Methodological Postscript

My arguments and conclusions concerning theories of capital and interest are complete. I shall now return to some questions concerning my methodology, and explain in general terms what a philosophical inquiry into an economic theory is, why such inquiries are worth undertaking, and what place such work should have in the philosophy of science.

Although philosophers of science have always been interested in the actual work of scientists, there was a strong turn, especially in the 1960s and 1970s away from prescribing how science ought ideally to proceed and toward studying more carefully how science has proceeded.1 In part this turn has been a reaction to previous work in philosophy of science, which to many seemed misguided and largely irrelevant to the sciences. In part this change reflects a general skepticism about the possibility of doing traditional foundationalist epistemology. Such skepticism is itself a reaction to the failure of the foundationalist program of the logical empiricists. The contemporary turn toward careful empirical study of the sciences constitutes a new program for the philosophy of science, which I shall call “empirical philosophy of science” or “the empirical approach to the philosophy of science.”

1. Empirical Philosophy of Science

The credo of the empirical approach may be stated trenchantly and simplistically as follows: 

The philosophy of science is itself an empirical science. All conclusions about the scientific enterprise that the philosopher of science

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draws are, or should be, scientific conclusions and must be defended in the same way or ways that the results of the sciences are defended. Pronouncements by the philosopher of science about the goals of science, the bases upon which scientists accept various theories, or any other feature of science, should be regarded as scientific claims. One should assess them as one would assess the various assertions the sciences make.

The empirical approach denies that epistemology can be distinct from empirical study of the human cognitive faculties, the history of the human search for knowledge, and the general progress of the sciences. In Quine's terminology (1969), epistemology is "naturalized." It aims no longer to justify kinds of knowledge claims in terms of an epistemologically prior (self-evident or indubitable) foundation. In justification one must always take for granted scientific and every-day knowledge. "Justifying" an assertion consists solely of showing that it is supported by evidence in the way or ways that scientific claims generally are. When claims to know are challenged, the best one can do is to explain scientifically how one knows (or can know) what one does. As empiricists, philosophers, scientists, and laypersons accept these explanations ultimately because they help organize, and are supported by, experience. There is no other ultimate warrant. The goal is to construct empirical theories of human knowing which are consistent with theories of other subject matters and which explain how one can know all these theories.

Is such a goal sensible? One might argue that a scientific explanation of the acquisition of knowledge is inconceivable because empirical scientists cannot explain why some methods of acquiring beliefs justify beliefs, while others do not. The claim is that determining standards for justified belief is not and cannot be the task of any empirical science. I do not find this claim compelling. A psychologist might, for example, by means of sufficiently cunning experiments, be able to show that certain methods of acquiring beliefs are more likely to lead to true beliefs in certain circumstances than are others. Moreover, a psychologist who could show why some grounds for believing lead to more reliable beliefs would be in a position to explain how in certain circumstances people acquire knowledge. There are obviously circularities here, but they are benign. I thus see no reason to believe it impossible that philosophers can explain how we know what we do.

The empirical approach to philosophy of science is not purely "descriptive." Although philosophers' claims about the sciences should be defended in part by showing their consistency with scientific practice, empirical philosophers of science can still assess the work of scientists and offer advice and instruction. Philosophers of science can sometimes contribute directly to the scientific disciplines they study. What scholars learn about acquiring scientific knowledge provides the basis for such assessment and advice. Empirical philosophy of science thus does not reduce to history of science. Not all of the history of science is relevant to the questions with which the philosophy of science is concerned. The precise details of how scientific results are reached are only important to philosophers of science when they help them understand how scientists come to know. On the other hand, there are other sources of evidence (for example, from psychology) about how humans acquire knowledge.

2. The Epistemological Circle

In attempting to study science as an empirical philosopher of science, one enters a logical circle with at least four forms or manifestations. Such an "epistemological circle" is, in fact, common to every theory of knowledge. Hegel states the predicament well:

We ought, says Kant, to become acquainted with the instrument [of cognition], before we undertake the work for which it is to be employed; for if the instrument be insufficient, all our trouble will be spent in vain. The plausibility of this suggestion has won for it general assent and admiration; ... In the case of other instruments, we can try and criticize them in other ways than by setting about the special work for which they are destined. But the examination of knowledge can only be carried out by an act of knowledge. To examine this so-called instrument is the same thing as to know it. But to seek to know before we know is as absurd as the wise resolution of Scholasticus, not to venture into the water until he had learned to swim. (1830:14)

Some theories of knowledge find their way through these difficulties easily. If one maintains that there are self-warranting truths, for example, then one can easily meet the demand that one know some of the results of epistemology in order to do epistemology. The empirical philosopher of science, on the other hand, has serious problems.

The first form of the epistemological circle is perhaps the most striking. Empirical philosophy of science is itself a science. In doing philosophy of science empirically, one should thus follow scientific method or scientific methods. But one of the goals of the empirical philosophy of science is to find out what scientific methods are. It thus seems that one must already know at least tacitly what one is supposed
to find out. If one does not already know how to do science, how can one find out (scientifically) how to do science?

The circularity is not vicious. Empirical philosophers of science disavow seeking any justification for scientific knowledge other than the broadest possible coherence among theories, including various theories of acquiring knowledge, and perceptual beliefs. There is nothing improper in beginning empirical philosophy of science in midstream, believing that one already knows something tacitly or consciously about how to acquire knowledge. Justification, although philosophically interesting, is not the immediate task. Investigating scientific knowledge in accordance with one's initial conception of scientific investigation, one improves and articulates this conception (and revises the procedures for carrying out this improving and articulating) as one proceeds. There is no guarantee that one will not be forced to change one's beliefs and procedures. (My own views concerning, for example, theory structure in economics changed enormously in the course of writing this book.) Although philosophers cannot start learning about the sciences from scratch, they can learn about the sciences. This circle remains disturbing, since many philosophers find it difficult to eschew wholeheartedly searching for justification for knowledge that goes beyond such broad coherence among beliefs. Contemporary philosophers, however, show little enthusiasm for any alternative. The talk of "coherence" here should not be misconstrued. Scientific and philosophical theories must be consistent with perceptual beliefs.

When one questions the philosophical theses upon which the empirical approach is based, the epistemological circle manifests itself in a second way. Suppose some traditional philosopher maintains, as many have, that there is knowledge to be gained in epistemology which is different in kind from the empirical knowledge the sciences provide. Such a philosopher could accuse the empirical philosopher of science of avoiding the real epistemological tasks of assessing and justifying (not merely explaining) scientific knowledge. Such a philosopher would deny that this book is a work of philosophy. In answer to such a challenge; the empirical philosopher of science must either deny that there are any such justificatory tasks or deny that there is any way to tackle them. But on what basis is either of these denials to be made? The grounds must themselves be the results of empirical philosophy of science (or of naturalized epistemology) or an anticipation of those results. But the traditional philosopher of science denies that philosophers ought to rely on (or ought to rely only on) such grounds. All empirical philosophers of science can do is to repeat their (scientific) reasons for surrendering the ambitions of traditional foundationalist epistemology. They can, of course, also criticize in detail epistemologies which attempt to do more.

The third way in which the epistemological circle manifests itself is somewhat different. Much of the evidence upon which empirical philosophy of science bases its conclusions comes from the history of science. Unless empirical philosophers of science are content only to describe all cognitive enterprises whatsoever, they must add to the presuppositions of their investigations discriminations between good and bad science, between science and pseudo-science, and between knowledge and conjecture. These initial discriminations are revisable as the inquiry proceeds, but they are indispensable. To contribute to understanding how humans acquire knowledge by investigating theories of capital and interest, I had to assess those theories. To make an informed assessment, I had to do economics—to find out what there is to be learned at present about capital and interest. The philosopher of economics must attempt to be a competent economic theorist. Standards to assess scientific work are also needed. Yet the standards of assessment and the methods to be employed in learning about economics had to be anticipated and could only be clarified in the course of the investigation. In trying to learn more, philosophers need to rely on all the knowledge they think they have, even if some of it is not well-founded and turns out not to have been knowledge at all.

Since empirical philosophers of science must begin by discriminating knowledge from superstition and science from pseudo-science, is not the way open for astrologers, for example, to begin by regarding astrology as the paradigm of a science? Might they not then come up with an empirical philosophy of science which shows how humans can acquire such astrological knowledge? After all, a crucial test, among astrologers, for any philosophical account of science will be whether it successfully shows how people can know astrology. But if astrologers can invent an empirical philosophy of science that "justifies" the claims of astrology, what does more orthodox empirical philosophy of science, which criticizes astrology, accomplish? Are not philosophers merely rationalizing prevailing prejudices?

Once one denies that there is any certain or self-evident foundation for human knowledge, the possibility of consistent and incommensurable knowledge systems cannot be ruled out. The fact that the astrologer (or theologian or paranoid) begins with different beliefs does not, however, itself show that such incommensurable knowledge systems can be constructed. Alternative philosophies of science are not easily created. It is hard enough to come up with one. Astrologers who attempt to develop a naturalized epistemology which coheres with both
their purported knowledge of astrology and their nonastrological knowledge of the everyday world will face a difficult task. If they find that epistemology is to be done as a nonastrological science is done, they will discover that their attachment to astrology is irrational. If they develop some other sort of epistemology, they might (in some sense) be able to come up with a coherent body of knowledge. Yet this body of knowledge would have to be drastically different from the body of scientific knowledge. Given this virtual incommensurability, one would not expect that orthodox philosophers could show astrologers that they are in error. Developing an empirical philosophy of science is not a trivial task of rationalization, but an arduous systematic task.

The fourth form of the epistemological circle concerns the relations between empirical philosophy of science and empirical philosophical investigations of particular sciences. The conclusions of empirical philosophy of science rest largely on investigation of the history and structure of actual sciences. To that extent empirical philosophy of science in general depends on empirical investigations of particular theories, disciplines, incidents, etc. General conclusions in the philosophy of science must rest on particular inquiries into particular sciences. Yet in order to investigate some limited area in science, like theories of capital and interest, one needs a great deal of philosophical apparatus. I had no choice except hesitantly and critically to rely on philosophical models of theories, explanations, laws, confirmation, objectivity, and the like. Once again the philosopher must anticipate the answers to his or her questions. If the conclusions of current philosophy of science were already well supported and already merited the esteem and confidence of philosophers and scientists, these anticipations would not be troubling. But a great deal of "established" philosophy of science is poorly confirmed and has been cast into doubt. In this last form the epistemological circle presents a pressing practical problem. I have made use of whatever philosophical wisdom I could; but the limitations in that wisdom have been palpable. Yet there is no way to contribute to the philosophy of economics or the philosophy of science in general except to rely on (while attempting to improve upon) conclusions of the past.

Empirical philosophers of science are caught in at least these four ways in the epistemological circle. Does this fact make dubious an empirical approach to the philosophy of science? Should the reader worry about whether the conclusions of this book are prejudiced by the philosophical presuppositions with which I began? Note that many of these presuppositions come from less self-conscious "investiga-

3. Philosophy of Science as a Social Science

Empirical philosophy of science, if itself a science at all, is a social science (where "social science" is understood to include history and psychology). Thus it may be that the structure, methods, etc. of philosophy of science will be unlike those of the natural sciences. As already mentioned, social scientific naturalists argue that, in crucial respects (goals, methods of justification, logical structure, fundamental ontology, or whatever), the social sciences are or should be identical to the natural sciences. Anti-naturalists argue for an essential difference in one or more of these respects. It seems to me that the empirical approach to the philosophy of science ought not itself to prejudice this debate. Both naturalists and anti-naturalists ought to be able to adopt empirical approaches to the philosophy of science. Otherwise it is hard to see how the philosophy of science can contribute to clarifying and resolving the many disputes between them. Individual empirical philosophers of science may prejudice the issue. I began by treating theories of capital and interest as if they were natural scientific theories.

The empirical approach to the philosophy of science does not presuppose that the structure, methods, etc. of the social sciences (and thus of the philosophy of science itself) are the same as those of the
natural sciences. In fact, philosophers of science rarely study the sciences the way physicists study motion or matter. The actual practice of empirical philosophy of science is diverse. Much of it will remain for the foreseeable future more like intellectual history than like physics. While the object of the philosophy of science is all of science, its structures, methodologies, and the like should be those of some of the social sciences. The worst social scientific naturalists can say of this methodological clarification is that it is empty.

Notice that the question of social scientific naturalism is only a special form of the question of whether the methods, structure, goals, and so forth are, at a suitable level of generality, one and the same for all sciences in all historical periods. Although philosophers may sometimes have to beg this general question, they should not forget that it is there. It should not be a condition of doing the philosophy of science empirically that this question have only one answer. Otherwise one could not learn its answer in doing philosophy.

4. Method and Presuppositions

If the above view is correct, one cannot make philosophical pronouncements about economics or any other science without studying the relevant theories, debates, experiments, communities, etc. in detail. Without such careful study one will lack evidence for one's philosophical conclusions. The comments above should make clear why I undertook a "philosophical inquiry" into capital theory. Only through such an inquiry was I able to establish my views of the definition, subject matter, structure, and methodology of economic theory. Let me add a few words about how I proceeded in making this inquiry.

Actually, I began with questions. I had general questions, as outlined in my introduction, as well as such specific questions as: What is the Cambridge Controversy all about? Why do the participants rely on fictions about oversimplified states of affairs? What can one learn about economics from one-or-two-commodity stationary equilibrium models? Do general equilibrium theories resolve the difficulties the Cambridge critics point to? Are general equilibrium theories adequate theories? Do they explain anything? What is Sraffa doing in his Production of Commodities by Means of Commodities? Does his work represent an alternative to general equilibrium theories? What, if any, role does ideology have in capital theory and controversies concerning capital theory? There were many other questions besides, and, of course, many more arose in attempting to answer these. How were such questions answered?

The general technique—to study the works of economists and philosophers which develop, apply, and discuss the issues—is certain not novel. In the actual course of my study, I had to rely heavily on the tentative results of contemporary philosophy of science and on initial judgments concerning the nature and worth of economic theory and of economics as a discipline. Merely to classify and to order what one finds when reading economics books demands that one have some idea of what a science is, what a theory is, what counts as a law, and so forth. The richness of philosophical work on the natural sciences and the extent of its influence made it tempting to suppose that a moderate naturalism is correct. Economists talk about their own work in many ways. As we have seen, they write about "models," "theories," "assumptions," and "predictions" and make use of previous work by epistemologists and philosophers of science. To interpret their comments and to describe accurately what they have done, one needs to know a great deal of philosophy of science. How else is one to decide, for example, whether the Austrian theory is even a theory?

Some of those most critical of traditional philosophy of science and most insistent on the need for a new empirical philosophy of science might object that no one knows enough philosophy now to understand the structure or methods of economics. There is some merit in this objection, but it is overstated. It would have helped, if I could have begun with solid and well-confirmed philosophical theses, but they are unavailable. A philosopher of economics studying economic theory is in the same philosophical position as any empirical philosopher of science seeking knowledge about the sciences. The only important difference is that philosophers of physics, for example, can begin with fewer doubts about the worth of the physics they study. Philosophers of physics are unlikely ever to conclude that Newton was a mediocre physicist. They can safely begin by regarding a large body of physics as "good physics." Revisions may be needed later. Philosophers of physics have, however, comparatively few practical problems deciding what to do when conventional philosophical wisdom does not fit the "good physics" studied. The difficulties facing a philosopher of economics are much greater.

Yet this contrast with philosophy of physics does not show that one should postpone philosophical examination of sciences like economics. What one learns about acquiring knowledge in physics may not apply to economics. Even if it does, philosophers of economics will probably
have to find this out through their investigations of economics. Furthermore, although the practical differences between the tasks of philosophers of economics and philosophers of physics are considerable, they are differences in degree, not in kind. Philosophers of physics cannot assume that Newton or Einstein never blundered.

How then was I to proceed, if I could not simply import categories and theses concerning theories, laws, and so forth upon which philosophers agree? When abstract general equilibrium theories failed to fit current philosophical conceptions, I could not automatically conclude that something was wrong with the economics. I had to trim, revise, and even invent philosophical categories and theses in trying to make sense of economic theory. Philosophy of economics can neither start from scratch nor rely on authoritative philosophical dicta. Cautiously and critically, the philosopher of economics must begin with the most plausible among current philosophical views of the sciences, as ill-founded and wrong-headed as they may be. There is no alternative.

Consider the long argument of chapter 7. In assessing the explanatory worth of general equilibrium accounts of the phenomena of capital and interest, I began with Hempel's models of scientific explanation. Since these have been questioned by philosophers and do not fit scientific practice easily, I could not take their validity for granted. In examining the various ways in which general equilibrium accounts of capital and interest apparently fail to fit the model, I had to ask whether the discrepancies were due to inadequacies or misapplications of the philosophical models or to inadequacies in the economics. Although I wound up defending Hempel's model of nonstatistical explanatory arguments, I argued that it must be applied loosely and that simplifications and inexact laws are legitimate in explanations. I had no choice except in this way to begin with and rely on previous work in philosophy of science.

To make sense of theories of capital and interest, one needs both philosophical apparatus to systematize what one finds, and an idea of the sort of sense to make of the theory. Histories full of rational decision-making and debate certainly "make sense" (fit many of the facts). So do histories full of stupidity, stubbornness, dishonesty, and ideological distortion. When the economist's practice conflicts with the philosopher's dicta, which should be criticized? This question often arose. The Cambridge Controversy differs considerably from philosophical models of how scientists assess competing theories. Should one "make sense" of this controversy as a different sort of rational debate? Should one "make sense" of it by concluding that it is shot full of confusion, misunderstanding, and ideological distortion? Obvious answers may be deceptive. Yet one must make such assessments. In this particular case I found plenty of misunderstanding and confusion. Locating the source of the controversy in a deep-seated disagreement about the strategy of economic theorizing, I was, however, able to find rational grounds for the convictions of both parties. From the perspective of the neoclassical theorist, the Cambridge criticisms only reveal that the problems concerning capital and interest remain unsolved. Since the models of the critics can be interpreted as equilibrium models, the neoclassical theorist sees nothing in the critic's claim to have exposed some great inadequacy in neoclassical theory. The Cambridge critics, on the other hand, are not engaged in equilibrium theorizing. They look to a piecemeal alternative. Emphasizing how little help neoclassical theory is with questions of capital accumulation, income distribution, and economic growth and development, and espousing an alternative vision of economics, the critics are prepared to reject the whole neoclassical program. The decision is not simple. There is little empirical evidence that bears on it. No wonder that the controversy has its own peculiarities.

To aggravate the difficulties of assessing the work of economists, discussions of economic issues are sometimes biased and distorted because of their importance to interests of individuals and of various social groups. As Marx luridly put it, "In the domain of Political Economy, free scientific inquiry meets not merely the same enemies as in all other domains. The peculiar nature of the material it deals with summons as foes into the field of battle the most violent, mean and malignant passions of the human breast, the Furies of private interest" (1967, 1:10). Although I doubt that it is possible to find a completely neutral starting point and to avoid commitments, the philosopher of economics can address a broader audience and a wider spectrum of issues if he or she does not start by taking any one school of economics as the paradigm for what economics should be. The philosophy of economics must struggle to avoid becoming apologetics.

My inquiry into theories of capital and interest leads me to believe that the task of the philosopher of economics should be to show that the state and development of economics manifest imperfect rationality. The standard of scientific rationality comes from or develops out of existing philosophy of science, as inadequate as it may be. One should expect to find deviations in economics because of the Furies' influence, but on the basis of what I found, I believe that these will be important exceptions and complications, not the center of the story. If one suc-
ceeds in providing a compelling account that is in accordance with these expectations, one thereby provides evidence that these expectations are correct.

Seeking to find imperfect rationality in economics comes down to looking for good reasons for whatever one finds unless there are specific grounds to expect or to substantiate bias. Given the dubiousness of many of the conclusions of economics, it is crucial to distinguish carefully between judging the enterprise to be rational and judging its results to be correct. When, according to the standards of accepted philosophy of science, some feature of, for example, equilibrium theories appears irrational, one should look both for ways of improving the philosophical model and for evidence of the influence of ideology or of simple error. I know of no precise rules to decide such cases.

The methodology of the philosophy of economics is thus vague and imprecise. It hardly evidences a dramatic new approach to the philosophy of science, such as the empirical approach might initially appear to be. What the empirical approach implies in practice are the following: (1) Philosophers should demand historical and psychological evidence for their conclusions and, insofar as that evidence is scanty (which it has been) should be hesitant about philosophical "wisdom" concerning the sciences. (2) Philosophers of science should be more willing to study and to learn from particular sciences than they sometimes have been. I hope I have shown that much can be learned by employing this modest advice.

Bibliography