Introduction

Premises assumed without evidence, or in spite of it; and conclusions drawn from them so logically, that they must necessarily be erroneous.

– Thomas Love Peacock, *Crochet Castle*

Ever since its eighteenth-century inception, the science of economics has been methodologically controversial. Even during the first half of the nineteenth century, when economics enjoyed great prestige, there were skeptics like Peacock. For economics is a peculiar science. Many of its premises are platitudes such as "Individuals can rank alternatives" or "Individuals choose what they most prefer." Other premises are simplifications such as "Commodities are infinitely divisible," or "Individuals have perfect information." On such platitudes and simplifications, such "premises assumed without evidence, or in spite of it," economists have erected a mathematically sophistical theoretical edifice, whose conclusions, although certainly not "necessarily erroneous," are nevertheless often off the mark. Yet businesses, unions, and governments employ thousands of economists and rely on them to estimate the consequences of policies. Is economics a science or isn't it?

This is a complicated question. What does it mean to assert or deny that economics is a science? To be called a science is, no doubt, an honor. As the scientific credentials of economists rise, so do consulting fees. But what question is one posing when one asks, "Is economics a science?" Is one inquiring about the goals of economics, about the methods it employs, about the conceptual structure of economic theory, or about whether economics can be reduced to physics? If economics is a science, is it the same *kind* of science as are the natural sciences?

During the last generation, interest in philosophical questions concerning economics has increased enormously. Twenty-five years ago, when I

was working on the first edition of this anthology, this interest was already growing, with philosophers, economists, other social scientists, and ordinary citizens all showing more curiosity about what sort of an intellectual discipline economics is and what sort of credence its claims merit. At the time, many turned to the literature on methodology because of doubts about the value of economics. After the economic successes of the generation following World War II, economic growth stalled in the 1970s, and many came to doubt that *anybody* knew how to restore prosperity without rekindling inflation.

A decade later, at the time of the second edition, things looked brighter for economics, although there were still doubts about how to restore prosperity without aggravating budget deficits, how to reinstitute markets in state-controlled economies without precipitating economic collapse, and how to alleviate widespread misery in the so-called developing countries. In that atmosphere, it is not surprising that economists turned to methodological reflection in the hope of finding some flaw in previous economic study or, more positively, some new methodological directive to improve their work. Nor is it surprising that ordinary citizens, whose opinions of economists are more influenced by the state of the economy than by systematic evaluation of economic theories, should wonder whether there might be something awry with the discipline.

Today, in 2007, in contrast, economists are riding high. Although there have been serious economic problems during past fifteen years, such as the international financial crisis in 1997, continued high unemployment in Europe, and a prolonged and severe recession in Japan, nevertheless, there has been significant economic growth in developed economies, which have generally prospered. Serious problems remain in the formerly socialist countries, but conditions have stabilized and for the most part improved. And rapid economic growth in the two most populous countries on earth, India and especially China, has transformed the economic landscape. Although it is overly optimistic to claim that the central economic problems have been solved (especially in the light of the disastrous performance of the economies of many of the poorest countries in the world), such a claim today, unlike a generation ago, would not strike most people as absurd.

While the doubts about the value of economics that helped fuel the interest in economic methodology that began in the 1970s have receded, the theoretical reasons to be interested in economic methodology have only grown stronger. In previous editions, I identified three theoretical reasons. First, not only economists but also anthropologists, political scientists, social psychologists, and sociologists influenced by economists have argued that

the "economic approach" is the only sensible theoretical approach to the study of human behavior. This provocative claim – that economics is the model that *all* social sciences must follow – obviously makes methodological questions concerning economics more important to other social scientists.

In the 1970s and 1980s, it was ironic that some economists were making grandiose claims for the universal validity of the economic approach to human behavior at the same time that others had serious qualms about their own discipline. As those qualms have faded, so has this irony. There is, however, a second ironical twist, which constitutes the second theoretical reason why interest in the methodology of economics has increased. During the same period that grand claims have been made for the economic approach to human behavior, cognitive psychologists and economists impressed by the work of cognitive psychologists have shown that many of the fundamental claims of modern mainstream economics are refuted by economic experimentation. The rapid expansion of experimentation, which is discussed in Vernon Smith's essay (Chapter 18) and of behavioral and neuroeconomics, which is discussed in Colin F. Camerer's essay (Chapter 19), raise intriguing methodological questions.

Finally, there are special reasons why philosophers have become more interested in the methodology of economics. Contemporary philosophers of science have become convinced that a great deal can be learned about how science ought to be done from studying how science actually is done. Although most philosophers who are interested in the sciences study the natural sciences, economics is of particular philosophical interest. Not only does it possess the methodological peculiarities sketched above, but moral philosophers, whether attracted or repelled by the tools provided by economists and game theorists, need to come to terms with welfare economics (which is discussed in Part III of this anthology).

For these reasons, it is not surprising that there is so much interest in the methodology of economics. At the same time that triumphant economists are claiming to have found the one true path for all the social sciences, psychologists, behavioral economists, and neuroeconomists are challenging the basic generalizations of economics and arguing for a different way of doing economics. Philosophers of science are at the same time turning their attention to the peculiarities of particular disciplines, such as economics. The renewed interest in economic methodology over the last generation comes after decades during which the subject was largely ignored by philosophers, while the philosophical efforts of economists – in many cases prominent ones – were sporadic and often polemical.

This volume aims to assist those interested in the methodology of economics by providing a comprehensive and up-to-date introduction to the subject. My hope is that this book will be useful both as a research resource and as a teaching tool. It provides an introduction to a wide range of methodological issues and and to a wide range of positions which have been taken with respect to these issues.

Unlike a textbook, this anthology also provides some historical perspective. Methodological questions concerning economics – questions about the goals of economics, the ways in which economic claims are established, the concepts of economics and their relation to concepts in the natural sciences and so forth – are all philosophical questions, and in philosophy it is generally a mistake to ignore the works of the past. Past wisdom cannot be encapsulated in a textbook, and original works cannot be consigned to intellectual historians. Much of what a philosophical text has to teach lies in its relationship to its intellectual context and in the nuances of its argumentative turns. There is, I believe, a great deal to be learned about economic methodology from studying directly how intellectual giants like John Stuart Mill or Karl Marx dealt with the problems. Those who wish to think seriously about the methodology of economics should know its history, too.

Some introductory material may help the reader to understand the essays reprinted here. At the beginning of each part, I offer a few comments about its contents. The remainder of this general introduction provides general background to make the various essays more accessible. Capsule introductions to the philosophy of science, to economic theory, and to the history and contemporary directions of work on economic methodology follow.

An Introduction to Philosophy of Science

As science is one sort of human cognitive enterprise, so philosophy of science is a part of epistemology (the theory of knowledge), although philosophers of science also face questions concerning logic, metaphysics and even ethics and aesthetics. One can find discussions of issues in the philosophy of science in the works of pre-Socratic philosophers, but philosophy of science as a recognizable subspecialty only emerged during the nineteenth century. Important names in the early development of modern philosophy of science are David Hume and Immanuel Kant in the eighteenth century, and John Stuart Mill and William Whewell in the nineteenth century. At the end of the nineteenth century, philosophy of science emerges as a subdiscipline with monographs mainly by scientists or historians of science such

as Ernst Mach, Pierre Duhem and Henri Poincaré. In the first half of the twentieth century, the so-called logical positivists (many of whom also had backgrounds in science) dominated thinking about the philosophy of science, although Karl Popper's views also were influential. Contemporary philosophy of science is a lively area of research and controversy. Although there is considerable agreement about fundamentals, the details concerning matters such as explanation or confirmation are hotly contested. There is no standard doctrine or detailed orthodoxy.

The issues with which the philosophy of science has been concerned that are most relevant to economics can be divided into five groups:

1. *Goals* What are the goals of science and of scientific theorizing? Is science primarily a practical activity that aims to discover useful generalizations, or should science seek explanations and truth?
2. *Explanation* What is a scientific explanation?
3. *Theories* What are theories, models, and laws? How are they related to one another? How are they discovered or constructed?
4. *Testing, induction and demarcation* How does one test and confirm or disconfirm scientific theories, models and laws? What are the differences between the attitudes and practices of scientists and those of members of other disciplines?
5. Are the answers to these four questions the same for all sciences at all times? Can human actions and institutions be studied in the same way that one studies nature?

This grouping of the questions with which philosophers of science have been concerned is intended only to help organize the discussion that follows. I have omitted issues concerning the unobservable postulates of scientific theories, which were of great importance to the logical positivists and their immediate successors, because they are less important to economics.

Contemporary philosophy of science is best understood against the background of positivist and Popperian philosophy of science, which are still influential among economists. So in discussing the questions listed here, I shall spend some time talking about the positivist and Popperian ancestors of contemporary views.

The Goals of Science

There are two main schools of thought. *Scientific realists* hold that in addition to helping people to make accurate predictions, science should *also* discover new truths about the world and explain phenomena. The goal is truth, and enough evidence justifies claims to have found the truth, although realists

recognize that the findings of science are subject to revision and correction with the growth and improvement of science. *Antirealists* may be *instrumentalists*, who regard the goals of science as exclusively practical, or antirealists may instead disagree with realists mainly about whether the unobservables postulated by scientific theories exist, whether claims about them are true or false, and whether observable evidence can establish claims about unobservables. Notice that instrumentalists do not repudiate theorizing. They agree with realists that theories are important. But they locate their importance exclusively in their role in helping people to anticipate and control phenomena. In his influential essay, "The Methodology of Positive Economics" reprinted in this anthology, Milton Friedman espouses a narrowly instrumentalist view of science.

Who is right, realists or antirealists? There is no settled opinion among philosophers, and the fortunes of realism and instrumentalism have oscillated over the past few decades.[1] Scientists themselves are divided. Realism has a firm foothold in many areas (how many people doubt that DNA exists or that it carries a genetic code?), but the problems and peculiarities of quantum mechanics have led many physicists to a modest view of the goals of science and to an antirealist view of claims about quantum phenomena. For a discussion of the relevance of realism versus antirealism to economics, see Uskali Mäki's and Tony Lawson's essays in Part V.

Someone who hopes that science can discover new truths about the world through its theorizing need not find theories *valueless* unless they are true. Ptolemy's astronomy, which places the earth in the center of the solar system, was used for navigational purposes for centuries after it was refuted. There is no reason why a realist cannot use Ptolemy's theory to navigate. The realist wants more from science than such merely useful theories, but that is no reason to throw away something that works.

Scientific Explanation

Explanations answer "Why?" questions. They remove puzzlement and provide understanding. Often people think of explanations as a way of making unfamiliar phenomena familiar, but in fact explanations often talk of things that are much *less* familiar than what they seek to explain. What could be more familiar than that water is a liquid at room temperature? Certainly not the explanation physicists give for its liquidity.

Philosophers disagree about what is central to a scientific explanation. Logical positivists and their logical empiricist successors took scientific explanations to show that the event or regularity to be explained follows

from a deeper regularity. A scientific explanation shows us that what is to be explained could have been expected to happen. This notion of explanation goes back to the Greeks, but it receives its best systematic development in the twentieth century in essays by Carl Hempel.[2] Hempel develops two main models of scientific explanation, the deductive-nomological and the inductive-statistical models. The latter, as its name suggests, is concerned with probabilistic explanations and attempts to extend the basic intuition of the deductive-nomological (D-N) model.

In a deductive-nomological explanation, a statement of what is to be explained is *deduced* from a set of *true* statements which includes *essentially* at least one *law*. Schematically, one has:

True statements of initial conditions
Laws
Statement of what is to be explained

The line represents a deductive inference. One deduces a description of an event or regularity from laws and other true statements. It is essential that there be at least one law. To deduce that this apple is red from the true generalization that all apples in Bill's basket are red and the true statement that this apple is in Bill's basket does not explain why the apple is red. "Accidental generalizations," unlike laws, are not explanatory.

The D-N model is an account of deterministic, or nonstatistical explanations. If one has only a statistical regularity, then one will not be able to *deduce* what is to be explained, but one may be able to show that it is highly probable, which is what Hempel's inductive-statistical model requires.

Even when limited to nonstatistical explanations, the D-N model faces counterexamples. An argument may satisfy all the conditions of the D-N model without being an explanation. For example, the fact that someone takes birth control pills regularly does not explain why they do not get pregnant, if the person never has intercourse or is a male. But not getting pregnant is all the same an implication of the "law" that those who take birth control pills as directed do not get pregnant.[3] One can deduce the height of a flagpole from the length of its shadow, the angle of elevation of the sun, and the law that light travels in straight lines, but doing so does not explain the height of the flagpole. A similar deduction does, however, explain the length of the shadow.[4]

What has gone wrong? The intuitive answer is that taking birth control pills has no causal influence on whether a woman who never has intercourse gets pregnant, and men cannot get pregnant whether or not they

take birth control pills. Similarly, sunlight and shadow have no significant causal influence on the height of flagpoles. It seems that explanations of events and states of affairs typically cite their *causes*.[5] There are, however, two problems with "explanations cite causes" as a theory of explanation. First, although most explanations of events and states of affairs are causal explanations, not all are. Second, saying that explanations cite causes is not by itself very informative. Without a theory of causation, a causal theory of explanation is empty, and even with a theory of causation, it only scratches the surface to maintain that to explain is to cite a cause. The existence of the sun is causally relevant to the wheat harvest, but it does nothing to explain the price of wheat.

The explanation of human behavior introduces special difficulties. Most explanations of human action take a simple form. One explains why an agent purchased some stocks or changed jobs by citing relevant beliefs and desires of the agent. When economists explain behavior in terms of utility functions, they offer explanations of just this kind.

This familiar kind of explanation is philosophically problematic. If one attempts to construe such explanations as elliptical or sketchy deductive-nomological explanations, one finds that it is hard to find any substantial and plausible laws implicit in them. What apparently do the explaining are platitudes such as "People do what they most prefer." Some philosophers have argued that generalizations like these are not empirical generalizations at all. They are instead implicit in the very concepts of action and preference.[6] According to these philosophers, explanations of human behavior differ decisively from explanations in the natural sciences. In explaining why someone did what he or she did, one does not subsume their action under some general regularity. Instead, one gives the agent's *reasons*.

It is true that in explaining an action one gives the agent's reasons for performing it. But do explanations in terms of reasons differ fundamentally from explanations in the natural sciences? Can they be seen as (roughly) deductive-nomological or as causal? Can they be assessed in the same way that explanations in the natural science are assessed? Philosophers disagree on these questions. Most writers on economics have attempted to assimilate explanations in economics to explanations in the natural sciences. Why cannot explanations in terms of reasons *also* be scientific explanations in terms of causes?[7] But there is a considerable minority, which includes distinguished economists such as Frank Knight (Chapter 4), who have argued that explanations of actions in terms of the reasons for the actions differ in some fundamental way from ordinary scientific explanations.

Scientific Theories and Laws

Most philosophers have argued that science proceeds by the discovery of theories and of laws, but economists are more comfortable talking about *models* than about laws and theories. Over the last two decades, philosophers have begun to catch up,[8] and there is a new philosophical literature that permits a more satisfactory characterization of theorizing in economics.

Economists do sometimes talk in terms of laws. They speak of the law of demand, Say's Law, the law of one price, and so forth. So let us begin with some words concerning laws and the role they play in science. The laws of sciences are not, of course, prescriptive laws dictating how things *ought* to be. (It is not as if the Moon would like to leave its orbit around the earth, but is forbidden to do so by a gravitational edict.) Scientific laws are instead (speaking roughly) regularities in nature. But they are not just regularities. Consider the generalization, "No gold nugget weighs more than 1,000 tons." Even if it is true everywhere and for all time, this generalization appears to be merely "accidental" and of no explanatory value. What then is the difference between an accidental regularity and a genuine law?

Rather than canvas the unsatisfactory answers philosophers have considered, let us step back and ask whether, however the analysis comes out, economics has any genuine laws. Consider, for example, the law of demand. It says, roughly, that when the price of something goes down, people seek to buy more of it, and when the price goes up, people want to buy less. Unlike physical laws such as Boyle's law, which states that the pressure and volume of a gas are inversely proportional, the "law" of demand is asymmetrical: it links causes (price changes) to effects (changes in demand). If an increase in demand comes first, the price will go up rather than down. Second, the "law" of demand is (at least when stated this way) not a universal truth. For example, if there is a change in tastes at the same time that the price drops, demand might not increase. So perhaps the concept of a law is not a useful one for those interested in economic methodology.

The issues here are complicated, because of the possibility of subtle reformulations of claims such as the "law" of demand. One might, for example, argue that such laws carry *ceteris paribus* qualifications: other things being equal, price increases lessen demand and price decreases increase demand. In my own work, I have defended this idea, which goes back to John Stuart Mill (the first selection in this volume). So I do not think that this project is misconceived. According to the deductive-nomological model of explanation, economists can use generalizations such as the law of demand to explain economic phenomena only if those generalizations are genuinely laws.

Nevertheless, there is a good deal to be said for adopting an explicitly causal view of explanation such as James Woodward's, which does not depend on citing any laws. Whether or not the law of demand is truly a law, there are specific domains in which the generalization is nearly always true and in which one can rely on it to pick out the causes of price changes.

The other intellectual constructs emphasized by the logical empiricists, scientific theories, also do not fit economics very well. One of the features the positivists took to be crucial to theorizing – the postulation of unobservable entities and properties to explain observable phenomena – is unusual in economics. (Even though beliefs and preferences are apparently unobservable, they are obviously not new postulations of economists.) More importantly, when economists talk about theories, they usually talk about branches of economics (such as game theory, or the theory of the firm, or the theory of monopolistic competition) rather than anything analogous to Newton's theory of gravitation or Maxwell's theory of electromagnetic radiation.

Theories in the natural sciences appear to be collections of lawlike statements that "work together" to help describe, predict, and explain phenomena in some domain. The logical positivists made the notion of "working together" precise, by arguing that theories form deductive systems. According to the positivists, theories are primarily "syntactic" objects, whose terms and claims are interpreted by means of "correspondence" rules.[9] Let me explain.

Influenced as they were by the dramatic breakthroughs in formal logic at the end of the nineteenth and the beginning of the twentieth century, the logical positivists conceived of deducibility as a *formal* relationship between sentences, which is independent of the *meaning* of the sentences. For example, one can infer the sentence "*r*" from the sentence "*s* and *r*" without knowing anything about what the sentences "*s*" or "*r*" assert. Logicians explored the possibility of constructing formal languages in which the ambiguities of ordinary languages would be eliminated. In these formal languages, there would be a sharp separation between questions concerning syntax and semantic questions concerning meaning and truth.

The logical positivists hoped to be able to express scientific theories in formal languages. From the axioms of the theory, all theorems would follow purely formally (just as "*r*" follows from "*s* and *r*"). For the theory to have meaning and to tell us about the world, it would still need an interpretation. "Correspondence rules" were supposed to provide that interpretation and to permit theories to be tested. Originally, correspondence rules were conceived of as explicit definitions for each of the theoretical terms, but the positivists