees of confirmation of an hypothesis.[17] It should be noted that concomitant with the rise of confirmability was a change in emphasis from individual sentences to systems of sentences (theories) as the focus for testing. A related development was the gradual abandonment of the analytic-synthetic distinction because it was not always possible to differentiate between the two, as was shown by W. V. O. Quine, and because it was not clear how statements which made reference to nonobservable theoretical entities should be handled.[18]

In their attempts to develop a criterion by which to separate legitimate synthetic propositions from nonsense assertions, the logical positivists discovered that a certain class of terms—theoretical—posed a particularly intractable problem. They were used in all branches of science, yet they often were not amenable to explicit definition in terms of observables. As such, sentences containing references to theoretical entities were problematical respecting the analytic-synthetic dichotomy. What was to be done?

Some positivists (such as Mach) called for the eventual elimination of theoretical terms from the language of science. Others favored explicitly defining these terms in a physicalist or protocol language, or forming reduction sentences which offer a partial rather than a complete specification of the entity in question.[19] Neither of these, however, adequately handled the problems posed by theoretical terms. The solution to which most mature positivists eventually came was to provide an "interpretative system," which contains not definitions "but statements to the effect that a theoretical sentence of a certain kind is true if and only if a corresponding empirical statement of a specified kind is true."[20] In addition, attention should be focused not on individual theoretical terms, but on theories as a whole, and hence some theoretical terms may be left undefined.[21]

This hypothetico-deductive (H-D) model of theories can be restated as follows. The formal structure of a theory is nothing more than a mechanical calculus, or a hypothetico-deductive system. A theory contains axioms, or primitive sentences, and theorems, or derivative statements. The axioms may refer either to observable or nonobservable entities. As a mechanical calculus, the system is devoid of meaning until given an empirical content by means of interpretative sentences, that is, when some of the sentences of the theory (often the derived ones) are translated into the observation language. Implicit is the idea that theories are to be judged as entire systems: The fact that there is no complete (or incomplete, for that matter) definition for every theoretical term is not to be held against a theory. All terms gain meaningfulness to the extent that the theory as a whole is confirmed, usually by checking the derivative theorems (or predictions) against evidence.

Nineteenth-century positivists claimed that theories do not explain phenomena, but are economical and eventually eliminable tools for the organization of complexes of sensations; that establishing correlations among phenomena is all that science can and should do; and that only metaphysicians would try to go beyond phenomena themselves in search of "ultimate explanations."[22] The logical positivists did not deal much with explanation, but later positivists denied the validity of the descriptivist position of their predecessors. They advanced what have been called the "covering law" models, in which scientific explanation was viewed as an essential part of the enterprise.

Hempel and Paul Oppenheim developed the deductive-nomological (D-N) model of scientific explanation in a 1948 paper, "Studies in the Logic of Explanation." According to their model, any valid explanation must be expressible in the form of a deductive argument in which a sentence describing the event to be explained (the explanandum) is a logically valid consequence of the explanans. The latter consists of a list of antecedent conditions which must exist and of one or more general laws. Thus, four "conditions of adequacy" must obtain: the explanandum must be a logical consequence of the explanans, the explanans must contain at least one general law, the explanans must have empirical content, and the sentences constituting the explanans must be true.[23]

The D-N model stresses the deductive nature of explanation: The implied logical necessity is due to the restriction that only laws of universal form are to be permitted in the explanans. If laws of a statistical nature are allowed, only a certain likelihood of the occurrence referred to in the explanandum can be maintained. Since it is clear that many explanations in science make use of statistical laws, the D-N model is inadequate for all situations. Accordingly, Hempel developed a second, inductive-probabilistic (I-P), covering law model.[24] In it the explanans, comprised now of sentences describing the requisite initial conditions along with *statistical* laws, "confers upon the explanandum-statement a high logical, or inductive, probability."[25]

Two further assertions were made by the covering law theorists. The first is the symmetry thesis: Explanation and prediction are structurally symmetrical, the only difference between them being temporal.[26] The second is that the two covering law models, between them, adequately describe virtually all legitimate explanation that occurs in both the natural and social sciences.[27]

To sum up, positivism experienced a number of changes in its evolution from the naïve positivism of the nineteenth century through the strict and sometimes dogmatic positivism of the Vienna Circle to the more mature positivism of the 1940s and 1950s. If such a person had existed, a "representative positivist" of the mid-1950s might have offered the following characterization of the structure, nature, and function of science.

The relationships and phenomena investigated by both the natural and social sciences can often be represented formally by axiomatic H-D structures known as theories. In their formal state, they have no empirical import; it can only be achieved when certain of the symbols are given an empirical content via interpretative sentences. Implied by this H-D model is the weak requirement that only some of the terms need have empirical counterparts. This is necessary because a certain group of terms used extensively in science—the theoretical—defy explicit interpretation into the neutral observation language. Rather than attempt to rid science of such terms, the current view recognizes the essential role played by theoretical terms and urges their retention. As such, the following modifications of earlier positivist views are necessary.

First, the individual statements contained in a theory should not be tested separately; rather, the entire theory should be tested to see if its observable deduced consequences correspond to reality. This rids science of the necessity of checking each synthetic statement for cognitive value. Confirmability, which states that theories must to some extent be supported by evidence, is the new criterion of cognitive significance. Because

it is a weak one, alternative nonempirical criteria for theory evaluation (for example, simplicity, elegance, consistency, theoretical support) should be systematically investigated and logically stated. To reiterate, cognitive significance is to be applied as a tool for *theory* evaluation, rather than as a means for distinguishing between meaningful and meaningless *sentences*.

If we insist on retaining theoretical terms in science, what is their status? Whereas the question of cognitive significance once turned on the testability of assertions, the present view allows theoretical terms to gain meaningfulness indirectly. Even when such terms are not directly expressible in the observation language, they are accorded cognitive significance upon the successful confirmation of the theory in which they are embedded. Whether or not theoretical terms make reference to real entities (the old realist-instrumentalist controversy) is moot; what counts is whether the hypotheses which contain them are confirmable and confirmed.

Finally, we follow Hempel in asserting that the goal of science is explanation, and we deny the naïve view that theories can only describe but not explain phenomena. However, in centuries past, humans have offered explanations for phenomena which should not be considered scientific (bodily functions are governed by vital forces [entelechies], natural disasters are sent by animistic spirits). To avoid such metaphysical excesses, the current view considers legitimate only those explanations which can be reconstructed in the form of either a deductive argument (following the D-N model) or a highly probable inductive argument (following the I-P model). A corollary of that view, which further ensures the legitimacy of our explanations, is that explanation and prediction are logically symmetrical, the only difference between them being temporal.

Positivism and Economics

Our discussion of positivism becomes meaningful to economists once it is realized that there are many interfaces between the philosophy of science and the methodology of economics. The relationship is, in fact, very similar to that between the methodology of economics and the practice of economics itself.

Economists make their living by studying those phenomena which are defined, for a given time and space, as economic.[28] It is the task of the economic methodologist to tell the practitioner which methods are best for studying those phenomena. In a totally analogous way, the philosophy of science is (at least in part) a study of the "proper" methodology for all of the sciences, of which economics is a particular instance. Thus, just as economists (in the best of all worlds) may look to their methodologists for direction, economic methodologists may look to the philosophy of science to see whether its methods are consistent with those generally employed by scientists. This is the prescriptive role of both disciplines.

At this point, a problem is encountered. What makes philosophers of science qualified to judge the methods of scientists? By what miracle of transcendental apperception have philosophers gained the ability to know which methods of study are most likely to yield successes in each of a number of specialized disciplines? These questions are even more significant given our topic, since most philosophers of science have made their prescriptions *vis-à-vis* the physical, not the social, sciences.

Indeed, philosophers of science have no transcendental apperception; there is no meta-methodology outside the philosophy of science by which its dictums may be evaluated. This seems to pose an intractable dilemma, but a possible solution is suggested by the notion that any methodology must include both prescriptive and descriptive elements. Economic methodologists have a duty to tell economists how to practice their trade, but their prescriptions must also be consistent with the general practice of economists. If the discrepancy between the two is large enough, either the behavior of economists or the prescriptions of methodologists must be changed. In a similar manner, the prescriptions of philosophers of science must be at once guideposts to behavior and mirrors of generally accepted scientific procedures. A constant interaction between prescription and description thus characterizes all methodological investigation. Methodology must prescribe behavior, it must distinguish between proper and improper methods, but because there is no recourse to a meta-methodology, the only possible check on those prescriptions is grounded in whether or not they are descriptive of actual (and, one would hope, sound) scientific practice.[29]

For our purposes, it is sufficient to note that the pronouncements of economic methodologists cannot simply be judged correct or incorrect in the light of the philosophy of science. However, a case can be made that a knowledge of that subject offers a unique vantage from which to come to grips with the methodological literature in economics. A few examples will buttress this claim.

Many aspects of economic methodological thought are more clear and understandable if the reader has some familiarity with the philosophy of science. This is true in part because many economists have looked to that field for ideas. T. W. Hutchison, Fritz Machlup, and Andreas Papandreou are three examples of economists whose methodological writings reflect a knowledge of the philosophy of science that was contemporaneous with their efforts.[30] Indeed, the reader who does not share some knowledge of

philosophy might well find the arguments of certain economic methodologists difficult to follow. Thus, Hutchison's frequent invocation (in his earlier writings) of such concepts as analyticity, syntheticity, and falsifiability can be better understood if one is familiar with the logical positivist program. Paul Samuelson's bizarre assertion that explanation is equivalent to description, an idea which goes against one's intuition, at the very least, is more comprehensible if one realizes that the same thought had been expressed by Ernst Mach and other early positivists. More recently, all of the readings in a volume edited by Spiro Latsis gain meaning and direction once one understands the work of Thomas Kuhn and Imre Lakatos.[31]

Of course, not all economic methodologists have drawn upon the philosophy of science in formulating their ideas: Milton Friedman's classic "The Methodology of Positive Economics" is perhaps the most dramatic instance. Moreover, ignorance of the philosophical literature can compound confusion if methodologists insist on using terms which have been formulated outside economics. The term *positivism* is an example. Although many economists have claimed that theirs is a positivist discipline, no consensus seems to exist among them as to just what that means. Does positivism mean that value judgments should be minimized? That the scientific method of the natural sciences should be followed? That correlations (and not explanations) are all that should be sought? That theories should be subject to constant empirical testing? Dissension over fundamental categories of analysis can only bring confusion to an already complex study. It is not suggested here that every economist interested in methodology should become familiar with the massive philosophy of science literature; however, a working knowledge can be of obvious value in categorizing alternative methodological views so that reasonable debate can ensue.

Along similar lines, the philosophy of science can be useful in evaluating the cogency (although not, I think, the ultimate validity) of the arguments of various economic methodologists. I have argued this point in an earlier article, taking Samuelson's descriptivist view of scientific explanation as an example.[32] A second example indicates both the advantages and limitations of using the philosophy of science for evaluating the methodological literature in economics.

During the mid-1950s, Machlup and Hutchison engaged in a debate over the necessity to test the assumptions of economic theory independently. Machlup distinguishes between two differing approaches to the problem of testing assumptions: *A priorists* "contend that economic science . . . is a system of pure deductions from a series of postulates, not open to any verification or refutation on the ground of experience," whereas ultra-empiricists "refuse to recognize the legitimacy of employing at any level of analysis propositions not independently verifiable."[33] Hutchison was cited as a representative of the latter position. Favoring neither extreme, Machlup states that the errors of both lie in failing to distinguish "the difference between hypotheses on different levels of generality and, hence, of different degrees of testability." Referring to such philosophers as Richard B. Braithwaite and Josiah Royce, Machlup claims that "fundamental assumptions" are not independently testable, and that the testing of hypothetico-deductive systems can only be effected by submitting deduced, "lower-level" hypotheses to test.[34]

Hutchison defended himself. Scoring Machlup's dichotomy of *a priorist* and ultra-empiricist as vague, the English economist stated that, in any case, he was no ultra-empiricist, since he had earlier written that propositions in science need only, conceivably, be testable or reducible to such propositions, which clearly allows for indirect tests of assumptions.[35] Hutchison then discussed Machlup's formulation of the maximization principle (Machlup's only example of a "fundamental hypothesis") and had difficulty in discovering just what content it was meant to possess. Hutchison suggested that the testable statement, "preferences can be arranged by consumers in an order," is more precise and clear than stating: "consumers maximize utility." He then approvingly cited the history of value theory from Irving Fisher and Vilfredo Pareto to Samuelson and I. M. D. Little as evidence of a trend toward more testable formulations in economic theory.[36]

The protagonists in this debate took different stands on the question of whether all statements in a theory should be independently testable.[37] Does the philosophy of science shed any light on the discussion?

The philosophy of science that was contemporaneous with their efforts would award Machlup the laurels. To be sure, the logical positivists had insisted that, to be legitimate, the nonanalytic statements of science had to be testable, which supports Hutchison's position. But by the 1950s it was recognized that statements containing theoretical terms (among which are included assumptions), whose role in science is essential, cannot be unambiguously evaluated using the analytic-synthetic distinction. All statements in a theory employing such terms cannot be independently tested; theories are tested by giving certain terms empirical counterparts and by comparing the deductions (predictions) of the theory with reality; Machlup's position is thus the more sophisticated.[38]

It is not, however, unassailable, for the pronouncements of philosophers of science on this topic were made with the *natural sciences,* and particularly physics, in mind. The point is succinctly made by Jack Melitz:

> Machlup's argument that "fundamental assumptions" are impossible to test directly rests essentially on the experience in physics. As frequently noted, the physical postulates cannot be directly tested. Like many observers, Machlup is extremely impressed with this point and prone to generalize on its basis. However, . . . it does not follow that there are significant barriers to direct testing of postulates in other disciplines, particularly in a field as distantly related to physics as economics.[39]

To conclude, the philosophy of science can be a useful tool for clarifying one's understanding of methodological issues in economics. The last example shows that its use for evaluating the dictums of economic methodologists is more limited but still existent.

Criticisms of Positivism

Many of the criticisms of the positivist program which have emerged in the last two decades grew out of the alternative visions of the scientific enterprise and of the way that the philosophy of science should be done. This is particularly true of the formulations of Kuhn, Lakatos, and Feyerabend. The purpose of this article, however, is to investigate positivism, not its heirs. For this reason, the three major categories of positivist analysis highlighted in an earlier section are the focus of our attention below, rather than the newer paradigms of contemporary philosophy of science. Recent applications in the methodology of economics are also mentioned, which indicates yet another role for the philosophy of science in aiding economic methodologists: that of suggesting new areas of research.

Confirmability

Confirmability, the most recent positivist criterion of acceptability, requires only that empirical evidence support to some degree the hypothesis being tested, with *hypothesis* used loosely to mean a single statement or a fully elaborated theory. Confirmability is a weak criterion of acceptability: If two hypotheses are confirmed "to some degree" by evidence, choosing between them on empirical grounds becomes problematic. Why must scientists be content with such a weak measure?

Confirmability is the strongest admissible criterion because of the conditional nature of all scientific hypotheses. A conditional hypothesis is of the following form: if P, then Q. Contained in P are the hypothesis (or hypotheses) under test, H; initial conditions, which should be specified, C; and any relevant auxiliary hypotheses, A. Q is the prediction of the hypothesis. The conditional form of scientific hypotheses may then be restated as follows: Under test conditions C, if hypothesis H and auxiliary hypotheses A are true, then we can expect a result of Q.[40]

For one to have any confidence that a test accurately confirms or disconfirms an hypothesis, initial test conditions and auxiliary hypotheses should be finite in number, empirically specifiable, technologically realizable, and met. That this is not always the case is obvious; it is literally impossible in the absence of omniscience to list all the variables which could affect the outcome of an experiment, especially in the social sciences. One must be content with listing only the most relevant, a task which, in the case of auxiliary hypotheses, can be very difficult. As Hempel has pointed out:

> Tycho Brahe, whose accurate observations provided the empirical basis for Kepler's laws of planetary motion, rejected the Copernican conception that the earth moves about the sun. He gave the following reason, among others: if the Copernican hypothesis were true, then the direction in which a fixed star would be seen by an observer on the earth at a fixed time of day, should gradually change. . . . Brahe, who made his observations before the telescope was introduced, searched with his most precise instruments for evidence of such "parallactic motions" of fixed stars—and found none. He therefore rejected the hypothesis of the earth's motion; but the test implication that the fixed stars show observable parallactic motions can be derived from Copernicus' hypothesis only with the help of the auxiliary hypothesis that the fixed stars are so close to the earth that their parallactic movements are large enough to be detected by means of Brahe's instruments.[41]

Thus, paradoxically, a number of auxiliary hypotheses may be implicit in any test situation, but their presence can go undetected until they fail to hold.

Two further difficulties regarding confirmability must be mentioned. First, certain "paradoxes of confirmation" have been discovered whose existence suggests that the distinctions among confirming, disconfirming, and irrelevant instances are not always easily perceived.[42] Second, efforts to construct an inductive logic, by which the relative degrees of confirmation of competing hypotheses might be assessed, have so far failed.[43] Some philosophers, notably Popper, view efforts to construct such a logic as misguided.[44] While certain of these questions are of purely philosophical interest, one methodological conclusion is inescapable: Unless the hypotheses in question are of the simplist variety (such as single statements), choice of a theory on empirical grounds is an ambiguous affair.

Although the reasoning may differ, the conclusions reached here share much with those of opponents of "predictivism" in economics, that term meaning a single-minded emphasis on prediction as the quintessential criterion of theory choice.[45] At this time, it seems that future work will be directed toward investigating the possibilities for rational theory choice in economics. Such investigations can be undertaken by studying how that choice has been effected in the past; more specifically, since prediction is not a straightforward criterion of theory choice, which criteria, if any, have economists consistently employed in the past in choosing between theories? We may also study how we want it to be done in the future, that is, can a set of rational canons of theory choice be reconstructed? The works of Kuhn, Lakatos, and Feyerabend are especially relevant here.[46]

Theoretical Terms

Mature positivists believed that all legitimate scientific theories can be reconstructed as mechanical, axiomatic, hypothetico-deductive systems. These are devoid of empirical content until certain terms are given an empirical interpretation. The ontological status of theoretical terms is unproblematic; instead of engaging in the instrumentalist-realist debates of earlier years (instrumentalists deny ontological status to theoretical terms; realists assert they make useful references), mature positivists circumvented the issue by declaring that theoretical terms do not gain meaningfulness directly, but are accorded cognitive significance to the extent that the theory in which they are embedded is confirmed.

Our remarks on the structure of theories will be brief. Peter Achinstein was among the first to recognize that there are many different types of theories in science. In listing six conditions which he feels are common to most theories, he was forced to enumerate only the most general characteristics.[47] A further broadening of the definition of *theory* is evident in many of the papers given at a symposium on the structure of theories in 1969.[48] Perhaps more directly relevant to the social sciences is the work of such philosophers as Abraham Kaplan and Paul Diesing, whose notions of hierarchical and concatenated theoretical structures have recently found two advocates in economics.[49]

The positivist solution to the problem of the status of theoretical terms rests on the possibility of clearly distinguishing between terms which are and are not theoretical. Indeed, one's interpretation of the structure of theories as well as one's ability to test a theory rest on one's being able to make such a dichotomization. Positivists traditionally drew the distinction on observational grounds: Nontheoretical terms can be expressed in the neutral observation language, while theoretical terms need not be so definable. This approach assumes, in good positivist style, that what is meant by "observation" and "observation terms" is known; that there is a "protocol domain" of "brute atomic facts" which are describable in a "neutral observation language," the existence and recognition of which poses no trouble for any competent observer. All of these beliefs have come under sustained fire by contemporary philosophers of science. The attacks may be summarized briefly. (1) There is no one-to-one correspondence between theoretical terms and nonobservables, on the one hand, and nontheoretical terms and observables, on the other.[50] (2) There exists no sharp distinction between what is observable and what is not.[51] (3) Any observation requires both *selection* and *interpretation* by the observer, and both activities will be colored by the observer's biases, interests, perspectives, past experiences, and anticipated results.[52] Both Kuhn and Feyerabend have extended this line of thinking in arguing that many disagreements between advocates of competing theories occur because scientists use the same words to refer to different phenomena. This confusion arises because one's observations are determined by the theoretical framework from within which one is working.[53] (4) The arguments above attempt to establish that both observation and the meaning of terms are theory laden; still others have argued that the facts themselves are theory dependent. Roman Harré, for example, notes that the only facts which are independent of theory are private to individuals, facts in the public domain being "affected by all sorts of influences, particularly from previous knowledge and upon which their exact form and our confidence in them depend."[54]

Recent work in the philosophy of science has abandoned the positivist distinction between theoretical and nontheoretical terms, focusing instead on the notion of theory dependence and its implications for the commensurability and comparability of competing paradigms. The objectivity of science is challenged if the theory dependence idea is taken too far.

Much of the work discussed above has applications in economics. That various frameworks for investigating economic reality may employ apparently similar but, because of theory dependence, actually different terms is a useful point of departure both for historians of thought and for economists trying to make sense of the conflicting claims of contemporary opposing camps, such as "the two Cambridges." It should also be mentioned

that the realist-instrumentalist debate has begun anew in the philosophy of science, with many (although not all) of the major protagonists aligning themselves with the realists. A reevaluation of Friedman's position on the status of "assumptions" thus seems in order.[55]

Explanation

Positivist philosophers of science accepted the two "covering law" models as adequate characterizations of explanation as it takes place in science. The covering law approach, it should be remembered, was a considerable advance over the ideas of nineteenth-century positivists, who either denied that explanation took place at all in science, or equated it with correlation. The goal of the covering law models is to reintroduce the notion of explanation in science, but to do so in a manner sufficiently cautious to avoid illegitimate pseudo-explanations. The optimal analysis of explanation, then, should place enough restrictions on the definition of *explanation* to rule out metaphysical or "ultimate" explanations, but should be sufficiently lax to allow those explanations which scientists traditionally accept as legitimate. Many critics of the covering law models feel that these are inadequate to the task.

Hempel's early statement that "an explanation is not fully adequate unless its explanans, if taken into account in time, could have served as a basis for predicting the phenomena under consideration"[56] has been termed the symmetry thesis. It has met considerable opposition, most notably from Michael Scriven. The two philosophers locked horns at a seminar on the philosophy of science in the early 1960s, but a careful reading of their exchanges indicates that they are discussing two distinct concepts. Hempel eventually backed off from his earlier position and insisted that his is a purely logical analysis of the structure of scientific explanation. Specifically, he asserts that the structures of an explanation and a prediction are symmetrical in terms of the deductive or inductive *inferability* of the explanandum, but *not* in terms of its "assertability per se."[57] Scriven accepts that Hempel is arguing only the symmetry of inferability,[58] but he stresses that such logical analyses should not mislead people into believing that all explanations can be potential predictions. He particularly emphasizes those asymmetrical cases in which the explanandum can be explained in terms of prior states, but that such knowledge does not allow predictions of the explanandum. For example, one may be able to explain a suicide by reference to certain antecedent conditions, but simply because those conditions hold does not mean there is an inductively high probability of the occurrence of a particular suicide. Hempel and Scriven apparently are talking beyond each other: The former is concerned with the logical structure of explanation, especially that of the D-N model; the latter focuses on explanations which he considers legitimate, but which could not be used to make predictions which have a high inductive probability.

Essentially, the same arguments are involved when we consider Hempel's assertion that virtually every legitimate explanation in science must be able to be reformulated in terms of one of the two covering law models. Opponents generally assert that there are many forms of legitimate explanation which do not meet that criterion. There are functional explanations whereby, for example, characteristics of an organism, society, or some other phenomenon are explained by reference to certain ends or purposes the characteristics are said to serve. There are motivational explanations, which cite purposive behavior or voluntary actions; in such cases the antecedent conditions comprising the explanans cannot be linked to the explanandum in any direct causal way. Hempel states of this latter form that "its soundness requires that the motivational assumptions in question be capable of test, and that suitable general laws be available to lend explanatory power to the assumed motives."[59] Nagel claims that either of these types of explanations, which he labels teleological, must be translatable into nonteleological ones if they are to be considered legitimate. Another mode of explanation is "the method of intuitive understanding" (*Verstehen*), the validity of which was hotly debated in earlier decades.[60]

The alternative forms of explanation mentioned above do not make use of general or statistical laws; thus they fit neither the D-N nor the I-P covering law models. Those explanations that can form the basis for predictions do not generally have inductively high probabilities of occurrence. However, this debate is not resolvable on the grounds expressed earlier. *If* one believes that the above are legitimate modes of explanation, then the covering law models must be viewed as unnecessarily restrictive. But as long as one *defines* explanation according to the D-N and I-P models, instances which do not fit those models can easily be dismissed by positivists as illegitimate.

Sylvain Bromberger takes another approach which is costly for the D-N model: He offers an example of an "explanation" which fits the D-N model, but which clearly should not qualify as a legitimate explanation.

> There is a point on Fifth Avenue, M feet away from the base of the Empire State Building, at which a ray of light coming from the tip of the building makes an angle of 0 degrees with a line to the base of the building. From the laws of geometric optics, together with the "antecedent" conditions that the distance is M feet, the angle 0 degrees, it is possible to deduce that the Empire State Building has a height of H feet. Any high school student could set up the deduction given actual numerical values. By doing so, he would not, however, have *explained* why the Empire State Building has a height of H feet, nor would he have *answered* the question, "Why does the Empire State Building have a height of H feet?" nor would an exposition of the deduction be the explanation of or answer to (either implicitly or explicitly) why the Empire State Building has a height of H feet.[61]

Bromberger's arguments do great damage to the D-N model, since its primary advantage is to rule out from science those explanations which scientists would not want to consider legitimate. Paradoxically, then, the covering law models have been criticized for being too stringent and for being too lax.

A number of serious doubts have been raised concerning the adequacy of the covering law models in describing all the types of explanation which occur in science. The problem is not that these models are useless aids in understanding certain types of explanation; rather, it lies in the positivist claim that any explanation which cannot be reconstructed to fit one of the models is somehow deficient. Alternative approaches to explanation now under development do not call for an elimination of the covering law models, but view them as a subset of the universe of explanation models.[62]

Until very recently, economists have had almost nothing of value to say about the nature of explanation in their discipline. One reason is that excellent discussions of this topic have taken place within the philosophy of social sciences, but economists have traditionally paid little attention to that discipline, favoring instead (when they listen to philosophers at all) pronouncements from the philosophy of the natural sciences. Another is that Samuelson's advocacy of the nineteenth-century view of explanation in the early 1960s obfuscated all intelligible discussion of this area until Stanley Wong's rebuttal in 1973. Happily, work has now begun on applying both the covering law models and their alternatives to analyses of explanation as it takes place in economics.[63]

Conclusions

One goal of this article has been to trace the rise and decline of positivist dominance within the philosophy of science. That vision was an awesomely powerful one, for it promised to make scientific thinking truly "scientific," to banish the speculative and the unverifiable. It captured and enthralled the minds of some of the greatest philosophers and scientists of this century.

However, that epoch is now at an end. As Frederick Suppe has written, "positivism today truly belongs to the history of the philosophy of science, and its influence is that of a movement historically important in shaping the landscape of a much-changed contemporary philosophy of science."[64] Significantly the attacks against positivism documented in the previous section were not made by disgruntled social scientists who disdain quantification, scientism, or predictivism. Rather, they are the criticisms of philosophers of science who, after studying the evolution of various (usually natural science) disciplines and the actual practice of scientists within them, find the positivist vision of the scientific enterprise lacking.

A second goal of this article has been to show that a working knowledge of the philosophy of science can aid economic methodologists in at least three ways: in clarifying various methodological positions and debates, in suggesting new areas of research, and, to a lesser degree, in evaluating the methodological positions of various economists.

One question not addressed here is whether economics should follow the lead of contemporary philosophy of science in rejecting positivism. Many economists today still believe that economics is a positivist discipline. The rejection of positivism within the philosophy of science at least suggests that such a belief may be misguided. It is to be hoped that future methodological work in economics will establish which aspects of positivism are to be retained and which are to be rejected by practitioners of the dismal science. Clearly, much work remains to be done.

A word of caution is in order: We must learn from the mistakes of the past. Most formulations within the current philosophy of science broaden the categories once so painstakingly delimited by positivist analysts. In the new environment, many more types of investigations will be granted legitimate status. Such a broadening can aid insight, but there are dangers. History tells us that positivism was (in part) a response to the speculative excesses of nineteenth-century idealistic philosophy. The pendulum must not be allowed to swing back; such speculative abuses must not be allowed to reenter science if future work in such fields as the methodology of economics is to have any meaning. Karl Popper called for a combination of bold conjectures and critical refutations in his vision of the scientific enterprise. Whether or not a workable and meaningful blend of rigor and creativity will characterize the analyses and pronouncements of the methodologists of tomorrow will be a crucial determinant of the progress of the discipline.

Notes

1. A fundamental problem in organizing an article such as this is how to keep it to a manageable length. Clearly, the comments on "positivist" economic methodology which comprise the third section could be developed at much greater length. The opportunity cost is high; my comments on philosophy of science would of necessity be shorter. I have opted for treating those topics least familiar to economists in greatest detail; hence, the philosophical discussions dominate.
2. As one referee commented, Popper has often been viewed as a positivist by economists. His rejection of this kind of interpretation is evident in *Conjectures and Refutations* (New York: Basic Books, 1962), especially chapters 1, 10, and 11. See also Imre Lakatos, "Falsification and the Methodology of Scientific Research Programmes," in *Criticism and the Growth of Knowledge*, edited by Imre Lakatos and Alan Musgrave (Cambridge: the University Press, 1970), pp. 91–196, for an analysis of Popper's role in twentieth-century philosophy.
3. It should be noted that the *Journal of Economic Issues* and the *History of Political Economy* have been leaders among the economic journals in bringing these new analyses to the attention of economists.
4. Auguste Comte first coined the term *positivism*; three early analysts whose work greatly influenced the Vienna Circle were Ernst Mach, Bertrand Russell, and Ludwig Wittgenstein. See Otto Neurath, *Le developpement du cercle du Vienne et l'avenir de l'empiricisme logique* (Paris: Hermann, 1935); and Joergen Joergensen, "The Development of Logical Empiricism," in *International Encyclopedia of Unified Science* (Chicago: University of Chicago Press, 1951), vol. 2, no. 9, pp. 27–28. The logical positivists shared many of the same general beliefs of their precursors, but they also saw many analytic differences. It is for this reason that Rudolf Carnap suggested in the 1930s that the logical positivists would be better labeled "logical empiricists." See his "Testability and Meaning," *Philosophy of Science* 3 (1936): 442, n. 2.
5. Abraham Kaplan, "Positivism," in *International Encyclopedia of the Social Sciences* (Chicago: University of Chicago Press, 1968), vol. 12, p. 389.
6. Carl G. Hempel, "The Empiricist Criterion of Meaning," in *Logical Positivism*, edited by A. J. Ayer (Glencoe, Ill.: The Free Press, 1959), p. 108. The analytic-synthetic distinction had predecessors in Western philosophy, for example, Hume's distinction between "Relations of Ideas" and "Matters of Fact." See David Hume, *An Inquiry Concerning Human Understanding* (Indianapolis: Library of Liberal Arts, 1955 [1748]), p. 40 ff.
7. Kaplan, "Positivism," p. 390.
8. Hempel, "Empiricist Criterion," p. 110.
9. Ernst Mach, *Popular Scientific Lectures,* translated by T. McCormack (Chicago: Open Court Publishing Co., 1898).
10. Percy Bridgman, *The Logic of Modern Physics* (New York: Macmillan, 1927), pp. 5–7.
11. For example, see his "Theories as Partially Interpreted Formal Systems,"

in *Readings in the Philosophy of Science,* edited by Baruch Brody (Englewood Cliffs, N.J.: Prentice-Hall, 1970), pp. 190–99.

12. A. J. Ayer, "Editor's Introduction," in Ayer, ed., *Logical Positivism,* p. 21.
13. Karl Popper, *The Logic of Scientific Discovery* (New York: Harper and Row, 1959), p. 36 (originally published as *Logik de Forschung* in 1934).
13. Ibid., pp. 40–42. The brief discussion of falsifiability in the text does not do justice to Popper's critique, which goes far deeper than a quibble over a criterion of cognitive significance. The idea of consciously trying to falsify (perhaps one's own treasured) hypotheses is tied in his work to a critical attitude which is the hallmark of the scientific enterprise: It is only through constant and unencumbered efforts to disprove hypotheses that science is able to establish provisionally true laws and ultimately to advance.
15. Ayer, "Editor's Introduction," p. 14.
16. Ayer, *Language, Truth and Logic,* 2d ed. (New York: Dover, 1946); Carnap, "Testability," section 4; and Hempel, "Empiricist Criterion," pp. 114–18.
17. For example, see his *Continuum of Inductive Methods* (Chicago: University of Chicago Press, 1952).
18. W. V. O. Quine, "Two Dogmas of Empiricism," in *From a Logical Point of View* (Cambridge, Mass.: Harvard University Press, 1953). Hempel's discussion of confirmability in his "Empiricist Criteron" is also helpful.
19. See Carnap, "Testability," sections 7 and 8; and the discussion in Carl Hempel, "The Theoretician's Dilemma," in *Minnesota Studies in the Philosophy of Science,* edited by H. Feigl, G. Maxwell, and M. Scriven (Minneapolis: University of Minnesota Press, 1956), vol. 2.
20. Hempel, "Dilemma," p. 72.
21. Ibid., p. 72.
22. Mach, *Scientific Lectures*; August Comte, *A General View of Positivism,* translated by J. H. Bridges, in *The European Philosophers from Descartes to Nietzsche,* edited by Monroe Beardsley (New York: The Modern Library, 1960).
23. Carl Hempel and Paul Oppenheim, "Studies in the Logic of Explanation," *Philosophy of Science* 15 (1948): 137–38.
24. Carl Hempel, "Explanation and Prediction by Covering Laws," in *Philosophy of Science: The Delaware Seminar,* edited by Bernard Baumrin (New York: John Wiley and Sons, 1963).
25. Ibid., p. 110.
26. Hempel and Oppenheim, "Studies," p. 138.
27. Ibid., p. 140.
28. The phenomena which have been defined as economic have changed through time. See Israel Kirzner, *The Economic Point of View* (Kansas City: Sheed and Ward, 1960).
29. Mark Blaug comments that the tension between description and prescription "surrounds the historiography of science: either we infer our scientific methodology from the history of science, which commits the fallacy of induction, or we preach our methodology and rewrite history accordingly, which smacks of 'false consciousness.' " See his "Kuhn versus La-

katos or Paradigms versus Research Programmes in the History of Economics," in *Method and Appraisal in Economics,* edited by Spiro Latsis (Cambridge: the University Press, 1976), p. 158.

30. T. W. Hutchison, *The Significance and Basic Postulates of Economic Theory* (London: Macmillan, 1938); Fritz Machlup, "The Problem of Verification in Economics," *Southern Economic Journal* 22 (July 1955): 1–21, and "Operationalism and Pure Theory in Economics," in *The Structure of Economic Science: Essays on Methodology,* edited by Sherman Krupp (Englewood Cliffs, N.J.: Prentice-Hall, 1966); and Andreas Papandreou, *Economics as a Science* (Chicago: J. B. Lippencott, 1958).
31. Hutchison, *Significance*; Paul Samuelson, "Discussion," *American Economic Review* 53 (May 1963): 231–36, and "Theory and Realism: A Reply," *American Economic Review* 54 (September 1964): 736–39; and Latsis, ed., *Method and Appraisal.*
32. Bruce Caldwell, "Two Suggestions for the Improvement of Methodological Work in Economics," *American Economist* 23 (Fall 1979): 352–57.
33. Machlup, "Problem of Verification," pp. 5–7.
34. Ibid., pp. 8–10.
35. T. W. Hutchison, "Professor Machlup on Verification in Economics," *Southern Economic Journal* 22 (April 1956): 476–79.
36. Ibid., pp. 479–82.
37. This contrast between the two positions is drawn too starkly; Hutchison, for example, does not "require" all statements to be independently testable, but he does view such attempts at testing as scientific progress.
38. See pages 54–60 for a more detailed discussion of the philosophical issues.
39. Jack Melitz, "Friedman and Machlup on the Significance of Testing Economic Assumptions," *Journal of Political Economy* 73 (February 1965): 54–55.
40. See Carl Hempel, *Philosophy of Natural Science* (Englewood Cliffs, N.J.: Prentice-Hall, 1966), chapters 3 and 4.
41. Ibid., pp. 23–24.
42. See the articles by Hempel, Nelson Goodman, Richard Grandy, and J. W. N. Watkins in Brody, ed., *Readings.*
43. Carnap's work along these lines is the most well known. See his "The Aim of Inductive Logic," in *Logic, Methodology and Philosophy of Science,* edited by Ernest Nagel et al. (Stanford: Stanford University Press, 1962). A critique is found in Ernest Nagel, "Carnap's Theory of Induction," in Brody, ed., *Readings.*
44. Popper, *Conjectures,* chapter 10.
45. Almost every economist writing on methodology has had to address, in one way or another, the problem of the role of prediction in economics. A sampling of contributors who have questioned the ability of economic science to generate adequate predictions would include Sidney Schoeffler, *The Failure of Economics* (Cambridge, Mass.: Harvard University Press, 1955); Ludwig Von Mises, *The Ultimate Foundation of Economic Science* (Kansas City: Sheed, Andrews and McMeel, 1962); Alfred Chalk, "Concepts of Change and the Role of Predictability in Economics," *History of Political Economy* 2 (Spring 1970): 97–117; Charles Wilber and

Robert Harrison, "The Methodological Basis of Institutional Economics: Pattern Model, Storytelling, and Holism," *Journal of Economic Issues* 12 (March 1978): 61–89; and Emile Grunberg, " 'Complexity' and 'Open Systems' in Economic Discourse," *Journal of Economic Issues* 12 (September 1978): 541–60.

46. Thomas Kuhn, *The Structure of Scientific Revolutions,* enlarged ed. (Chicago: University of Chicago Press, 1970); Lakatos, "Falsification"; and Paul K. Feyerabend, *Against Method: Outline of an Anarchistic Theory of Knowledge* (London: New Left Review, 1975). Examples of applications for economics are found in Latsis, ed., *Method and Appraisal.*
47. Peter Achinstein, *Concepts of Science: A Philosophical Analysis* (Baltimore: Johns Hopkins Press, 1968), chapter 4.
48. Frederick Suppe, ed., *The Structure of Scientific Theories,* 2d ed. (Urbana: University of Illinois Press, 1977). Suppe's "Critical Introduction" and "Afterword," which comprise almost half of the volume, offer a superlative summary and analysis of mature positivism, the growth of knowledge theories, and new directions in the philosophy of science. His lengthy bibliography is nearly comprehensive, omitting only some of the most recent contributions.
49. Paul Diesing, *Patterns of Discovery in the Social Sciences* (Chicago: Aldine-Atherton, 1971); and Abraham Kaplan, *The Conduct of Inquiry: Methodology for Behavioral Science* (San Francisco: Chandler Publishing Co., 1964); see Wilber and Harrison, "Methodological Basis," for an application to economics.
50. Hilary Putnam, "What Theories Are Not," in *Theories and Observation in Science,* edited by Richard Grandy (Englewood Cliffs, N.J.: Prentice-Hall, 1973).
51. Grandy, "Introduction," in Grandy, ed., *Theories,* p. 6.
52. Norwood Hanson, *Patterns of Discovery* (Cambridge: the University Press, 1958), chapters 1 and 2.
53. Kuhn, *Scientific Revolutions*; P. K. Feyerabend, "Explanation, Reduction and Empiricism," in *Minnesota Studies in the Philosophy of Science,* edited by H. Feigl and G. Maxwell (Minneapolis: University of Minnesota Press, 1962), vol. 3.
54. Roman Harré, *The Philosophies of Science* (London: Oxford University Press, 1972), pp. 43–44.
55. Friedman is interpreted as an instrumentalist in Stanley Wong, "The F-Twist and the Methodology of Paul Samuelson," *American Economic Review* 63 (June 1973): 312–25. If this interpretation is correct, then a reevaluation of that position may be in order.
56. Hempel and Oppenheim, "Studies," p. 138.
57. Hempel, "Explanation and Prediction," pp. 125–31.
58. Michael Scriven, "The Temporal Asymmetry of Explanations and Predictions," in Baumrin, ed., *Philosophy of Science,* p. 102.
59. Hempel and Oppenheim, "Studies," section 4. See Hempel, "The Logic of Functional Analysis," in Brody, ed., *Readings,* pp. 121–43; and Donald Davidson, "Actions, Reasons and Causes," *Journal of Philosophy* 60 (Fall 1963): 697.

60. Ernest Nagel, "Teleological Explanations and Teleological Systems," p. 107, in Brody, ed., *Readings*; see the readings on *Verstehen* in *Philosophy of the Social Sciences: A Reader,* edited by Maurice Natanson (New York: Random House, 1963).
61. Sylvain Bromberger, "Why-Questions," in Brody, ed., *Readings,* p. 71.
62. Alternatives include the models of Kaplan and Diesing, already mentioned; Harré's realist approach, in Roman Harré, *The Principles of Scientific Thinking* (Chicago: University of Chicago Press, 1970), chapter 2; and Hesse's explanation as "metaphoric redescription" in Mary Hesse, *Models and Analogies in Science* (Notre Dame: University of Notre Dame Press, 1966).
63. Wong, "F-Twist." For alternative models, see Wilber and Harrison, "Methodological Basis." These authors' application of alternative models of explanation to institutionalist explanation is an insightful addition to the literature; their linking of Friedman (who rejects explanation in science) with the covering law models, and their implicit identification of positivism as simply those models, must be questioned, however.
64. Suppe, *Scientific Theories,* p. 632.