

Introduction

This book defends the old-fashioned view that the basic axioms of economics are “inexact” and that economics proceeds by deducing the consequences of these axioms in particular circumstances. The method of economics is deductive, and confidence in the implications of economics derives from confidence in its axioms rather than from testing their implications. In looking back two generations to this traditional methodological wisdom, I shall be defending economics and economists from common but unwarranted criticisms. I shall also be taking issue with the views defended in other recent monographs.¹ In my view many of the basic principles of economics can be regarded as inexact laws, and the methods of theory appraisal that economists employ in practice are scientifically acceptable.

But there is another aspect of economic methodology I shall not defend: the commitment to economics as a “separate science.” To insist that any acceptable economic theory must, like current theory, aspire to capture the entire economic “realm” has no justification and leads, I shall argue, to stagnation. The keys to the methodological peculiarities of economics lie in its structure and strategy.

What is economics?

This book will be concerned only with contemporary microeconomic theory and general equilibrium theory. These theories are the best known of economic theories, the theories that have most influenced work in the other social sciences, and the theories which have been most discussed by philosophers, economists, and other social theorists.

In focusing on neoclassical economics, I am avoiding and begging questions about the definition and subject matter of economics. Phenomena do not come with the label “economic” attached to them.

¹ Blaug 1980a, Boland 1982b, 1986, 1989, Caldwell 1982, Klant 1984, Pheby 1988, Redman 1990, and Rosenberg forthcoming. My views are closest to those expressed in Stewart 1979.

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On the contrary, theorists have had to decide what counts as an economic phenomenon. Like every other science, economics must define its object.

We are so accustomed to thinking about economies, that we often fail to notice how remarkable it is that there are such “things.” As Marx points out with particular brilliance, market societies are strange human creations. Although they are constituted by the attitudes, actions, and artifacts of human beings, markets possess a very real objectivity, and they dominate the people whose actions perpetuate and constitute them. Although the “naturalness” of the domination of markets over human beings and the inevitability of market relations are, in Marx’s view, illusory, there is nothing illusory about the domination itself.

The fact that these human activities and products so control human beings in market societies is part of what Marx means when he discusses “alienation.” Consider the following story:

A man was terribly down on his luck, out of work and desperate. He had only a few dollars left in his bank account. He decided to try prayer. He went down to his cash machine, got down on his knees, and prayed. Then he checked his balance and found that he was worth millions!²

Whether this is a story of divine intervention or electronic failure, the picture of a man on his knees in front of a cash machine makes vivid the objectivity of market relations and the subjection of individual human beings to them.

Markets not only constrain the choices of individuals; they determine the fate of nations. Lester Thurow argues, for example, that in order to compete with Japan, the United States must increase its rate of investment (1980, pp. 96–7), otherwise it will suffer economic decline. What enforces this supposed necessity?

The world market. But what is that? What are markets? How do they work? How can they dominate a powerful nation of a quarter of a billion people? What are “economies”? What are the systems, norms, attitudes, and actions that economists study? What is “economics”? Attempts to answer these questions and to define economics are central to landmark works on economic methodology such as Mill’s “On the Definition of Political Economy and the Method of Investigation Proper to It” (1836) and Lionel Robbins, *An Essay on the Nature and Significance of Economic Science* (1932, 1935).

Mill defines economics as “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

² This story was reported to me by students in a philosophy of science course I was teaching in 1979 at the University of Maryland.

not modified by the pursuit of any other object” (1836, p. 323). Such “substantive” definitions take economic phenomena to be linked to matters of wealth, but most also carry with them, as in Mill’s words, commitments to a mode of explanation and a kind of theory.³ Robbins, in contrast, offers a “formal” definition of economics as “the science which studies human behavior as a relationship between ends and scarce means which have alternative uses” (1932, p. 15). According to Robbins, economics is not concerned with production, exchange, distribution or consumption as such. It is instead concerned with an aspect of all human action. Although economists have not been able to draw the boundaries of their discipline in this way, they nevertheless like to think of their subject matter, as Robbins urges, as the consequences of rational choices in circumstances of scarcity. This vision has a determining influence on the questions theorists ask and the answers they are willing to accept. However, it is not the only possible vision of economics, and we shall see some of its limitations, but no alternatives will be explored here. *Indeed, to avoid unnecessary repetition, I shall usually omit the adjective “neoclassical” and just speak of “economics” when I am discussing neoclassical economics.* This is merely a convenience, not a covert attempt to denigrate other schools of economics or to define them out of existence.

Methodology and the problem of theory assessment

This is a book on economic methodology. But what is THAT? Just what might an investigation of economic methodology accomplish? There are at least four distinct answers.

First, investigators may simply want to know how the discipline of economics “works” now and how it has worked in the past. They may want to know answers to questions such as: How does one succeed as an economist? What character traits, stylistic preferences, or values are encouraged among economists? To what extent are the aims of economists bound up with the policy demands that are made of them? One may want to know the answers to these sociological and historical questions simply because one wants to understand the discipline, or one may have further aims, which answers to these questions may help one to achieve. One might want, for example, to learn how to get tenure in an economics department, to understand how empirical knowledge is possible, or to convict some group of economists of idiocy.

³ Indeed Mill also defines economics in terms of the causal factors with which it is concerned. This dual specification in terms of causes and domains is crucial to the notion of economics as a separate science. The contrast between Mill and Robbins is thus less than it may appear, especially since the notion of a specifically economic “realm” has persisted. See section 6.4 below.

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Second, one may study methodology to help assess aspects of economics from a practical or policy perspective. The questions which motivate such assessments are varied: What role should economics play in the curriculum of secondary schools or colleges? What role should economists play in policy-making? To what extent should other inquiries model themselves after economics? Philosophers are supposed to have a central role in such practical evaluation, which has not been sharply distinguished from philosophical inquiry (see Rorty 1979, p. 4).

The third reason for being interested in economic methodology is my reason. I would like to understand better how people manage to learn about the social world around them. By seeing how economists have succeeded – and failed – in acquiring such knowledge, one may be able to determine how best to study social phenomena: to what extent social inquiry ought to resemble inquiry in physics, how much humans can know about social phenomena, and what limits social inquiry encounters. Since such philosophical inquiry is in my view itself a kind of social inquiry, the whole project might appear absurd. I will defend it below in chapter 14.

Most of those who study economic methodology do so because they want to improve it or to help economists to practice it better. Just as economists may seek to improve monetary policy or the tax structure or compliance with the ideals of either, so students of economic methodology may seek to improve the way economic theories are generated and tested and the incentives that encourage economists to undertake certain kinds of study and to avoid others. Such ambitions make sense only if there is some way to determine whether one methodological rule is superior to another. Practical efforts to improve economic methodology will thus be heavily influenced by philosophical theories concerning knowledge acquisition. For one of the most important senses in which methodological norm N may be superior to norm M is if one is more likely to learn something if one follows N than if one follows M . The practical methodological implications of my views are drawn together and defended in chapter 14.

Many people regard economic methodology as concerned exclusively with the problem of theory *appraisal*, the problem of distinguishing good theorizing and good economic theories from bad theorizing and bad theories. Although theory appraisal is a central issue, about which I shall have a great deal to say, there are other philosophically interesting questions to ask about economic theory. One should also inquire about the *structure* of microeconomics and general equilibrium theory, about the *strategy* and *heuristics* that guide work in contemporary economics, about the *goals* of economic theorizing, and about the relations between

economic theory and policy questions. As this book will show, these questions are crucial both in themselves and in order to understand the peculiarities of theory appraisal in economics. One should also ask more detailed questions that do not fall neatly under any of these general rubrics. Notice that many questions besides those related to theory appraisal are also normative. For example, to ask what the goals of economics are is to ask not only what they have been, but also what they ought to be.⁴

A reader's guide

The central problem of theory appraisal cannot be broached before one understands the content, structure, and strategy of economic theory as well as a good deal of philosophy of science. Yet readers would be impatient with so much introductory material. Accordingly I have placed the general discussion of philosophy of science in an appendix, which has been organized for easy use. I hope readers will find it a helpful reference. Those without any background in the philosophy of science may want to read it straight through before starting chapter 1.

Introductory material concerning economic theory could not be placed in a second appendix, for how one understands this material determines how well one grasps the structure and strategy of economics, which are the subject matter of part I. I think that the way in which the economic background is presented should be of value to students of economics and that it may even be of interest to trained theorists. Although the first four chapters contain many familiar analyses and can be skimmed by readers with a solid background in economics, they should not be skipped altogether, for they define the questions that the rest of the book attempts to answer, and they provide initial sketches of important philosophical distinctions. I urge readers not to skip sections 3.6, 3.7, and 4.6.

Chapter 1 focuses on the conception of rationality that is embodied in contemporary economics and is central to it. After presenting ordinal utility theory, I offer a critique of revealed preference theory and an introduction to expected utility theory. If one wants to understand economics, the theory of rationality is the place to begin.

Chapter 2 presents consumer choice theory and an example of a simple economic *model*, and it makes preliminary comments on the apparent empirical anomalies the theory faces. Its material is well-known, although

⁴ It is not obvious how one should go about answering such questions, but rather than address explicitly such "metamethodological questions" – such questions concerning the methodology of the methodology of economics – I shall show how to answer them by doing methodology.

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textbooks rarely develop the connections between specific models and fundamental theory so explicitly. Section 2.6 provides an illustration of “bootstrapping,” a contemporary theory of confirmation discussed in section A.10.3, and it can be skipped by those who are not interested in confirmation theory.

Chapter 3 carries out the same tasks for the theory of the firm and for general equilibrium theory. In doing so, it pulls together the discussions of the first three chapters to offer a general sketch of the causal structure and basic principles of economics. It takes issue with the common view that general equilibrium theory is the fundamental theory of contemporary economics. Equilibrium theory, not general equilibrium theory, is fundamental.

Chapter 4 sketches the contemporary theory of economic welfare. It shows that welfare economics is an esoteric discipline, whose questions are determined more by equilibrium theory than by practical problems of economic welfare. In section 4.6 I explain why economists embrace perfect competition as a moral ideal. The argument I explore also explains why one finds among welfare economists a seemingly paradoxical combination of moral authority and moral agnosticism.

In the remaining three chapters of part I, I attempt to say more abstractly and precisely what economic theories and models are and to characterize their overall structure and strategy.

Chapter 5 is concerned with theories and models in economics. It surveys philosophical conceptions of theories and defends a common-sense view of theories as sets of lawlike statements that are systematically interconnected. Models are conceptual explorations without empirical commitments. They are definitions of predicates or kinds of systems. Models can be *used* to theorize, explain, or predict, when one offers “theoretical hypotheses” asserting that parts of the real world belong to the extension of the predicate a model defines.

Chapter 6 is one of the most important chapters in the book. It is concerned with the global strategy and structure of economic theory. After arguing that Thomas Kuhn’s and Imre Lakatos’ notions of “paradigm” and “research program” are misleading and not sufficiently detailed, I sketch the structure and strategy of economics as an inexact and separate science and comment on the role of abstract general equilibrium theories in this enterprise.

Chapter 7 concludes part I with an illustrative case study of Paul Samuelson’s famous overlapping-generations model.

Part II, “Appraisal,” focuses on problems of theory assessment. I develop my views in chapters 8 and 12, which are the most important chapters in this part. Chapters 9 to 11 are devoted to criticizing the views

of others and may be skipped by those who are not interested in the views I criticize.

In **Chapter 8** I develop and defend the traditional conception that economics is an inexact science that investigates deductively the implications of assumptions that are known to be approximately true. I consider several interpretations of the problematic notion of inexactness or approximate truth and develop a concept of inexactness as vague implicit qualification. I explain how statements with vague implicit qualifications can be true and what conditions must be met if one is to have good reason to accept them. Chapter 8 also presents an interpretation of J. S. Mill's deductive method, which still appears to dominate methodological practice in economics.

This view of theory assessment was challenged and rejected several decades ago and has been replaced by more "positivistic" or "modernist" views of economic methodology, which are the subject of criticism in **chapter 9**. In developing and criticizing the views of Terence Hutchison, Paul Samuelson, Fritz Machlup, Milton Friedman, and Tjallingis Koopmans, this chapter highlights the "methodological schizophrenia" of contemporary economics, in which methodological pronouncements and practice regularly contradict one another.

Chapter 10 criticizes Karl Popper's views on the philosophy of science, which have been particularly influential among writers on economic methodology. Popperian critics of economics are right to claim that economists seldom practice the falsificationism that many preach, but the problem is with the preaching, not with the practice: falsificationism is not a feasible methodology.

Chapter 11 turns to Popper's disciple and then critic, Imre Lakatos, whose influence on economic methodologists is second only to Popper's. Although Lakatos provides more resources with which to defend economics than Popper, his views are also inadequate and for a similar reason. Both Popper and Lakatos deny that one can judge how close to the truth or how likely to be true any scientific statement is. One is consequently unable to use such judgments either in engineering or in theoretical science. Popper and Lakatos are implicitly calling for a radical and destructive transformation of human practices.

Chapter 12 returns to Mill's inexact deductive method, as developed in chapter 8. I concede that it is too dogmatic, but I show how economics can be scientifically respectable, even though economists appear to conform to this method. The peculiarities of theory appraisal in economics follow more from the difficulties of testing in economics than from an aberrant view of confirmation. Chapter 12 considers some of the anomalies to which expected utility theory gives rise, to show how

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disconfirmation of basic principles of economics is possible and to expose the large and legitimate role that pragmatic factors play in theory appraisal in economics.

Chapter 13 concludes part II with a case study of the reactions of economists to experimental work on so-called “preference reversals.” In this case the profession has not relied on an unacceptably dogmatic view of theory appraisal. Such dogmatism as there is (and there is some) stems from the commitment of economists to a vision of economics as a separate science.

Part III, “Conclusions,” pulls together this long argument. In **chapter 14**, I defend the critical implications of chapter 12 against two further arguments that would justify dismissing anomalous experimental results, such as those concerning preference reversals. I then draw out some of the implications of my philosophical conclusions for the practice of economics, and defend the legitimacy of my “preaching” against criticisms such as those voiced most compellingly by Donald McCloskey.

In **chapter 15**, I summarize the argument and show that the methodological peculiarities of economics depend to a considerable extent on the fact that it is a social science. The fact that equilibrium theory includes a theory of rationality helps to explain why positive and normative economics are so intermingled, why economists are so strongly committed to their theory, and why they pursue such a distinctive strategy.

Although this book is an extended argument for a particular vision of economic methodology, it is also designed to serve as a reference work and an advanced textbook on economic methodology. It is written mainly for an audience of economists and graduate students in economics, but the issues with which it is concerned are also of interest to philosophers, other social scientists, and to policy-makers. The introductory material is designed to make the book accessible to these different audiences. I have tried to lay out a rich and coherent vision of the unique neoclassical theoretical enterprise, its special handicaps, and its brilliant, fascinating but unsuccessful strategies for overcoming them.

Sources and acknowledgments

When I began writing this book, almost four years ago, I thought I could pull together the methodological views I had expressed in *Capital, Profits, and Prices* (1981a) and in journal articles and produce a monograph in a few months. How wrong I was! In developing the extended argument of this book I changed my mind both about details and on many fundamental issues. I am gratified to be able to correct so many mistakes in previous works and am appalled that there were so many to correct.

Echoes of earlier works remain. The most distinct are to be found in chapters 5, 8, 9, 10, 12, and the appendix. The view presented in chapter 5 of theories and models is essentially that of chapter 3 of *Capital, Profits, and Prices*. The view of economics as employing an inexact deductive method receives a truncated exposition and defense in “John Stuart Mill’s Philosophy of Economics” and chapter 7 of *Capital, Profits, and Prices*. “The Deductive Method” (1990a), which was drawn from early versions of chapters 8 and 12, is much closer to them in its content. Chapter 9 draws on “Economic Methodology and Philosophy of Science” (1988b) and “Economic Methodology in a Nutshell” (1989b). The discussion of Popper’s views in chapter 10 follows my “An Appraisal of Popperian Methodology” (1988a). A version of chapter 13 entitled, “On Dogmatism in Economics: The Case of Preference Reversals,” will be appearing late in 1991 in the *Journal of Socio-Economics*. The appendix incorporates material from the first half of the introduction to my *The Philosophy of Economics* (1984b).

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