

Introduction: What is philosophy of economics?

I wound up doing philosophy of economics almost by chance. As a graduate student in philosophy at Columbia University, I was searching for a dissertation topic when John Eatwell came to town and delivered an exciting series of lectures on the so-called Cambridge Controversy in capital theory. The history he described was so unlike anything suggested by the philosophy of science I had studied that it seemed that there might be something of interest to be said about it. The slogan in philosophy of science at the time was, after all, that one should study what scientists actually do. And applying that slogan to economics seemed particularly interesting, since it was by no means uncontroversial that economics was a science. Furthermore, I had a tremendous resource ready to hand in Sidney Morgenbesser, who knows more about these issues than any other living philosopher. So over the Christmas holidays in 1976 I decided to do a dissertation on philosophy of economics.

It was a lucky choice. I had more than one great teacher in the subject, for Isaac Levi gradually convinced me of the centrality of problems concerning rational choice, and Ronald Findlay generously coached me into a modest competence in capital theory. My timing was perfect, too, for the great wave of contemporary interest in philosophy of economics was just beginning. Hollis and Nell's *Rational Economic Man*, Rosenberg's *Microeconomic Laws: A Philosophical Analysis*, and Latsis's collection *Method and Appraisal in Economics* had just been published, and the authoritative works or collections in the field by Blaug (1980), Boland (1982), Caldwell (1982), Hutchison (1981), Pitt (1981), Samuels (1980), Stegmüller et al. (1982), and Stewart (1979) were shortly to appear. Conferences in 1979 at Virginia Polytechnic Institute and Michigan State University permitted me to meet many of the other philosophers and economists working on issues in economic methodology, and my career was launched.

The essays collected here are a sample of what I have written over the last dozen years, and they reflect the development of my knowledge and perspective. In its most general outline, my view of how economics ought to be practiced has changed relatively little, but I have made and, I hope, corrected many mistakes in the details. Good interdisciplinary work demands an awful lot of knowledge, which I am still acquiring. The few major revisions in my views concern the connections between "positive" economic theory and theories of rationality and welfare. These have come largely from the wide exposure to work on rationality

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and economic welfare that has resulted from my editing *Economics and Philosophy*.¹ Were I rewriting these essays today, I would not only patch up many details, but I would place greater emphasis on the fact that economics is built around a normative theory of rationality. For, as I argue in *The Inexact and Separate Science of Economics*, that fact is central to an appreciation of the methodological peculiarities of economics.

When a philosopher sets forth upon a career of peering over the shoulders of economists, he or she should expect not only a pinched neck but some loss of identity. For there is no way to immerse oneself in the goings on of one discipline without feeling oneself becoming a part of it. The role of spectator or voyeur can also be frustrating, for after a while one wants to take part. I cannot participate as a full-blown economist, but increasingly I have come to feel that some of the insights and arguments I have developed are of value to economists. Yet with few exceptions the essays included here have appeared in books and journals read mainly by philosophers. One motive in collecting these essays is to make them accessible to economists.

Many of my ideas have been incorporated into my two books – *Capital, Profits and Prices: An Essay in Philosophy of Economics* (1981) and *The Inexact and Separate Science of Economics* (1992) – but some of the discussions, particularly of historical matters and of causality, do not appear in the books and many of the particular arguments may be more accessible and salient when not integrated into a monograph. Since few philosophers or economists are likely to have come across more than a few of the essays collected here, their assembly and republication may be of some value.

ISSUES IN ECONOMIC METHODOLOGY

The essays in this book are concerned with “economic methodology.” I use these words to cover everything that bears on how economics ought to be practiced and how the products of economics ought to be appraised. These essays are thus all at least implicitly normative, although the advice and preaching are typically subdued and are grounded in an attempt to appreciate clearly how economists do their various jobs. The first chapter, “Economic methodology in a nutshell,” provides a thumbnail sketch of the history and contemporary state of the discipline.

1 With Michael McPherson. Another bit of wonderful good luck. Colin Day, then editorial director at Cambridge University Press, was the matchmaker and encouraged us to found the journal. Working with authors, referees, and editorial board, and especially Mike has been a tremendous education.

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My writings on economic methodology have been concerned with a wide range of issues, which can be grouped under the following eight headings.²

Problems of appraisal, idealization, and inexactness

When most people think of economic methodology, these are the issues that first come to mind. One striking fact about economics is that it is built on generalizations which, if taken literally, are false. People are not able to rank all objects of choice. Their preferences are not always transitive. They do not always want more. Firms do not always attempt to maximize net returns. How can economics contain such claims and still be “good science?” How can economists make these claims and still be “good scientists?” Like everybody else writing on economic methodology, I have a great deal to say about these problems, particularly in Part I, “Theory appraisal.” But I would insist that these are neither the only nor necessarily the best questions to focus on.

Up until the 1930s the dominant view of theory appraisal in economics was that economics proceeds by deducing the consequences of well-established generalizations about human behavior and technology when these are conjoined with premises concerning specific circumstances. Since these generalizations concern only the “major causes” of economic phenomena, the deduced consequences will be inexact and sometimes badly mistaken. Such predictive failures are only to be expected, for there are many “disturbing causes” left out of the theory, and the consequences follow only *ceteris paribus*. Since the initial generalizations are well-established by introspection or everyday experience, the failures do not cast doubt on economic theory itself. This is John Stuart Mill’s view, which I reformulate and defend in Chapter 3, “John Stuart Mill’s philosophy of economics.” With various qualifications I still think it is the correct view of theory appraisal in economics.

Over the past fifty years most economists have come to the opposite conclusion. They have repudiated the deductive method, but only in principle, not in practice. Some of the reasons for this repudiation have been bad ones. As economic methodologists who have been influenced by the work of Karl Popper have pointed out, *ceteris paribus* claims are not logically falsifiable. They are not logically inconsistent with any possible observation reports. Apparent disconfirmations can always be attributed to some disturbing cause. But as I argue in Chapter

² One set of important issues that is badly underrepresented both in the methodological literature as a whole and in my writing are those especially concerned with econometrics. Although the status and accomplishments of econometrics are controversial, it is obvious that econometrics links economic theory to statistical data. The practices and problems of econometrics are thus relevant to discussions of the appraisal of economic theories. But writers on economic methodology have typically paid little attention to econometrics, and writers on econometric methodology have often focused on problems of mathematical statistics without explicitly drawing the connections to philosophical problems of theory appraisal.

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6, “An appraisal of Popperian methodology,” individual theories in science are never logically falsifiable. As I stress both in Chapter 6 and in Chapter 7, “Is falsification unpracticed or unpracticable?” there is no reason to criticize economists for failing to meet a standard that no scientist can meet.

Not all of the reasons for repudiating the deductive method have been bad ones.³ If observations are capable only of confirming economic theories or of demonstrating the influence of some “disturbing cause,” then what can economists learn? And if there is no way to learn from observation, then economics cannot be an empirical science. Any method of theory appraisal that rules out the possibility of learning from experience must be repudiated.

In Chapter 4, “The deductive method,” I present and answer this criticism. It is fair to insist that disconfirmation must not be ruled out by methodological fiat, but I argue that the sort of deductive method that economists still cling to does not do so. Anomalous observations count so little in economics, not because of economists’ commitment to any unreasonably dogmatic view of theory appraisal, but because these observations constitute poor data with no clear evidential relevance. If the price of wheat goes up and demand for wheat rises, it is more likely that some disturbing cause is at work than that the law of demand is mistaken. When better data are available, as in the case of the preference reversal phenomena discussed in Chapter 15, leading economists are prepared to admit that fundamental postulates of economics are false and should be revised. Some dogmatism remains, not because of any mistaken view of testing or confirmation, but because economists place unjustifiable constraints on which revisions they are willing to consider.

In my view there is nothing particularly remarkable about the views of theory appraisal that most economists accept. As I argue in Chapter 4, “The deductive method,” and Chapter 8, “The limits of economic science,” the peculiarities of theory appraisal in economics arise from the special circumstances economists must contend with and from the strategies they have adopted. Economists are blessed with good reason to believe their fundamental behavioral postulates, but they are cursed with such problems in experimentation and data gathering that they are largely unable to learn from experience. The strategy they have adopted is to require that the domain of economics be spanned by a single unified theory in which rationality and acquisitiveness have central roles.

Problems of bad philosophy

Writers on economic methodology have, sensibly enough, looked to contemporary philosophy of science for guidance. Unfortunately much of contemporary

3 As Abraham Hirsch and Neil de Marchi helped me to see.

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philosophy of science, particularly the grand systems of Popper, Lakatos, and the logical positivists, contains many mistakes and provides poor guidance. Consequently a good deal of methodology has been devoted to undoing the damage done by philosophers and economists under their influence. Chapter 5 is a brief attempt to make vivid exactly what is wrong with the message of Milton Friedman's influential essay, "The methodology of positive economics." Chapters 6 and 7 explore deep difficulties in the Popperian views that have been so prominent among economic methodologists.

Problems of economic modeling

Theoretical economics is devoted to the formulation of economic models and to essentially mathematical inquiry into their implications. Why? What are economic models? What are the differences between models and theories? Is this concern with models a strength or weakness of theoretical economics? These central questions have commanded too little attention in the literature on economic methodology. In Chapter 3, "On the conceptual structure of neoclassical economics," I sketch the view of these matters that I defend (with a few differences concerning details) in both my books. Models are like definitions, and in working on them, economists engage in the sort of conceptual exploration and development which has been essential in the growth of all the sciences. To say that individuals are rational if and only if their preferences are complete and transitive and they choose what they most prefer is to offer a model of rationality. Theories, on the other hand, are sets of lawlike assertions, which employ the concepts developed in the formulation of models. Modeling is ultimately of value in an empirical discipline only insofar as it leads to improvements in theories.

Global theory structure

As philosophers have come to emphasize over the past generation, sciences are not collections of unconnected theories. To the contrary, as philosophers and historians such as Kuhn, Lakatos, Laudan, and Shapere have stressed, one finds enormous continuity in science, and typical day-to-day theoretical work is heavily constrained by a set of shared values, heuristics, models, fundamental laws, and so forth. Since particular theories in orthodox economics – whether concerned with human capital, rational expectations, or economic welfare – show intimate connections, these philosophical insights were eagerly seized upon by economists, who saw a "paradigm" or a "research program" under every economic bush or tree. In my view, which is sketched in Chapter 2 and defended at length in Chapter 6 of *The Inexact and Separate Science of Economics*, the particular characterizations of the global theory structure of scientific disciplines defended by Kuhn and Lakatos

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do not fit economics very well. And much more importantly, even if they did, they have too little structure to provide one with much help in understanding economics. As I hint in Chapter 3, “On the conceptual structure of neoclassical Economics” and Chapter 12, “What are general equilibrium theories?” it is more useful to see standard economics as developing the implications of a single fundamental theory.

Problems of definition and scope

Classic works on economic methodology such as John Stuart Mill’s “On the Definition of Political Economy and the Method of Investigation Proper to It” (1836) and Lionel Robbins’s *An Essay on the Nature and Significance of Economic Science* (1932, 1935) devote considerable attention to the definition of economics. To modern readers this emphasis may seem old-fashioned. Surely it is obvious enough what economics is. On the contrary, it seems to me that economists typically accept competing definitions of their subject as concerned with a particular set of causal factors (rational acquisitiveness) and as concerned with a particular realm of social behavior. Central to contemporary economics is the implicit but highly contestable conviction that the two definitions coincide, that (at a suitable level of approximation) the causal factors with which economists are concerned provide a complete theory of their subject matter. The discussion in Chapter 3 of Mill’s view of economics as a “separate science” and the inquiry in Chapter 12 into the role of general equilibrium theory broach these issues, but they call for deeper treatment. These same issues are also lurking in Chapter 13, “Arbitrage arguments.” I argue in Chapter 6 of *The Inexact and Separate Science* that the core of the methodological distinctiveness of economics lies here.

Problems of social science

Economics is concerned with social phenomena, and the entities with which social theories are concerned are in important ways unlike the entities with which the natural sciences are concerned. In particular, human beings can learn the theories that purportedly describe their behavior and can adjust their behavior in consequence. Second, human beings act for reasons, and their actions can be appraised for their rationality (and morality) as well as causally explained. Irrational action is consequently likely to be unstable. Third, “folk-psychological” accounts of human action (of which standard economic theory is a variant) see action as depending on constraints, beliefs, and preferences, and beliefs and preferences introduce elements of “intentionality.” For human action depends not only on the “facts” but on how the facts are viewed by the agents involved. For example, in *Hamlet*, the fact that the wine was poisoned had physiological consequences for

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Gertrude regardless of her subjective state. But the fact that the wine was poisoned had consequences for Gertrude's choices only insofar as she was aware of it.

There is a large literature in philosophy of the social sciences concerning whether the distinctive features of social phenomena require that social inquiries differ in crucial ways from inquiries about nature, and this literature finds echoes in writings on economic methodology. I have until recently been inclined to downplay the differences between the natural and social sciences, and in Chapter 14, "Explanatory progress in economics," I offer a partial defense of scientific progress in economics against Alexander Rosenberg's view that the social sciences have not progressed.

Problems of causality

Although many economists, especially those who have been most influenced by logical positivism, are uneasy about the use of causal language, economics is full of causal claims. Over the last two generations only econometricians have been explicitly concerned about causality, and the literature on the topic they have produced is rarely cited by writers on economic methodology. Although the three essays in Part II draw on this econometric literature, they do not attempt to contribute to it. They are instead concerned with theoretical issues in economics to which concepts of causation are crucial. As argued in Chapter 11, "Supply and demand explanations and their *ceteris paribus* clauses," causal concepts are at the bottom of the so-called *Methodenstreit* over demand curves in the 1950s and 1960s. As Chapter 9, "Are there causal relations among dependent variables?" documents, causal questions are particularly pressing and puzzling when addressing macroeconomic problems. Chapter 10, "Classical wage theory and the causal complications of explaining distribution," illustrates how causal problems arise in functional distribution theory and how the classical economists overcame them.

Detailed problems

Finally there are the details, where all the substance and hard work are to be found. Some of these detailed problems, such as, "In what sense does the intersection of an IS and an LM curve determine the rate of interest?" are related to general issues, in this case concerning causation. But there are many puzzles that stand alone, too. For example, abstract general equilibrium theories, which are discussed in Chapter 12, are peculiar. They take the form of mathematical investigations of the existence, uniqueness, stability, and welfare properties of equilibria of nonexistent and indeed typically quite impossible economies, in which, for example, everybody has perfect knowledge of all present and future commodities and prices. What's the point? Chapter 13, "Arbitrage arguments," provides a

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second example. Economists often are inclined to deny the relevance of findings of irrationality on the grounds that competitive markets will prevent irrationality from having any major consequences. Chapter 13 provides a detailed analysis of the structure of the argument they are relying on.

STRUCTURE AND ACKNOWLEDGMENTS

I have divided the seventeen essays of which this collection consists into four sections: Part I, "Methodology and theory appraisal," is concerned with the issues that many people think of as constituting the whole subject. The section as a whole offers my view of the special problems of theory appraisal in economics and the methods economists do and should employ. Two of the eight essays, "On the conceptual structure of neoclassical economics" and "Why look under the hood?" are published here for the first time. The first is a revised version of a talk delivered to the Allied Social Sciences meetings in 1982. A revised version of the second is incorporated into chapter 9 of *The Inexact and Separate Science of Economics*.

"Economic Methodology in a Nutshell" is reprinted from *Journal of Economic Perspectives* 3 (1989), pp. 115–27.

"John Stuart Mill's Philosophy of Economics" is reprinted from *Philosophy of Science*, 48 (1981), pp. 363–85.

"The Deductive Method" is reprinted from *Midwest Studies in Philosophy*, 15 (1990), pp. 372–88.

"An Appraisal of Popperian Methodology" is reprinted from N. de Marchi, ed., *The Popperian Legacy in Economics*. Cambridge: Cambridge University Press, 1988, pp. 65–86.

"Is Falsificationism Unpracticed or Unpracticable?" is reprinted from *Philosophy of the Social Sciences*, 15 (1985), pp. 313–19.

"The Limits of Economic Science" is reprinted from N. Rescher, ed., *The Limits of Lawfulness*. Pittsburgh: Center for Philosophy of Science, University of Pittsburgh, 1983, pp. 93–100.

Part II, "Causality in economics," documents the importance of causal concepts in theoretical economics. The three essays in this section reveal mistakes economists and sociologists have made as a consequence of supposing that they could avoid making causal claims, and the essays show how causal notions help to resolve disputed issues, such as the nature of the *ceteris paribus* clauses attached to demand and supply functions.

"Are There Causal Relations Among Dependent Variables?" is reprinted from *Philosophy of Science*, 50 (1983), pp. 58–81.

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“Classical Wage Theory and the Causal Complications of Explaining Distribution” is reprinted from J. Pitt, ed., *Change and Progress in Modern Science*. Dordrecht: Reidel, 1985, pp. 171–97.

“Supply and Demand Explanations and Their *Ceteris Paribus* Clauses” is reprinted from *Review of Political Economy*, 2 (1990), pp. 168–87.

Part III, “Cases and puzzles,” collects four essays which are concerned with detailed questions about particular theories, arguments, episodes, and theoretical developments in economics.

“What Are General Equilibrium Theories?” is reprinted from W. Sieg, ed., *Acting and Reflecting*. Dordrecht: Kluwer, 1990, pp. 107–14.

“Arbitrage Arguments” is reprinted from *Erkenntnis*, 30 (1989), pp. 5–22.

“Explanatory Progress in Economics” is reprinted from *Social Research*, 56 (1989), pp. 361–81.

“On Dogmatism in Economics: The Case of Preference Reversals” is reprinted from *The Journal of Socio-Economics* 20 (1991), pp. 205–25.

Part IV, “Postscripts,” is concerned with “meta-methodology.” Both essays reflect on the nature and difficulties of the project of doing philosophy of economics.

“How to do Philosophy of Economics” is reprinted from P. Asquith and R. Giere, eds., *PSA 1980*. East Lansing: Philosophy of Science Association, 1980, pp. 352–62.

“Reflections on Philosophy and Economic Methodology” was written especially for this volume, although it draws heavily on “Philosophy and Economic Methodology,” published in P. Asquith and P. Kitcher, eds. *PSA 1984*, vol. 2. East Lansing: Philosophy of Science Association, 1986, pp. 231–49.

Although I wrote these essays and am responsible for the errors they contain, I could not have written them without the criticisms and suggestions of virtually everyone who has been working in this area. I have learned from all of my many co-workers in reflecting upon that remarkable human enterprise called “economics.”

PART I

Methodology and theory appraisal

The eight essays in this part are concerned with the issues of theory appraisal that dominate writings on the methodology of economics. Chapter 1 offers a general overview of economic methodology and emphasizes that the subject is not exhausted by questions of theory assessment. Chapters 2, 3, 4, and 8 develop my views that the traditional deductive method provides (with some revisions) the best overall basis for assessing economic theories and that the difficulties involving theory assessment in economics result from the complexities of the subject matter economists study and the strategies or heuristics they use to simplify it. Chapters 5, 6, and 7 take issue with the influential views of economic methodology defended by Milton Friedman and Karl Popper.