

Current Issues in the Philosophy of Economics

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Philosophy of science and economic theorizing

This volume represents a contribution to the philosophy of economics with a distinctive point of view. The contributors to the volume have not by and large taken up the general and abstract issues that have usually occupied philosophers in their discussions of the philosophy of economics in the past. Instead, they have selected particular areas of economics and have probed these areas for the philosophical and methodological issues that they raise: for example, the meaning of causal ascriptions in econometrics, the reliability of large economic models, the status of the assumptions of rationality that go into the public goods argument, the explanatory import of equilibrium analysis, and the role of experiment in economics. The primary essays are written by philosophers, who were invited to concentrate on philosophical issues that arise at the level of the everyday theoretical practice of working economists. Commentary essays have been provided by working economists, who were asked to respond to the philosophical arguments from the standpoint of their own disciplines. The volume thus represents something of an "experiment" in the philosophy of science, striving as it does to explore methodological issues across two research communities. The fruits of the experiment are available for the reader's own assessment; but it is the editor's judgment that these exchanges between philosopher and economist have been fruitful indeed.

The purpose of the volume is very specific: to stimulate a discussion of the epistemology and methodology of economics that works at the level of detail of existing "best practice" in economics today. The contributors have been asked to design their contributions in such a way as to stimulate productive conversation between philosophers and economists on topics in the methodology of economics. Collections in the philosophy of economics that are currently available focus on very general philosophical issues--falsifiability, realism, the status of economic laws, and so forth. However, a more detailed level of analysis is likely to be more fruitful for working economists.

The rationale for this approach to the philosophy of economics is that philosophy of science ought to be useful for the scientific practitioner. It is the editor's conviction that the social sciences raise important philosophical problems, and that adequate analysis of these problems can actually facilitate the theoretical and empirical work of the social scientist. But it is evident that these advantages can only be realized if the philosopher takes seriously the details and techniques of the social science with which he or she is concerned. And the working social scientist will only find the results of the philosopher's inquiry useful and worthwhile to the extent that the philosopher's analysis gives adequate expression to real problems in the practice of the social scientist.

Economics is no exception to these observations. The science of economics raises myriad difficult philosophical problems: What is the status of economic generalizations? What counts as evidence for economic hypotheses? How do economists explain events in the real world? In what sense do economic circumstances exert causal influence on other factors? What evidence gives us reason to believe a causal ascription in economics? How are economic models to be empirically evaluated? What is the status of predictions premised on economic models?

There is no question, then, that economics as a science raises difficult and important philosophical issues--problems which repay the careful analysis made possible by a philosopher's training. There is little question either that the way in which these problems are resolved has consequences, not only for philosophy, but for the science of economics as well. One has only to think of the mischievous consequences that early twentieth century philosophy had on the methodology of psychology in the form of radical behaviorism, to recognize that bad philosophy leads to bad science.

The essays

The reader will find the following essays dense in detail and rich in analytical insight. It will be useful to have a brief sketch of the topics that emerge from these essays.

The first pair of essays by James Woodward and Nancy Cartwright represent challenging discussions of problems of causal reasoning in econometrics. Woodward focuses on the idea of a "structural" or autonomous relationship, as being fundamental to our understanding of a causal relationship between two or more variables. As a first effort, Woodward defines an autonomous relation in these terms: "if [the equation] is autonomous, the relationship expressed by the equation--its functional form, its coefficients and so on--will remain unaltered under some relevant classes of changes" (p. 4). Woodward provides a philosophical analysis of the suitability of understanding causality in econometrics in terms of the notion of autonomy. Perhaps Woodward's most significant conclusion is that "econometric techniques take domain-specific causal information as inputs and combine these with statistical evidence to yield conclusions about causal relationships expressed in structural equations" (23). In other words, the causal relations cannot be extracted directly from the data; it is necessary to have a hypothesis about the underlying causal mechanisms in order to extract the structural equation.

Nancy Cartwright's essay, "Causal Structures in Econometrics Models," focuses on related issues. Cartwright's central point is that econometrics ought not be led to a theory of causation out of a bad social ontology. Her concern is with the idea that facts about causal relations are simply facts about probabilities of association among variables. She argues that there are good philosophical reasons for doubting the stability and depth of such probabilities--in the natural sciences as well as in econometrics. Instead, she urges econometrics to conceive of its subject matter in terms of causal structures embodying causal powers and capacities. She argues as well that econometrics is no more able than any other area of science to provide an algorithm for arriving at a good explanation; in econometrics the upshot of this observation is that causal analysis cannot be done mechanically, guided by standard statistical tests. On Cartwright's view, what econometrics permits is a form of controlled experimentation for economists (pp. 4-5). Cartwright considers the issues of autonomy and invariance raised by Woodward, and maintains that the more fundamental issue is the causal structure within which a set of economic processes takes place.

Kevin Hoover's commentary on the econometrics essays provides a thoughtful and detailed discussion of both essays. Hoover takes up the important topic of the status of laws within economics (both Cartwright and Woodward deny that even strongly supported econometric findings constitute laws of the variables in question). And he offers illuminating discussion of the role of economic theory in causal judgments in econometrics; the problem of measurement; and the issue of how to identify an underlying causal structure.

Christina Bicchieri takes on the central idea of a Nash equilibrium within game theory in her essay. The foundational concept of game theory is the notion of a Nash equilibrium: a combination of strategies each of which is each player's best (or as good) reply to each other strategy. An outcome is explained (and predicted) if it is a Nash equilibrium: such an outcome will be stable, because no player has an incentive to alter his or her strategy. Bicchieri raises a series of important difficulties with this concept as a basis for explanations of outcomes, beginning with the question of how players might reach a Nash equilibrium. Her concerns proceed from the "epistemic foundations" of a Nash equilibrium: that is, the assumptions we are forced to make about players in order for the Nash equilibrium to have a genuine explanatory role. Her central result is that the bare assumption that the players are rational and know the structure of the game does not entail that they will arrive at the equilibrium. She arrives at the skeptical conclusion that "the epistemic conditions required for a given strategic profile to be a Nash equilibrium . . . are seldom met in a static context . . . ; in such cases, we must be prepared to admit that Nash equilibrium has no particular claim on us" (p. 2). The best hope she offers for the salience of Nash equilibrium is in the dynamic case, in which repeated interactions permit learning (p. 52). In this case the players can eventually converge on equilibrium through updating of their beliefs.

Bart Lipman's discussion of Bicchieri's essay provides useful commentary on the significance of game theory within the discipline of economics--briefly, it is Lipman's view that game theory provides a framework from which to analyze the significance of institutions in competitive markets. Lipman's most telling comment on Bicchieri's approach is his suggestion that her desiderata concerning a justification of Nash equilibrium are too idealized. Lipman would settle for a set of beliefs and assumptions specific to a context that would permit players to arrive at an equilibrium position; whereas Bicchieri typically assumes that what we need are purely formal and context-independent assumptions about the participants' beliefs that are sufficient to entail convergence on the equilibrium. Lipman is persuaded that there are no such assumptions; he argues, however, that we can do second-best by attributing context-dependent assumptions to the players. Lipman proposes that we should ask, "in a real-world setting . . . what would we predict" about the outcome of a game of a given structure (4)? He also expresses skepticism, however, about Bicchieri's notion that learning might improve the availability of Nash equilibria.

David Schmitz introduces the topic of experimentation in economics in his essay on public good provision. He details experimental efforts to test the behavioral assumptions underlying public goods analysis and the prisoners' dilemma. The experimental results are clear: participants in controlled public good problems provide positive though suboptimal levels of contribution to the public good. How are we to interpret these results? Schmitz breaks the prisoners' dilemma into an assurance problem and an exploitation problem. Schmitz maintains that these problems have independent effects on decision makers--with the result that altering a public goods problem in a way that addresses one or the other ought to result in a change in behavior. And this is what experiments incorporating a money-back guarantee show, according to Schmitz: by eliminating the assurance problem, the money-back guarantee induces participants to contribute. Schmitz concludes that the experiments he describes permits us to probe rather accurately the decision-making rules employed by actual subjects (in contrast to the sparse abstraction of the rationally-self-interested utility maximizer of microeconomic theory). Jeffrey Baldani's insightful commentary piece draws out points of contrast and continuity between Schmitz's argument and that of Christine Bicchieri. Baldani focuses on the logical issues raised by experimental economics: what assumptions in

fundamental microeconomic theory are amenable to testing through the sorts of experiments described by Schmidt? Baldani doubts that experimentation in economics could serve the function of calibrating quantitative parameter values, and he suspects that many areas of economics are complex enough to defy experimentation. Baldani concludes that the most valuable contribution of experimentation is in probing the effects of alternative institutional arrangements on economic behavior.

Margaret Schabas shifts our attention to problems of reasoning about economic history. The "new economic history," cliometrics, is the focus of her analysis. The new economic history applies the analytical framework of neo-classical economics to processes of economic change in the past. Schabas reviews the problems of data paucity and the framing of counterfactuals that have received much attention in the past several decades in debates over cliometrics, and then focuses her attention on the suitability of neo-classical models in economic history. Is it reasonable to use linear models and Cobb-Douglas production functions in modeling historical economies? Is the notion that economic history represents a series of neo-classical equilibria a sustainable one? Schabas gives sustained attention to the arguments constructed by Fogel and Engerman in *Time on the Cross* concerning the economics of American slavery.¹ Her central criticism focuses on the lack of correspondence between the abstract and general assumptions of microeconomic theory--agents possessing complete, transitive, and continuous preferences, simple Cobb-Douglas production functions--and the complex and changing economic arrangements to which these assumptions are applied. Her essay concludes by linking some of the methodological characteristics of cliometrics to recent work in the philosophy of science on the status of law and rationality in historical explanations. A. W. Coats's commentary piece offers a defense of the new economic history against some of Schabas's criticisms. Coats argues that most work in this area is less ruthlessly abstractive than Schabas's account would suggest, and that the field is in better shape than Schabas would allow.

Harold Kincaid puts forward a different concern for specificity within economic analysis in his treatment of the theory of the firm. The focus of Kincaid's analysis is the use of optimality explanations in the theory of the firm: such and so characteristic of the firm is explained on the basis of the assertion that the trait is optimal--it maximizes profitability, self-interest, efficiency, or other economic features. Kincaid's essay, more than any other in the volume, focuses as well on problems of the empirical confirmation of economic models; he provides an admirably clear exposition of Bayesian confirmation theory. Kincaid examines several recent neo-classical theories of the firm--a transaction cost theory, a principal-agent theory, and an implicit contract theory--and argues persuasively that the empirical basis for accepting any of these theories is weaker than initially appears. Kincaid's point is not that these theories are discredited, but rather that the right sort of empirical data has not been presented on the basis of which to evaluate them. Marina Bianchi provides a thoughtful discussion of both the epistemic and the substantive issues raised by Kincaid's argument. Her discussion complements Kincaid's, in that she attempts to assess the transactions costs theory of the firm on the grounds of its theoretical cogency rather than its direct empirical support. In line with the conclusions of several of the essays in the volume, she argues that the abstract analysis of the firm based on rational agents arriving at efficient outcomes must be

¹ Robert William Fogel and Stanley L. Engerman, *Time on the Cross: The Economics of American Negro Slavery* (Boston: Little, Brown, 1974).

supplemented with more detailed analysis of the specific circumstances and arrangements within which the firm took shape.

Finally, Daniel Little's essay examines some methodological issues raised by "computable general equilibrium" models in macroeconomics. A CGE model is designed as a multi-sectoral representation of a functioning economy; it is aimed at assessing the effects of a variety of policy interventions (changes in tariffs, energy prices, taxes, etc.). The CGE simulation is often described as a way of performing "experiments" in macroeconomics. Little attempts to identify some of the epistemic problems raised by such models. The essay lays out a framework of evaluation in terms of which one might undertake to assess the credibility of the results produced by a CGE simulation. Issues of abstraction and the correspondence between models and the world arise again in this context. Lance Taylor's commentary piece provides a spirited discussion of some of the assumptions of Little's piece; he doubts, for example, that CGE models are often put forward as assertively as Little assumes. Taylor's view is that a typical CGE model is no stronger, and usually no weaker, than the underlying body of theory upon which it rests. So undertaking to assess the independent empirical warrant of the model is fruitless.

Themes

As is evident from these brief descriptions, there is a wide range of topics included in the essays below. But several themes emerge from most or all of the essays. A central theme in the volume is the epistemic status of economic models, hypotheses, and theories. The authors of the essays in this volume concern themselves with problems of assessing and interpreting the results of various areas of economics. How reliable are various economic techniques? What implicit assumptions are being made in applying a given economic model to a particular empirical case? How do data limitations constrain the evaluation of hypotheses in economic history? How appropriate are the behavioral assumptions of the model? To what extent have contextual institutional factors been adequately represented within the analysis? To what extent do arbitrary features of the framework affect the analysis in particular cases? How sensitive are outcomes to variations in the values of parameters? These questions make up what one might call the epistemology of economics; and the essays included in this volume offer philosophically insightful frames of analysis in efforts to formulate answers to them.

A related theme involves the issue of how best to understand the relation between an abstract and highly simplified model, on the one hand, and the textured and complex reality to which it is asserted to correspond. Economists employ models for a variety of purposes, with greater and lesser ambition. Most ambitiously they use models to simulate the behavior of existing economies to provide a basis for predictions. At the other end of the spectrum, models are used to explore the dynamics of multiple theoretical assumptions; on this use, it is the overall behavior of the model rather than its predictive consequences of the model that is of central interest. In what sense can the literally false model succeed in explaining the concrete reality? Is there a credible sense in which we may say that economic models correspond to economic reality?

Third, many of the essays focus attention on the behavioral assumptions of neoclassical economics: the abstract assumption of the rational self-interested agent. This assumption is unavoidably central to economic theorizing; at the same time, it is evident that the theory corresponds only weakly to actual human psychology. So how does this lack of correspondence affect the credibility of theorizing based on it?

A related theme in the volume is the need for greater specificity in economic analysis--whether in analyzing historically specific institutions, the firm, or developing economies. These points converge with a number of developments in the recent economics literature itself--institutional economics, increased attention to transaction costs, and attention to the importance of market imperfections, incomplete information, and problems of incomplete rationality.

Finally, many of the essays raise problems in the logic of confirmation that find a priori formulation in the philosophy of science literature, but which are applied with detail and finesse to particular areas of economic reasoning in the essays below. If economics is a science, it needs to explain, and it needs to be grounded in some appropriate way on empirical evidence. But in almost all of the essays below, it emerges that the problem of confirmation of economic theory and economic model continues to be unresolved--both within the economics profession and among those philosophers who have observed economic theorizing over the years.

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In short, there is ample room for productive exchange between philosophers and economists. But it is apparent that philosophers will only succeed in entering into fruitful debate with economists to the extent that they take seriously the particular details of current practices and controversies in economics. Philosophers of physics have learned this lesson well in the past several decades, and extremely fruitful interactions have occurred between philosophy and contemporary physics. Philosophers of economics must likewise link their work to specific technical details in problematic areas of economics.

The philosophers who have contributed to this volume fit the bill admirably for this perspective. Each has taken up an issue that arises directly out of current economic research. And each has treated the subject matter in sufficient detail to make the results of interest to the working economist. Equally, the economists who have provided commentaries to the philosophers' essays have taken up the challenge of considering with seriousness and attentiveness the philosophical problems raised in the philosophers' essays. There is a genuine sense of productive conversation that emerges out of these essays, and it is the hope of this volume that these conversations will stimulate continuing productive relationship between economics and philosophy.