THE PHILOSOPHY OF ECONOMICS

AN ANTHOLOGY

Third Edition

Edited by
Daniel M. Hausman

CAMBRIDGE

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This is a comprehensive anthology of works on the philosophy of economics, including classic texts and essays exploring specific branches and schools of economics. Other than the classics, most of the selections in the third edition are new, and both the comprehensive introduction and the bibliography have been revamped to bring the volume up to date. The volume contains twenty-six chapters organized into five parts: (I) Classic Discussions, (II) Positivist and Popperian Views, (III) Ideology and Normative Economics, (IV) Branches and Schools of Economics and Their Methodological Problems, and (V) New Directions in Economic Methodology. It includes crucial historical contributions by figures such as Mill, Marx, Weber, Robbins, Knight, and Veblen, as well as works by the leading contemporary figures writing on economic methodology, including five Nobel Laureates in Economics.

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The Philosophy of Economics

An Anthology

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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 soon realized that the relationship between theory and observation is more intricate.

Scientific theories cannot usually be formalized in the way in which the logical positivists hoped, and the positivist view of theories does not do justice to the way in which theories are constructed or used. Furthermore, the problems of relating theory to observation, in the form in which the positivists posed them, are intractable, and problems about characterizing lawlike statements remain. Many philosophers of science now settle for a looser informal construal of theories as collections of interpreted lawlike *statements* rather than uninterpreted, purely syntactic sentences, which are systematically related to one another.

The really pressing philosophical task for those interested in economics is to come up with an understanding of scientific *models*, because economic theorizing relies mainly on models. Models in the sciences, unlike theories, may be material (like the scale models of airplanes tested in wind tunnels) as well as linguistic; however, like laws and theories, they are representational. Unlike laws and some theories, models are manipulated, explored, and modified. Although it is sometimes appropriate to ask whether parts of models are true or false, economists more often assess models in terms of their fruitfulness or usefulness.

One view of models, which I have defended (and which is criticized in the essay by Sugden, reprinted as Chapter 26), takes them to be of the same logical type as are predicates such as "has two legs," or definitions of such predicates.[10] According to this view, a model of consumer choice among two commodities does not make assertions about the world. It is instead a predicate such as "is a two-commodity consumption system" or a definition of such a predicate. Of course, economists do make claims about the world. They do so by *using* models, by asserting that the predicates that models constitute or define are true or false of systems of things in the world.

Drastically oversimplifying this view, it maintains that instead of offering "theories" like "All bodies attract one another with a gravitational force," scientists offer "models" like "Something is Newtonian system if and only if all bodies in it attract one another with a gravitational force and . . . ," and that scientists then use such 'models' to make empirical claims such as "The universe is a Newtonian system." Given this parody, one might wonder why serious philosophers defend the predicate view of models.

There are two reasons. First, if one hopes to be able to reconstruct the claims of science formally, the predicate view has significant technical advantages. Second, the predicate view offers a useful way to schematize the *two* kinds of achievements involved in constructing a scientific theory. Although what ultimately count are the claims that models permit scientists to make about the world, science does not proceed by spotting correlations among already known properties of things. An absolutely crucial part of the scientific endeavor is the construction of new concepts, of new ways of classifying phenomena. And much of science is devoted to thinking about these concepts, relating them to other concepts and exploring their implications. This kind of endeavor is prominent in economics, where economists often explore the implications of perfect rationality, perfect information and perfect competition, without immediate concerns about empirical application or testing.

Assessment and Demarcation

Most people are empiricists about theory assessment: they believe that the evidence that ultimately leads scientists to accept or to reject claims about the world should be perceptual or observational evidence. According to empiricists, economists should believe that individuals generally prefer more commodities to fewer, if and only if this claim is borne out by experience.

Empiricism is not completely uncontroversial. Kant argued in his *Critique of Pure Reason* that there are some "synthetic" truths about the world such as the axioms of Euclidean geometry that can be known "*a priori*" – that is, without specific sensory confirmation. He maintains that these propositions are implied by the very possibility of having any conscious experience of the world. No specific observations or experiences could ever lead us to believe that such propositions were false.

Modern physics has not dealt kindly with Kant's view that the axioms of Euclidean geometry are *a priori* truths, but the Kantian view that there are synthetic *a priori* truths still has supporters among so-called Austrian economists, especially Ludwig von Mises and his followers. They argue that the fundamental postulates of economics are synthetic *a priori* truths.[11] I shall not discuss the Austrians' epistemological views, but the reader should be aware that some methodologists question empiricist views on assessment.

Despite their "obviousness," empiricist views of the assessment of claims about the world encounter serious problems. First, it seems implausible to claim that definitional truths such as "Triangles have three angles" require testing or that our confidence in such claims rests on the results of observations. Nor do we need experiments to know that a claim such as "This square is circular" is false. The logical positivists responded by distinguishing synthetic claims – claims about the world – from analytic or contradictory claims whose truth or falsity depend solely on logic and on the meanings of the terms in such claims.[12]

Even confining oneself to synthetic claims, serious problems remain. As Hume argued in the eighteenth century, observation only establishes the truth of singular statements about particular events or about properties of things at particular times and places. On what, then, is our confidence in generalizations or in singular statements about instances not yet observed based? As Hume put it:

> If a body of like color and consistency with that bread which we have formerly eaten be presented to us, we make no scruple of repeating the experiment and foresee with certainty like nourishment and support. Now this is a process of mind or thought of which I would willingly know the foundation.[13]

In other words, Hume is issuing a challenge: Show me a good argument whose conclusion is some generalization or some claim about something not observed and whose premises include only reports of sensory experiences. Such an argument cannot be a deductive argument, because such inferences are fallible: the next slice of bread might be fatal. Nor will an "inductive" argument do, as we have only inductive and thus question-begging grounds to believe that such arguments are good ones.

This is Hume's *problem of induction*. It is primarily a problem concerning how singular claims about unobserved things or generalizations are to be *supported* or *justified*. It is not mainly a problem about the discovery of generalizations. In my opinion, Hume's problem of induction is, as stated, insoluble.

If this problem of induction cannot be solved, there are two options. One is to deny that there are ever good reasons to believe generalizations about the world, no matter how much purported evidence one has. This is the skeptical conclusion Hume drew – although he confessed that when he left his study he could not act on it. Alternatively, one can criticize Hume's description of the problem. I prefer the latter course. What is wrong with Hume's problem of induction is Hume's view of what justification demands. Hume wants a separate argument for every generalization with only reports of sensory experiences as premises. If instead one relaxes the demands on justification and one permits the premises in justificatory arguments to include all of our purported scientific knowledge about the world, then one faces the difficult but not impossible problems of inductive inference that scientists actually grapple with. Observations and experiments play a crucial role in the expansion and correction of empirical knowledge, but people need not trace their knowledge claims back to an experiential foundation.[14] To borrow a metaphor, learning about the world is like rebuilding a ship while staying afloat in it. In learning more about the world, people rely both on observation and on the vast body of knowledge that they think they already have.

The ship metaphor is due to Otto Neurath, who was a member of the Vienna Circle, the main wellspring of logical positivism. Yet the logical positivists did not for the most part endorse such a holistic view of scientific knowledge. Instead considerable efforts were made by Rudolf Carnap and others to develop an inductive logic, a canon of thought whereby conclusions could be established with a certain probability from premises that included only basic logic and mathematics and reports of observations.[15] These efforts were not successful, but Carnap's work helped lead to more promising modern approaches.[16]

Karl Popper's views on induction are more radical. Popper recognized in the 1930s that the results of experiments and observations bear on the truth or falsity of claims about the world only within the context of a body of tentatively accepted beliefs.[17] But he then introduced a further twist. He argued that generalizations such as "All copper conducts electricity" can be *falsified* by singular statements reporting the results of observations, even though they cannot be *verified*. In fact, Popper argued that there is no such thing as confirmation! (He says, instead, that scientific generalizations may be "corroborated," but he maintains that corroboration provides no grounds to believe that a theory is correct or a reliable basis for prediction.) Generalizations remain no more than tentative conjectures, no matter how often we fail to falsify them.

Many have read Popper as suggesting that generalizations can sometimes be conclusively proven to be false on established premises which include only reports of observations. The problem of induction is thus "solved" by accepting half of Hume's skeptical conclusion: There are never good reasons to believe that universal generalizations are true. What saves us from skepticism and generates scientific progress is the possibility of finding good reasons to believe that generalizations are false. Science proceeds by making bold conjectures and eliminating errors.

Popper explicitly disavowed this simple interpretation of his position.[18] In his view, reports of observations are fallible and open to revision. As a matter of convention one accepts them as true in the course of testing a generalization. In doing so, one is taking an unavoidable risk of rejecting the generalization, even though it is true. Moreover, one can rarely infer the falsity of interesting claims in science merely from singular observation reports. For example, to use observations of choices in the economics laboratory to test game theory, one has to make assumptions concerning what factors influence preferences. In testing a theory, scientists deduce an implication from that theory, *conjoined with* subsidiary hypotheses and statements of initial conditions. If the implication is not borne out by observation, scientists must take risks and decide that the problem lies in the particular theory being tested, not in the unavoidable additional premises.

In autobiographical comments, Popper maintains that what drove him into the philosophy of science was what he calls “the problem of demarcation”: What is the difference between a scientific theory and a theory which is not scientific?[19] Although formulated differently, this was a driving question for the logical positivists, too. They wanted to be able to distinguish scientific theories from “meaningless” metaphysics and to contribute to the further development of science. As stated earlier, the problem of demarcation concerns the distinction between scientific *theories* and other sorts of theories. But Popper is often concerned instead to distinguish those *attitudes, rules and practices* that distinguish a scientific community from other attitudes and practices. What matters is often not the theory, but what people think of it and what they do with it. Newton’s theory of motion could become the dogma of some strange sect, while, in contrast, astrology can be subjected to scientific scrutiny. The more important problem of demarcation concerns the difference between the attitudes of scientists and nonscientists, not the difference between scientific theories and other sorts of theories.

According to Popper, what is special about scientists is that they have a “critical attitude.” They follow methodological rules directing them to make bold conjectures and then seek out the harshest possible tests of them. These rules require that when the conjectures fail those tests, scientists do not make excuses. Instead they should regard the theories as refuted, and they should then propose and scrutinize new conjectures.[20] As many have noted, including Thomas Kuhn and Imre Lakatos, it is a good thing that scientists do not follow these rules.[21] Because theories always face unresolved difficulties, these rules demand that they all be rejected. But theories are too important to the practice of science to be surrendered until alternatives are available. And alternatives are not easily generated.

The questions Popper asks may be more important than the answers he argues for. Successors such as Kuhn and Lakatos and a number of sociologists of science have followed Popper in attempting to clarify what sort of disciplines the sciences are. Yet current investigations of assessment and demarcation differ not only from the positivists’ efforts, but from Popper’s as well. As completely opposed as the Popperian and positivist approaches were, both conceived of theory assessment in terms of the confrontation of single theories with data. Most contemporary philosophers of science reject this way of approaching the problems. Instead of thinking about the problems of theory *assessment*, they are concerned with the problems of theory *comparison* and *choice*. Testing is a many-sided confrontation among alternative theories and data. Furthermore, there are many choices to be made among theories, not just one. A scientist may for example reasonably believe that theory T is better confirmed than theory T', but that T' offers more interesting research possibilities. Although most contemporary philosophers of science agree that there are many different problems of theory assessment and that one must address them in terms of *choices* among alternatives, disagreements remain about what conclusions to draw.

One view, which many attribute to Thomas Kuhn, is to question whether theory choices are rationally defensible. In his classic *Structure of Scientific Revolutions*, Kuhn offers a view of science and of philosophy of science that differed sharply from the logical empiricist orthodoxy at the time he was writing. With the help of vivid examples from the history of science, Kuhn emphasizes how extensive are the constraints on ordinary scientific research. To determine the magnitude of a particular constant or to solve a detailed theoretical problem takes resources and energy, which scientists will not be willing to expend unless they are convinced that the general theoretical framework ("paradigm") within which they are working is more or less correct. Without such commitments, detailed esoteric research efforts would not be undertaken. Although the workaday, perhaps even dogmatic "normal science" that results does not aim at discovering novelties, it nevertheless, Kuhn argues, uncovers "anomalies" – problems that resist solution within the particular normal scientific tradition. Such anomalies can undercut the scientific community's confidence in the accepted paradigm and, given the construction of an alternative paradigm, can lead to a scientific revolution.

Kuhn's view of scientific revolutions is especially controversial. He seems to argue that disagreements in scientific revolutions can be so pervasive that no rational choice can be made.[22] Because scientists in different camps will have distinct views about standards of theory assessment and about how to conceive of the subject matter and practice of the science, consensus can be reached only through nonrational persuasion. According to this irrationalist interpretation of Kuhn, the paradigm that triumphs in a scientific revolution need not be objectively "better" than the paradigm it replaced.

Kuhn disavows such an extreme interpretation of his views, and many historians and most philosophers of science have found such irrationalist conclusions to be unjustified. Yet they live on in the work of some sociologists of science, who have defended even more extreme views. Some go so far as to deny that the phenomena that scientists study have any influence at all on the views that scientists defend.[23]

Yet in rejecting Kuhn's apparent irrationalism, one can still recognize the significance of his contribution to contemporary philosophy of science. Not only did Kuhn make philosophers aware of the complexity of scientists' commitments, but he did as much as anyone to convince philosophers that theorizing about science without careful attention to scientific practice was likely to be misleading. Even though few philosophers of science regard themselves as Kuhnians, most follow Kuhn on these points. Although Popper and many of the logical positivists were scientifically literate and intensely interested in the sciences, including particularly physics, contemporary philosophers of science tend to address problems in the philosophy of science at a lower level of abstraction and with greater attention to the details of scientific practice. Just as economists can only offer advice to a firm if they have learned what in fact makes firms run well, so philosophers can only offer advice to scientists if they have learned what in fact makes for good science. And, in my view, there is in general no way to learn about firms or science without studying firms or scientists.

A number of prominent philosophers of science have developed accounts of theory evaluation that recognize the complexities of scientific work without denying the rationality of science. Many approaches merit discussion, especially the work of the modern "Bayesians," but this introduction is not long enough to discuss them.

Something must, however, be said about Imre Lakatos's "Methodology of Scientific Research Programmes," which had a considerable influence on economic methodology in the 1970s and 1980s. Lakatos began his work on the philosophy of science as a follower of Popper. Although critical of many details, including Popper's view that scientific honesty demands an immediate readiness to surrender one's theory in the fact of an apparent disconfirmation, Lakatos insists that Popper's basic point remains valid: if scientists make empty excuses for their theories when they run into apparent difficulties, then they will never learn from experience. What philosophy of science should be concerned with, according to Lakatos, are not rules for assessing theories, but rules for *modifying* and *comparing* theories. Rather than asking, "Is theory *T* well or poorly supported by the data?" scientists want to know whether a new version of *T* is an improvement over the old. The central question concerning assessment is whether the proponents of *T* are making as much progress improving it as are the proponents of competing theories.

According to Lakatos, a modification of a theory is an improvement if it is not *ad hoc*. Modifications may be *ad hoc* in three ways.[24] If a modification of a theory has no new testable implications at all, it is empty and unscientific. Modifications that are not *ad hoc* in this first sense are "theoretically progressive." If the testable implications of theoretically progressive modifications are not confirmed by observation, then these modifications are not "empirically progressive," and they are thereby *ad hoc* in the second sense. Lakatos maintains that an extended process of theory modification is progressive overall, if the modifications are uniformly theoretically progressive and intermittently empirically progressive. As scientists revise their theories in the hope of improving them, the changes must always have new testable implications; and those testable implications must sometimes be borne out by experiment and observation. In addition, there must be continuity throughout this history of repeated modification. Economists do not make theoretical progress by tacking on unrelated generalizations from chemistry. Adding the generalization that copper conducts electricity to monetary theory results in new testable implications, but such a modification is *ad hoc* in a third sense.

Lakatos insists that science is and should be dominated by scientific research programs. These consist of a series of related theories that possesses a "hard core," which the "negative heuristic" insists must be preserved through all modifications of particular theories within the research program. In addition, the research program contains a "a positive heuristic" that directs scientists in making modifications. Particular changes within a research program should be assessed by considering to what extent they are theoretically and empirically progressive and to what extent they are in accordance with the positive heuristic of the research program. Competing research programs should be compared by examining their overall progressiveness. In Lakatos's view (in contrast to Kuhn's), science suffers when a single research program becomes dominant.

Lakatos's methodology of scientific research programs has some dubious features. The single-minded emphasis on progress is questionable. The fact that a series of theories T, T', T'' may be progressing splendidly tells one nothing about whether T'' fits the data well. Why should only the "novel predictions," the new implications of T'' over T', matter? Lakatos's insistence on a specific hard core, which defines a particular research program is also too strict. The supposed "hard core" of every research program is always being reformulated and, in various ways, modified.

If one goes to contemporary philosophy of science in search of hard and fast rules for assessing theories in the light of data, one will be disappointed. Nonphilosophers may find this state of affairs discouraging, and they might draw skeptical or relativist conclusions. But skepticism and relativism are cold comfort when one needs to decide what to do about crushing poverty or the problems of achieving economic growth without environmental disaster. And, as this brief summary shows, philosophers have learned a great deal about theory assessment, even if that knowledge cannot be codified into detailed and exceptionless rules.

The Unity of Science

In studying economics, one not only faces standard problems in the philosophy of science, but one also wants to know whether social sciences like economics should model themselves after natural sciences like physics. Human beings and their social interactions are different objects of study than are planets or proteins. Should the goals and methods of social theorists be the same as those of natural scientists?

As mentioned at the beginning of this introduction, those who have asked whether the social sciences can be "real" sciences have been concerned with several different questions concerning the structure or concepts of theories and explanations in the social sciences and concerning the goals of social theorizing. Philosophers have argued that in addition to or instead of the predictive and explanatory goals of the natural sciences, the social sciences should aim at providing us with *understanding*. This issue receives its classic discussion in the selection in this volume by Max Weber, although Frank Knight also touches on it.[25]

Weber and many others argue that the social sciences should provide understanding "from the inside," that permits social theorists to empathize with the agents and to find what happens "understandable." He argues that social theorists inevitably classify social phenomena in terms of various culturally significant or meaningful categories, and that explanations must be in these terms or they will not tell people what they want to know. This seems to introduce an element of subjectivity into the social sciences that is avoidable in the natural sciences. But, provided that social theorists explain the phenomena in these meaningful terms, Weber has no objection to causal (indeed deductive-nomological) explanation. Yet even here there is a difference in emphasis. Weber maintains that however interested theorists may be in regularities, people want to understand particular happenings in their details and individuality, rather than, as in the natural sciences, as instances of general regularities. I see this as a difference in emphasis, not as demanding a different kind of explanation.

Contemporary philosophers who have been influenced by Weber and by developments in the philosophy of language (especially the work of Wittgenstein), have made stronger claims. These philosophers contend that regularities in human behavior are not natural laws, but the result of *rules* or *institutions.* To "understand" some human action is to discover the rules that guide it. And to understand rules, according to Peter Winch and others, is the same sort of task as understanding meanings. It is a task requiring interpretation, not empirical theorizing and testing. Winch's views seem to rule out applying the methods of the natural sciences to the study of human behavior and institutions, and they have been vigorously contested.[26]

Human free will suggests additional doubts about the possibility of a social science. One wonders whether, given free will, human behavior is intrinsically unpredictable and thus not subject to any laws. As tempting as this line of thought may be, it is a mistake. Even if there are no *deterministic* laws of human behavior, there are, in fact, many regularities in human action. Of course, if Winch and others are right, these regularities differ from laws of nature, but the regularities exist nevertheless. Not only can we predict the behavior of people we know well, but we often know what strangers will do. Every time we cross the street in front of cars stopped for a red light, we stake our lives on such knowledge. Whatever one thinks about free will, there are still uniformities in human behavior, which social theorists may reasonably seek to identify.

The mistaken assertion that human free will makes social science impossible lies, I believe, behind other arguments for the impossibility of any science of society. Expectations and beliefs, including beliefs about social theories, influence behavior. It is thus possible to make both self-fulfilling and self-defeating claims about people. These possibilities suggest that there may be paradoxes lurking within the notion of a social science. But the difficulties are specific and limited rather than fundamental.[27] A social theorist can "factor in" the reactions of those who become aware of any particular theory.

As economists have come increasingly to recognize, human beliefs and expectations, not just the realities about which people have beliefs and expectations are crucial to understanding human behavior. For people can, as Frank Knight points out, make mistakes or fail to recognize things. As a first approximation, economists abstract from such difficulties. They assume that people have perfect information. By assuming that people believe whatever the facts are, economists can avoid worrying about what people actually believe.

Once economists go beyond this first approximation, difficulties arise which have no parallel in the natural sciences. For claims about beliefs (and desires) are, in philosophical jargon, "intentional."[28] They possess a different logic. From a *non*intentional statement such as "The United States invaded

Iraq in 2003," and the second premise, "The invasion of Iraq in 2003 was a huge mistake," one can infer "In 2003, the United States made a huge mistake." But from the same second premise and the *intentional* statement, "President Bush wanted the United States to invade Iraq in 2003," one cannot deduce "President Bush wanted the United States to make a huge mistake." The logic of belief, desire and other such "intentional" terms is in some ways "subjective." These logical peculiarities and the subsequent need for a "subjective" treatment of expectations distinguish economics from the natural sciences (with the possible exception of a small part of biology). However, the significance of the differences is not clear. Members of the Austrian school (represented by James Buchanan and Viktor Vanberg in Chapter 20) argue that these differences are of great importance.

One final special difficulty about the social sciences concerns their role in guiding conduct. One view is that economics serves policy in the same way that the natural sciences guide policies – that is, by helping policy makers to choose means that will achieve their ends. Such a practical role for scientific knowledge seems unproblematic. Agents have some goal that they want to accomplish, and the scientist provides the needed "know-how." On this view, economics matters to policy only as a source of descriptive or "value-free" information. It matters so much, simply because it is so relevant. This view of the policy relevance of economics is defended in many of the essays reprinted in this anthology.

Many disagree. They argue that the links between economics, policy, and values go deeper. The demands and interests of public policy makers or of private employers influence which questions social theorists ask and the range of possible solutions that are seriously considered. The influence can sometimes be crude: economists are people after all, and they can be corrupted by the lure of money and prestige. Or there may be more subtle influences from customs, mores, and rhetoric to avoid what seems "unreasonable" or "irresponsible." Although it is hard to deny that ideological forces have influenced many social scientists, the extent of such ideological and evaluative influences requires sober assessment. What looks like ideology to an unsympathetic critic may in fact be work of unimpeachable intellectual integrity.

There are other less nefarious ways in which economics is entangled in values. Because policy makers rarely turn to economists with precisely formulated goals, economists may help determine the goals. Indeed, as philosophers such as John Dewey have argued, the distinction between means and ends, as plausible and useful as it may sometimes be, may mislead here. The major economists of the past two centuries have also been social philosophers who have found in economic theory inspiration for their social ideals. Although some have argued that normative or welfare economics, which is discussed in Part III of this anthology, is really a part of "positive" economics, investigating means to ends, most would concede that it is driven by moral commitments. Michael S. McPherson and I explore the philosophical foundations of normative economics in Chapter 13.

In providing the reader with both some glimpse of findings in philosophy of science and some sense of how much remains to be found out, this introduction may have discouraged readers who were looking for more detailed guidance. But in recognizing how much there is to be done, readers should not overlook how much has been done. Although logical positivism finds few supporters today, this is because the positivists were so devoted to clarity and precision and so intellectually honest and courageous that they uncovered the inadequacies in their own positions and ultimately refuted themselves. The more historically and empirically oriented philosophy of science and the sometimes exaggerated sociological views that have succeeded them have, no doubt, many inadequacies, but they begin with knowledge that the positivists gained. Similar comments apply to Popper's seminal work.

These words are cold comfort to the citizen, policy maker, economist or social scientist who wants to know whether economics is a science, whether he or she should rely on particular economic theories for practical or theoretical purposes or how he or she can best contribute to economics or to some other social science. But there is nothing to be done other than to make use of what has been learned. Philosophy of science has many insights to offer, and those who do not take it seriously are doomed to repeat its past mistakes. On the basis of such knowledge and on the basis of their own experience, economists and other scientists offer useful rules of thumb. But there is no well-founded general philosophical system to resolve the many real difficulties economists, policy makers, and citizens face.

An Introduction to Economics

To understand the essays collected in this anthology, it helps to know something about economics. What follows does not aim to provide the reader with any technical competence. Its goal is only to give some sense of (a) the basic approach of mainstream economists (b) the different branches of economics and (c) the different schools or approaches of economics.

Although one can find discussions of economics in ancient and medieval philosophy, economics is a modern subject. With the exception of some writing on monetary theory and on the purported benefits of exporting more goods than one imports, economics begins in the eighteenth century with the writings of the French physiocrats, of Cantillon and Hume, and especially of Adam Smith. What set these thinkers apart from the predecessors was their growing recognition of the existence of *mechanisms* whereby individual actions would have systematic consequences without any need for government control of the processes. Smith and others came to see the economy as to a large extent a self-regulating system. Economics came into being when it was realized that there were such things as economic mechanisms and systems to study.

Economics has been concerned mainly with understanding how a capitalist economic system works. (A capitalist economic system is a market economy in which the means of production are for the most part privately owned, and workers are free to accept or decline offers of employment.) Many economists believe that their theories apply to other economic arrangements, too, and a good deal of work has been done on other kinds of economies. But the core of economic theorizing has been devoted to understanding capitalist economies.

Since Adam Smith, a particular vision of such economies has dominated economic theorizing. One conceives of an economy as made up of a large number of independent firms and households, whose interactions with one another consist of voluntary exchanges of goods and services. Everybody knows that people have all sorts of other relations to one another, but the economist assumes as a first approximation that these can be ignored when one is addressing economic problems. Economic agents are conceived of as well-informed, rational, and self-interested agents, with firms seeking to maximize profits and households seeking wealth or what best satisfies their preferences. Agents exchange with one another because they prefer their after-exchange circumstances to their before-exchange circumstances. In the background is an institutional setting that ensures that contracts are kept, violence, coercion and fraud prevented, and so forth. Adam Smith formulates these conditions more loosely than I have, whereas contemporary theorists formulate them much more precisely. But the basic vision has persisted.

Given these assumptions, economists such as Adam Smith have for the most part believed that voluntary exchange would result in an efficient organization of economic life, which would be beneficial to all. In Smith's view, and in the view of most economists since, such a market economy also respects individual liberty more than does any other economic arrangement. One thus has a strong justification for capitalism. It delivers the goods and leaves individuals free to pursue their own objectives. Smith could not, however, prove rigorously that voluntary exchanges of well-informed self-interested agents lead to efficient economic outcomes.

Shortly after World War II, mathematicians and economists such as von Neumann, Arrow, Debreu, and McKenzie proved something like what Smith conjectured. They demonstrated that if agents are rational, self-interested, and well informed, and if they interact only through voluntary exchange in a perfectly competitive market, then a general equilibrium exists, which is Pareto efficient. In addition, they proved that every Pareto efficient outcome is a general equilibrium of voluntary exchanges among rational and self-interested agents, given the proper initial distribution of resources among the agents.[29] A general equilibrium is a situation in which there is no excess demand on any market. An economic outcome *O* is Pareto efficient if and only if one cannot depart from *O* without frustrating someone's preferences. All possibilities for uncontroversial improvement have been seized. In an inefficient economic state of affairs, in contrast, there are ways of better satisfying some people's preferences without lessening the preference satisfaction of others. The "efficiency" in question here is efficiency in satisfying preferences.

Although inefficiency in satisfying preferences is arguably a bad thing, lots of things are worse. Whether a state of affairs is Pareto efficient is generally independent of the distribution of goods, and accordingly some Pareto efficient states of affairs may be intolerable. For example, almost everyone favors a great many nonoptimal economic circumstances over a Pareto efficient state of affairs in which one man had everything he wanted and most others were miserable. One should be skeptical about the significance of proofs of the existence and efficiency of general equilibria both because of the weakness of the notion of Pareto efficiency and because of the extremely restrictive assumptions needed for the proofs.

But I have jumped directly from the beginning to near the end of the story. Let us see how, over the last two centuries, the image of rational, well-informed, and self-interested agents exchanging with one another has been refined. The "classical" economists, of whom Adam Smith, David Ricardo, and John Stuart Mill are the most prominent, did not have much to say about the choices of consumers. Their emphasis was on production and on the factors that influence the supply of consumption goods. They regarded agents as seeking to maximize their financial gains and divided both agents and basic inputs into three major classes: capitalists with their capital (which they conceived of as stocks of accumulated goods or the value thereof), landlords with their land, and workers with their ability to work. The classical economists offered two main generalizations concerning production. First, they assumed that at any given moment all reproducible goods (thus excluding things such as rare paintings) could be produced in any quantity for the same cost per unit. Except for temporary price fluctuations in times of crop failures or rapid changes in demand, prices should be determined by these constant costs of production. Second, classical economists discovered diminishing returns. Unless there is some technological innovation, as more and more labor is devoted to a fixed amount of land, the amount that output increases when an additional laborer is employed will eventually decline.

Given these generalizations concerning production and the view (most forcefully expressed by Malthus) that higher wages cause rapid increases in population, economists in the early nineteenth century drew gloomy conclusions. With economic growth, demand for workers increases and wages rise. The higher wages result in an increase in population. More workers need more food, and so capitalists (whom the classical economists thought of as renting rather than owning land) must rent additional and less fertile land, or they must cultivate existing land more intensively. Either way, the proportional return (rate of profit) on the additional investments will be lower. Landlords will consequently be able to increase rents on more fertile land and the rate of profit throughout the economy must decline. Ricardo argues that eventually the rate of profits will decline to the point where it is no longer worthwhile for capitalists to invest at all. In the resulting "stationary state," there are more workers, but they are no better off than their predecessors, since their wages will decline to that point where population no longer increases. Capitalists are better off than workers, but the rate of profit is low and their returns are modest. The big winners are landlords, who do nothing but collect rents. There is, in the view of most classical economists, little to do about this gloomy prospect except to agitate for the elimination of tariffs impeding the importation of foodstuffs and to preach "restraint" to the working class.

Fortunately, things did not turn out as Ricardo predicted. With improvements in the standard of living, population growth slowed, and by the late nineteenth century, economists recognized that population need not grow explosively in response to higher wages. Moreover, technological improvements brought about increases in productivity beyond the wildest dreams of the classical economists, who vastly underestimated the ability of technological improvements to stave off diminishing returns.

By the end of the nineteenth century, economics was no longer such a dismal science. Economists for the most part stopped worrying about population growth, and through the so-called neoclassical or marginal revolution, they focused their attention on individual choice and exchange. In the 1870s, William Stanley Jevons in England, Carl Menger in Austria, and Leon Walras in France began paying systematic attention to preferences of consumers, to exchange, and to demand for commodities.[30] In doing so, they filled in more of the basic vision of a market economy and transformed economic theory.

Many of the early neoclassical economists, particularly Jevons, were influence by *utilitarianism*, an ethical theory expounded earlier by Jeremy Bentham and John Stuart Mill.[31] According to the utilitarians, questions of social policy are to be answered by calculating the consequences of alternatives for the total happiness of individuals. The policy that maximizes the sum of individual utilities is the morally right one. Bentham held that the utility of something to an individual is a sensation that might in principle be quantified and measured. He also believed that individuals act so as to maximize their own utility (which raises the question of how they can be motivated to carry out actions that instead maximize the sum of everybody's utility).

Jevons developed the essentially Benthamite notion of a utility function. Every option open to an individual results in a certain amount of utility for that person. One can then clarify the notion of rationality by maintaining that people act so as to maximize some consistent utility function. In addition, the neoclassical economists assumed that consumers are generally not satiated – that they will always prefer a bundle x of commodities or services to another bundle y if x is unambiguously larger than y. Nonsatiation is both a plausible first approximation, and it articulates the notion of self-interest. All that matters to agents are the bundles of commodities and services that they are giving up or receiving.

With the addition of one more generalization, one has the core of modern economic theory. The early neoclassical economists noted that as one consumes more of any commodity or service, each additional unit increases one's utility at a diminishing rate. One's first computer may raise one's utility considerably. A second computer doesn't contribute nearly as much. This law of diminishing marginal utility explains why the price of essential but plentiful commodities such as water is lower than the price of inessential but scare commodities such as diamonds. Thinking in terms of marginal utility also enables one to give an integrated account of the "forces" affecting both demand and supply. Instead of regarding costs as reflecting physical requirements, most neoclassical theorists take costs to be the disutilities incurred when individuals devote resources or service to production or to be the utilities that would result from alternative uses of resources that individuals forgo (although these are in turn influenced by technical factors). The forces governing supply and demand are ultimately the same. The role of the market is to equilibrate these forces and to bring into harmony the efforts of individuals to secure what they want. With the further simplification that commodities are infinitely divisible, it became possible to apply the calculus to economics and to formulate this theory mathematically. In principle, the single theory of general equilibrium should enable one to explain virtually all the significant features of an economy.

In the 130 years since the neoclassical revolution, this theory has been tremendously refined. In speaking of utility, for example, contemporary economists are no longer speaking of some sensation that individuals want to maximize. "Utility" is now just another way to speak about preferences. The utility of some object of choice *x* to agent *A* is larger than that of option *y* if and only if *A* prefers *x* to *y*. In taking utility to reflect merely the ordering of preferences, economists had to surrender talk of utility differences and hence of marginal utility. Fortunately, the law of diminishing marginal utility can be reformulated in terms of the diminishing rates with which individuals are willing to substitute units of one commodity for another. Roughly speaking, one can replace the "law" of diminishing marginal utility with the generalization that people are willing to pay less for additional units of commodities that they already have a lot of than for commodities that they have very little of. Despite these refinements, mainstream theory is still recognizably the theory developed by the early neoclassical economists.

This fact may seem surprising, as most people know that in response to the Great Depression of the 1930s, John Maynard Keynes proposed a dramatic overhaul of economics. Before Keynes, most theorists of any reputation had maintained that a prolonged depression was impossible. There might be a crisis of confidence, which would lead to a temporary hoarding of money and a temporary interruption in the general cycle of exchange (in which firms as a whole purchase resources from their owners, then sell the commodities produced to the latter in their role as consumers, who then sell resources to firms again and so on). But with an excess demand for money and excess supplies of resources and commodities, prices are bound to drop and real interest rates rise. Any tendency to hoard would be self-correcting.

Keynes challenged this orthodoxy in part by emphasizing the importance of liquidity to both firms and individuals when they are faced with the uncertainties that a business crisis causes, and in part because he questioned the efficacy of the supposed self-correcting mechanisms. Prices, especially wages, do not drop easily, and lower wages can lead to less spending, which would suppress demand for commodities and lead to an even deeper slump. Keynes argued that government policy could increase aggregate demand for commodities and encourage investment and in that way move the economy out of its unemployment "equilibrium."

Despite Keynes's influence, his work did not shake the fundamentals of neoclassical theory. Initially, neoclassical theory instead divided into microeconomics, on the one hand, which is concerned with individuals, firms, and industries, and macroeconomics, on the other hand, which is concerned with aggregate demand and the performance of the economy as a whole. Although vestiges of this bifurcation persist, there ought, one would think, to be important connections between microeconomics and macroeconomics, and most economists nowadays insist on relating macroeconomic theories to stylized microeconomic foundations. For further discussion, see Chapter 17.

In the 1970s and 1980s, Keynesian economics was seriously challenged. Not only was it ill-suited to deal with the simultaneous inflation and unemployment of the 1970s, but economists grew increasingly impatient with the gap between micro- and macroeconomics and increasingly enamored of microeconomics. Unlike previous economists who made use of Keynesian macroeconomics while hoping to reconcile it with microeconomics, members of the so-called new classical school refused to employ any models that did not at least purport to derive from microeconomics or general equilibrium theory. Some, such as Robert Lucas, even went so far as to deny on the basis of microeconomic considerations that there was such a thing as involuntary unemployment,[32] and Lucas and others argued that the rational expectations of economic agents tend to undermine the effectiveness of monetary and fiscal policy as tools to manage the economy.

It is hard to say whether the new classical research program has triumphed or failed. On the one hand, its econometric predictions were no improvement over its predecessors, and the experience of the 1990s made it hard to believe that policy (especially monetary policy) had only a very limited effect on the economy. Updated versions of Keynesian economics remain influential. On the other hand, the concerns about modeling rational expectations that the new classical economists emphasized are now widely accepted, and new classical economics lives on in a different form as so-called real business cycle theory, which argues that business cycles are largely a response to "real" as opposed to monetary or policy factors.[33] Variations on real business cycle models are currently very influential. As this brief description suggests, macroeconomics is an unsettled area of economics.

Although microeconomics and macroeconomics are the two main branches of mainstream economics, they do not include all of it. Over the past three generations, there has been an enormous expansion of *econometrics*, which is discussed in Chapter 16. Econometrics is a branch of applied statistics as well as a branch of economics. Beginning in the 1930s, it was hoped that the claims of economic theorists might be tested and refined with the help of statistical techniques. Since then econometric techniques have become much more sophisticated. Exactly what this work means for economic theory (as opposed to narrowly focused practical inquiries) is controversial, with some prominent economists arguing that econometrics is incapable of providing good reasons to believe or disbelieve any significant causal claims.[34]

Microeconomics, macroeconomics, and econometrics together include most of mainstream economics, although there are of course specific subareas such as international trade, labor economics, and so forth. There are also competing schools of economics, although in most cases they have relatively few proponents. A generation ago, there was still a good deal of interest in Marxian economics. Although Marx was heavily influenced by Ricardo's work, he had a different view of the nature of economics and of its relationship to other social sciences than classical or neoclassical economists have. According to Marx's *historical materialism* (which is sketched in Chapter 5), the relations among people in the course of their productive activities are the most fundamental social relations. Relations of production strongly influence not only other relationships but also the personalities and consciousness of individuals. In studying economics, one is studying much more than how individuals produce, exchange, and distribute goods and services; one is also studying how human beings shape the development of their species.

Marx regards capitalism, despite the miseries it may cause (which he meticulously documents), as an enormous step forward for human beings. Capitalism relates individuals everywhere to one another through the world market, and it expands the needs and horizons of people. But, as argued in his early essay "Estranged Labor," it does not allow people to decide rationally and consciously how society and human nature should develop. The market creates both a reality and an illusion of helplessness. Given the market, people cannot in fact consciously determine their collective future. At the same time, it is an illusion to regard capitalism as eternal or natural. Marx believes that people can and will transcend capitalism and organize production and distribution in some rational way.

Those who will carry out the socialist revolution are the workers, who are "exploited," because they do all the producing but receive only part of the output. Capitalists (who, on Marx's view, possess little more real freedom than do workers) would resist any revolution that attempts to take their property and to prevent them from hiring workers and making profits. But, or so Marx argues, in expanding the size of their enterprises, capitalists unwittingly enlarge and strengthen the working class and lay the foundations for socialist revolution.

Given the collapse of the Soviet Union and the transformation of its economy and of the economies of Eastern Europe, interest in Marxian economics has collapsed as well. From one perspective, this is peculiar, because the Russian and Eastern European economies had only a tenuous connection with Marx's economics. But economic theories do not hover above the political waves. They are instead tossed about and, in the case of Marx's economics, possibly drowned.

Institutionalist (or "evolutionary") economists make up another major contemporary alternative to mainstream economics. The neoclassical attempt to capture all relevant aspects of the economy in one elegant theory leaves out a great deal. In the view of the institutionalists (and their nineteenth-century predecessors, the German Historical School), it leaves out too much and in abstracting from institutional development, it misses central aspects. The essays by Thorstein Veblen (Chapter 6) and Geoffrey M. Hodgson (Chapter 21) exemplify the institutionalist critique of mainstream economics and provide some sense of the institutionalist alternative. Although institutionalists do not ignore individual decision making, they emphasize the evolving constraints on agents occupying specific economic roles. The institutionalists do not constitute a tightly organized sect. The writings of the central historical figures (Thorstein Veblen, Wesley Mitchell, and John R. Commons) are very different from one another. The emphasis is on historically situated and evolutionary theorizing. Economists are divided on how successful institutional theorizing has been and is likely to be.

A third contemporary alternative to mainstream economics, about which there is currently heated disagreement, is behavioral economics, including neuroeconomics.[35] Behavioral economics has been heavily influenced by the increasingly important experimental work that economists have been doing, which is discussed in Chapter 18 by Vernon Smith. The general dissatisfaction many economists have felt with the highly simplified assumptions that mainstream economics makes concerning individual beliefs and preferences has been superseded by carefully delineated behavioral anomalies that have been established through economic experimentation. It is now possible to study the influence on preferences of a wide variety of cognitive, motivational, and even neurological features of human beings and to develop theories of economic behavior that are more psychologically nuanced. Whether and to what extent this work will help economists to address the questions concerning monetary policy, tax incidence, or economic welfare are hotly contested matters.[36]

These are but three of many approaches, which, in addition to mainstream neoclassical economics, occupy contemporary economists. A few others deserve to be mentioned. Neo-Ricardians believe that one can do better in understanding economies by employing modern mathematical reformulations and extensions of Ricardo's economics than by employing its neoclassical successor.[37] Austrian economists agree with neoclassical economists on the central generalizations of economics, but stress the importance of uncertainty, disequilibrium, and a subjective point of view (Chapter 20). Because of these factors, they regard sophisticated mathematical analyses of equilibria as misleading.[38] Post-Keynesian economists often offer similar criticisms of high theory, but unlike the Austrians, they tend to defend interventionist policies.[39] Economic forecasters often depend very little on any specific economic theory. And the list could be extended. Although contemporary economics is dominated by mainstream microeconomics, macroeconomics, and econometrics, there is lots more going on.

An Introduction to Economic Methodology

John Stuart Mill's 1836 essay, with which this analogy begins, is one of the first discussions of the methodology of economics, and it is still one of the best. From the perspective of a staunch empiricist like Mill, economics is a puzzling science. Its conclusions, which Mill accepts, are rarely tested, and they sometimes appear to be disconfirmed. Specific predictions based on economic theory are inexact and sometimes dead wrong. How can Mill reconcile his confidence in economics and his empiricism?

In Mill's view, the basic premises of economics are either psychological claims, which are established by introspection, or technical claims, such as the law of diminishing returns, which are established directly by experimentation. These premises state how specific causal factors operate. If the only causal factors that affect economics were those that economists consider, then the conclusions of economics would be correct, because they follow deductively from its well-supported premises. In fact, Mill argues, the conclusions economists draw must be treated cautiously, because so much is left out of their theory. Economists must be ready to make allowances for various disturbances, and economists must recognize that their predictions may be badly mistaken even though their theory is fundamentally correct. They should regard economics as hypothetical – as a science of tendencies, whose influence may be overwhelmed by interferences. Because it is only a science of tendencies, economists and policy makers cannot be confident that its predictions are always correct.

Mill's view was influential throughout the nineteenth and early twentieth century. It is, for example, still alive in John Neville Keynes's authoritative summing up in *The Scope and Method of Political Economy*, excerpts from which were reprinted in the first edition of this anthology. Despite differences in language, tone, and emphasis, Weber adopts a similar position in his discussion of "ideal types" in Chapter 2.

The transition from classical to neoclassical economics brought substantial changes in economic doctrine and changes in methodology. In its focus on individual decision making, neoclassical theory is a more individualist and subjective theory than was its classical predecessor, and the appreciation of this fact is an important contribution of early twentieth-century methodological writing. The major figures in developing this subjective turn are the Austrians (including especially von Mises), Frank Knight and Lionel Robbins. Knight's distinctive methodological contribution lies in his distinction between risk, on the one hand, (where the alternatives and their probabilities are known) and error and true uncertainty, on the other hand. Knight and the Austrians agree that as soon as one abandons the subjective point of view and thinks of economics as if it were a natural science, one loses sight of the central features of the subject.

Lionel Robbins, in his classic *An Essay on the Nature and Significance of Economic Science* (Chapter 3), comes close to the view of the Austrians, but he is better known for his definition of economics as "the science which studies human behavior as a relationship between ends and scarce means which have alternative uses" (1935, p. 85). According to this definition, economics is not concerned with any particular class of social phenomena (such as production, distribution, exchange, or consumption). Economics is instead concerned with a particular aspect of human behavior. One's decisions to have children or to be unfaithful to one's spouse are, on this definition, as much a part of economics as supply and demand for tuna. Robbins is, in effect, defining economics as the science of rational choice – that is, as neoclassical theory. Such redefinitions are characteristic of scientific development.[40] Robbins's definition remains controversial, since it excludes from economics some work that most people regard as economics, such as Keynesian theory.

Robbins, Knight, and the Austrians stress the individualism and subjectivity of economics, and they all emphasize the peculiarities of human action as an object of scientific investigation. They also agree with Mill that the basic premises of economics are well established and that these premises are not impugned by the empirical failures of the theory. In fact, the Austrians go further and argue that the basic premises are *a priori* truths.

With the intrusion of the views of the logical positivists in the 1930s came the first important change in the profession's views on the justification of economic theory. In 1938, Terence Hutchison published *The Significance and Basic Postulates of Economic Theory*. In this landmark book, Hutchison argues that economics, like other sciences, must formulate testable generalizations and subject them to serious tests. The statements of "pure theory" in economics are, Hutchison argues, empty definitional truths. Claims in economics are so hedged with *ceteris paribus* qualifications that they are untestable. With the weight of contemporary logical positivism behind him, Hutchison insisted that it was time for economists to start behaving like responsible scientists. The development of revealed-preference theory and Paul Samuelson's defense of what he calls "operationalism" also supported the demand that economics be recast into testable theories.

Hutchison's criticisms were immediately rebutted by economists such as Knight, but they remained disturbing. Could it be that economics did not meet the standards of empirical science? Some, such as Knight and the Austrians, were prepared to say that the standards of the natural sciences did not apply to economics. But most writers on economic methodology attempted to show that economics satisfied the more sophisticated (and weaker) criteria to which the logical positivists had already retreated.[41] Although Milton Friedman's well-known essay, "The Methodology of Positive Economics" (1953; see Chapter 7 in this anthology) does not refer to contemporary philosophy of science, it, too, attempts to show that economics satisfies broadly positivist standards.

For decades after its publication, Friedman's essay dominated work on the methodology of economics. Although almost all the many essays that have been written in response to it have been critical (like the brief comments in Chapters 8 and 9 of this volume), Friedman's essay has nevertheless remained the most influential work on economic methodology of the twentieth century.

One should not forget that there are many different methodological questions that one can ask about economics. The different branches and schools of economics face special methodological problems of their own, which are discussed in the six essays reprinted in Part IV of this anthology. Questions concerning the relations between positive and normative and the character of normative economics are the topic of Part III, although selections in other sections bear on this issue, too.

The field of economic methodology, including methodological studies of the details of branches and schools of economics has blossomed during the last fifteen years. Part V turns to some of the new directions within economic methodology, and the widespread changes in the contents of the other parts of this anthology reflect this blossoming. The extent to which the field has matured was brought home to me vividly by how hard it was to decide on what to include. In the first edition of this anthology, I noted that at least nineteen books specifically devoted to economic methodology had been published in English between 1975 and 1983. In the decade between the first and the second edition, I counted fifty. Since the second edition, there have been about one hundred more, and the outpouring of essays has increased at a greater pace. Just after the first edition of this anthology was published, a new journal, *Economics and Philosophy* began publishing works on methodology, the theory of rationality, and ethics and economics. Just before the second edition of this anthology came out, the *Journal of Economic Methodology* began publishing essays and reviews specifically focused on methodology. And the pace of publication of essays on economic methodology in journals in economic theory, philosophy of science, and history of economics or history of science has increased rapidly, too. Were it not for the generous advice of many others, who are expert in particular sub-domains of economic methodology, I would not have been able to do a competent job of designing this edition of the anthology.[42] The literature is now just too large!

The methodological questions economics raises are varied, difficult, and for the most part unanswered. When I compiled the first edition of this anthology, I was optimistic that collaboration between philosophers and economists would tame, if not answer, these questions. To some extent that optimism has been rewarded: progress has been enormous. Just compare the essays in a current version of *Economics and Philosophy* or *The Journal of Economic Methodology* with the essays in the early issues of either journal. I would like to think that this anthology, now in its third edition and its third decade, has contributed to that progress.

I am perhaps a little less optimistic now (or perhaps just older). The methodological problems economics raises are difficult, and progress is slow when philosophical argument has to contend with social forces and the reward structure within academic disciplines. There is so much more to be learned about the nature of economic models, how to compare and assess them, how to relate them to policy recommendations and empirical studies, and, most important, how to improve them. May this third edition continue to play a role in tackling these questions.

Notes

1. For defenses of scientific realism, see R. Boyd, "On the Current Status of Scientific Realism," *Erkenntnis* 19 (1983): 45–90 and R. Miller, *Fact and Method: Explanation, Confirmation and Reality in the Natural and the Social Sciences* (Princeton: Princeton University Press, 1987), Part III. For an influential antirealist position, see B. van Fraassen, *The Scientific Image* (Oxford: Oxford University Press, 1980).
2. See C. Hempel, *Aspects of Scientific Explanation and Other Essays in the Philosophy of Science* (New York: Free Press, 1965), and for an overview of debates about scientific explanation, W. Salmon, *Four Decades of Scientific Explanation* (Minneapolis: University of Minnesota Press, 1990).
3. This is Wesley Salmon's example from "Statistical Explanation," in W. Salmon, ed. *Statistical Explanation and Statistical Relevance* (Pittsburgh: University of Pittsburgh Press, 1971), p. 34.
4. The example is adapted from S. Bromberger, "Why Questions," in R. Colodny, ed. *Mind and Cosmos: Essays in Contemporary Science and Philosophy* (Pittsburgh: University of Pittsburgh Press, 1966), pp. 86–111.
5. In both *How the Laws of Physics Lie* (Oxford: Clarendon Press, 1983) and *Nature's Capacities and Their Measurement* (Oxford: Clarendon Press, 1989), Nancy Cartwright argues compellingly for the importance of causal notions in science, particularly physics. Explicitly causal accounts of explanation are developed in D. Lewis, "Causal Explanation," pp. 214–40 of his *Philosophical Papers*, vol. 2 (Oxford: Oxford University Press, 1986), R. Miller, *Fact and Method*, Part 2, and W. Salmon, *Scientific Explanation and the Causal Structure of the World.* The most sophisticated current account of causal explanation is in J. Woodward, *Making Things Happen* (Oxford: Oxford University Press, 2003).
6. See G. von Wright, *Explanation and Understanding* (Ithaca, NY: Cornell University Press, 1971) and, for a more introductory treatment, chapter 2 of A. Rosenberg, *Philosophy of Social Science* (Boulder, CO: Westview Press, 1988).
7. See D. Davidson, "Actions, Reasons and Causes," *Journal of Philosophy* 60 (1963): 685–700 and A. Rosenberg, *Microeconomic Laws: A Philosophical Analysis* (Pittsburgh: University of Pittsburgh Press, 1976), ch. 5.
8. Although the logical positivists and logical empiricists also talked about models, their notion, which was borrowed from formal logic, has little to do with the models that economists employ.
9. See F. Suppe, *The Structure of Scientific Theories*, 2nd ed. (Urbana: University of Illinois Press, 1977), esp. pp. 3–118.
10. This predicate view of models derives from P. Suppes, *Introduction to Logic* (New York: Van Nostrand Reinhold, 1957), ch. 12, J. Sneed, *The Logical Structure of Mathematical Physics* (Dordrecht: Reidel, 1971), and W. Stegmueller, *The Structuralist View of Theories* (New York: Springer-Verlag, 1979). For a convenient introductory treatment that follows the terminology that I prefer, see R. Giere, *Understanding Scientific Reasoning*, 2nd ed. (New York: Holt, Rinehart and Winston, 1982), ch. 5. These authors all develop this view as an account of theories rather than models. For a more detailed exposition of the view with reference

to economics, see my *The Inexact and Separate Science of Economics* (Cambridge: Cambridge University Press, 1992), ch. 5.

11. See Ludwig von Mises, *Human Action: A Treatise on Economics* (New Haven: Yale University Press, 1949). Other aspects of the Austrian approach are developed in Chapter 20.
12. The classic critique of the analytic-synthetic distinction is W. V. O. Quine, "Two Dogmas of Empiricism," in *From a Logical Point of View*, 2nd ed. (New York: Harper & Row, 1961), pp. 20–46. See also H. Putnam, "The Analytic and the Synthetic," in H. Feigl and G. Maxwell, *Minnesota Studies in the Philosophy of Science*, vol. 3, pp. 350–97, and C. Hempel, "A Logical Appraisal of Operationalism," in his *Aspects of Scientific Explanation*, pp. 123–33.
13. *An Inquiry Concerning Human Understanding* (1748; rpt. Indianapolis: Bobbs-Merrill, 1955), p. 47.
14. In rejecting foundationalism, I am following many contemporary philosophers. Recent influential antifoundationalists include Quine in "Two Dogmas," and "Epistemology Naturalized," in *Ontological Relativity and Other Essays* (New York: Columbia University Press, 1969), pp. 69–90 and I. Levi in *The Enterprise of Knowledge* (Cambridge, MA: MIT Press, 1980). The antifoundationalism of Quine and Levi derives in large part from the American pragmatists.
15. R. Carnap, *Logical Foundations of Probability* (Chicago: University of Chicago Press, 1950).
16. For an excellent overview of recent work on confirmation, see the entry on "Confirmation Theory" by P. Maher in *The Encyclopedia of Philosophy*, 2nd ed., edited by D. Borchert (London: Macmillan, 2005). For an overview of issues concerning theory appraisal with special reference to economics, see the entry by E. Eells and D. Hausman in the second edition of the *New Palgrave Dictionary of Economics* (London: Macmillan, 2007).
17. See K. Popper, *The Logic of Scientific Discovery* [1935] (London: Hutchinson & Co., 1959).
18. Contrast K. Popper, *Objective Knowledge: An Evolutionary Approach* (Oxford: Clarendon Press, 1972), ch. 1 with *The Logic of Discovery*, ch. 2, esp. p. 50. Despite this disavowal, Popper continues to stress the asymmetry between falsification and verification.
19. K. Popper, "Science: Conjectures and Refutations," in *Conjectures and Refutations: The Growth of Scientific Knowledge*, 3rd ed. (London: Routledge, 1969), pp. 33–65.
20. *The Logic of Scientific Discovery*, ch. 5. For contrasting views of the merits of Popper's philosophy of science and their applicability to economics, see B. Caldwell, "Clarifying Popper." *Journal of Economic Literature* 29 (1991): 1–33, D. W. Hands, *Testing, Rationality and Progress* (Lanham, MD: Rowman and Littlefield, 1992), ch. 11, and my *The Inexact and Separate Science of Economics*, ch. 10.
21. T. Kuhn, *The Structure of Scientific Revolutions* 2nd ed. (Chicago: University of Chicago Press, 1970), I. Lakatos, "Falsification and the Methodology of Scientific Research Programmes," rpt. in his *Philosophical Papers* (Cambridge: Cambridge University Press, 1978), vol. 1, pp. 8–101.
22. Especially in chapters 9 and 10 of *The Structure of Scientific Revolutions.*

23. See J. Brown, ed., *Scientific Rationality: The Sociological Turn* (Dordrecht: Kluwer, 1984) and, for an overview with applications to economics, D. W. Hands, *Reflection without Rules: Economic Methodology and Contemporary Science Theory* (Cambridge: Cambridge University Press, 2001), chapter 5 and U. Mäki, "Social Conditioning of Economics," in Neil deMarchi, ed. *Post-Popperian Methodology of Economics: Recovering Practice* (Boston: Kluwer, 1994), pp. 65–104.
24. See "Falsification and the Methodology of Scientific Research Programmes," pp. 116–38, and especially pp. 124n, 125n, and 175n.
25. For a modern collection of views concerning "understanding" in the social sciences, see F. Dallmayr and T. McCarthy, eds., *Understanding and Social Inquiry* (Notre Dame: University of Notre Dame Press, 1977).
26. See P. Winch, *The Idea of a Social Science and its Relation to Philosophy* (London: Routledge, 1958). Other prominent figures arguing that explanations in the social sciences involve interpretation are G. Anscombe, P. Geach, A. Meldon, and G. von Wright.
27. These general philosophical issues are linked to controversies concerning rational expectations in economics. See K. Hoover, *The New Classical Macroeconomics: A Sceptical Inquiry* (Oxford: Basil Blackwell, 1988).
28. See, for example, R. Chisholm, *Perceiving* (Ithaca, NY: Cornell University Press, 1957), ch. 11 and W. Quine, *Word and Object* (Cambridge, MA: MIT Press, 1960), chs. 4, 6.
29. For the bright spots in this history, see E. Roy Weintraub, *General Equilibrium Analysis: Studies in Appraisal* (Cambridge: Cambridge University Press, 1985). For some of the less successful aspects, see Weintraub's *Stabilizing Dynamics* (Cambridge: Cambridge University Press, 1991).
30. W. Jevons, *The Theory of Political Economy* (1871); C. Menger, *Grundsätze der Volkwirthschaftslehre* (1871); and L. Walras *Elements of Pure Economics* (1874), tr. W. Jaffé (1954). Many others were important, too, especially Alfred Marshall, whose *Principles of Economics* (1st ed., 1890) was for decades the main text of neoclassical economics. The common features in the work of Jevons, Menger, Walras, and Marshall or in the work of their immediate intellectual descendants should not be exaggerated. There also were sharp differences.
31. See particularly Mill's *Utilitarianism* (1863).
32. R. Lucas, "Unemployment Policy," *American Economic Review* 68 (1978): 353–57.
33. For an excellent overview, see G. Stadler, "Real Business Cycles," *Journal of Economic Literature*, 32 (1994): 1750–83.
34. L. Summers, "The Scientific Illusion in Empirical Macroeconomics," *Scandinavian Journal of Economics* 93 (1991): 129–48.
35. For an overview, see C. Camerer, G. Loewenstein, and M. Rabin, eds. *Advances in Behavioral Economics* (Princeton: Princeton University Press, 2004).
36. See particularly, A. Caplin and A. Schotter, eds., *Perspectives on the Future of Economics: Positive and Normative Foundations* (Oxford: Oxford University Press, 2007).
37. Leading neo-Ricardian theorists include J. Eatwell, P. Garegnani, and L. Pasinetti. The major theoretical work influencing the neo-Ricardians is P. Sraffa, *The*

Production of Commodities by Means of Commodities (Cambridge: Cambridge University Press, 1960).

38. Prominent Austrian economists include L. von Mises, F. von Hayek, and M. Rothbard.
39. See Alfred Eichner, ed., *A Guide to Post-Keynesian Economics* (White Plains, NY: M.E. Sharpe, 1978), and the *Journal of Post-Keynesian Economics.*
40. See Kuhn, *Structure of Scientific Revolutions*, chapters 9 and 10.
41. Fritz Machlup's work, some of which appeared in the first two editions of this anthology is particularly noteworthy in this regard. See F. Machlup, *Methodology of Economics and Other Social Sciences* (New York, Academic Press, 1978). I have argued that Mill's views are useful in allaying such empiricist qualms. See *The Inexact and Separate Science of Economics* (Cambridge: Cambridge University Press, 1992), esp. chs. 8 and 12.
42. I would in particular like to thank Roger Backhouse, Peter Boettke, Richard Bradley, Bruce Caldwell John Davis, Francesco Guala, Wade Hands, Kevin Hoover, Harold Kincaid, Uskali Mäki, Deirdre McCloskey, Michael McPherson, Philippe Mongin, Julie Nelson, Alex Rosenberg, Don Ross, and Margaret Schabas, to whom I sent appeals for help. Wade Hands, Kevin Hoover, and Elliott Sober read drafts of this introduction and helped improve it. One of great privileges of having worked so long on economic methodology is being able to count such wonderful people and wonderful intellects as friends.

PART ONE

CLASSIC DISCUSSIONS

The six selections reprinted in this section are a good sample of the major contributions to the philosophy and methodology of economics before the late 1930s, when logical positivism became influential. Not all the significant works could be included – even in abridged form – but many of the methodological insights of authors omitted here, such as J. E. Cairnes, J. N. Keynes, Carl Menger, W. S. Jevons, Alfred Marshall, and Ludwig von Mises appear in other essays in this anthology.

The materials collected in this section represent a number of different perspectives and have stood the test of time. Although economic theory has changed considerably since Mill or Marx or Veblen wrote, their appreciation of the methodological difficulties of economics still rewards careful study. One might, in fact, argue that thinking on economic methodology has advanced very little beyond the stage to which the authors in this section brought it.

ONE

On the Definition and Method of Political Economy

John Stuart Mill

John Stuart Mill (1806–73) was born in London, His father, James Mill, was a friend of Bentham and of Ricardo and did important work himself in psychology and political science. As John Stuart Mill explains in his autobiography, he was educated at home by his father, starting Greek at age 3 and Latin at age 8. By age 13 Mill had been through a complete course in political economy. Mill spent most of his life working for the East India Company. His *Principles of Political Economy* (1848) was the nineteenth century's most influential text in economics, and his *A System of Logic* (1843) was the century's most influential text in logic and the theory of knowledge. His essays on ethics and contemporary culture, such as *Utilitarianism* and *On Liberty*, continue to be extremely influential. Mill was an early defender of women's rights and of a moderate democratic socialism. The following selection is an abridgment of Mill's "On the Definition of Political Economy and the Method of Investigation Proper to It." Approximately the first quarter of the essay, in which Mill discusses the definition of economics, is omitted.

What is now commonly understood by the term "Political Economy" is not the science of speculative politics, but a branch of that science. It does not treat of the whole of man's nature as modified by the social state, nor of the whole conduct of man in society. It is concerned with him solely as a being who desires to possess wealth, and who is capable of judging of the comparative efficacy of means for obtaining that end. It predicts only such of the phenomena of the social state as take place in consequence of the pursuit of wealth. It makes entire abstraction of every other human passion or motive; except those which may be regarded as perpetually antagonizing principles to the desire of wealth, namely, aversion to labour, and desire of the present enjoyment of costly indulgences. These it takes, to a certain

Excerpted from "On the Definition of Political Economy and the Method of Investigation Proper to It" (1836). Reprinted in *Essays on Some Unsettled Questions of Political Economy* (1844), 3d ed., London: Longmans Green & Co., 1877, pp. 120–64.

extent, into its calculations, because these do not merely, like other desires, occasionally conflict with the pursuit of wealth, but accompany it always as a drag, or impediment, and are therefore inseparably mixed up in the consideration of it. Political Economy considers mankind as occupied solely in acquiring and consuming wealth; and aims at showing what is the course of action into which mankind, living in a state of society, would be impelled, if that motive, except in the degree in which it is checked by the two perpetual counter-motives above adverted to, were absolute ruler of all their actions. Under the influence of this desire, it shows mankind accumulating wealth, and employing that wealth in the production of other wealth; sanctioning by mutual agreement the institution of property; establishing laws to prevent individuals from encroaching upon the property of others by force or fraud; adopting various contrivances for increasing the productiveness of their labour; settling the division of the produce by agreement, under the influence of competition (competition itself being governed by certain laws, which laws are therefore the ultimate regulators of the division of the produce); and employing certain expedients (as money, credit, &c.) to facilitate the distribution. All these operations, though many of them are really the result of a plurality of motives, are considered by Political Economy as flowing solely from the desire of wealth. The science then proceeds to investigate the laws which govern these several operations, under the supposition that man is a being who is determined, by the necessity of his nature, to prefer a greater portion of wealth to a smaller in all cases, without any other exception than that constituted by the two counter-motives already specified. Not that any political economist was ever so absurd as to suppose that mankind are really thus constituted, but because this is the mode in which science must necessarily proceed. When an effect depends upon a concurrence of causes, those causes must be studied one at a time, and their laws separately investigated, if we wish, through the causes, to obtain the power of either predicting or controlling the effect; since the law of the effect is compounded of the laws of all the causes which determine it. The law of the centripetal and that of the tangential force must have been known before the motions of the earth and planets could be explained, or many of them predicted. The same is the case with the conduct of man in society. In order to judge how he will act under the variety of desires and aversions which are concurrently operating upon him, we must know how he would act under the exclusive influence of each one in particular. There is, perhaps, no action of a man's life in which he is neither under the immediate nor under the remote influence of any impulse but the mere desire of wealth. With respect to those parts of human conduct of which wealth is not even the principal object, to these

Political Economy does not pretend that its conclusions are applicable. But there are also certain departments of human affairs, in which the acquisition of wealth is the main and acknowledged end. It is only of these that Political Economy takes notice. The manner in which it necessarily proceeds is that of treating the main and acknowledged end as if it were the sole end; which, of all hypotheses equally simple, is the nearest to the truth. The political economist inquires, what are the actions which would be produced by this desire, if, within the departments in question, it were unimpeded by any other. In this way a nearer approximation is obtained than would otherwise be practicable, to the real order of human affairs in those departments. This approximation is then to be corrected by making proper allowance for the effects of any impulses of a different description, which can be shown to interfere with the result in any particular case. Only in a few of the most striking cases (such as the important one of the principle of population) are these corrections interpolated into the expositions of Political Economy itself; the strictness of purely scientific arrangement being thereby somewhat departed from, for the sake of practical utility. So far as it is known, or may be presumed, that the conduct of mankind in the pursuit of wealth is under the collateral influence of any other of the properties of our nature than the desire of obtaining the greatest quantity of wealth with the least labour and self-denial, the conclusions of Political Economy will so far fail of being applicable to the explanation or prediction of real events, until they are modified by a correct allowance for the degree of influence exercised by the other cause.

Political Economy, then, may be defined as follows: and the definition seems to be complete:

> The science which traces the laws of such of the phenomena of society as arise from the combined operations of mankind for the production of wealth, in so far as those phenomena are not modified by the pursuit of any other object.

But while this is a correct definition of Political Economy as a portion of the field of science, the didactic writer on the subject will naturally combine in his exposition, with the truths of the pure science, as many of the practical modifications as will, in his estimation, be most conducive to the usefulness of his work.

The above attempt to frame a stricter definition of the science than what are commonly received as such, may be thought to be of little use; or, at best, to be chiefly useful in a general survey and classification of the sciences, rather than as conducing to the more successful pursuit of the particular science in question. We think otherwise, and for this reason; that, with the consideration of the definition of a science, is inseparably connected *that of the philosophic method* of the science; the nature of the process by which its investigations are to be carried on, its truths to be arrived at.

Now, in whatever science there are systematic differences of opinion – which is as much to say, in all the moral or mental sciences, and in Political Economy among the rest; in whatever science there exist, among those who have attended to the subject, what are commonly called differences of principle, as distinguished from differences of matter-of-fact or detail, – the cause will be found to be, a difference in their conceptions of the philosophic method of the science. The parties who differ are guided, either knowingly or unconsciously, by different views concerning the nature of the evidence appropriate to the subject. They differ not solely in what they believe themselves to see, but in the quarter whence they obtained the light by which they think they see it.

The most universal of the forms in which this difference of method is accustomed to present itself, is the ancient feud between what is called theory, and what is called practice or experience. There are, on social and political questions, two kinds of reasoners: there is one portion who term themselves practical men, and call the others theorists; a title which the latter do not reject, though they by no means recognize it as peculiar to them. The distinction between the two is a very broad one, though it is one of which the language employed is a most incorrect exponent. It has been again and again demonstrated, that those who are accused of despising facts and disregarding experience build and profess to build wholly upon facts and experience; while those who disavow theory cannot make one step without theorizing. But, although both classes of inquirers do nothing but theorize, and both of them consult no other guide than experience, there is this difference between them, and a most important difference it is: that those who are called practical men require *specific* experience, and argue wholly *upwards* from particular facts to a general conclusion; while those who are called theorists aim at embracing a wider field of experience, and, having argued upwards from particular facts to a general principle including a much wider range than that of the question under discussion, then argue *downwards* from that general principle to a variety of specific conclusions.

Suppose, for example, that the question were, whether absolute kings were likely to employ the powers of government for the welfare or for the oppression of their subjects. The practicals would endeavour to determine this question by a direct induction from the conduct of particular despotic monarchs, as testified by history. The theorists would refer the question to be