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# The boundaries of economics

GORDON C. WINSTON &  
RICHARD F. TEICHGRAEBER III



## **The boundaries of economics**

**Murphy Institute Studies in Political Economy**

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The books in this series are occasional volumes sponsored by the Murphy Institute of Political Economy at Tulane University and Cambridge University Press, comprising original essays by leading scholars in the United States and other countries. Each volume considers one of the intellectual preoccupations or analytical procedures currently associated with the term "political economy." The goal of the series is to aid scholars and teachers committed to moving beyond the traditional boundaries of their disciplines in a common search for new insights and new ways of studying the political and economic realities of our time. The series is published with the support of the Tulane-Murphy Foundation.

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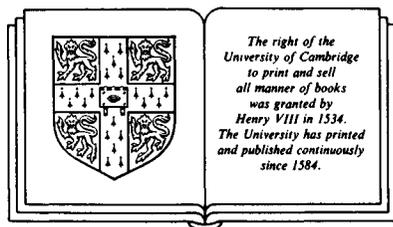
John Dunn (ed.), *The economic limits to modern politics*

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## Preface

“When English-speaking philosophers think of economics, they usually have a particular kind of pure theory in mind. This is the class of theories predominantly taught in western universities and often called neo-Classical. Purity here is a matter of conceiving *homo economicus* in abstraction from his social setting and, more excusably, of forswearing the attempt to make economics part (or all) of a general theory of society. By contrast, political economy, as the term is now used, is just such an attempt and its champions insist that no economic theory can be as pure as neo-Classicals pretend.”<sup>1</sup> This view of the links between philosophy and economic theory, espoused by Frank Hahn and Martin Hollis, is one that the contributors to this volume embrace, even if none of them is a practitioner of “political economy” precisely as it is defined here. This book had its origins in an effort to place neoclassical economic theory (especially conventional textbook microeconomic theory) in the broader context of modern economics with special concern for the boundaries between economics and the other social sciences. The widespread use of textbook theory in business, economic, and political analysis is a clear testament to its power. Yet the restrictions and artificialities of neoclassical economic assumptions also give cause for worry to some of the finest minds in the discipline. These chapters examine two related themes that complicate the conventional “economist’s” view of conduct and thereby provide a more complex (and humane) subject of study than the traditional *Homo economicus*. The first is the extent to which the economist’s paradigm – that humans are to be characterized chiefly by (1) self-interested goals and (2) rational choice of means – is useful in studying

<sup>1</sup> Frank Hahn and Martin Hollis, eds. 1979. *Philosophy and Economic Theory*. New York: Oxford University Press, p. 1.

traditionally noneconomic fields such as philosophy, political theory, and rhetoric. The second is the way in which insights from other disciplines are changing (or might improve) the current practice of economics.

Four of the lectures published here were delivered as a series with the general title “The Boundaries of Economics” at Tulane University in 1985. The series was sponsored by Tulane’s Murphy Institute of Political Economy. The chapter by Professor Hausman was presented at the 1984 Philosophy of Science Association meetings. Each contributor explores the dividing line between economics and the other social sciences from his own point of view, and there is no claim to have exhausted every facet of this issue. What this volume provides is a series of linked case studies showing how economics interpenetrates other forms of inquiry into human conduct and is penetrated by them as well.<sup>2</sup>

Richard F. Teichgraber III

<sup>2</sup> For a clear and provocative account of the current practice of economics, see Jack Hirshleifer. 1985. The expanding domain of economics, *American Economic Review* 75: 53–68.

## **Acknowledgments**

At virtually every turn in the editors' production of this volume – from organization to proofing to index – Gwen Steege played a central role. Without her, the manuscript would surely have been misspelled, disorganized, and late. Her efforts were complemented by the copy editing of Mary Nevader, whose deft sense of style and clarity is reflected throughout the book.



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## **Introduction**

**GORDON C. WINSTON**

All but one of the chapters in this volume were written as public lectures for a general university audience. All examine the boundaries – at least parts of the boundaries – of the discipline of economics with two aims in mind. First, they seek to give noneconomists insights into the way economics works, the way economists think about human behavior, and the way their mode of thought differs from that prevailing in other disciplines. Second, they attempt to give economists fresh insights into the way they do their work, on the premise that looking at, and across, the boundaries of a subject – looking at what it is not – is often useful for understanding what it is. And to both groups these chapters are meant to give a sense of the often significant changes now taking place in these boundaries.

The lectures were given at Tulane University in 1985. The Murphy Institute of Political Economy and Policy Analysis was just then establishing an interdisciplinary undergraduate program in political economy, and I was invited for the spring term as the first Murphy Institute Visiting Professor. I was assigned three duties: to teach an undergraduate class on economics in the context of other disciplines, to organize an interdisciplinary faculty seminar, and to arrange for a series of public speakers. In all instances, the topics to be addressed were broadly the same. As a device for increasing the productivity of a visiting professor, this combination of activities was surprisingly effective; the same sorts of ideas could be discussed at the same time with eager but naïve undergraduates and with skeptical and sophisticated colleagues from economics and other disciplines (economics, history, philosophy, sociology, and political science were represented in the faculty seminar), and I could invite speakers whose ideas I found interesting and exciting and to the point – which I did.

The experience was exceptionally fruitful for me and for the participants. Its tangible results are presented here with the conviction that they will prove useful to a wider audience of economists and noneconomists.

Under the “Boundaries of Economics” title, the lectures considered, in various ways, economists’ understanding of human behavior and the relation of their understanding to that of other disciplines. The intellectual traffic went both ways. The emphasis was on the bending and shaping and exporting and invading of the conventional stuff of economics.

The “boundaries” metaphor seemed particularly appropriate and serves now to unite the chapters in this volume. Boundaries define and delineate and differentiate, in this case setting economic inquiry off from other ways of understanding human behavior. Boundaries define the limits of the subject, from both sides. The two-way intellectual traffic – imperialist forays of economic methodology into philosophy, psychology, and the law, and the invasion of our territory by psychologists, sociobiologists, and rhetoricians – has recently flowed across the borders of economics in high volume. It appears we are in something of a rush hour. And partly in consequence, shifts and changes in the boundaries of economics seem to be taking place at an accelerated rate – even things like personal relationships, self-control, emotion, and addiction are becoming legitimate areas of economic research (Schelling 1984; Elster 1985; Schultze 1985; Winston 1985; Frank 1986).

Of course, the boundary that encircles economics is a long and winding one. Economics is intellectually adjacent to many other disciplines; no single collection of essays can avoid large gaps and holes. So the chapters in this volume do not systematically march around the perimeter of the subject; instead, they conduct a selective walking tour of a few important parts of it. The most serious omission, in terms of currently exciting and productive activities over the border, is the interaction between economics and psychology, and economics and political science (as distinct from political philosophy) – the work of people like Kahneman and Tversky (1982), Scitovsky (1985), and Frank (1986) and that of Axelrod (1984), Cohen and Axelrod (1984), and Elster (1985). Gary Becker gave a talk on addiction in the lecture series – he returned to the rational addict theme of the Stigler–Becker (1977) model – but it was not based on a written paper and so could not be included in this collection. McCloskey’s emphasis on persuasion and mine on repetitive behavior in perspective time should be of some value to psychologists and sociologists, though neither can be said

to be a significant border crossing in those directions. But if there is little here on the psychological and political parts of the boundary, those between economics and philosophy are well explored by McPherson and Hausman, the segment between economics and political philosophy is examined by Gray, and that between economic methodology and literary criticism by McCloskey.

Two powerful themes of modern economics are central to these chapters: information and the economic role of morality. The first has arguably been the most important area of research in conventional economics for the past two or three decades; in Chapter 2, McCloskey calls expected utility “the leading novelty in economic theory since the 1940s.” Information has come to be recognized as, simultaneously, a commodity that is produced with costly resources and an essential component of rational economic choice: Information is both a component and an object of rational choice. And more recently Herbert Simon’s (1955) idea of bounded rationality is playing an increasingly central role in economic thinking as the implications of its constraint on rational decision making are extended and developed. Williamson’s (1985) transactions cost analysis alone would justify that statement, but there are others, like Heiner’s (1983) examination of cognitive capabilities and situational complexity. “Efficient markets” describe markets as information-processing institutions. Asymmetries of information among economic actors are ubiquitous.

In all this there has been a transformation of – and growing sophistication in – the way we treat information. If expected utility has been the leading novelty since the 1940s, its step away from determinism was, in retrospect, a small and rather timid one – perfect knowledge about events was there replaced by perfect knowledge about the probabilities of those events. Bayesian learning could relax that strong probabilistic information requirement, but only for some kinds of events (the repetitive ones on which part of my chapter focuses).

Things opened up more sweepingly with Akerlof’s (1970) introduction of information asymmetries between economic actors – his formal recognition of the fact that buyers and sellers and employers and workers typically *know* very different things and typically know different amounts about the same things. Most recently, there has been a renewed appreciation of “Knightian uncertainty” or (surprise) an appreciation of the fact that many important things that happen cannot be predicted with sufficient

clarity even to be included in the list of events over which probabilities can be specified. There are important things – unique things, my chapter stresses – that will not be on anyone’s list of possible states of nature. For them, expected utility is without meaning and Bayesian learning cannot take place. Appreciation of the fact that information has important *unknowable* components, of course, goes along with the resurgence of interest in Austrian subjectivism (see Kirzner 1973) and with Shackle’s (1958) persistent efforts to induce economists to take time – past, present, and future – more seriously in their analysis, a task I have carried another step farther in my chapter.

As John Gray makes admirably clear in Chapter 4, the structure of Hayek’s conception of society rests crucially on information, knowledge, for he sees a free society as a device by which individuals can utilize information they do not possess – actors can benefit from knowledge they do not, themselves, have. One function of institutions, including market institutions, is this dissemination of the results of socially usable information. McPherson, in Chapter 5, notes how Rawls’s “veil of ignorance” protects one’s choices about social organization from the personalistic contamination of self-interest, allowing collective choice without personal competition: People achieve a Kantian universalism simply because they cannot guess how they, as individuals, will fare.

McCloskey’s discussion of rhetoric is a discussion of the way information is transmitted *persuasively* among people – in this case among economists. One of his definitions of “rhetoric” is simply “the paying of attention to one’s audience.” From his perspective as a philosopher of science, Hausman asks in Chapter 6 how economists evaluate information and how, particularly, they make peace with the largely unverified nature of economic information. Finally, my own chapter examines information in time: the different pieces of information available to analyst and subject because of their time perspectives; the difference in the degree to which the information requirements of rational choice are satisfied by repetitive and by unique events; the information revealed when transactions are examined with a sharply focused unit time.

It is no coincidence that economists’ attention to information has led them recently, if reluctantly, to an increased attention to morality, the second recurrent theme of these chapters. That movement is uncomfortable to a number of economists, sometimes deeply so, as the threat of

being enveloped by something inherently squishy challenges the scientist's white-coated dispassion. Aside from McCloskey's question of whether we really do persuade one another with that official, positivist, and scientific rhetoric, the explicit consideration of morality in economics is forced on us by our consideration of asymmetric information.

As long as we dealt only with fully informed actors – and fully informed, it must be assumed, about the intention of their fellow actors – it was possible to take for granted the moral context of their actions. Fully informed people could, acting rationally, protect themselves, and any quirky moralisms they wanted to bring in could be consigned to the preference function with all those other messy psychological imponderables. But as Akerlof's (1970) early lemons papers made clear, once we analyzed dealings between uninformed people or, worse still, between people when one knew more about the transaction than the other – the city slicker and the bumpkin – it became necessary to ask how far those people with superior information would push their advantage. The opportunities opened up, by virtue of their superior knowledge, to doctors, people buying insurance, and used-car salesmen trying to unload their lemons inevitably raise questions of morality and of the strength, direction, and source of internalized, self-imposed constraints on pure self-interest (see Bok 1978; Reder 1979; McPherson 1984).

Moral behavior is no longer peripheral to our understanding of economic behavior. "Cheating," "altruism," "guile," "opportunism," "deception," and "moral hazard" have become key words in modern economic theories. Information asymmetries introduce the dark strategic side of economic behavior, calculating and deceitful. And they thereby sharply increase the relevance of economic analysis to the tasks of disciplines like the law that must deal with those behaviors.

This Machiavellian side of rational economic behavior is unexpectedly revealed when private transactions are examined with temporal meticulousness in the last section of my chapter. It is shown that the problems of free riding usually attributed to public-goods transactions adhere, too, to private transactions as they take place in time. Gray's and McPherson's chapters address different aspects of morality. McPherson describes Rawls's use of welfare economics to think carefully about key issues of moral philosophy – of economic justice – and, at the same time, to broaden the domain of economic inquiry more explicitly to recognize

economic justice. Gray's critique of Hayek's intellectual scheme questions Hayek's presumption of a benign evolution of social institutions that rejected the demands of a theistic morality.

Though the themes of information and morality run through all of these chapters, a brief precis of each will give a sense of their strong individuality.

In "Towards a Rhetoric of Economics," Donald McCloskey further develops the theme that has increasingly dominated his work (McCloskey 1983, 1985) – the need for economists to recognize that the way they persuade one another, their rhetoric, is not what they think it is and that they will be better economists, practicing better economics, if they become aware of the actual rhetoric of their discipline. This chapter is both an extension of that theme and an excellent introduction to it for those who have not before sat through one of McCloskey's delightful sermons. He urges economists (and others, incidentally) to look into their souls and recognize the rhetoric that lies there, often denied but rarely hidden. We are not the objective scientists of positivist mythology, but quite fallible humans, trying to understand a complicated reality and to persuade one another that we do. We persuade with a wide variety of devices including, but certainly not restricted to, mathematical and scientific devices. The device of primary interest in this chapter is our pervasive use of metaphor and analogy and the various forms it takes in economic discourse – as when Gary Becker likens a family's children to a stock of capital or when mathematical metaphor is used in modeling economic reality. Finally, McCloskey demonstrates the promise of a two-way flow across the boundary that separates economics from literary criticism, calling on I. A. Richards's insights to tell economists more about preference functions and ways we can better understand "metapreferences" – our preferences about preference functions.

My own chapter, "Three Problems with the Treatment of Time in Economics," has a simple theme – that economists (and other social scientists, though less is made of them) properly shy away from the metaphysical conundrums of the Nature of Time that preoccupy physicists and philosophers of science and theologians, but that economists err on the other side, paying too little careful attention to the time context of the people and activities they study. And it costs them. That argument is supported by three instances. The first is economists' tendency to attribute to the people we study the same omniscient ability to move about in time that we enjoy

as analysts – a tendency illustrated by our long-standing ambivalence about the rationality of discounting the future. The second instance is our failure to distinguish between, on the one hand, economic decisions about repetitive events – where the information requirements of the rational choice model are nicely satisfied and one has the ability to adjust toward the real-world equivalent of an optimum – and, on the other, economic decisions about unique events – where rational choice theory is less obviously helpful. The final instance of a too casual treatment of time involves the selection of an analytical time unit: What seems a quintessentially pedestrian matter turns out to have unexpected consequences for economic analysis, as illustrated by the appearance of a genuine free-rider problem of classic proportions in private transactions, when they are viewed carefully as a sequence of actions in time.

With John Gray's "Hayek, the Scottish School, and Contemporary Economics," the discussion shifts away from admonitions to economists about the way they ought to do economics, which might characterize the first two chapters, to the examination of a leading economist who has never been accepted within the boundaries of economics, yet has won the Nobel Prize in Economics and has had formidable influence on the development of the field. The chapter is both an effective introduction to the distinctive features of Hayek's economics by a leading Hayek scholar and a thoughtful critique of its consistency, achieved by comparing Hayek's conceptions with those well-analyzed ideas of Burke, Smith, Hume, and Hobbes.

The central Hayekian theme is knowledge – information – and its embeddedness in and development through social institutions including, most explicitly, the market. Hayek's "knowledge" is much more than the "information" of much modern economics, information as the explicit knowledge of the individual rational decision maker à la Becker or von Neumann–Morgenstern (1944) or even Simon (1955). In Hayek, knowledge is the massive accumulated sum of individual information, the results of which are made available to people within society through social institutions; the *results* of knowledge are usable without explicit or even articulable forms of knowledge. Social institutions, then, allow the *use* of that tacit, unarticulated, even inchoate knowledge.

This is not a theme unfamiliar in modern economics. Arrow's (1974) *Limits of Organization* and much of the information economics that preceded it reach similar conclusions, if in a narrower setting, as does

Williamson's (1985) systematic examination of the implications of bounded rationality for social interaction in organizations and the evolution of institutions to protect (Hobbesian) individuals in their social and market interactions. And the study of "relational exchange" between individuals (see Goldberg 1980; Okun 1981; Schultze 1985) has brought increasing attention to the information and economic role of noneconomic institutions.

All of this makes more important Gray's argument that this "epistemological turn" in Hayek's thinking is independent of and not supported by his presumption of an optimistically evolving set of institutions, increasingly in the service of improved human well-being. The temptation to slide from the careful functionalism of Arrow's analysis to the optimism of Hayek's is great and quite useful to identify since it seems clear that mainline "information economics" is moving toward the more sweeping Hayekian issues – from the explicit information available to individual decision makers to organizations and their ability to use information that few individuals within them have – and toward the optimistic temptations to which Hayek succumbed.

Michael McPherson's "Reuniting Economics and Philosophy" looks at the sharply increased interest economists have shown in moral philosophy in the past decade and the sharply increased interest moral philosophers have shown in economics. He asks what caused it and what has come of it. An important part of the answer to the first question lies in the contradiction between the social ferment of the late 1960s and the sterility of the economist's neoclassical analysis or the moral philosopher's linguistic analysis. Each discipline provided a push, inducing socially concerned scholars to search for ideas more relevant to the world they lived in. Into this came the strong pull, too, of John Rawls's (1971) *Theory of Justice*. In one stroke, a major philosopher, in a major book, demonstrated that one could think carefully and substantively about so passionate an issue as justice. And he did it by using welfare economists' tools – their rhetoric – in unaccustomed ways to illuminate deeply meaningful questions far outside the economist's accepted purview. This was heady stuff for the unsatisfied young economist and an eloquent demonstration to philosophers of the power of the economist's vocabulary. Though the traffic over the boundary between economics and moral philosophy turned out to be more complicated than was originally

expected – McPherson gives us what amounts to a general theory of such boundary crossings as they evolve from discovery to high promise, then disappointment growing with familiarity – an association has been established in which both moral philosophy and economics are now significantly different.

Aside from its insights and thoughtful analysis of an important event in both economics and philosophy, McPherson's chapter is a nearly perfect, if unplanned, illustration of much of what McCloskey has been saying to economists about disciplinary rhetoric, the way scholars actually persuade each other, and the benefits to understanding of using a richer rhetorical menu. The chapter is autobiographical, using the story of one economist's seduction by moral philosophy as a personalized way of discussing the interaction between the two disciplines. It describes a genuine conversation between moral philosophers and economists and, what is even more important, a conversation in which each group has broadened its own disciplinary rhetoric to incorporate the alien rhetoric of the other. It is important to both the McPherson and McCloskey stories that these rhetorical assimilations were not made at the expense of the disciplines' traditional ways of thinking but, instead, augmented those conventional vocabularies and significantly expanded the discipline in the process. Social philosophy has gained a rigor of analysis and a capacity to say something careful about issues of great passion; economics has acquired a broader compass and a capacity to use its tools in a richer domain.

In Daniel Hausman's "Economic Methodology and Philosophy of Science," a philosopher of science asks why there has been so little traffic over a boundary one would expect to be quite narrow and heavily traveled – that between economic methodology and the philosophy of science. Why, particularly, has most economic methodology been written (and rather badly) by economists who do not have a very sophisticated understanding of the philosophy of science? In Hausman's useful sketch of the history of economic methodology, starting with Mill and, really, coming back to Mill, he describes the power of a defunct positivism, aided by Friedman's persuasive rhetoric, to make economists highly uncomfortable with the untested propositions that ground their discipline.

If McPherson concludes that the traffic over the boundary between economics and moral philosophy has carried considerable mutual gains from trade, Hausman concludes that it is a mistake to expect that

economists and philosophers of science will have the same objectives motivating their interest in economists' methodology. Economists are interested in economic methodology because they want to do better economics (witness the McCloskey and Winston arguments on methodology) whereas philosophers are interested in economic methodology because they hope to understand more fully the development of knowledge in an important discipline. Their objectives are different, and in the greater modesty of postpositivism, the role of the philosopher of science is not to tell economists how to get their methods right but to study and make sense (if possible) of the way economists *do* their economics. And if McCloskey is successful in inducing economists to recognize the very large role that rhetoric plays in the way they do their economics, it would appear that philosophers of science will soon be studying literary criticism in order to understand how social scientists go about their jobs.

Finally, it will be useful to note that none of the chapters indulges in economist bashing. That may be clear by now, but it is important. The authors are often critical of the current state of economics – they argue for more coherence, more recognition of its limits, more respect for its methods of argumentation – but they do so with a respect for the value of economics as a way of illuminating human behavior. In some of the chapters some of the time, this attitude is almost a passion. Economics is limited in its capacity to penetrate the mysteries of human behavior, not by some lamentable stupidity on the part of its practitioners, but by the inherent and inescapable complexity of the subject – human behavior – it has taken on. The volume implicitly, like McPherson's chapter explicitly, has as a major objective the identification of the boundaries of our ignorance with the conviction that, as we become clearer about what we do not (and often cannot) know, we will be clearer, too, about the way people behave. It is simply uninteresting, in the implicit view of these authors, that economics is an imperfect "science" – it exists in an imperfect world and confronts issues of mind-boggling complexity. What matters is that economic understanding be improved, not that it aspire to perfection.

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## **Towards a rhetoric of economics**

**DONALD N. McCLOSKEY**

Most of the ways economists talk, if they were translated into English, would sound plausible enough to noneconomical folk like farmers and poets and business executives. The talk is hard to follow at first, in the usual way of specialized talk, because the culture of the conversation makes the words arcane. But the people in an unfamiliar conversation are not Martians. Underneath it all (the economist's favorite phrase), conversational habits are similar. Even mathematical models and statistical tests, which sound alien to literary ears, grow out of ordinary talk. Under scrutiny they reduce to words that even an earthling might use.

All the conversational devices of economics, whether words or numbers, may be viewed as figures of speech. They are all metaphors, analogies, ironies, appeals to authority. Figures of speech are not mere frills. They think for us. Someone who thinks of a market as an "invisible hand" and the organization of work as a "production function" and coefficients as being "significant," as an economist does, is giving the language a great deal of responsibility. It seems a good idea to look hard at this language.

If the economic conversation were found to depend heavily on its verbal forms, this would not mean that economics is "not a science" or "just a matter of opinion" or some sort of confidence game. Good poets, though not scientists, are serious thinkers about symbols; good historians, though not scientists, are serious thinkers about data; good scientists, too, use language. What is more, though it remains to be shown, they use the cunning of language, without particularly meaning to. The language used

Portions of this chapter appear in McCloskey (1985).

is a social object, and using language is a social act. It requires cunning (or, if you prefer, consideration), attention to the other minds present when one speaks.

The paying of attention to one's audience is called "rhetoric." One uses rhetoric, of course, to warn of a fire in a theater or to arouse the xenophobia of the electorate. This sort of yelling is the vulgar meaning of the word, like the president's "heated rhetoric" in a press conference or the "mere rhetoric" that our enemies employ. Since the Greek flame was lit, however, the word has acquired a broader and more amiable sense, to mean the study of all the ways of accomplishing things with language: inciting a mob to lynch the prisoner, to be sure, but also persuading readers of *Emma* that its characters breathe or bringing economists to accept the better argument and to reject the worse (Burke 1950; Corbett 1965; Booth 1974).

The question is whether economic scholars – who usually fancy themselves announcers of "results" or staters of "conclusions" free of rhetoric – speak rhetorically. Do they try to persuade? It would seem so. Language, I just suggested, is not a solitary accomplishment. Economists do not speak into the void, or utter monologues, but speak to a community of voices. They desire to be heeded, published, imitated, en-Nobeled. These are their desires.

The devices of language are the means. Rhetoric is the proportioning of means to desires in speech. Rhetoric, one might say, is an economics of language, the study of the way scarce means are allocated to the insatiable desires of people to be heard. It seems on the face of it a reasonable hypothesis that economists are like other people in being talkers who desire listeners when they go to the library or the computing center as much as when they go to a cocktail party or the polls. The purpose is to see if this is true and to see if it is useful: to study the rhetoric of economic scholarship.

The point of thinking about economic conversations is to help the field mature, not to attack it. A rhetorical study of an economic text need not be hostile, no more than a rhetorical study of "Ode on a Grecian Urn." It is simply a literary study. The service that literature can perform for economics is to exhibit literary criticism as a model for self-understanding. Literary criticism does not merely pass judgments; in its more recent forms the question seems hardly to arise. Chiefly it is concerned with making readers see how poets and novelists accomplish their results. A

rhetorical criticism of economic science would not merely pass judgment on economics, as did an older philosophy of science. Rhetoric and philosophy have had different programs, at least since Socrates embodied them both. A rhetoric of economics would be a way of showing how the science accomplishes its results. It would apply the devices of literary criticism to the literature of economics.

Not many economists think this way (Klamer 1983, 1984; McCloskey 1983). A larger though small proportion of other social scientists do; it is not unheard of in anthropology or sociology (Geertz, in press). What the French call the “human sciences” – the disciplines from English to paleoanthropology that study humankind – can assemble nowadays quite a few people who think critically in this literary sense about their own thinking. And many scholars in mathematics, physics, computer science, biology, paleontology, communication, political science, law, sociology, anthropology, history, history of science, philosophy, theology, comparative literature, and English have seen merit in “a rhetoric of inquiry” (Nelson, Megill, and McCloskey 1987).

I propose, then, a rhetoric of inquiry in economics. The proposal has wider purposes as well. It uses an ancient rhetorical device, the figure *a fortiori*, “from the stronger”: If even the study of hog farmers and railroads is literary as well as mathematical, if even the science of human maximization under constraints is as much a part of the humanities as of the sciences, then all the stronger is the hope for the rest.

### The metaphorical character of economics

The most important example of economic rhetoric is the economic metaphor. To say that markets can be represented by supply and demand “curves” is to be as metaphorical as to say that the west wind is “the breath of autumn’s being.” “Game theory” is a transparent example from the most mathematical part of economics, the very name being a metaphor. It is obviously useful to have before us the notion that the arms race (an arms “race”) is a two-person, negative-sum, cooperative “game.” The metaphor displays its persuasiveness, and some of its limitations. (Someone remarked recently that game theory has a nice name but no results.) The noneconomist finds it easier to see the metaphors than does an economist, for the economist is habituated to them

by daily use, accustomed to mental cartoons showing cement coming out of a “function” and business failures swinging in “cycles.”

Certain of the metaphors are self-conscious, as revealed for instance by faith or doubt in speaking of the “invisible hand.” And everyone understands that a metaphorical question is at issue when it is asked whether a mechanical or a biological analogy best suits the economy as a whole (Boulding 1975; Georgescu-Roegen 1975; Kornai 1983). Some economists, again quite self-aware, think metaphorically in ways that no one can mistake: J. K. Galbraith with his countervailing powers, for example, or Albert Hirschman (1970) with his exits and voices.

But few economists recognize the metaphorical saturation of economic theories believed to be quite literal. One economist to have done so is Willie Henderson (1982), who has written illuminatingly on the subject. It is more common to find people aware of their metaphors in other fields: A volume of essays by philosophers, linguists, and psychologists is entitled simply *Metaphor and Thought* (Ortony 1979). In physical or biological sciences the case is plain, though the official rhetoric rejects metaphors nonetheless. Jacob Bronowski (1965:36) noted that the scientist needs “the exploration of likenesses; and this has sadly tiptoed out of the mechanical worlds of the positivists and the operationalists, and left them empty. . . . The symbol and the metaphor are as necessary to science as to poetry.” One might better say that even positivists and operationalists are tied to metaphor, the metaphor of “objectivity,” for instance, and in any case the metaphors of their discipline. The philosopher Richard Rorty (1979:12) said it well: “It is pictures rather than propositions, metaphors rather than statements, which determine most of our philosophical [and economic] convictions.”

Each step in economic reasoning, even the reasoning of the official rhetoric, is metaphorical. The economic world is said to be “like” a complex model imagined in serried equations stretching out to an infinite number of traders. The complex model is said to be like a finite model for actual thinking, which is in turn like an even simpler model for actual calculation, with variables said to be like the easily measured proxy variables to hand. For purposes of persuading doubters, the model is said to be like a toy model that can be manipulated inside the head of the doubter or pushed about on a crowded blackboard. John Gardner (1978:118–19) wrote:

There is a game – in the 1950s it used to be played by the members of the Iowa Writers' Workshop – called "Smoke." The player who is "it" [thinks of] some famous person . . . and then each of the other players in turn asks one question . . . such as "What kind of weather are you?" . . . Marlon Brando, if weather, would be sultry and uncertain. . . . To understand that Marlon Brando is a certain kind of weather is to discover something (though something neither useful nor demonstrable) and in the same instant to communicate something.

On the contrary, I shall argue, in economics the comparable discovery of metaphor is useful and, by recourse to rhetorical standards, demonstrable. What kind of a curve is a market? What kind of a material is a worker?

But metaphor is commonly viewed as mere ornament. From Aristotle until the 1930s even literary critics viewed it this way, as an amusing comparison capable of affecting the emotions, yet inessential for thought. "Men are beasts": If we cared to be flat-footed about it, the notion was, we could say in what "literal" way we thought them beastly, removing the ornament to reveal the core of plain meaning underneath. The notion was in 1958 common in philosophy, too:

With the decline of metaphysics, philosophers have grown less and less concerned about Godliness and more and more obsessed with cleanliness, aspiring to ever higher levels of linguistic hygiene. In consequence, there has been a tendency for metaphors to fall into disfavor, the common opinion being that they are a frequent source of infection. (Horsburgh 1958:231)

Such suspicion of metaphor is widely recognized by now to be unnecessary, even harmful. That the very idea of "removing" an "ornament" to "reveal" a "plain" meaning "underneath" is itself a metaphor suggests why the removal might not work. Perhaps thinking is metaphorical. Perhaps to remove metaphor is to remove thought.

### **The case of Gary Becker**

The question is whether economic thought is metaphorical in some nonornamental sense. The most obvious metaphors in economics are those used to convey novel thoughts, one sort of novelty being to compare economic with noneconomic matters. "Elasticity" was once a mind-stretching fancy; "depression" was depressing; "equilibrium" compared an economy to an apple in a bowl, a settling idea; "competition" once induced thoughts of horse races; money's "velocity," thoughts of swirling bits of paper. Much of the vocabulary of economics consists of dead metaphors taken from noneconomic spheres.

Comparing noneconomic with economic matters is another sort of novelty, apparent in the imperialism of the new economics of history, law, politics, crime, and the rest, and most apparent in the work of that Kipling of the economic empire, Gary Becker. Among the least bizarre of his many metaphors in economic poetry, for instance, is that of children as durable goods, like refrigerators. The philosopher Max Black (1962:236) points out that “a memorable metaphor has the power to bring two separate domains into cognitive and emotional relation by using language directly appropriate to the one as a lens for seeing the other.” So here: The subject (a child) is viewed through the lens of the modifier (a refrigerator).

A beginning at literal translation would be, “A child is costly to acquire initially, lasts for a long time, gives flows of pleasure during that time, is expensive to maintain and repair, has an imperfect second-hand market. . . . Likewise, a durable good, such as a refrigerator . . .” That the list of similarities could be extended farther and farther, gradually revealing the differences as well – “children, like durable goods, are not objects of affection and concern”; “children, like durable goods, do not have their own opinions” – is one reason that, as Black (1962:237) says, “metaphorical thought is a distinctive mode of achieving insight, not to be construed as an ornamental substitute for plain thought.” The literal translation of an important metaphor is never finished. In this respect and in others, an important metaphor in economics has the quality admired in a successful scientific theory, a capacity to astonish us with implications yet unseen.

But it is not merely the pregnant quality of economic metaphors that makes them important for economic thinking, not mere ornaments. The literary critic I. A. Richards (1936:93) was among the first to make the point that metaphor is “two thoughts of different things *active together*, . . . whose meaning is a resultant of their interaction” (italics added; cf. Barfield 1947:54; Black 1962:46). A metaphor is not merely a verbal trick, Richards (1936:94) continues, but “a borrowing between and intercourse of *thoughts*, a transaction between contexts” (his italics). Economists will have no trouble seeing the point of his economic metaphor, one of mutually advantageous exchange. The opposite notion, that ideas and their words are invariant lumps unaltered by combination, like bricks (Richards 1936:97), is analogous to believing that an economy is a mere aggregation of Robinson Crusoes. But the point of economics since Smith has been that an islandful of Crusoes trading is different from and often better off than the mere aggregation.

Another of Becker's favorite metaphors, "human capital," invented at Chicago by Theodore Schultz, illustrates how two sets of ideas, in this case both drawn from inside economics, can mutually illuminate each other by exchanging connotations. In the phrase "human capital" the field in economics treating human skills was at a stroke unified with the field treating investment in machines. Thought in both fields was improved – labor economics by recognizing that skills, for all their intangibility, arise from abstention from consumption; capital theory by recognizing that skills, for all their lack of capitalization, compete with other investments for a claim to abstention. Notice by contrast that, because economists are experts only in durable goods and have few (or at any rate conventional) thoughts about children, the metaphor of children as durable goods has, so to speak, only one direction of flow. The gains from the trade were earned mostly by the theory of children gaining from the theory of durable goods (fertility, nuptiality, inheritance), not the other way around.

What is successful in economic metaphor is what is successful in poetry, and can be analyzed in similar terms. Concerning the best metaphors in the best poetry, comparing thee to a summer's day or comparing A to B, Owen Barfield (1947:54) argued:

We feel that B, which is actually said, ought to be necessary, even inevitable in some way. It ought to be in some sense the best, if not the only way, of expressing A satisfactorily. The mind should dwell on it as well as on A and thus the two should be somehow inevitably fused together into one simple meaning.

If the modifier B (a summer's day, a refrigerator, a piece of capital) were trite – in these cases it is not, although Shakespeare was more self-critical of his simile than economists usually are of theirs – it would become, as it were, detached from A, a mechanical and unilluminating correspondence. If essential, it fuses with A to become a master metaphor of the science, the idea of "human capital," the idea of "equilibrium," the idea of "entry and exit," the idea of "competition." The metaphor, quoth the poet, is the "consummation of identity."

### **The metaphors of mathematics**

Few would deny that economists often use figurative language. Much of the pitiful humor in a science devoted to calculations of profit and loss comes from talking about "islands" in the labor market or "putty clay" in the capital market or "lemons" in the commodity market. The more

austere the subject the more fanciful the language. Economists have “turnpikes” and “golden rules” in mathematical growth theory, for instance, and long disquisitions on what to do with the “auctioneer” in general equilibrium theory. A literary person with advanced training in mathematics and statistics who turned up by mistake in a seminar in economics would be astonished at the metaphors, would be lost in a land of allegory. Allegory is merely long-winded metaphor, and all such figures are analogies. Analogies can be arrayed in terms of explicitness, the most explicit being simile (the businesspeople behave “as if” they were calculating machines) and the symbol (“the demand curve”) the least. These rhetorical siblings of metaphor dominate the conversations of economists.

Mathematical theorists, for instance, frequently spin “parables,” as the more self-conscious of them put it, or tell “stories.” The word “story” has in fact come to have a technical meaning in mathematical economics, though it is usually spoken in seminars rather than written in papers. It means an extended example of the economic reasoning underlying the mathematics, often a simplified version of the situation in the world that the mathematics is meant to characterize. It is an allegory, shading into extended symbolism. The literary theories of narrative could make economists self-conscious about what use the story serves. Here the story is the modifier, the mathematics the subject. A tale of market days, traders with bins of shmoo, and customers with costs of travel between bins illuminates, say, a fixed point theorem. “Tales well told endure forever,” an economist and poet once said.

The critical question is whether the opposite trick, illuminating human behavior with mathematics, is also metaphorical. If it were not, one might acknowledge the metaphorical element in verbal economics about the “entrepreneur,” for instance, or more plainly about the “invisible hand,” yet argue that the linguistic hygiene of mathematics leaves behind such fancies. This, indeed, was the belief among advanced thinkers of the 1930s, who later imposed their modernist methodology on economics: Samuelson, Friedman, and others. When engaging in verbal economics we are more or less loose, they say, taking literary license with our “story”; when we do mathematics, however, we put away childish things.

But mathematical theorizing in economics is metaphorical, and literary. Consider, for example, a relatively simple case, the theory of production functions, the notion prevalent in economics since the early twentieth

century that any product, such as the national product, can be viewed as being mathematically dependent on inputs, such as aggregate capital and labor. Its vocabulary is intrinsically metaphorical. "Aggregate capital" involves an analogy of "capital" (itself analogical) with something – sand, bricks, shmoos – that can be added up in a meaningful way; so does "aggregate labor," with the additional peculiarity that the thing added is no thing, but hours of conscientious attentiveness. The very idea of a "production function" involves the astonishing analogy of the subject, the fabrication of things, about which it is appropriate to think in terms of ingenuity, discipline, and planning, with the modifier, a mathematical function, about which it is appropriate to think in terms of height, shape, and single-valuedness.

The metaphorical content of these ideas was alive to its inventors in the nineteenth century but is largely dead to twentieth-century economists. Its deadness does not eliminate the metaphorical element. In the Battle of the Two Cambridges in the 1960s, the metaphor got out of its coffin in a most alarming fashion. The Marxists of Cambridge, England, battled the liberal capitalists of Cambridge, Massachusetts, over the meaningfulness of the capitalist's friend, the aggregate production function.

The very violence of the battle, which continues down to the present in sporadic sniper fire, suggests that it entailed something beyond mathematics or fact. The something was more than politics. Mere politics could not explain such fury; some intellectual matter was at stake. The combatants hurled mathematical reasoning and institutional facts at one another, but the important questions are those one would ask of a metaphor – is it illuminating, is it satisfying, is it apt? How do we know? How does it compare with other economic poetry? The production function is a metaphor and should be judged on grounds relevant to metaphors. One does not score points against Shakespeare by telling him that his metaphor is "literally" wrong: "Come, my good fellow, I have here a biological and mathematical proof that all this talk of a woman being a summer's day is rubbish; the one is flesh and blood, the other rough winds and darling buds." The remark is at best unhelpful.

After some tactical retreats by Cambridge, Massachusetts, on points of logic mostly irrelevant to the metaphorical issue, the two sides withdrew exhausted. On mathematical grounds the British Cantabrigians had won. Since they won on grounds agreed to by both sides, the grounds of mathematical proof of consistency, they are understandably annoyed that

people go on using production functions. But the important questions were literary, not mathematical or statistical, and went unanswered. No one noticed. The continued vitality of the idea of an aggregate production function in the face of mathematical proofs of its impossibility, and the equal vitality of the idea of aggregate economics as practiced in parts of Cambridge, England, in the face of statistical proofs of its impracticality, would otherwise be a great mystery.

Even when the metaphors of one's economics appear to stay well and truly dead there is no escape from literary questions. The literary man C. S. Lewis pointed out in 1939 that any talk beyond the level of the-cow-standing-here-is-in-fact-purple, any talk of "causes, relations, of mental states or acts . . . [is] incurably metaphorical" (p. 47). For such talk he enunciated what may be called Screwtape's Theorem on Metaphor, the first corollary being that the escape from verbal into mathematical metaphor is not an escape:

[W]hen a man claims to think independently of the buried metaphor in one of his words, his claim may . . . [be] allowed only in so far as he could really supply the place of that buried metaphor. . . . [T]his new apprehension will usually turn out to be itself metaphorical. (Lewis 1939:46)

If economists forget and then stoutly deny that the production function is a metaphor, yet continue talking about it, the result is mere verbiage. The word "production function" will be used in ways satisfying grammatical rules but will not signify anything.

The charge of meaninglessness, applied so freely by modernists to forms of argument they do not understand or like, sticks in this way to themselves. Lewis's second corollary is that "the meaning in any given composition is in inverse ratio to the author's belief in his own literalness" (p. 27). Economists speaking (they believe) literally about the demand curve, the national income, or the stability of the economy are engaging in "mere syntax." Lewis cuts close to the bone here, though sparing himself from the carnage:

The percentage of mere syntax masquerading as meaning may vary from something like 100 percent in political writers, journalists, psychologists, and economists, to something like forty percent in the writers of children's stories. . . . The mathematician, who seldom forgets that his symbols are symbolic, may often rise for short stretches to ninety percent of meaning and ten of verbiage. (p. 49)

If economists are not comparing a social fact to a one-to-one mapping,

thus bringing two separate domains into cognitive and emotional relation, they are not thinking:

I've never slapped a curved demand;  
 I never hope to slap one.  
 But this thing I can tell you now:  
 I'd rather slap than map one.

### Why it matters

Metaphor, then, is essential to economic thinking, even to economic thinking of the most formal kind. Self-consciousness about it would be an improvement on many counts. Obviously, unexamined metaphor is a substitute for thinking – which is a recommendation to examine the metaphors, not to attempt the impossible by banishing them.<sup>1</sup> To repeat, saying that economic reasoning is metaphorical is not saying that it is bad. People have a hard time understanding this point. Robert Kuttner (1985), for instance, summarized it as saying that “economics is adrift in metaphors that have no application to empirical reality but are taken literally, because they happen to be in the language of mathematics.” The misunderstanding reflects a powerful dualism in our culture contrasting literary and mathematical expression. It must be clear by now that I do not think much of the dualism and find that it serves only to mislead otherwise intelligent people into reversing the point. Economics is indeed “adrift” in metaphors, but they *are* the “empirical reality,” being the worlds we make by our talk (Goodman 1978, 1983). It does not matter whether we choose a mathematical or a nonmathematical metaphor to make the world with a way of speaking. The point is not to attack metaphor, but to attack the notion that we can do without it, speaking “literally.” We cannot, and until this is recognized the conversations among schools of economics will be dark and bitter.

Metaphors, furthermore, evoke attitudes that are better kept in the open and under the control of reasoning. This is plain in the ideological metaphors popular with parties: The invisible hand is so very discreet, so soothing, that we might be inclined to accept its touch without protest; the

<sup>1</sup> An example of a naïve attack on economic metaphors, and of a failure to realize that economic theory is itself armed with metaphor, is the first page of McCloskey (1970).

contradictions of capitalism are so very portentous, so scientifically precise, that we might be inclined to accept their existence without inquiry.

But even metaphors of the middling sort carry freight. The metaphors of economics often carry in particular the authority of Science, and often carry, too, its claims of ethical neutrality. It is no use complaining that one did not *mean* to suggest with the metaphor of, say, “marginal productivity” that one had solved the moral problem of distributing the things we make, a problem made difficult because we make things together and not alone. It is irritating that it carries this message, because it may be far from the purpose of the economist who uses it to show approval of the distribution arising from competition and capitalism. It is better, though, to admit that metaphors in economics can contain such a political message than to use the jargon innocent of its potential.

A metaphor, finally, emphasizes certain respects in which the subject is to be compared with the modifier; in particular, it leaves out the other respects. Max Black (1962:41), speaking of the metaphor “men are wolves,” notes that “any human traits that can without undue strain be talked about in ‘wolf-language’ will be rendered prominent, and any that cannot will be pushed into the background.”

Economists will recognize this as the source of the annoying complaints from nonmathematical economists that mathematics “leaves out” some feature of the truth or from noneconomists that economics itself “leaves out” some feature of the truth. Such complaints are often trite and ill-informed. The usual responses to them, however, are hardly less so. The response that the metaphor leaves out things in order to simplify the story just for the moment is disingenuous, occurring as it often does in contexts in which the economist is simultaneously fitting fifty other equations. The response that the metaphor will be “tested” eventually by the facts is a stirring promise, but seldom fulfilled.

A better response would be to affirm that we like the metaphor of, say, the selfishly economic person as a calculating machine on grounds of its prominence in successful economic poetry or on grounds of its greater congruence with introspection than alternative metaphors (of people as religious dervishes, say, or as sober citizens). In *The New Rhetoric*, Perelman and Olbrechts-Tyteca (1958:390) note that “the acceptance of an analogy . . . is often equivalent to a judgment as to the importance of the

characteristics that the analogy brings to the fore.” What is remarkable about this unremarkable assertion is that it occurs in a discussion of purely literary matters, yet fits so easily the matters of economic science.

### Literary economics: the case of Albert Hirschman

That, then, is one example of literary thinking applied to economists’ talk. Only an economist, however, would think first of applying literary methods to economists’ talk. The lay person confronted with the sentence “Economics is literary” would think, “Aha, yes. We can better see the *economy* – forget about the talk of economists – if we see it as rhetorical. Here is an opportunity to get rid of that great stick of a character, *Homo economicus*, and replace him with somebody real, like Madame Bovary.”

It may be. It may be that economic theory can be improved by a literary approach to economics. I doubt it, and rest the case for a rhetoric of economics on the improvement in temper and self-control to be expected from it. Yet one can think of ways in which a literary economics might be better, and here is one. Both economists and literary critics talk about “preferences.” Economists mean by this simply “what people want,” in the sense of wanting some candy when the price is right. With a few other economists, Albert Hirschman (1984:89–96) has observed that stopping at mere wants causes economists to overlook higher-level preferences, wants about wants. Elsewhere these are known as taste or morality or, west of the Sierras, life style. Hirschman’s notion is that if you wish to be the sort of person who enjoys Shakespeare you will sit through a performance of *Two Gentlemen of Verona* as part of your education. You impose a set of preferences on yourself, which you then indulge in the usual way. You have preferences about preferences: metapreferences (cf. Elster 1979).

Now it would not be shocking if literary critics could teach economists a thing or two about metapreferences. Literary criticism, after all, is largely a discourse about them, and people like I. A. Richards, Northrop Frye, Wayne Booth, and Kenneth Burke are fair canny. One might think that the older line of critics – Sir Philip Sydney, Johnson, Coleridge, Arnold – would have in fact the most to teach, being more concerned than the recent kind with matters of value (matters of how well, as against simply how). A passage from the younger line, though, can illustrate how literary

notions might be used to understand the economy of taste. Richards wrote in 1925:

On a pleasure theory of value [a theory using only preferences, not metapreferences] there might well be doubt [that good poetry is better than bad], since those who do enjoy it [namely, bad poetry, such as that collected in *Poems of Passion*] certainly appear to enjoy it in a high degree. But on the theory here maintained, the fact that those who have passed through the stage of enjoying the *Poems of Passion* to that of enjoying the bulk of the *Golden Treasury*, for example, do not return, settles the matter. . . . [A]ctual universal preference on the part of those who have tried both kinds fairly is the same (on our view) as superiority in value of the one over the other. (pp. 205–6)

An economist will notice right away that Richards's argument is the same as the economics of "revealed preference" or, on a national level, the "Hicks-Kaldor test of welfare improvements." To use the reasoning developed by Paul Samuelson, an early economic exponent of antiliterary methodology in economics, one bundle of groceries is revealed-preferred to another if you *could* buy either bundle (could *afford* to buy either) but in fact chose one. In your view, clearly, the bundle you could afford but did not take must be inferior.

The point is that Richards's test is a revealed preference test *for (good) taste*. In other words, it is a way of ranking metapreferences. You could have read the Classic comic book but in fact chose to read Dostoevski, because you wanted to be that sort of person. The Dostoevski-reading personage is revealed-preferred by you. That someone passes through the stage of enjoying "The Love Boat" on television to that of enjoying the bulk of modern drama and does not return settles the matter. That someone passes through the stage of enjoying modern drama to that of enjoying the bulk of Shakespeare and does not return settles it again: Shakespeare is metapreferred to modern drama, which is in turn metapreferred to "The Love Boat."

The same applies to nonliterary preferences, which is why Richards's notion can be used by economists. To be sure, it is more complicated than that. We do drift slowly from one metapreference to another and sometimes, gyrelike, return to elementary pleasures. But the notion is a good beginning. People who learn Cajun cooking may never return to meat and potatoes. The style of life in New Orleans – that is, the preferences one chooses to indulge – may be revealed-preferred to those in Atlanta, and those in Atlanta to those in New York. It would be so

revealed if one observed people trekking from New York to Atlanta and thence to New Orleans but never back again. In like fashion a capitalist democracy may be revealed preferred to a workers' democratic republic by the direction in which the guns on the border point.<sup>2</sup>

What is attractive about the test is that it replies in a suitably modernist way to the modernist argument that "you can't say anything about ranking tastes." The Richards test is similar to Rawls's test of political constitutions from behind a veil of prenatal ignorance. It is similar, likewise, to the tests of social preferences proposed earlier by the economists Harsanyi, Sen, and others. And these are in turn extensions from the individual to the society of the leading novelty in economic theory since the 1940s – expected utility. The Richards test, in short, may be literary criticism, but it is also economics. Even by the economist's narrow standard of sayability there is nothing intrinsically can't-sayable about changes in preferences guided by taste. Or at any rate it is no more can't-sayable than ordinary remarks about ordinary choice, the usual sayings of economic theory.

Economics, then, can be seen in many ways as an instance of literary culture. That it can also be seen as an instance of mathematical culture is no contradiction. The two cultures are more similar than they realize. As Max Black (1962:243) wrote, discussing "archetypes" as extended metaphors in science, "When the understanding of scientific models and archetypes comes to be regarded as a reputable part of scientific culture, the gap between the sciences and the humanities will have been partly filled." This is in the end the significance of metaphors and of the other rhetorical machinery of argument in economics: Economists and other scientists are not as separate from the concerns of civilization as many think. Their modes of argument and the sources of their conviction – for instance, their uses of metaphor – are not very different from Cicero's speeches or Hardy's novels. And this is a good thing.

<sup>2</sup> Milton Friedman (1975:188) uses this very figure of speech to support his argument against conscription in peacetime: "I have observed many persons initially in favor of the draft change their opinions as they have looked into the arguments and studied the evidence; I have never observed anyone who was initially in favor of a volunteer force reverse his position on the basis of further study. This greatly enhances my confidence in the validity of the position I have taken."

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## **Three problems with the treatment of time in economics: perspectives, repetitiveness, and time units**

GORDON C. WINSTON

Time has usually been treated in two very different ways in economic and social analysis. An analytical high road ponders the Nature of Time and the metaphysical conundrums of St. Augustine and Einstein, whereas a low road considers the time context of human behavior and its analysis to be self-evident, uncomplicated, and not very interesting. Both of these are appealing but both, I will argue, are inadequate: the high road because of its irrelevance to social analysis, the low road because its inattention to temporality creates important obstacles to understanding social behavior. A middle road needs building – or at least widening and surfacing – a road that acknowledges the importance of the time structure of social behavior and our analysis of it while avoiding both mystification, on the one hand, and carelessness, on the other. I hope this chapter will make a contribution to that end.

### **The high road, the low road, and a middle road**

Consideration of the Ultimate Nature of Time is mind boggling. Sympathy with Augustine's (1955:354) famous complaint has certainly increased in the past fifteen hundred years: "What, then, is time? If no one asks me, I know what it is. If I wish to explain it to him who asks me, I do not know." Philosophers have asked whether time has reality, whether the passage of

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time has reality, whether time exists apart from human observation of it, and whether statements about time can be made independent of time itself. Einstein resolved the conflict between Maxwell's wave theories of electromagnetic force and Newton's classical mechanics by keeping the speed of light a constant and making time itself the variable. So even modern physics, with its considerable prestige, tells us that time is not a solid part of our reality but, instead, one that is highly malleable, depending ultimately on the velocity and position of the observer.

Fortunately, these physics and metaphysics of time are largely irrelevant to economic concerns. They have little to do with the role of time in *social* analysis. The broad, but still limited objective of that analysis is a better understanding of *human* behavior. So if some metaphysicians are in one sense quite right and time does not exist except as an illusion of the human mind, that illusion has to be taken as a datum for the analysis of the behavior of humans. The challenge of relativity theory dismisses itself. That theory holds that even the old Newtonian conceptions of an external, uniform time are quite adequate for slow-moving entities within a given inertial frame, and since all of human society certainly moves slowly within the same inertial frame, relativity theory approves our neglect in social analysis of the temporal complications of relativity theory.

What emerges as relevant to social behavior and social analysis is a rather ordinary, workaday conception of the ultimate nature of time. Leibniz's description of a relative time as ordered events may be more comfortable for us than Newton's absolute time as external clock, but either one serves social analysis well. For people, time *is* external. A big clock *does* tick away out there, independent of our individual actions or feelings, and we coordinate our activities with one another on its basis. Our big clock uses geophysical events – the movement of the planets and the accumulated vibrations of quartz crystals – complemented by peoples' conformity to those rhythms. So to our limited sensitivities, Leibniz's event series is Newton's cosmic clock.<sup>1</sup>

So much for the temporal high road. It is exciting, mysterious, and full of difficult and unexpected turns, but it does not take us very far toward

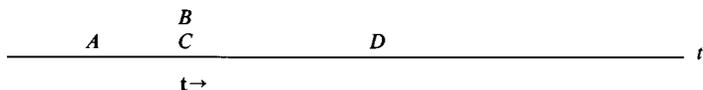
<sup>1</sup> It matters very much to social analysis, of course, which external clock is used. A crude diurnal clock of days and nights adequately coordinates social interactions like rural Nigeria's "market days," whereas a clock based on the oscillations of excited atoms is necessary for the microsecond discriminations on which loran, multiplexing, and computers rely.

describing and understanding – toward modeling – social behavior. But those facts do not constitute an endorsement of a low road – the implicit suggestion that wise scholars who hope to get anything done will shun such encounters with philosophical abstractions. A number of problems arise in a social analysis that impatiently or uncritically assumes that its time dimension is always obvious. Indeed, to economists, Alfred Marshall (1961:vii) wrote, “the element of Time . . . is the centre of the chief difficulty of almost every economic problem.” In this chapter I shall examine what seem to be three of the most fundamental sources of difficulty in taking the low road of casual temporal thinking in the analysis of social behavior. They illustrate that analytical middle road that this chapter advocates – a probing and careful consideration of time, but one that remains analytically pragmatic.

Careless attention to time can mislead economic and social analysis when the temporal perspective of an analyst-observer is confused with that of the actor as the subject of analysis; careless attention to time can lead to the use of inappropriate methodology when the difference between repetitive and unique behavior is ignored; and careless attention to time will hide important economic relationships when too crude a time unit is used. Let me consider these three problems in turn and suggest how their identification will lead to a more effective analytical treatment of time.

### Analytical time and perspective time

Time orders events. A fundamental temporal complication in social analysis is that the same events must always be ordered under two different, though compatible schemes. Under one ordering, any event can be described with respect to another event as taking place *before*, *after*, or *simultaneously*. Under the other ordering, a person or a society is introduced whereupon the same events can also be ordered as *future*, *now*, or *past*.<sup>2</sup> A very simple graph is useful:



<sup>2</sup> This distinction in ordering schemes is a common one; indeed, the first is McTaggart's (1927) "B-series" and the second is his "A-series." His labels properly imply the priority of the subjective future–present–past ordering, I think. I have taken the other priority as more appropriate to our *analysis* of social behavior, where the before–after–simultaneous order is the more common starting point.

Moving from left to right, event *A* clearly happens before events *B*, *C*, and *D* (so they happen after *A*), and events *B* and *C* happen simultaneously. A person moving through time *Now*, at *t*, will, at the moment depicted, see event *A* as past, events *B* and *C* as happening *Now*, and event *D* as taking place in the future. As *Now* (*t*) moves through time (to the right, by convention), future event *D* will approach, happen, and then recede into the past with the rest.

It is useful to make this difference stark by describing these alternative orderings of events as two different “kinds” of time. An *analytical time* involves only the before – simultaneous – after ordering. A *perspective time* shows events the way people experience them in future – present – past ordering.

The problem caused by these two temporal orderings is that the same social behavior can look very different depending on whether it is set in an analytical or in a perspective time context. Analytical time describes the timing of events abstracted from their experiencing. It permits analytical mind games, thinking about events that take place in time while ignoring the constraints placed on an actual participant in those events. Analytical time lets us wander over the time line freely from event *D* back to *B* and *C* and to *A* and *D* again, examining one event and then another, selecting and rearranging events to search for patterns, causality, consequences; it is the temporal context in which we analyze and theorize. In analytical time we approach each event with knowledge of what is to come. This freedom to *think* about events apart from their actual temporal order – taking them “out of time” – is, of course, an essential tool of the social analyst, and more generally of rational behavior, the behavior of the subjects of our analysis. It allows us to see behavior whole, with precedent and antecedents, in such a way that we can hope to make sense of it, as analysts and as actors alike (Loasby 1986).

But if we can *think* in analytical time, we can *live* and act and decide only in perspective time. Though we can rise above time in *thinking about* human behavior – our own and other people’s – we cannot rise above time in *doing* it – thus Kierkegaard’s (1944:206) “Life can only be understood backward; but it must be lived forward.” Perspective time, and particularly *Now*, is the inescapable context of all actual social behavior. And therein lies the potential for mischief and confusion on the analytical low road, because we tend, too easily, to neglect that fact. So immersed do we become in the insights we derive from the Olympian view of human

activity provided by analytical time – so impressed with its genuine analytical power – that we often forget that the people whose behavior we would understand do not and cannot share that view and the omniscience it provides. They behave – they make their decisions that are the object of our study – in perspective time. They must make all decisions Now, and they do so without the capacity to control the pace of events or to recreate the past, without knowing the events that lie in the future.

Three differences between analytical and perspective time are most relevant to social behavior. One involves information: knowledge of the actor's future. Since his or her movements are unconstrained over time, the observer in analytical time can see what is coming at any moment. The actor in perspective time can, at the most, know only what is happening Now and what has happened in the past and guess about what will happen in the future. In analytical time there is full information; in perspective time, the future is not just unknown but *unknowable*. Shackle (1958) has made this a central aspect of his subjectivist critique of neoclassical economics. The second central difference is that in analytical time any moment is the same as any other – none enjoys a special status – whereas in perspective time, the moment Now is different from all others. All action must take place Now; the future is always anticipation and the past is memory. Shackle (1958), indeed, argues that only Now – “the moment in being” – has reality: Past and future can have meaning only insofar as they impinge on the moment Now. The third difference is that perspective time is not subject to control, and, particularly, it is irreversible; in analytical time we roam about and wander back and forth in time at will, but in perspective time, we are stuck in an uncontrollable present.

The analysis of economic behavior, or of our own behavior, in the contexts of analytical and perspective times is likely to yield very different conclusions. Reflecting on one's past week – or on U.S. economic policy – one may see a particular act or decision as remarkably inept. In analytical time one's judgment may be “That was a mistake, a dumb thing to do.” Yet recognizing the limitations of one's time perspective when one made the mistake often leads to a gentler judgment: “That would have been stupid if I'd known then what I know now, but I had little time to think it through and knew I'd not be able to go back and second-guess my decision. Under the same circumstances, I'd probably do it again.” Hicks (1976) saw a more humane and humble economic analysis emerging from

a greater respect for perspective time; he called it an “economics *in* time,” in contrast to our “economics *of* time.”<sup>3</sup>

Both temporal perspectives, of course, are essential to living as well as to social analysis, the one because it allows analysis and juxtaposition – comprehension – of time-differentiated behaviors, the other because it allows insight into the circumstances and constraints within which a given behavior was performed. Hicks (1979:10) described this as the economist’s “need for double vision.” The error of the analytical low road lies in trying to get along without one of these temporal perspectives and getting hold, thereby, of a very partial conception of human behavior. People *are* analytical, rising in their thought processes above the time frame of their actions, but people *are* limited and restricted in the temporal context of their actions; they are confined in time and space, and that restriction has to be part of our understanding of their behavior.<sup>4</sup>

Perhaps the clearest illustration of the seductive power of analytical time lies in the economist’s periodic assertion that discounting the future is irrational, an error due to the fact that “our telescopic faculty is defective,” in Pigou’s (1929:25) famous phrase.

In the context of analytical time, it clearly is foolish to differentiate events solely because of their timing, their futurity. If events *B* and *C* in our simple graph have the same objective value to the analyst-observer – and will have the same value to the actor-participant when he or she has

<sup>3</sup> Derek Parfit’s (1984) effort to demonstrate that using a social discount rate is immoral is flawed by his failure to recognize perspective time and its temporal parallel to individuation in social space. (Of course, analytical time and society [Us-Forever] are the restricted perspectives of much of moral judgment, presumably to weigh against the powerful seductions of imprudence and selfishness from our inevitable individuation in perspective time [Me-Now]). Parfit’s inconsistency lies in his accepting the moral legitimacy of the individual’s perspective in society through the “commonsense morality” of kinship and relationships but, having previously relied on the analogy between time and social space in an “equal treatment” argument, failing to explain why the equally commonsense legitimacy of Now in perspective time does not make an equally strong moral claim differentiating the present from the future.

<sup>4</sup> It is worth noting that the freedom analytical time gives the economist to reorder events, to juxtapose and search for patterns, is similar to that in literature where flashbacks, compression, and anticipation draw meaning from events that would be obscure if described in perspective time (a statement made in deliberate neglect of the deeper issues of poststructuralism). When playwrights were freed from verisimilitude, they – like social analysts – were released from the requirement that their story run in strict perspective time. Making sense of events (interpretation, searching for meaning) is not, of course, the only motive for temporal reordering in literature; it can clearly serve a broader aesthetic-emotive purpose, too.

gained sufficient temporal distance – the mere fact that they occur at different times is easily seen to be irrelevant. Bertrand Russell (1918:21–2) captured this with characteristic clarity in saying that

there is some sense – easier to feel than to state – in which time is an unimportant and superficial characteristic of reality. Past and future must be acknowledged to be as real as the present, and a certain emancipation from slavery to time is essential to philosophic thought. . . . Both in thought and in feeling, even though time be real, to realise the unimportance of time is the gate of wisdom.<sup>5</sup>

But that judgment is sustained only by analytical time. To the participant-actor locked in the unique Now of perspective time, events *B* and *C* are different, and essentially so. At the moment depicted in the graph, *B* is happening and *C* is not. It does not seem irrational for the actor to recognize that fact and in consequence to place a different value on the present event than on the future event – to care more about what *is* happening than what *will* happen. No hint of that difference is inherent in analytical time.

Economists face a special temptation to blur the distinction between analytical and perspective time because of the sheer analytical power they get by endowing their subjects with the perfect knowledge they would have if they lived in analytical time. It yields firm answers to difficult questions that are of considerable social importance; it lets economists utilize the powerful tools of the calculus and optimal control theory to model people as maximizers of unambiguous objectives over a known set of alternative actions that have known outcomes. That is the essence of the paradigmatic neoclassical model: the powerful economic conception of human beings in which rational actors maximize the value of known preferences under the constraints of known prices, incomes, and technologies to achieve optimal, equilibrium behavior. But power corrupts and analytical power corrupts analytically. To recognize that the people we describe are often confused, in the dark about what will happen, groping, doing their best to act intelligently, Now, in the uncertain world of

<sup>5</sup> Russell's authoritative assertion is too good not to quote more fully: "The felt difference of quality between past and future, therefore, is not an intrinsic difference, but only a difference in relation to us: to impartial contemplation, it ceases to exist. . . . *Whoever wishes to see the world truly, to rise in thought above the tyranny of practical desires, must learn to overcome the difference of attitude towards past and future, and to survey the whole stream of time in one comprehensive vision* (Russell 1918:22; italics added).

perspective time, removes the sharp edge of our conclusions, and economists are not eager to do that without good reason.

Yet there has been considerable movement, especially in the past two decades, toward developing an “economics *in time*” that recognizes the limited and time-bound perspective of the people we study. Indeed, much of the most interesting theory of the past twenty years has focused on imperfect information and an actor’s temporal position as an important source of that imperfection. The initial formal incorporation of peoples’ uncertainty – and still the dominant one in formal analysis – was von Neumann and Morgenstern’s (1944). It gave away little of the power of formal modeling since it introduced uncertainty about the future as known probability distributions over a known list of possible future states: Expected values of variables could simply be substituted as “certainty equivalents” in place of deterministic variables, and the old models of perfect certainty could roll on.

But economists now go much farther, at a cost of losing a certain amount of formal rigor, in recognizing both the unknowability of the future – “Knightian uncertainty,” the surprise events that are on no one’s list of possible outcomes – and the implications of those asymmetries that exist between people in what is known. “Bounded rationality” now more often replaces omniscience (Simon 1955; Williamson 1979); surprise replaces known lists of possible events (Knight 1921; Shackle 1958; Williamson 1979); search (Nelson and Winter 1982) and discovery (Schumpeter 1934; Kirzner 1973) replace maximization. All of these recognize the temporal perspective of the subjects of social analysis as being inherently different from the temporal perspective of their analysts.

A question lurks under the surface of the distinction I have drawn between analytical and perspective time: Is it anything more than the unsurprising observation that behavior that is rational when based on full information is not the same as behavior that is rational when based on limited information? I think it is. The distinction does include that, to be sure, but it goes on to identify *why* information is limited. The literature includes a variety of sources of imperfect information, notably limited search activity (Stigler 1961), deceptions of other parties (Akerlof 1970; Williamson 1979), and limited cognitive processing capacities (Simon 1955; Heiner 1983). But here we are saying that some of the limitation on information is existential, the result of the human condition, because people are embedded in time. No amount of effort spent in search

activities, no amount of interpersonal honesty or intellectual expansion will overcome the source of limited information that is revealed by perspective time.

The unknown is different from the unknowable. Something can often be done about the unknown – one can spend more resources on search, trick or cajole or command more information from ones' trading partners, employ more or better minds to increase cognitive capacities. These may not be deemed optimal strategies, but they are always *possible*. The absence of information about the future in perspective time is absolute, however. No expenditure of resources can change the fact that we really do not know what will happen. We can, to be sure, improve our guesses about and forecasts of repetitive events (to which we turn in the next section), but we cannot acquire knowledge about the future. It remains unknowable. The distinction between perspective and analytical time embodies a more fundamental aspect of modeling behavior than does the generic imperfection of information.

So the first pitfall awaiting the unwary social analyst on the temporal low road is the unwitting attribution to the subjects of study, who must act always Now in perspective time, of the wisdom, insights, and temporal mobility that accrue to analysts who observe them and consider their actions in analytical time. We analysts move freely among undifferentiated moments and we know what the future holds, but our subjects do not. The analytical middle road would recognize the utility of analytical time and our employment of its Olympian perspective, but at the same time it would keep firmly in mind the fact that the subjects of our analysis must make their decisions Now on the basis of rushed and imperfect guesses about the future and their consequences.

### **Repetitive events and unique events**

Some things, like eating breakfast or choosing a commuting route, happen frequently and others, like choosing a cancer therapy or starting a war, happen rarely. This difference is empirically observable, and the activities and events of social behavior lie on a continuum between two extremes from continuous to once in a lifetime, or even less. I shall focus now on that characteristic of events and activities – what we can usefully dichotomize for now as their *repetitiveness* or *uniqueness*. It has not, to my knowledge, been explicitly and systematically examined in economic

analysis,<sup>6</sup> yet it appears to do much to determine, for any particular application, the kind of methodology that is appropriate: What is useful for understanding repetitive events, I will argue, is dubious for the analysis of unique events, and vice versa.

Repetition generates experience, which brings learning and the accumulation of knowledge about those events. Learning and knowledge are central. Consider the graph of a set of events:



An individual moving in perspective time *t*, Now, through this event sequence will repeatedly encounter *ABC*. The individual will come to learn that events *B* and *C*, and in that order, follow *A*. Through the repetitiveness of the *ABC* sequence, an individual, even though confined to perspective time, learns what is coming; at least, observing *A*, he or she comes to expect *B* and after it *C*.

Of course, the individual is guessing, and only if *ABC* is perfectly repetitive (never *A* without *B* then *C*) will he or she always guess correctly. But a significant degree of repetitiveness allows the individual a significant degree of confidence in at least some of what will happen in the future. The future no longer appears unknowable – and on this basis, practical people will deny that it is. Indeed, in the extreme of frequently and perfectly repetitive events like the sunrise, the distinction I have just elaborated between analytical and perspective time – between the time perspectives of the analytical observer and the actor – is blurred since the actor, though he or she must operate in perspective time, “knows” as much about the future as does the observer who is in analytical time. To the extent that events are repetitive, the future is a duplicate of the past.

Now consider unique events like *U* and *Q* in the graph. Because they occur only once, there is no basis for anticipating them – they must come as a surprise. Nor is there any source of insight into some relationship between *U* and *Q* – their independence or causality – because there *is* no pattern except that, once they have happened, they have happened and that

<sup>6</sup> See Georgescu-Roegen (1971) for a survey of the literature on “sameness” and identity. See also O’Driscoll and Rizzo (1984) on “typicality” of events – a broader and more subjective concept that begs the central question here of the source of knowledge of what is typical. Finally, see Langlois’s (1984) thoughtful analysis of typicality.

$Q$ , in the particular instance pictured, followed  $U$ . Novelty denies theory.<sup>7</sup> The scholar of truly unique events is restricted to an ex post description of those events, to purely narrative history. Theoretical generalizations – identifying patterns that have implications for the future – are possible only to the extent that events are repetitive (or have repetitive aspects).

The significance of this is that very different methodologies are appropriate to the understanding of repetitive and unique behavior. Repetitive behavior allows “scientific” analysis, searching for the patterns of repetition and testing hypothesized patterns against future repetitions; *prediction* can play the central role that Milton Friedman (1953) assigned it.<sup>8</sup> It produces the classical mechanics model of behavior identified with physics, a model familiar, of course, in the economist’s neoclassical analysis of the rational, fully informed, utility-maximizing consumer or profit-maximizing producer. That model can deal with human behavior in which, thanks to repetition, most people *are* well informed, and they come, with experience, to adjust their behavior to something not unlike an “optimal equilibrium” pattern. It can employ the devices of mathematics and statistics to form and test its hypotheses about behavior. The parameters of behavior – prices, incomes, preferences, and production relationships in the economic model – change slowly relative to the learning and the adjustment that repetition allows, making the idea of an equilibrium a useful construct for repetitive events (Simon 1959; Coddington 1983). Indeed, even the most extreme textbook version of the neoclassical model is a fairly helpful description of highly repetitive behavior: The strictly mechanistic model *does* work for analysis of this kind of behavior.

The analysis of unique events stands in sharp contrast. Here the full force of the limits imposed by perspective time comes into play, the effect

<sup>7</sup> “[T]he *raison d’être* of a theoretical edifice is the economy of thought it yields. If novelty is an immanent feature of a phenomenal domain . . . a theoretical edifice, even if feasible at all, is uneconomical: to build one would be absurd” (Georgescu-Roegen 1971:116).

<sup>8</sup> Heiner (1983) asserted that predictability is due solely to uncertainty, implicitly denying repetitiveness as a basis for prediction and optimization and resting all predictability on defensive rule making – choice restriction. In his enthusiasm for his interesting discovery that uncertainty can explain some regularity of behavior, Heiner is guilty of having slipped from offering us *an* explanation of predictable behavior to giving us *the* explanation. Within his own model, the introduction of regular, repetitive time-specific changes in environment, like light and dark, and winter and summer (Winston 1982), will generate the sorts of repetitive behavior described here and even the optimizing responses.

of the elemental *unknowability* of the future that often forces people to make decisions and take action hastily with only the vaguest sense of where they will lead. As James March (1978) described it (and Bohm-Bawerk [1891] before him), behind such decisions are two guesses: a guess about the results of any action and a guess about how the actor will feel about those results. The uniqueness of the events, itself, denies people the sense of knowledge that comes with repetition: In place of confidence are tentative guesses, hunches, and hope. And in place of statistical verification of scientifically derived hypotheses are historical description and the unresolvable competition among plausible but inherently speculative alternative “stories.” Scholarship is inescapably different for repetitive and novel events. Economists can achieve a far higher level of scientific rigor than historians because they deal more typically with repetitive events: Among the disciplines seeking social understanding, differences in the tasks we set ourselves go far to explain the differences in our methods.

But economics, as currently defined, sets inconsistent tasks for itself because it encompasses everything from highly repetitive to highly unique behavior, from the routine behavior of commuters to the most idiosyncratic elements of economic history. It does so – and this is the source of a major problem in treating time casually – without recognizing that fact, without identifying the repetitiveness of behavior as an important variable, different for different kinds of events. Therefore, economics typically proceeds without discriminating methodologically between its analysis of unique and repetitive events. We search for a single best model of behavior, one that will predict consumers’ repetitive demand for breakfast food or commuting routes and equally well understand their unique choice of medical treatment, or producers’ choice of capital investments, without recognizing the methodological incompatibility of those decisions.

In making prediction the only acceptable test of explanatory validity in economic theory, Friedman played down the restriction imposed on the *kinds* of economic behavior we can therefore study. Elsewhere neoclassical economics is criticized as being inadequate for generating useful insights because it does not recognize the unknowability of the future – hence Shackle’s (1958) often nihilistic subjectivism, which leads him to hold that it is inherent in the nature of time that nothing at all can be known about the future; all is “dark forces of ignorance,” in Keynes’s

phrase. Even Georgescu-Roegen's (1971) penetrating analysis of time in economics seems to dismiss the relevance of these mechanical clock models – scientific models of perfect knowledge – because they neglect the influence of history. But that seems too sweeping in its own neglect of repetitive events.

Repetitiveness similarly appears to have a special relationship to rationality. Only repetitive events that confer full information admit of an unambiguous judgment of what is rational. Indeed, it becomes quite unclear just what “rational behavior” might mean when an actor faces unique events with their inherently unknowable consequences – one's best guess can turn out to be very bad, and pure chance can make triumph out of raw stupidity.

But if it is recognized that the behaviors explained by “rational” or “scientific” modes and by “historical” or “literary” modes are both important and essentially different – and different in the observable characteristic of repetitiveness – the search for and contest between all-purpose economic models and master theories can be replaced by the use of different models appropriate to different tasks. Mechanistic, neoclassical analysis can inform our understanding of repetitive behavior, and more subjectivist, even Austrian, analysis can describe more unique behavior. In a critique of Keynesian economics, Coddington (1983) accused Keynes of a self-serving arbitrariness in asserting that household consumption spending is stable – mechanistically predictable – whereas business investment spending is inherently volatile – dominated by uncertainty and “animal spirits.” Coddington seems to have overlooked the great degree of repetition in consumption behavior relative to the novelty inherent in business investment behavior, yet that difference alone would justify Keynes's different analytical treatment of the behaviors, without arbitrariness.<sup>9</sup>

Repetitiveness determines the range of applicability of neoclassical economic models of behavior. Two things would follow from taking that fact seriously. One is simply the increase in the analytical power of the

<sup>9</sup> The other difference Coddington ignored is the differential durability of consumption and investment goods, which forces more of the anticipated benefit of capital goods farther into an unknowable future, reducing the degree of confidence surrounding any estimate of benefits and making that guess more susceptible, therefore, to “unsubstantial” sources of information like others' opinions.

mechanistic, neoclassical analysis that would result if it were applied more narrowly, if it were used less often where it is inappropriate, applied less often to novel decisions where its information requirements cannot be satisfied. The discovery that saws and hammers are not equally suitable for cutting wood and driving nails surely increases the usefulness of both saws and hammers.

The other implication of recognizing repetitiveness as an important characteristic of events and behavior is, paradoxically, the potential increase in the analytical domain of the economist's mechanistic model of the rational maximizer. If this is a model of repetitive behavior, with well-informed people making rational choices, does it necessarily have to be a model only of repetitive *economic* behavior? Or might it provide useful insights into noneconomic areas of behavior where repetition is also significant? Might not this identification of the role of repetition be a license for even more of the "economic imperialism" – the application of the economist's analytical paradigm to other fields – that is already taking place in law, sociology, and social psychology? I think it is. It is the repetitiveness of behavior that justifies the mechanistic model – and novelty that denies it – rather than its "economicness" per se. So economic imperialism, or cross-fertilization at least, seems highly promising.

Let me end this discussion of repetitiveness with some caveats. First, expositionally convenient though it has been to play down that fact, events and behaviors cannot be neatly and unambiguously divided into those that are repetitive and those that are unique. No event is other than unique; like snowflakes and tree leaves, each event is essentially different from all others. But like snowflakes and tree leaves, events and behaviors are usefully treated as if they were the same. So we must recognize that events will be similar, hence repetitive, in some aspects but not in others. "The same" breakfast may be eaten in different places at different times while dressed differently: For decisions on menu, that breakfast may be one of a repetitive set of events; for decisions on location or dress, it is novel. But repetitiveness can often be identified in those aspects of events and behavior that are relevant: The demand for breakfast food would not seem to depend on what people wear while eating it.

Second, the degree of repetitiveness describes a frequency of events *relative* to what has to be learned about them. No purely mechanical

measure of repetitiveness is always sufficient, and some behaviors (like falling in love) may be so complex that even a very great deal of repetition does not make most of us fully informed and rational actors.

Finally, though experience is an excellent teacher, it is not the only teacher, and one's own experience with repetitive events will be augmented by experience borrowed from others – by “reputation,” broadly defined. This is a source of information that gives us knowledge about personally unique events, reducing our effective ignorance about the future without experience. Reputation, then, often serves as a substitute for repetition – one's decision about a cancer therapy, for example, need rarely be made in total ignorance of others' experiences.<sup>10</sup>

So the second analytical peril of the temporal low road is the use of a methodology appropriate to repetitive events in an attempt to understand behavior that is relatively unique. The analytical middle road will recognize that the information assumptions of even the strictest neoclassical economic models are often approximated by repetitive behavior even though they are silly when applied to unique events and novel behavior. There subjectivism and a greater scientific modesty are more appropriate.

### The size of the time unit

Unique events are *dated*; repetitive events are *counted*. Pearl Harbor was bombed on December 7, 1941, and 9.3 million cars and trucks were made in the United States in 1979. The third and final problem of the analytical low road that I shall consider is the apparently mundane matter of choosing the duration of the analytical time unit over which to count or describe events – the year or the business cycle or the day or the second. The problem is, simply, that any time unit suppresses information about the timing of events *within* it. Since economics deals so heavily in repetitive events, in contrast, say, to history, it is especially vulnerable to the problems generated by such temporal abstraction.

Knowing yearly auto production, we know nothing about when those cars were produced within the year – whether all were made in January or in June or produced evenly throughout the year. We can impose a time

<sup>10</sup> It should be noted that literature, again, plays an important role in conveying information about unique events; though things may happen only once or twice in one's lifetime, an avid reader (or movie goer or television watcher) can vicariously learn from others' experiences.

pattern using external common sense, but nothing in the data supports it. The count of events over a time unit is much like the statistical average of a characteristic over a population. Both are useful single summary numbers, so both abstract from the variety of their underlying observations, temporal variety in the first instance and individual variety in the second. But because we have traveled so long on the low road in thinking about time, we are less alert to the dangers of temporal abstraction than to those of statistical abstraction. Often it does not matter, but often it does.

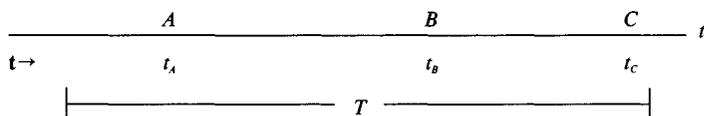
I could illustrate the importance of this information loss within the analytical time unit from my own work in production analysis. The use of a typical day to measure the wage rate long obscured the fact that wages are usually higher at night, which makes it prohibitively costly for some plants to operate at that time – a fact that has a significant effect on productive capacity and capital productivity, matters of considerable concern to economists and policymakers. But free-rider transactions make a more general illustration, with broader implications outside of economics.

“The free-rider problem” can be summarized as follows. Because of their technical natures, some of the economic goods and services that people want – public goods – if provided to anyone, must be provided to everyone. National defense and clean air are archetypical examples. This technical hitch might be resolved by voluntary payments from those who wanted such goods; then the goods could be supplied by the market just as private goods are. But, clearly, any self-interested citizen would then have an incentive to free-ride, denying his or her true preference for the public good in order to avoid making a voluntary payment for it. If others provided the public good, he or she would get it for free: A free rider gets something for nothing. Because of free riding, too little of a public good will be supplied. In the extreme, if everyone is a “rationally self-interested” free rider, the public good will not be provided by a private market; if it is to be had at all, it must be provided by the state with its ability to make people pay through coercive taxation.

That is all conventional economic wisdom. Conventional wisdom also holds, by contrast, that *private* goods can be left to market transactions because private transactions are self-enforcing, free of free riders. It is the technical nature of private goods that they can be used *exclusively* – they are appropriable – so free riding is eliminated among self-interested

actors. The classic illustration for undergraduates is a case of beer, the consumption of which can easily be denied to any person who is not willing or able to pay for it: no payments, no beer. So there is thought to be no free riding in private transactions.

But now consider another graph of a sequence of events in time:



These are the elementary events that make up a private transaction, shown here as they occur in time. Event *A* is the agreement between buyer and seller to exchange goods for money and money for goods; event *B* is the payment by the buyer; and event *C* is the delivery of the goods by the seller. (Payment can follow delivery, making *B* the delivery of the goods and *C* the payment, as on a MasterCard.)

Now consider the time perspective of the buyer and seller as they move through this event sequence, actually carrying out the transaction – graphically, as Now, *t*, moves from left to right. At time  $t_A$  they agree to go through with the transaction, exchanging money for goods because each of them feels he will be made better off by the completed transaction. The buyer wants the goods more than the money; the seller wants the money more than the goods. Both expect to gain from the trade. All goes smoothly until the payment takes place (*B*) at time  $t_B$ . Suddenly there is a problem: The seller has the buyer’s money but he has not yet delivered the goods. In terms of pure self-interest, he has every incentive to take the money and run.

In the middle of this private transaction, then, the free-rider problem appears because of the *timing* of the events that make up the transaction. The seller can get something for nothing, at the expense of the buyer.<sup>11</sup> And just as the potential public-goods free rider is sometimes declared to be “irrational,” in light of his self-interest, if he *does not* lie about his preferences for the public goods in order to avoid making a voluntary payment, so the private-transactions free rider is “irrational” if he does not opportunistically renege on his part of the transaction. So opportunism

<sup>11</sup> The underlying story remains the same, of course, if the seller first delivers the goods in the expectation of the buyer’s subsequent payment, except that the buyer is then the opportunistic free rider who can take the goods and run.

is *required* by the conventional assumption of raw self-interest on both parts.<sup>12</sup>

Contrary to conventional wisdom, incentives to free-ride are not unique to public-goods transactions but permeate private transactions, too; without restraints, their corrosive effect should, with minimal learning by would-be transactors, prevent that private kind of transaction, just as it is held to prevent public-goods transactions.

The immediate question is how private transactions actually do survive this incentive to free-ride that is revealed in the time-shape of transaction. Despite strong free-rider incentives that would destroy even private transactions, the bald fact is that markets do work, that opportunism does not bring the private economy to a grinding halt.<sup>13</sup> Why? Laws exist, of course, making it a crime to renege too blatantly on transactions agreements, but it would be hard to argue that fear of arrest and prosecution explains the success of most private transactions. The deeper reasons for the triumph of private trading despite its temptations and risks are revealing, I think, of limitations of the motivations attributed to economic man. A more complex and humane picture of private trading and dealing emerges from the time-specific view of transactions – a picture of those things that must soften the hard drive of self-interest so central to economic models of behavior.

Most centrally, honesty, trust, morality, and codes of ethics will clearly

<sup>12</sup> Williamson (1979, 1985) has, for some time, suggested that a “deeper” sort of self-interest must be involved, that guile, trickery, and deception must be added to self-interest *per se* to get full opportunism. Though it borders on quarreling about what self-interest “really” means – whether and to what extent we implicitly constrain the play of self-interest in our use of it – it is important that no guile need be involved here, but only myopia. Only if the seller knows that he will, when his opportunity comes, renege on the deal need there be guile or deception. The entirely naïve self-interested trader would be required to renege, too, when he recognized his opportunity to free-ride. Certainly, as I have argued elsewhere (Winston 1982), this kind of self-interest is no different from that assumed in the public-goods arguments in which the deception and guile of misstated preferences is taken for granted.

<sup>13</sup> Although the private free-rider problem may have been neglected by modern economists, it was not neglected by Thomas Hobbes. In *Leviathan*, he wrote: “If a covenant be made, wherein neither of the parties perform presently, but trust one another; in the condition of mere nature, . . . upon any reasonable suspicion, it is void. . . . For he that performeth first, has no assurance the other will perform after; because the bonds of words are too weak to bridle men’s ambition, avarice, anger, and other passions, without the fear of some coercive power; which in the condition of mere nature, where all men are equal, and judges of the justness of their own fears, cannot possibly be supposed. And therefore he which performeth first, does but betray himself to his enemy; contrary to the right, which he can never abandon, of defending his life, and means of living” (Hobbes 1962:108).

allow private transactions to take place; free-rider opportunism does not destroy the transaction if self-regarding behavior is modified by other-regarding values. A higher value, a “metapreference” in Sen’s (1977) analysis, restricts the free play of self-interest attributed to economic man. Honest behavior and morality are then instrumental in the private economy as they “attenuate opportunism,” in Williamson’s (1979) phrase.

But it need not even really be honest or moral or other-regarding behavior – in Reder’s (1979) strong sense of behavior that carries a cost. Honest-like behavior will often serve, too, to restrict opportunism in private transactions. If transactions between two people are, to return to an earlier theme, repetitive over time, the gain to the opportunist from renegeing on any one transaction may be more than offset by his consequent loss of future gains from continued trade with that partner. If so, it pays to be honest, or at least honest-like, even in terms of the crassest self-interest. So the market will function with repetitive transactions between the same individuals as the promise of future trade disciplines present temptations to renege.<sup>14</sup> And reputation will serve much the same role in nonrepetitive transactions if there is inexpensive communication between actual and potential trading partners; people are more honest in small towns, except when they deal with tourists.

But repetitive transactions create other complications for the economists’ model because people who deal with each other repeatedly over time will develop *relationships* in which behavior will be far more complex and subtle – far less amenable to classical mechanistic models – than in the timeless, anonymous auction markets with which economists are most comfortable. This is a subject of considerable current research in the economics of relational exchange (Goldberg 1980; Klein and Leffler 1981; McPherson and Winston 1983; Schultze 1984).

The importance of trust, honesty, and the longer-run self-interest of repetitive, relational transactions is underlined by the nature of those transactions in which trust and honesty do not exist – where one-shot exchanges take place between people who have no reason to trust or be trusted. The much maligned encyclopedia or used-car salesman comes

<sup>14</sup> This theme of the discipline of opportunism by repetitive transactions has been developed by Telser (1981) and by Benjamin Klein and K. B. Leffler (1981) in a series of excellent articles. It is, more generally, a special case of the hostage issue identified by Telser (1981) and Williamson (1983), a case in which the valuable resource that is put at risk as a hostage to reduce opportunism is the gains from potential future transactions.

immediately to mind, but even more to the point are kidnappers' ransom payments or, on a grander scale, the Iranian hostage exchange of 1980. In these extreme cases, the ultimate protection to both transactors is invoked by altering timing through managed simultaneity, by making the payment and delivery events occur at the same time so that the period of time-specific vulnerability to opportunism – the period from  $t_B$  to  $t_C$  – simply does not exist.

But how does this illustrate the role of the analytical time unit? Because all information about the temporal order and distance of events within a time unit is lost when transactions are analyzed in a large time unit, like the period  $T$  on the graph above, we can know only that events  $A$  and  $B$  and  $C$  together – the complete transaction – happened sometime within  $T$ . We cannot know when or in what order those events occurred, considered separately. But, of course, the private free-rider problem arises *only* because of *when* things happen, only because between  $t_B$  and  $t_C$  one party is vulnerable to the other's opportunism. That vulnerability is wholly invisible if events  $A$ ,  $B$ , and  $C$  are simply counted as having occurred within time unit  $T$ . The private free-rider problem appears only when explicit attention is given to their order and timing, and it is that from which the use of the inappropriately large time unit  $t$  abstracts. Conventional analyses of private transactions implicitly use a big  $t$  as the time unit, hiding the potential nonsimultaneity of events  $B$  and  $C$  that creates the problem. In the view of private transactions described here, in contrast, a much smaller (instantaneous) time unit exposes the timing of the transactions event sequence and with it the source of temptation to engage in opportunism and private free riding.

So the third pitfall on the low road is that the careless use of an analytical time unit hides the timing of events and with it important aspects of the problems we would understand. A more temporally meticulous analysis would self-consciously choose a time unit short enough to reveal the relevant social behavior, a time unit that would suppress only that information deemed analytically uninteresting after an effort at explicit consideration.

### Conclusion

The argument of this chapter is that, although few of the temporal complexities and bafflements of the metaphysician's concern with the nature of time – the philosophical high road – are relevant to the analysis

of social behavior, it is nonetheless true that a careless and offhanded treatment of the role of time in economic analysis – the analytical low road – often yields murky confusions and inappropriate models. We surveyed an analytical middle road here by identifying three shortcomings inherent in the low road and by suggesting how their recognition can lead to a more effective understanding of social processes. Recognition of the difference between analytical time and perspective time induces analysts to be more attentive to the limitations – temporal, informational, and cognitive – under which real people act, choose, and live; it helps us resist the intellectual Monday-morning quarterbacking inherent in viewing behavior only as temporally omniscient analysts. Recognition of differences in the repetitiveness or uniqueness of the events and behaviors we study, and of the importance of that neglected characteristic, will induce those who would better understand social behavior to use their methodological tools, ranging from scientific to literary, more appropriately and therefore more effectively. Finally, recognition of the way the analytical time unit always hides temporal information about events within it will encourage more careful selection of that time unit and less frequent use of handy calendar periods that may or may not fit the problem at hand.

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## **Hayek, the Scottish school, and contemporary economics**

**JOHN GRAY**

In his own account of his intellectual formation, F. A. Hayek has always acknowledged his indebtedness to the thinkers of the Scottish school and, above all, to Ferguson, Smith, and Hume. Indeed, in his contributions to the intellectual history of classical liberal political economy and social philosophy, Hayek has gone so far as to distinguish two divergent and opposed intellectual traditions – that of the French Enlightenment, which he sees as inspired ultimately by a variation of Cartesian rationalism, and that of the Scottish Enlightenment, with its roots in a Christian and skeptical recognition of the limits of human understanding – and has identified himself explicitly with the Scottish tradition. That Hayek's thought converges with that of the leading Scottish political economists on many fundamental questions is not in serious doubt and can easily be demonstrated. At the same time, the thought of the Scottish school is only one of the influences that have shaped Hayek's complex intellectual makeup, and these other influences, especially that of his teachers in the Austrian school, are responsible for many of the points of sharp and real divergence between Hayek and the Scottish philosophers. It is by virtue of these other influences that we may say that Hayek's thought diverges from that of the Scottish philosophers as often as it converges with it – and, often enough, in ways Hayek has not himself perceived.

It is by virtue of his debts to the Austrian economists, also, that Hayek's thought has been neglected in the mainstream of economic theory. Hayek's intellectual relations with his Austrian forebears are so complex, and his own methodological and theoretical perspective so distinctive, that explicit acknowledgment of his chief insights is rare in the literatures of conventional economic theory. Yet many distinctively Hayekian themes – such as the role of tacit information in coordinating market processes, the subjectivity of economic phenomena, and the function of markets as

discovery procedures for preferences – surface in the more centrally situated works of Shackle, Arrow, and other mainstream economists. In short, whereas Hayek's explicit presence has at least since the Second World War been on the margins of economics, his characteristic theoretical insights are found at many growth points in the literatures of economics since that time. One of my goals in this chapter is to account for this paradoxical circumstance.

In seeking to uncover the complicated connections between Hayek, the Scottish writers, and contemporary economics, I address two central themes: the conception found in Hayek and the Scottish writers of the uses of knowledge in society and of the role of morality in sustaining a market order. One conclusion of my investigation is that Hayek has sometimes ascribed a uniformity of view to the Scottish thinkers that historical inquiry does not support. Another conclusion, which may be of interest chiefly to Hayekian scholars but which may be of broader concern to political economists and social theorists, is that Hayek's own thought is beset by conflicts and tensions that finally disable it as a system. The conclusion I reach is that, notwithstanding its ultimate failure, Hayek's system of thought retains considerable contemporary interest, both for the many important insights it encompasses and for its achievement in keeping alive political economy as an intellectual tradition whose subject matter and concerns transcend many contemporary disciplines. Notwithstanding Hayek's comparative neglect, the continuing vitality of his theoretical work is indicated by the many occasions on which his chief insights surface, often stated in different terminologies and within other conceptual frameworks, in a variety of areas in contemporary economics.

### **Distinctive elements in Hayek's system of thought**

Hayek's system of thought comprises two major elements. The first is expressed in what I have elsewhere<sup>1</sup> called his *epistemological turn* in social philosophy. By this is meant his insistence that, against the dominant tradition in social and political theory, social institutions be assessed and compared by reference to their capacity to conserve, generate, transmit, and make use of knowledge rather than by their conformity to some preferred principles of political morality. It is not, to be sure, that Hayek sees the production of knowledge by social institutions

<sup>1</sup> See Gray (1986:134–40).

as an ultimate or intrinsic good. Instead, he argues that human goals, whatever they may be, are best assured of achievement when the growth of knowledge is promoted and existing knowledge effectively utilized. It is of vital importance in any attempt to understand Hayek's thought to grasp that he believes the knowledge-producing function of social institutions to apply across the board – not only to the central institution of market pricing, but also to law, morality, religion, and language. For Hayek, all these important social institutions are best conceived as vehicles for the conservation, transmission, and generation of knowledge. They are (so to speak) carriers or embodiments of knowledge held in practical form – as skills, traditions, and practices – and they are an indispensable condition of our developing and using knowledge in its theoretical forms.

For Hayek, the problem of knowledge – the problem of how we are to make best use of the knowledge we have as well as the problem of how we are to acquire new knowledge – is the central problem of social order. It is also the central problem of economic theory. As he has put it:

The peculiar character of the problem of a rational economic order is determined precisely by the fact that the knowledge of the circumstances of which we must make use never exists in concentrated or integrated form but solely as the dispersed bits of incomplete and frequently contradictory knowledge which all the separate individuals possess. The economic problem of society is thus not merely a problem of how to allocate "given" resources – if "given" is taken to mean given to a single mind which deliberately solves the problem set by these "data." It is rather a problem of how to secure the best use of resources known to any of the members of the society, for ends whose relative importance only these individuals know. Or, to put it briefly, it is a problem of the utilisation of knowledge which is not given to anyone in particular. (Hayek 1976a:77–8)

For Hayek, then, the economic problem is not one of allocating scarce resources to competing ends, but one of coping with the most fundamental scarcity of all, human knowledge. Market institutions are then understood as rational devices for the division of knowledge in society, as institutions whereby knowledge scattered and dispersed across society becomes nevertheless available through the price mechanism to the society as a whole. The market process is not according to this view an allocative mechanism but rather an epistemological device, a discovery procedure in which knowledge that could not be collected by a single mind (or committee of minds) is yet rendered accessible and usable for human purposes.

This conception of market institutions as an epistemological device is,

perhaps, Hayek's most fundamental and original contribution to economic thought. It has echoes, no doubt, in recent studies in the economics of information, in which information is itself viewed as a costly commodity and in which practices such as advertising are theorized as rational devices for lowering information costs in the economy. At the same time, Hayek's conception of market institutions as a discovery procedure remains very different from that which animates studies in the economics of information. It is not that market processes lower the costs of information to economic agents, but that they allow participants to make use of information that would not otherwise be available to them at all. Invoking his contributions to the famous calculation debate between the Austrian economists and the theorists of market socialism such as Lange and Lerner, Hayek maintains that market prices embody information about preferences and relative scarcities that is available in no other way. One crucial argument he advances in support of this claim is the Polanyian<sup>2</sup> argument that much of the knowledge transmitted by market prices is tacit and local knowledge untheorized (and perhaps untheorizable) by its possessors. This kind of knowledge – the knowledge that may be expressed in entrepreneurial insight, for example – may well be unknown to its possessor. Market prices then embody knowledge of which market participants themselves may be ignorant. Arguing that social institutions in general are adaptations to our inevitable ignorance of most of the facts of the social world, Hayek maintains that market institutions in particular enable us to make use of knowledge we do not know we have.

The epistemological turn in Hayek's thought leads him, as we have seen, to view social institutions as knowledge-bearing phenomena. The second element in Hayek's thought seeks to promote an evolutionary conception of the emergence and development of social institutions. Generalizing Menger's account of the rise of money as an unintended consequence of human action, Hayek submits that social institutions are (in Ferguson's phrase) results of human action but not of human design; they are artifacts but not constructs, having evolved as adaptations to changing circumstances and needs. No one could have planned the central social institutions – law, morality, language and the market – and no one understands their detailed functions. In general, Hayek develops an

<sup>2</sup> Michael Polanyi invokes the idea of tacit knowledge in the context of an argument against central economic planning in his *Logic of Liberty* (1951:114–22). The notion of tacit knowledge is developed in Polanyi's major work, *Personal Knowledge* (1962).

*evolutionary-functional* conception of the development of the major social institutions, in which it is maintained that their emergence and subsequent history can be explained by their contribution to the survival chances of the social groups that subscribe to them.<sup>3</sup> This second element of Hayek's thought is not a simple claim but a complex thesis. In part it is the claim that social institutions are the unintended consequences of the actions of individuals; it is a methodological individualist thesis about the explanatory reconstruction of social institutions. As such it remains controversial but not clearly indefensible. Hayek's evolutionary-functional claims are far more clearly disputable. To begin with, human history is too riddled with sheer contingencies for any monocausal model of institutional development to be at all plausible, and for this reason, a Darwinian explanation of the rise and fall of institutions comes up against many strong counterexamples. For example, Hayek's suggestion that there is a sort of natural selection of religions,<sup>4</sup> in which religions favoring private property and family life prevail over others by virtue of the enhanced survival chances they afford the offspring of their practitioners, neglects the role that the capture of state power has often played in accounting for the triumph of religions over their rivals. In fact, the evolutionary turn in Hayek's thought seems open to all the criticisms and objections that disable the evolutionary-functional sociologies of Herbert Spencer, W. G. Sumner, and (perhaps) Marx.<sup>5</sup>

The two fundamental elements that distinguish Hayek's thought having been sketched, a few remarks on their difficulties and mutual relations may be in order. In the first place, though Hayek connects it with his thesis of the knowledge-bearing role of social institutions, the thesis that there is a sort of natural selection of institutions and practices is evidently wholly independent of it. One may grant the epistemological functions of social institutions and admit the vital contribution these functions make to human well-being without in any way endorsing Hayek's cultural Darwinism. This is to say that, from the competition or rivalry among social

<sup>3</sup> Hayek's evolutionary functionalism in respect of social institutions is discussed and criticized in Gray (1986:135-38).

<sup>4</sup> Hayek's conception of the natural selection of religions is advanced in his as yet unpublished work, *The Fatal Conceit: The Intellectual Error of Socialism*.

<sup>5</sup> That Marx's historical materialism is a form of functionalism with affinities to Darwinian evolutionary theory has been argued by G. A. Cohen (1978) in his *Marx's Theory of History: A Defence*. I have criticized Cohen's Marxian evolutionary functionalism in my "Philosophy, Science and Myth in Marxism" (Gray 1982:71-96).

institutions, practices, and systems, nothing guarantees the success or survival of the institutions that are best in terms of productivity, efficiency, capacity to sustain human populations, or contribution to the growth of knowledge. It is, after all, one of the larger implications of the calculation debate that attempts at socialist central planning only squander available knowledge by making it unusable. In other words, the attempted suppression of market pricing in Stalinist-style socialist economies results only in a depletion of the common stock of human knowledge – knowledge that had hitherto been preserved or stored in market prices. If socialist economic institutions come to prevail over market institutions, then according to Hayek's own account this will lead to a massive impoverishment not only of living standards but also of the human capital of knowledge in society. There is nothing about human cultural evolution or institutional development that assures us that the institutions that are best in epistemological terms will be those that prevail. For this reason, the two fundamental theses of the Hayekian system are not only independent of one another; they may even on occasion come into conflict with each other. But what does all this say about Hayek's relations with the Scottish school?

### **Hayek and the Scottish philosophers on the use of knowledge in society**

The point of closest convergence between Hayek and the Scottish writers lies in their anti-Cartesian skepticism of systematic or comprehensive social reform by the use of the human reason. As Adam Smith puts it in a famous passage:

The man of system . . . seems to imagine that he can arrange the different members of a great society with as much ease as the hand arranges the different pieces upon the chessboard. He does not consider that the pieces upon the chessboard have no other principle of motion beside that which the hand impresses upon them; but that, in the great chessboard of human society, every single piece has a principle of motion of its own, altogether different from that which the legislature might choose to impress upon it. If those two principles coincide and act in the same direction, the game of human society will go on easily and harmoniously, and is very likely to be happy and successful. If they are opposite or different, the game will go on miserably, and human society must be at all times in the highest degree of disorder.<sup>6</sup>

<sup>6</sup> Hayek (1978:269) quotes this passage from Smith in his *New Studies in Philosophy, Politics, Economics, and the History of Ideas*.

The skeptical perspective expressed in this passage, in which is asserted the incapacity of human intelligence to capture the vast complexity of social life, is one found in most of the Scottish thinkers, and it has many echoes in Hayek. Like the Scottish thinkers, Hayek constantly asserts a contrast between civilization and barbarism – between societies with developed systems of law and institutions of property, extended networks of trade and commerce, and a considerable measure of social and geographical mobility, on the one hand, and societies of a more autarchic, traditional, and agrarian sort, on the other. According to Hayek, the point of demarcation between these two categories lies in the uses made of knowledge in society. As he puts it, “It might be said that civilisation begins when the individual in the pursuit of his ends can make use of more knowledge than he has himself acquired and when he can transcend the boundaries of ignorance by profiting from knowledge he does not himself possess” (Hayek 1960:22). Human constitutional ignorance is not, however, abolished in civilized society; it is acknowledged and thereby diminished by reliance on institutions such as market pricing and by subscription to knowledge-bearing cultural traditions. Hayek (1960:24) further qualifies the kind of ignorance to which the institutions of a civilized society are a response when he observes that “the knowledge which any individual mind consciously manipulates is only a small part of the knowledge which at any time contributes to the success of his action.” And again: “Scientific knowledge does not exhaust even all the explicit and conscious knowledge of which society makes constant use” (p. 25). In summary, Hayek remarks that “concurrent with the growth of conscious knowledge there always takes place an equally important accumulation of tools in this wider sense, of tested and generally adopted ways of doing things” (p. 27).

Despite their many obvious points of affinity and convergence, the Hayekian doctrine of human constitutional ignorance is not merely a reiteration of the skeptical Scottish insistence on the limitations of human understanding. It is hardly a theory of ignorance at all, but rather an account of the primordially practical character of our social knowledge. Hayek’s argument is a version of the thesis of the primacy of practice in the constitution of social knowledge that has been advanced in other forms by such philosophers as Wittgenstein, Ryle, Oakeshott, and Heidegger. In Hayek’s work, as in Oakeshott’s, the thesis that much social knowledge is always buried or stored in social practices and is often unsusceptible to

conscious recovery or rational reconstruction is used as a battering ram against various schemes for comprehensive social planning and reform – schemes that Hayek lumps together under the pejorative label of “constructivism.” In this respect, Hayek’s criticism of constructivist rationalism has something in common with Pascal’s critique of Cartesianism. Both insist that much knowledge cannot be formalized in any systematic way and affirm that the project of reconstructing knowledge on indubitable foundations can only end in an impoverishment of our knowledge, including our moral knowledge. (We know that Descartes himself retreated from the project of reconstructing moral knowledge and opted instead for a provisional and conventional morality.) In Pascal’s case, but not straightforwardly in Hayek’s, the upshot of the critique of constructivist rationalism – which means here only the doctrine that all knowledge worthy of the title can, and perhaps should, be given a statement in theoretical or formal terms – was a sort of skeptical traditionalism in social and economic matters. Because Hayek is concerned, as Pascal was not and could not have been, with the necessary conditions of a large industrial society, his inference from the impossibility of constructivist rationalisms is different. For Hayek, the positive normative implication of the thesis that our social knowledge has always a profoundly practical character lies in the demand that individuals have an assured space of independence within which they may act on their own goals and with the aid of the tacit knowledge each uniquely possesses. In political terms this becomes the demand for strong private property rights under the rule of law. The primacy of tacit knowledge then yields a knowledge-based defense of property rights. This is, in effect, the reverse side of Hayek’s epistemological arguments against socialist calculation.

Whereas their normative and political implications are the same, there is little in the writings of the Scottish philosophers that parallels Hayek’s use of the idea of tacit knowledge. In Hume there is, as a result of his complex skeptical argumentation, the insistence that in the end we rely on natural belief in all our dealings with the world, but there is little that could be construed as a recognition, however oblique, of the importance of social institutions as stores or bearers of practical knowledge. In this connection, Hume differs radically from Burke, who explicitly defends traditions as bearers of knowledge otherwise inaccessible to any one generation of people and in whose writings there is an informal but

pervasive reliance on a conception of tacit or practical knowledge. Burke here deviates, however, not only from Hume but from all the other major Scottish writers.

Hayek's position on tacit knowledge may well be an entirely original feature of his thought, since it seems to predate Polanyi's writings on the subject (see Gray 1986:14–15). At the same time, it may also be interpreted as a development of the subjectivist analysis of social phenomena that Hayek inherited from his Austrian teachers. Though at first it figured as a theory of value, Austrian subjectivism soon came to be extended to such important institutions as money and to be used against objectivist positions in the theory of expectations. In Hayek's work a subjectivist account of social life is explicitly advocated (see, in particular, Hayek 1952), which has significant implications for the character and method of political economy. Among the Scots, as Myrdal and many others have observed, objectivist conceptions of economic value survived that led to an endorsement of the "communitistic fiction" of the economy as a household. For this reason, political economy was for the Scottish thinkers incompletely emancipated from the idea of a science of wealth, or *plutology*, which had figured in much mercantilist thought. Hayek, by contrast, makes a sharp distinction between an *economy*, in which resources are known and ranked in a hierarchy of importance by reference to an agreed scale of values, and a *catallaxy*, which is the vast network of exchanges, taking place without a hierarchy of values or common knowledge of the available resources, that constitutes the real economic life of society. Economic theory has as its subject matter this network of exchanges, not the allocational decisions that occur in a firm or a household, and is better called catallactics than economics. Hayek's subjectivist value theory leads naturally, then, to a conception of the subject matter of economic thought that is radically different from that of the Scots (based as it was on an objectivist, labor-based value theory). This does not mean, however, as I shall later maintain, that the Scots writers do not converge with Hayek on a broad general conception of political economy. What we have established is only that Hayek's neo-Austrian subjectivism leads him to a conception of market institutions and indeed economic life that is very different from any conception of them to be found among the Scots. Is there any greater convergence in their views of morality?

### Morality and society in the Scottish writers and in Hayek

The upshot of a powerful current in recent historiography is that the Scottish Enlightenment was significant in effecting a radical secularization of political morality. In its most profound statement, John Dunn's (1985:chs.1-3) interpretation of the relations of John Locke with the Scottish philosophers, it is argued that Locke's thought is embedded (and is, indeed, barely intelligible outside of) a context of Christian theism. Whatever epistemological problems Locke may have in accounting for our knowledge of the divine will, all the moral force of natural law and natural rights in his system derives from their source in the will of the Deity. By contrast, in the Scottish writers, according to Dunn, a dissolution of all such theistic schemes is effected, with the result that morality, particularly political morality, is given a wholly naturalistic form. It is one of Locke's least appreciated anticipations of future intellectual developments, Dunn (1985:54) avers, that the naturalistic reconstruction of morality by the Scottish writers may in the end prove unstable or at least inadequate as a basis for political legitimacy.

That the Scottish writers inherited a Lockean vision of civil society while at the same time divesting it of its foundations in a certain kind of Christian piety, and attempted to rest its justification on a largely secular basis, are claims that are easily substantiated by a survey of the principal Scottish writings. At the same time, it is easy for an account of this sort to neglect important differences among the Scottish writers. Whereas the secular character of David Hume's reconstruction of political morality is unambiguous and incontestable, the case of Adam Smith is more equivocal. The coincidence of private with public interest – which in the later and wholly secular thought of Bentham, James Mill, and the other Philosophic Radicals, was a contingency grounded institutional artifice – has in Smith the status of a natural law whose guarantor is in the end a beneficent Providence. It seems for this reason an exaggeration to assimilate the uncompromising secularism of Hume into the deistic naturalism of Smith. An even starker contrast is that of Hume with Burke, whose debts to Smith are well known. In Burke, the Whig presumption of progress in history rests securely on an explicitly providentialist historiography that has no echo in Hume. The latter's thought is largely lacking in any notion of progress, history being represented in pagan and Machiavelian fashion as a cycle of civilization and barbarism. Whereas the lack of

historical dimension in Hume's political thought has been over stated by many writers and has now been amply criticized,<sup>7</sup> it remains the case that his account of political life has a static quality that leaves him closer to Hobbes and Spinoza than to some of the other major Scottish thinkers such as Ferguson.

These differences in the degree of secular commitment among the Scottish writers have important implications for their links with Hayek. One way of illuminating this complex structure of influences is to note that Hayek himself fails to mark the contrasts I have mentioned above between Hume on the one hand and Burke and Smith on the other. He assimilates Burke together with all the Scottish philosophers into a single tradition of "English" liberal thought (Hayek 1960:55-6) – an assimilation that neglects the differences in degree of secular commitment and the diversity of conceptions of historical development among the thinkers so grouped together. In spirit and outlook, Hayek is closest to Burke, though Burke's providentialism undergoes in Hayek's thought a secularist metamorphosis into a form of cultural Darwinism. The view common to both Hayek and Burke of traditions as carriers of knowledge accumulated across the generations fits well with the epistemological turn in Hayek's social theory. At the same time, it suggests a number of hard problems for Hayek's defense of liberal civil society and the market order. One problem has already been intimated – the fact that nothing in Hayek's thought guarantees that civil societies will in the end prevail over tyrannous ones. Such an outcome could be assured only, if at all, in a historical theodicy that, like Burke's, had an explicit theistic context. (Even in Burke, providentialism faces notorious difficulties, vividly ventilated in the closing sections of the *Reflections*, about how the victory of the French ideologues is to be accounted for in providentialist terms.) The expectation that liberal civilization will become universal is in Hayek's thought, as it was in the earlier evolutionary sociology of Herbert Spencer, a form of moral optimism that is justified by nothing in the evolutionary theory itself.

Hayek's Burkean conception of moral tradition confronts another problem created by his wholly un-Scottish endorsement of the moral attitudes of Mandeville. As is well known, most of the Scottish thinkers, and in particular Smith and Hume, were strongly critical of Mandeville's

<sup>7</sup> For example, by Forbes (1975).

moral radicalism, in which the achievements of civil society are shown to be dependent on a toleration of human attributes – greed, lust, envy – condemned by inherited Christian moral traditions. None of the Scottish writers was prepared to accept Mandeville’s equation, part ironic, part speculative, of private vice with public virtue. In this respect, the Scottish writers were able to combine their moderate political radicalism with a robust moral conservatism. For Smith as much as for Hume, the stability of civil society depended on the flourishing within it of the traditional virtues, and in particular of honesty, sobriety, deference to superior rank, and so on. The attitude of the Scottish thinkers to the moral inheritance of Christianity may well have been a complex and ambivalent one, since, as a whole school of intellectuals following Pocock has shown (see, e.g., Winch 1978), Scottish thought during this period was not uninfluenced by civic humanism with its sympathies for pagan moral life. None of the Scottish writers followed Mandeville, however, in his readiness to deploy a subversive critique of inherited moral traditions in the service of a defense of civil society. What is noteworthy in Hayek is that he is ready to do precisely this, finding in Mandeville the germs of many of the central themes of Scottish social theory. As he puts it:

. . . His (Mandeville’s) main contention became simply that in the complex order of society the results of men’s actions were very different from what they had intended, and that the individuals, in pursuing their own ends, whether selfish or altruistic, produced useful results for others which they did not anticipate or perhaps even know; and, finally, that the whole order of society, and even all that we call culture, was the result of individual strivings which had no end in view, but which were channelled to serve such ends by institutions, practices and rules which had also never been invented but had grown up by the survival of what proved successful.

It was in the elaboration of this wider thesis that Mandeville for the first time developed all the classical paradigmata of the spontaneous growth of orderly social structures: of law and morals, of language, the market, and of money, and also the growth of technological knowledge. (Hayek 1978:253)

The problem generated for Hayek by his identification of Mandeville as the precursor of the Scottish school lies precisely in the contradictory relations between Hayek’s Burkean moral conservatism and Mandeville’s moral radicalism. This problem emerges explicitly when, in a recent book, Hayek (1976b) notes the emergence in modern cultures of “unviable moralities,” that is, moralities that condemn the institutions on which contemporary civil societies rest. Hayek is here demanding a revision of the moral inheritance of modern society – a moral inheritance replete with

elements he stigmatizes as tribal and atavistic.<sup>8</sup> In so doing, however, he relinquishes his Burkean moral traditionalism and adopts a critical rationalist stance in regard to customary morality of precisely the sort he has elsewhere condemned. Furthermore, in criticizing elements of inherited and contemporary morality for their inconsistency with the conditions of stability of civil society, Hayek effectively abandons the evolutionary ethics he has expressed ever more insistently in his recent writings. In abandoning evolutionary ethics, however, Hayek reveals the absence in his thought of any plausible moral theory – an absence that dissolves the unity of his system and leaves it without a compelling normative defense for the market order.

Hayek's relations with the Scottish school on questions of morality and society are, then, no less complex than those on questions of epistemology and the use of knowledge in society. His view of morality is less like that of the Scottish writers than he supposes and least like that of the Scottish philosopher he most admires, David Hume. This is not to say that Hayek's overriding concern with the moral foundations of a civil society is any less urgent than that of the Scots thinkers. He shares with them, and particularly with Smith and Ferguson, an anxiety that the actual evolution of commercial societies may throw up moral outlooks that are incompatible with the stability of the societies that have produced them. But it would be idle to pretend that there is in Hayek, any more than there is in Smith or Ferguson, a compelling moral response to what have later been called the cultural contradictions of capitalism.<sup>9</sup>

### **Hayek's relations with contemporary economic theory**

We have seen that Hayek's intellectual relations with the Scottish writers are complex, sometimes obscure, and not always as Hayek himself conceives them. Hayek's account of his affinities with, and debts to, the writers of the Scottish school, like his intellectual historiography as a whole, is idiosyncratic and partisan. It is not surprising, then, that the authentic and distinctive contributions Hayek has made to the intellectual history of economic thought have found little echo in the dominant voices in the recent history of ideas. The manifest neglect of Hayek's theoretical work by the larger profession of economists is harder to explain. Part of

<sup>8</sup> See Hayek (1976b) for an elaboration of this claim.

<sup>9</sup> I refer here, of course, to the writings of Daniel Bell.

the explanation, no doubt, is to be found in the accidents of Hayek's intellectual biography. His influence in the profession was probably at its height during his theoretical controversy with Keynes. Unlike Keynes, however, Hayek never embodied his economic thought in a general theory whose implications for public policy were made intelligible and explicit. For this reason, though Hayek's economic theory is eminently systematic in conception and intent, its systematic character was far from obvious. Furthermore, its policy implications appeared to many to be irrelevant or absurd during the two or three decades after the Second World War, when it seemed that Keynesian macroeconomic policies were being implemented, and implemented successfully. Part of the reason for the neglect of Hayek's work in mainstream postwar economics is the brute historical fact that for several decades he appeared to be on the losing side in intellectual terms. Finally, his intellectual efforts after the war were not, in fact, principally in economic theory in any easily recognizable form, but instead in social philosophy.

These incidents in Hayek's intellectual biography tell us something, but not much, about his neglect in postwar economics. A much deeper explanation lies in the elusive quality of Hayek's thought itself. If, as I have maintained, his relations with the Scottish writers are complicated and unclear, his relations with his Austrian forebears are no less difficult to specify. In methodology, Hayek never subscribed either to the Kantian a priori, apodictic-deductive method of von Mises or to the Aristotelian essentialism of Menger. His methodological position appears in fact to have owed much to Mach and to have anticipated in important respects that of Popper (though there are no less important differences between Hayek's and Popper's accounts of scientific methodology).<sup>10</sup> In particular, it remains very unclear what status Hayek ascribed to "economic laws." His opposition to the German historical school in economic theory is well known, but at times Hayek comes close to the view adumbrated explicitly by his sometime pupil, G. L. S. Shackle,<sup>11</sup> that economics has more in common with law and medicine than it does with physics or mathematics. At times, indeed, Hayek comes close to the view, stated in postmodernist idiom in this volume by Donald McCloskey (see Chapter 2), that the

<sup>10</sup> I have discussed the similarities and differences between Popper's views on scientific methodology and those of Hayek (Gray 1986:10-13, 110-15, 136-7).

<sup>11</sup> See Shackle's (1972:28-39) masterpiece, *Epistemics and Economics: A Critique of Economic Doctrines*, on self-subsistent or non-self-subsistent sciences.

economic way of thinking is but one way of talking about the social world – one that seeks to find intelligibility in certain kinds of patterns or *gestalten* that recur in our social experience.<sup>12</sup>

It is in the elusive originality of Hayek's methodological and theoretical outlook, then, that a large part of the explanation for his neglect by mainstream economics is plausibly to be found. It is to be found, most fundamentally, however, in certain specific features of Hayek's theoretical perspective, which deviate radically from the ruling paradigm in much conventional economic thinking. In his contributions to philosophical anthropology, Hayek has always theorized human beings as rule-following animals rather than utility maximizing creatures. His theoretical contributions have, for this reason among others, been resistant to formulation in the utilitarian terms of neoclassical economics. Hayek has never subscribed to the fiction of *Homo economicus* or its related fiction, the notion of a distinct mode of economic life. For Hayek, the market process is only social exchange in its most explicit and accessible form. At the same time, Hayek has always repudiated the imperialist claims of a paneconomism (such as Gary Becker's) that theorizes all human activity on a model of rational choice – a model that, for Hayek, only consecrates the Hobbesian (and Cartesian) myth of *Homo calculans*. The sphere of economic life is not, then, restricted to the market process; but neither is it all pervasive.

Hayek's marginality in contemporary economics may be accounted for, in significant part, accordingly, by reference to the elusive originality of his theoretical perspective, which assorts badly with the ruling idioms of theoretical discourse in postwar economics. It may well be, indeed, that Hayek's own methodological and philosophical perspective on economic theory failed to gain adherents or to exercise a compelling interest, not only by virtue of its subtlety, but because of unresolved problems in its foundations – problems that persuaded the few who have studied his thought that the systemic unity of his work is at bottom thoroughly compromised. Among economists, the most fundamentally important of those who have found incoherences in Hayek's system is Shackle, who has argued powerfully that Hayek's insistence on the limitation of human knowledge, and, most particularly, on the subjective character of our beliefs about the future, introduces a disequilibrating factor in economic life that Hayek's account of the market process does not sufficiently

<sup>12</sup> For Hayek's notion of pattern explanation and prediction, see Gray (1986:79–81).

acknowledge. At the level of theory and policy, this is to say that Hayek's account of the market process fails to respond adequately to the insights, only partially developed by Keynes, into the macroinstability of the market process under conditions in which subjective expectations produce large-scale crisis (see Shackle 1972:ch. 22). Stated in the most general terms, Shackle's critique of Hayek lies in the proposition that Hayekian conceptions of social knowledge and unknowledge, when taken to their natural limit, have implications that are not recognized by Hayek himself and are ultimately subversive of his system. It is in the likely incoherence of Hayek's system of ideas taken as a whole, then, as well as in the fact of its considerable subtlety and originality, that we find the most persuasive explanation of the weakness of his direct influence. It is in these facts, again, that we can find an explanation of the many oblique borrowings of Hayekian insights in recent economic literatures.

#### **Concluding remarks: Hayek, the Scottish school, and contemporary economics**

The outcome of the survey I have conducted of the affinities between Hayek and the thinkers of the Scottish school on questions to do with the use of knowledge in society and the role of morality in sustaining a civil order is that the points of divergence are at least as striking as the elements of clear affinity between them. The distinctive elements in Hayek's own intellectual formation to which I alluded at the start, and above all the inheritance of Austrian subjectivism, in any case render the result of my survey *prima facie* plausible. It seems that Hayek in his contributions to the history of ideas has found continuities where discontinuities are more easily demonstrable and, at least in respect of his view of social institutions as epistemic devices, may even have underrated his own originality. Whereas an account of this general sort is attempted by Burke of moral traditions, I can think of no one before Hayek who has represented the central institutions of the market itself as epistemic devices.

Distinctive and original as his own contributions to it are, Hayek shares with the Scottish thinkers a commitment to a discipline – political economy – and he has in common with them a definite conception of its subject matter. Though the term “political economy” is not itself commonly used by the Scottish writers, there is in all of them, and especially

in the writings of Smith, Ferguson, and Hume, the conception that it is the market process, and the conditions and characteristics of commercial society, that are the central subject matter of political economy. Both Hayek and the Scottish thinkers could not but view as retrograde the fragmentation of intellectual life that has occurred in the twentieth century, in which the subject matter of political economy has come to be viewed from a variety of rationally incommensurable disciplinary viewpoints – those of jurisprudence, sociology, moral psychology, political science, and economics, for example. Equally, both could not help but regret the fragmentation of economics itself into a host of specialist subdisciplines whose mutual relations and shared presuppositions were neglected and untheorized.

For all their many points of divergence, then, Hayek and the Scottish thinkers are at one in their commitment to political economy as a distinct discipline. They have in common a suspicion of conventional categories of understanding – a suspicion based on the belief that such disciplinary categories render elusive to us a social world that, insofar as it can be understood at all, must be understood in its totality. This holistic perspective shared by Hayek and the Scottish thinkers has been little evident in contemporary economics, with the interesting and significant exception of those who still work in a Marxian tradition. It is, perhaps, in carrying on the intellectual tradition that informs the present-day discipline of political economy, and injecting into it the distinctive Austrian themes of subjectivism and concern with the epistemic role of market institutions, that Hayek has made his chief contribution to intellectual life. Even if his own system of thought founders, his insights both contribute to the project of theorizing social life as a whole that the Scottish philosophers initiated, and intimate a holistic perspective on the conditions and character of the market process from which conventional economics may still have something of fundamental importance to learn.

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## Reuniting economics and philosophy

MICHAEL S. McPHERSON

This is an exciting time to be engaged in economics and philosophy, a time when a number of unusually able and serious people from the two disciplines are converging on common themes and issues and are learning from one another. This renewed activity follows upon several decades when economics and moral philosophy, once closely aligned, had grown apart; and indeed, partly as a result of this separation, both disciplines had come to back away from engagement in serious social problems.

I would not deny that such disengagement continues for many economists and philosophers, who seem to regard any hint of moral judgment in their work as a derogation of professional standards. But for those who *want* to do serious work on the borders of economics and social philosophy, the opportunities are there, in abundance.

I offer here some thoughts, rather heavily autobiographical, about how we have come to this happy state of affairs. These will provide the basis for a bit of reflection on where, after ten or fifteen years of renewed collaboration, we seem to have gotten and where we may be headed.

The current mutual interest between economists and philosophers is dramatically different from what it was even twenty years ago. A useful test of that difference is to ask what philosophical works most economists think they should know. (A more honest version might be: Which ones would they feel slightly embarrassed to admit their ignorance of publicly?) Twenty years ago, the list would, I think, have been limited to a couple of

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books in the philosophy of science – Popper, Kuhn, perhaps Hempel or Nagel. In social philosophy, one would be hard-pressed to name any book of the fifties or sixties that had even an arguable relevance to economics – in fact, one would be hard-pressed to name *any* memorable book in analytical social philosophy from that era. (In the midfifties, Isaiah Berlin [1955] could write an article asking, without irony, “Does Political Theory Still Exist?”)

Now, of course, the economist hardly exists who does not have an opinion about Rawls and Nozick (I am not making any claims about whether economists have actually read them), and many economists are at least vaguely aware that folks like Parfit, Dworkin, and Elster and a journal called *Philosophy and Public Affairs* are doing work that “bears on the subject.”

There has been a parallel shift in philosophical concerns. The idea that moral or political philosophy might have something to say about right and wrong or about good social arrangements was passé in the fifties and sixties, at least within the analytical tradition that dominated Anglo-American philosophy. Linguistic analysis had defined the philosopher’s role as that of tidying up the language of morals; philosophers had chosen to become, in Sidney Alexander’s (1967) memorable phrase, “janitors in the Mansions of Truth, leaving the premises otherwise untenanted.” There was no reason to suppose that reading an economics book would help much with those rather refined linguistic exercises.

The change on the philosophical side is, if anything, more dramatic than the change among economists. It is hard to see how one could hope to read Rawls’s (1971) *Theory of Justice* or Nozick (1974) or any number of other recent contributions to social and moral philosophy without a solid background in economic theory. Similarly, philosophers of science have come around more and more to the view that the road to understanding problems of knowledge acquisition or epistemology runs through the disciplines themselves: One cannot say anything very interesting about the character of economic knowledge without studying in some depth the way in which economists go about discovering things.

Obviously, I think that these changes, in both economics and philosophy, are all to the good, but I do not want here so much to praise or endorse these movements as to see if we can understand them a little better. Why has there been this fairly abrupt and substantial uptick in the

mutual interest of economists and philosophers? What are the products of that mutual interest? Where are we headed?

The only answers I can give to these questions are rather personal ones: the products of reflection on my own engagement with philosophy. I think the observations that follow generalize beyond my own situation, at least in some measure, although I am sure another person would tell a somewhat different story. One limitation in particular should be mentioned: I shall focus on relations between welfare economics and social philosophy, which is where my own interests have centered. There is an equally important story about the relations between “positive” economics and philosophy of science that I am not equipped to tell.

We can think of the recent tendency for some economists to journey toward a serious interest in philosophy – and for some philosophers likewise to take economics seriously – as a kind of intellectual migration. And like most migrations, this one seems to have roots in both “push” and “pull” factors, in *dissatisfaction* with the existing environment and in *hope* that new surroundings will prove more appealing or satisfying.

In the case of someone like me, coming to the study of economics in the mid-1960s, the dissatisfaction side of the equation is easy enough to understand. If one views economics as basically a way of advancing the understanding of important social problems and, one hopes, of helping to change society for the better, the neoclassical synthesis that prevailed in the 1960s could seem to be pretty thin gruel. Especially at a time of considerable social upheaval, the “official view,” embodied in neoclassical welfare economics, of what economists (*qua* “professional economists”) were permitted to talk about was notably abstemious. They were not supposed to have much to say about the distribution of income and wealth or about questions concerning the social formation of preferences or about the role of power in shaping social and economic outcomes. This abstemiousness, this unwillingness to talk about things that, arguably, one does not know much about, has, of course, its good side. But clearly, and again in a time of general bad conscience about the role social science was playing in our society, such reticence could and did generate a considerable sense of unease. And, indeed, that unease was being expressed in rather prominent forums, such as Kenneth Boulding’s 1968 Presidential Address to the American Economics Association (Boulding 1969). In that talk, Boulding coined the phrase “the immaculate conception of the

indifference curve” to call attention to economists’ trained incapacity to think about some of the broader social aspects of their discipline.

That set of factors helps explain the push. The pull toward philosophy as a potential solution to these troubles is, perhaps, a little harder to account for. Economists could just as well have looked toward psychology or sociology or history in attempting to broaden and deepen their understanding. And indeed some have. But the movement toward social and moral philosophy has been a prominent one, and it is an interesting question why at least some young economists were pulled that way.

It seems to me that the publication of John Rawls’s *Theory of Justice* played a crucial role in that regard, for reasons that are partly obvious and partly not so obvious. For those who were looking for a way out of the narrow confines of neoclassical economic theory, Rawls’s book was a considerable source of hope. I remember reading Marshall Cohen’s (1972) review of the book in the *New York Times Book Review*. Cohen described *A Theory of Justice* as the most significant work in moral philosophy since Kant. At the time, I had only the vaguest notion of what the significance of Kant’s moral philosophy might be, but I knew enough to know that that was high praise indeed – a hopeful omen. And I remember with some vividness reading that book on warm summer nights in 1972 when I was bored with working on my dissertation but too driven by guilt about not working on it to do something sensible, like read a mystery novel.

What I especially remember was turning over to page thirty-seven of Rawls’s book and seeing there an indifference curve–budget constraint diagram. Indifference curves in a philosophy book! To noneconomists, this may not seem like much. But to a budding economist, seeing this apparatus put to use is rather like seeing a fellow in a poolroom pull out his own cue. The message is: This guy is serious. What the use of indifference curves announced, and what became progressively clearer as one read further into Rawls’s argument, was that here was a thinker who, though not in any obvious way bound by the usual strictures and limits of the “economist qua economist,” had clearly learned an enormous amount from economics and was not afraid – indeed, was rather proud – to show it.

This, from the point of view of someone in my position (and looking back with the wisdom of hindsight), had several advantages. First it meant that one could, by mastering Rawls (in the first blush of discovery, one might have thought that “mastering Rawls” was possible), step beyond the

narrow boundaries of orthodox economics without abandoning the tools and skills one had already acquired. The attraction here was partly the crass one of valuing the capital sunk in learning that stuff, but I think it was more than that: I, and I suspect many others who entertained doubts about the limits of received analysis, also had considerable respect for its beauty and power and for the insights it conveyed. Our hope was to enrich and not to abandon it. Rawls's book also confirmed some strong intuitively held economists' value judgments – about the virtues of markets and competition, and the attractions of procedural notions of justice.

Second, Rawls did not make a fool of himself. Too much criticism of the “narrowness” or “formalism” of modern economics is know-nothing. Critics embarrass themselves, and harm their cause by displaying ignorance and misunderstanding. Not so Rawls. Kenneth Arrow (1973:245) commented on Rawls's acumen in his review essay in the *Journal of Philosophy*. “As an economist accustomed to much elementary misunderstanding of the nature of an economy on the part of philosophers and social scientists, I must express my gratitude for the sophistication and knowledge which Rawls displays.” As an economist who has at times ventured beyond his domain, I am aware of how easy it is to make embarrassing mistakes – and, more embarrassingly still, not to know one is making them. Rawls avoided this trap. Given the wealth of Rawls's learning and the intricacy of his argument, one did not risk accusations of being softheaded or an intellectual lightweight in taking his work seriously – and those are accusations of no small moment to a young economist.

Finally, Rawls's work had a powerful technical side. Rawls, for example, argued (to put it crudely) that economic policy should replace the aim of maximizing aggregate social welfare with the aim of maximizing the lowest incomes in society. Exploring the implications of that proposal could generate (and has generated) plenty of work for tax theorists. More abstractly, Rawls proposed a new method for determining principles of justice. He proposed that one could characterize impartial rules as those that would be chosen by rational, self-interested actors from behind a “veil of ignorance” – deprived, that is, of knowledge of features like their wealth, sex, and race that might bias their choices.<sup>1</sup> This bid to

<sup>1</sup> Rawls, as he himself acknowledged, was preceded by some economists in characterizing impartiality in this way. See Harsanyi (1953) and Vickrey (1960).

transpose the debate about justice into a problem in rational choice tends to move the subject squarely into economists' territory.

At the same time, though, that Rawls reassured the dissatisfied young economist that embracing Rawlsian social philosophy would not put him out of work, it was also evident that there was a great deal more to Rawls than a few new twists on the good old social-welfare function. Rawls's work provided a window for economists and other social scientists on the most interesting work being done in modern social philosophy. In part, these were innovations introduced by Rawls himself during the twenty years of work that preceded his magnum opus. His presentations of metaphors and concepts like "reflective equilibrium," "pure procedural justice," "the veil of ignorance," and the "original position" were enormously suggestive of productive ways of organizing thought about society (a good illustration, in fact, of Donald McCloskey's stress on the role of metaphors in shaping thought). Just as important, Rawls's book functioned for me – and no doubt for some of my contemporaries – as an introductory social philosophy text, albeit an unusually complex one. The message was that there really are some people over there in the philosophy department who actually suppose that you can *think* about questions of right and wrong, good and bad. The phrase "That's a value judgment," we saw, is not in all discussion circles, as it has tended to be among economists, a conversation stopper.

It was not that Rawls provided decisive *arguments* against the views that had hamstrung normative discussions in economics – views, for example, that utilities are noncomparable or that "ought" statements are independent of "is" statements or that distributive judgments, unlike efficiency judgments, are purely a matter of taste. Rawls's effect on disputes about the possibility of fruitful normative discussion of economic issues was rather like the Wright brothers' effect on disputes over the possibility of air transport: He got in and flew the goddamn plane.

Moreover, the effort to think systematically about social justice, as embodied in Rawls, turned out to be absorbing and analytically challenging. In fact, it turned out to draw on many of the intellectual capacities economists pride themselves on, including a flair for abstraction and a determination to think dispassionately about issues that carry a high emotional charge.

For all these reasons, Rawls provided a bridge across which economists could walk into the broader realm of moral and social philosophy. If one

took the walk, of course, one discovered there was lots there besides. There was, first, Robert Nozick's (1974) powerful anti-welfare-state book, *Anarchy, State, and Utopia*, demanding to be read almost before one could limp through Rawls's last long chapter on the congruence of the right and the good. But there was other good analytical social philosophy too, much predating the publication of *A Theory of Justice*, Brian Barry's (1965) *Political Argument*, for example, and some good work on utilitarianism. And Rawls's discussions of the historical background of social contract theory pointed back to earlier writing: Kant and Rousseau, Mill and Sidgwick – even Hegel, for the brave.

Many economists, I think, now see and understand the relevance of these ranges of inquiry to their business, certainly many more than did two decades ago, although of course the time and energy that most devote to these matters is quite limited. And I think it is fair to greatly credit Rawls (as well as Sen, Arrow, and some other adventurous economists) for helping them to make that connection.

It is just as important to say, however, that the bridge Rawls built from economics to philosophy carries traffic in both directions, for economic theory is, as I said before, deeply integral to Rawls's book, and the message sent thereby to philosophers must have been, I believe, at least as striking as the one many economists heard. Rawls simply cannot be read intelligently without a very considerable understanding of economic theory. I have already noted Rawls's reliance on the theory of rational choice under uncertainty. It is further true that the institutional structure of Rawls's "just political economy" is an artful amalgam of the four-branch analysis of governmental functions articulated in Richard Musgrave's (1959) *Theory of Public Finance* with the analysis of property institutions in James Meade's (1964) *Efficiency, Equality, and the Ownership of Property*, two classic economic texts. More broadly, it is apparent that Rawls's whole conception of the notion of just social institutions has been heavily influenced by the economist's conception of the workings of an ideal market system.

The reading of Rawls – and of Nozick, too, for that matter – says to philosophers that if they want to think about economic justice, or about desirable social institutions more broadly, they had better learn some economics. And just as many economists felt pushed and pulled toward philosophy, so many philosophers must have felt the pressure the other way. The ordinary language philosophy of the fifties and sixties, like the

neoclassical economic synthesis, produced dissatisfactions of its own. The social upheavals of the sixties must have made it seem peculiarly inadequate to confine one's professional discussion of morals to analysis of the meanings of moral terms. Such dissatisfaction provided a push away from the strictures of logical positivism and linguistic analysis, but philosophers have also been pulled, I think, by the hope that the methods and substance of economics can help make social philosophy better.

This hope I am attributing to philosophers has at least two dimensions, one pertaining more to the methods of modern economics, the other to its substance. Each is clearly present in Rawls's work. Economics provides a reasonably clear, even if in some important respects impoverished, conception of rationality, and it says a great many interesting things about the relations between individual and collective rationality. It is this relatively precise method of rational choice analysis that Rawls borrowed for his central "veil of ignorance" argument and that turns up in discussions of the free-rider problem and the prisoners' dilemma elsewhere in his book. Philosophers already had a strong interest in formal theories of rational decision; what Rawls did was to help them see the connection between these theories and moral philosophy. The practice of using such simple rational choice models, especially in game theoretic form, to illuminate problems in social philosophy has since become quite widespread. (I should note that the bridge here from philosophy to economics was perhaps constructed as much from the writings of Thomas Schelling and of theorists of social choice as from Rawls's work.)

I said that Rawls's work suggests that the substance of economics, as well as its methods, matters for social philosophy. Rawls's approach supposes that we cannot deliberate intelligently about what principles of justice to adopt without developing views about what consequences will follow from attempting to embody those principles in social institutions. The relevant institutions and consequences include, though they are not limited to, economic ones. Rawls, in fact, devotes about a third of his long book to sketching a plausible institutional embodiment of his principles, the aim being to see if that sketch can be reconciled with our intuitive ideas about just institutions and how they function.<sup>2</sup>

<sup>2</sup> It may be worth noting that Nozick makes his defense of his "entitlement" principles of justice largely independent of the social consequences of embodying those principles in institutions. This seems to me methodologically a step backward from Rawls.

This effort illustrates Rawls's "reflective equilibrium" approach to social philosophizing – the notion of a back-and-forth movement between principles and their consequences, seeking a fit between our principles and our considered judgments on specifics. That approach makes the links between empirically grounded social science, and economics in particular, and moral and social philosophy very tight. As long as philosophers remained content with metaethical discussion, that is, with discussion about the nature, rather than the content, of ethical discourse, they could hope to keep themselves insulated from inquiry into facts about how the social world works. But once attracted by the prospect of actually *saying* something about what principles and institutions are worth seeking, they were forced to make empirical commitments.

And Rawls again was a major influence in pulling philosophers in this direction. The parallel with the influence on economists is worth underlining: Just as Rawls helped to legitimize serious normative discussion among economists, he helped legitimize serious interest in empirical social science among philosophers. (The outpouring of work on medical ethics [Daniels 1983 is the best example] is one illustration.) And in both cases he did it, not so much by persuasive *argument* that doing it was possible in principle, but by the much more persuasive *evidence* of doing it in fact.

I have suggested that the mutual movement of (some) economists toward philosophy and of (some) social philosophers toward economics has been grounded in a mixture of dissatisfaction and hope. But human affairs being what they are, hope almost inevitably leads to disappointment, a point Albert Hirschman (1982) has reminded us of in his book on swings in political sentiment. The movements I am discussing here have not entirely escaped that fate.

I am sure, if I may return to autobiography, that the degree of hope I myself placed in Rawls's theory as a kind of all-purpose framework for settling social and economic issues was exaggerated. No doubt, this was partly a result of my inexperience with philosophy. I had not yet learned that philosophers' ability to construct apparently seamless and final arguments is exceeded only by their ability to blow such arguments to pieces. But I suspect that there are also some special features of Rawls's work – or, in fairness to Rawls, features of my *reactions* to Rawls – that helped produce this exaggerated reaction.

One is the power of the claims that Rawls makes, not so much on behalf of his own theory as on behalf of what good social theory might do. Rawls speaks in more than one place of a sound theory of justice as providing an “Archimedean point,” suggesting the notion of an objective place to stand, outside the world, from which the world might be judged and indeed, to pursue the analogy, might be moved. Surely that is an ambition worth getting excited about, especially relative to an economic tradition that accepted so much about the world (inequality, acquisitiveness, private property) as given. Rawls’s theory promised to provide an objective, detached, in a certain sense neutral standpoint from which to judge social institutions. And yet his book was not, as one might expect from something written from such an Olympian standpoint, terribly removed from concrete social issues and institutions: Rawls’s principles seemed to carry quite tangible implications for welfare and tax policy, for education, and so on.

A second consideration that was important in bolstering my hope was that economists’ critiques of Rawls were often incredibly crude, based on simple misreadings or highly partial readings. They sometimes attacked views Rawls never held – for example, that Rawlsian justice required maximizing the minimum *utility* (rather than the minimum of a certain bundle of goods). More generally, critics often ignored key features of his view – not seeing, for instance, that the proposal to maximize the minimum income share actually plays a subordinate role in his structure of principles. It was tempting to believe that if Rawls’s critics had it wrong, then we (i.e., Rawls and those of us clever enough to understand him) must have it right.

Finally, being clever enough to understand him was important, too. It is rare to find a book that fills up such a large space with *arguments*, and ones that are so intricately balanced and cross-referenced. It took me something like three years of intensive, albeit intermittent, study to achieve a reasonably good grasp of Rawls. After that sort of commitment, one has an understandable desire to defend one’s investment by believing the theory. Moreover, Rawls’s argument is so rich and intricate that there is available to any move against it some countermove; if one has not found a good reply to some objection, it is easy to suppose that somewhere, in the vast stretches of that book, an answer is to be found. Moreover, there is the fact I noted earlier that I and probably many other social scientists learned what social philosophy we know from *A Theory of Justice*;

objections and alternatives were seen through Rawls's eyes. And, naturally, through those eyes, Rawls's view of the world looks pretty good.

No doubt my own case of attachment to Rawls has been somewhat extreme – I am one of the few people I know who actually likes his *prose* – but I think the effect on a number of other economists and social scientists was similar in kind, if not in degree. Certainly in fields like legal theory, moral psychology, and international relations there are figures who could be appropriately labeled disciples of Rawls. The galvanizing effect that Rawls has had on substantive moral philosophy suggests a similar process at work among philosophers. Indeed, the surprising energy, sometimes venom, with which Rawls is routinely attacked in philosophical circles is backhanded testimony to his influence. I suspect that some of this reaction (which, more in talk than in print, is sometimes fairly strident) is the product of disappointment of exaggerated hopes.

The *character* of that disappointment is illuminating. What Rawls provided in his book was a rather thoroughly worked out portrait of an ideally just society. He presented that conception of a just society in a way that detached it from the existing preferences and practices of any particular society – in principle, applicable as well to ancient China or medieval Europe as to the contemporary United States. In that sense, the theory was universalistic in its ambitions. The theory was also essentially complete and determinate in its description of just institutions: not many major decisions about the character of just social institutions were left open by the theory. One had the sense that the book was trying to present a blueprint of *the* just society. I have already said that these rather grand ambitions of the theory were part of its initial appeal, but in retrospect they seem problematic.

It is not just that Rawls's own theory is not convincing on all points – perhaps, for example, not winning every argument with Rawls's favorite opponents, the utilitarians. Rather, it is a question of whether *any* theory that tries to deliver so much can plausibly be what we are after, especially given our vast ignorance about moral matters. In this respect Rawls's theory is troubling in rather the same way utilitarianism itself is. As Sen and Williams (1982:1) put it in the introduction to their important volume *Utilitarianism and Beyond*, such theories may represent “an attempt to do too much, to give too comprehensive and extensive an answer to problems of personal or public choice. . . . It is not simply utilitarianism that is at fault, but any theory which displays that degree of ambition.”

Economists have perhaps special reason to be suspicious of their attraction to the universalistic and univocal quality of Rawls's work. Orthodox economics is itself strongly drawn toward unhistorical, universal theorizing, and many economists – I do not except myself – are temperamentally disposed that way. In that sense, the path from welfare economics to Rawlsian justice may have been *too* easy to travel.

Rawls, I should say, probably never took the universality and finality of his theory as seriously as the language of his text sometimes implies. His real claim (made clearer in more recent writing [Rawls 1985]) is that theorizing in this ambitious mode may clarify our ideas and push our thought on real social questions farther than it would otherwise get. That claim has certainly been amply justified by the developments his writing helped set in train.

In fact, the exaggerated claims of Rawls's theory as written were probably of real help to those of us who used it as a bridge for getting beyond neoclassical welfare economics into the broader fields of social philosophy. Albert Hirschman (1962) and John Sawyer (1952) have made familiar the point that people would probably never try anything ambitious or risky if they fully grasped the complications that lay ahead. I think that some of us who plunged ahead into social philosophy in the naïve hope that it could *settle* for us the deep questions normative economics raised without answering, are now in the process of coming to grips with those complications.

The result has been something of a movement in recent work away from the spectacularly large theories of writers like Rawls and Nozick toward more focused and deliberately partial inquiries. Some of this work is technical. One good example is the effort by Sen and others to develop the notion of “metapreference” as a vehicle for describing more adequately some of the complexities of personal and moral choice (Sen 1982: chs. 3 and 4). There is also well-known work that aims to give an adequate formal characterization of liberty (Sen 1982; Pressler 1987), Hal Varian's work on formal notions of justice based on symmetry (Thomson and Varian 1984), John Roemer's work on exploitation (1982a, b), and much else.

Equally important are more informal attempts by writers like Thomas Scanlon (1982), Thomas Nagel (1986), Michael Walzer (1983), and Bernard Williams (1985) to give more limited and partial accounts of moral matters, ones that provide a proper space for social variation and

disagreement and that recognize that there may be wide spaces of moral controversy that simply cannot be settled by theory.

Michael Walzer's (1983) book, *Spheres of Justice*, illustrates these points especially well. Walzer argues that deliberation about the just distribution of any good must always be relative to the meaning attached to that good in the life and discourse of a particular place and time. It is of interest that Walzer in his preface specifically contrasts his approach to that of Rawls, noting that Rawls was heavily influenced by psychology and economics, whereas his own work draws instead on history and anthropology.

What seems quite clear is that economists like me, who moved toward social philosophy with exaggerated hopes of finding satisfyingly firm answers to vexing questions about social conduct, were bound to encounter some disappointment. The problems are just too hard, too many-sided – and too human – to yield to any simple global approach. I think the parallel statement applies to philosophers who have looked to economic theory to help give substance to their work in social philosophy. Neither rational choice methods nor the substantive findings of economics provide as quick a resolution as some have hoped of philosophical uncertainties about justice.

But it is just as clear that this disappointment will not push us, neither economists nor philosophers, back to where we were before. A decade's intensive work on this borderline between welfare economics and political philosophy has, I think, provided persuasive evidence that we have come upon a fruitful and enlivening field of inquiry. The old formulas of Paretian welfare economics and positivist metaethics look increasingly inadequate.

So far, at least, a good deal of what we have gained is a better understanding of the scope and character of our ignorance. The neoclassical tradition has rather surprisingly firm things to say regarding what economists might and might not know something about. Concerning the character of rational choice from given preferences, we might, that tradition says, in principle know everything; about the social origins of those preferences, nothing. About the efficiency consequences of policy choices, everything; about their fairness or distributive merits, nothing. And so on. The traditions of linguistic analysis and logical positivism in philosophy were similarly firm. About the logical implications entailed in the voicing of moral commitments, we might know everything; about the merits of such commitments, nothing. Philosophical activity pertaining to

moral matters could, that tradition said, be totally disconnected from contingent empirical knowledge about the consequences of embedding different moral practices or institutions in society.

This way of drawing the map of our ignorance – and hence of our actual and potential knowledge – is remarkably crude. We probably have just as firm reasons for believing that certain extreme distributions of resources are unjust as we have for believing that some allocations of resources are inefficient. And certainly there are many situations in which our judgments about efficiency, as well as distributive justice, are extremely uncertain. Or, again, upon the kind of close examination Amartya Sen (1982:ch. 4) has given it, our seemingly firm notion of the “rational consumer” proves much slipperier than we thought; on the other hand, there are important aspects of the process of preference formation about which economists, despite the conventional wisdom, can have something to say (McPherson, 1987).

Thomas Pynchon (1984:15), in a remarkable introduction to a recently published collection of his early stories, has suggested that coming to a greater understanding of one’s ignorance is a mark of growing maturity.

At the earlier stages of life we think we know everything – or to put it more usefully, we are often unaware of the scope and structure of our ignorance. Ignorance is not just a blank space on a person’s mental map. It has contours and coherence, and for all I know rules of operation as well.

I suggest that we might extend this metaphor from the maturing of a person to the maturing of a discipline – or a pair of disciplines, in this case. To learn something about the complexity of what it is one does not know is an exercise in humility. The mutual engagement of philosophers and economists during the seventies and eighties should have produced an outpouring of modesty on both sides (although academics being what they are, we have perhaps had a trickle rather than a flood). At the same time, fashioning better maps of our ignorance is an important step toward knowledge (as Socrates was perhaps the first to notice explicitly). We are learning, I think, to ask more constructive questions, questions that conform to a greater extent to the complexities of the moral and social realities we have started to glimpse more adequately. As we figure out how to ask questions to which we have a good chance of finding at least tentative answers, we have good reason to think our efforts will pay off. The product of those efforts, if they are made, will no doubt be a further succession of hopes and disappointments. That is all we could ask.

### Postscript

When I set out to prepare this essay, I had no idea that Rawls would play such a central role. I think that centrality is in fact deserved, both in terms of my own experience and in terms of the wider influence of Rawls's monumental work. Still, that emphasis calls for one further remark.

This is the fairly obvious point that Rawls has not provided the only bridge in recent years between economics and wider issues of social philosophy. For some, it is clear that Marx and neo-Marxism have provided that function; for others it has been social choice theory as pioneered by Arrow and Sen; for still others, of the "Austrian" persuasion, Hayek and von Mises may have been the path. But I would like to offer the hypothesis that along any of these paths, the trajectory has been essentially like that described in this chapter. That is, one is initially pulled by a theory with fairly universalistic and decisive claims and ambitions and then, once involved, discovers unforeseen complications. This discovery, then, forces one to come to grips with complexities and limitations and with what Pynchon calls the "scope and structure of our ignorance." Although I do not want to minimize the differences in view among people who have moved from the various starting points I just named, there do seem to be an unusual number of serious social thinkers who have arrived at a point where they share both a passion for rigorous argument and a willingness to acknowledge the many-sidedness of social phenomena, and hence the inevitable partiality of all rigorous argument.

These reflections have provided at best a description of the evolution of this state of affairs, and not really much of an *explanation* of why we should have arrived at such a point at this historical moment. But since this point that we have somehow arrived at provides such an excellent standpoint from which to approach social theory, perhaps it is time that I stopped looking this particular gift horse in the mouth.

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## **Economic methodology and philosophy of science**

**DANIEL M. HAUSMAN**

Most methodological writing on economics is undertaken by economists. Although the bulk is produced by lesser-known members of the profession, almost all leading economists have at one time or another tried their hand at methodological reflection. Almost everybody agrees that the results are usually poor. If one read only their methodology, one would have a hard time understanding how Milton Friedman or Paul Samuelson could possibly have won Nobel Prizes. It is less surprising that the economics profession professes such scorn for philosophizing than that its members spend so much of their time doing it.

In this chapter I am concerned with three related puzzles concerning work on economic methodology. In addition to saying something about the reasons for its current mediocrity, I address its peculiar relations to philosophy of science and the strange fact that the currently dominant views in economic methodology are drastically inconsistent with the practice of economists. The solutions to these puzzles are related, and the villains responsible for them are philosophers.

To clarify the strange relations between writing on economic methodology and work in philosophy of science and to articulate and explain the inconsistency between methodological dictum and practice, I shall offer a sketch of the history of reflection on economic methodology with a special

This chapter is derived from Hausman (1986), and I thank the Philosophy of Science Association for permission to reprint parts of that essay. A discussion with Larry Laudan and Gary Downing was instrumental in making me realize the point I try to argue at the end of this chapter. Comments and suggestions from Neil de Marchi, Clark Glymour, Frederick Lee, Michael McPherson, Paul Thagard, Gordon Winston, and the audience at the 1984 Philosophy of Science Association meetings were also extremely helpful.

emphasis on the recent history. We shall see that, although the literature concerning economic methodology is heavily influenced by philosophy – both current and, especially, outdated – it is cut off from philosophical discourse. This quasiautonomy is puzzling, since this literature is concerned with questions that appear to be instances of, if not the same as, the questions with which philosophers of science are concerned. Writers on economic methodology are, for example, concerned with theory appraisal and construction, with the nature of theories, models, and explanations, with the goals of science, and with the problems of confirmation or statistical inference. Yet although economic methodologists read philosophy of science, they do not contribute to it. This separation is currently decreasing, and with this reunion should come an improvement in work in economic methodology and a resolution of the contradiction between methodological preaching and performing. But as I shall argue at the end, economic methodology has different aims than does philosophy of economics, and some separation will remain.

If philosophers of science and economists concerned with methodology are really asking the same questions, why has there been so little effective dialogue? Indeed, one has here an instance of a more general question. In other fields that, like economics, are insecure about their methodology (and thus *not* in such disciplines as physics and chemistry), one finds continuing discussions that look like philosophy but are not quite philosophy after all. These discussions are concerned with philosophical problems, and the participants, who are themselves only rarely trained as philosophers, look outside to philosophers for solutions. At large universities, literally dozens of courses concerning such semiphilosophy are offered in fields ranging from criminology and nursing to recreation and family and community development.<sup>1</sup> Once one looks, one can find philosophy of a sort scattered all over the academy.

Why is economic methodology a separate subdiscipline? Is it the same inquiry as philosophy of economics under another name? What are the prospects for bridging the disciplinary boundaries and for making the ineffective *de facto* collaboration between economists and philosophers more vital, self-conscious, and effective? Is the isolation of methodologists from philosophers responsible for the general mediocrity of method-

<sup>1</sup> For example, at the University of Maryland, where I used to teach: CRIM 610, Research Methods in Criminal Justice and Criminology; Recreation 432, Philosophy of Recreation; FMCD 610, Research Methods for Family and Community Development.

ological writing? To what extent is it responsible for the inability of economic methodologists to recognize and rationalize the practice of economics – even when, as in the case of major economists such as Friedman or Samuelson, it is their own practice they are commenting on? To answer these questions, let us turn to the history of reflection on economic methodology.<sup>2</sup>

### **The traditional mainstream**

John Stuart Mill's (1836) essay, "On the Definition of Political Economy: and on the Method of Investigation Proper to It," is one of the earliest discussions of the methodology of economics, and it is still one of the best (see also Mill 1843:bk. 6). From the perspective of so staunch an empiricist as Mill, economics is a puzzling science. Its conclusions, for which Mill has immense respect, 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, such as "People seek more wealth," which are firmly established by introspection, or technical claims, like the law of diminishing returns, which are confirmed directly by experimentation. Mill believes that these established premises (although not universal laws) state accurately how specific causal factors operate. They are inexact laws or statements of tendencies. Economics is devoted to exploring the consequences of these established premises in diverse circumstances. If the only causal factors that affect economics were those that economists consider, the conclusions of economics would be secure, because they would follow deductively from its well-supported premises.

But as Mill points out, the conclusions economists draw must in reality be treated cautiously, because so much is left out of the theory. One must be ready to make allowances for various disturbances, and one must recognize that the predictions economists make may be badly mistaken even though their theory is fundamentally correct. Economics is thus only "hypothetical." It is a science of "tendencies," which may be over-

<sup>2</sup> The account of the history of discussions of economic methodology follows the introduction to Hausman (1984:38-42).

whelmed by disturbances or interferences. Application is a risky business that requires good judgment and broad experience. Despite these empirical shortcomings, one can continue to have a high regard for the science of economics, because its premises are well supported and because, once one abstracts from all interferences or disturbances, its conclusions follow deductively from its premises. Ricardo's theory of rent was thus for Mill not tarnished by the failure of *all* its apparent predictions (de Marchi 1970). Various temporary "disturbances" left out of the theory (especially technological improvements) had kept rents from rising and profits from falling as Ricardo predicted.

In a nutshell, Mill's view is that economics is a science, for economists know the basic causes. But it is an inexact science, for there are myriad disturbing causes. The confidence of economists in this science is based on the direct confirmation of its premises, not on econometric tests of their implications; and this confidence is not shaken by the disconfirmation of those implications, for economists know full well that many significant causal factors have been left out of their models. Update the language and the economic theory and you have got the view to which most orthodox economists, regardless of what they may say in methodological discussion, still subscribe.

Mill's view was influential throughout the nineteenth and early twentieth centuries and, despite some problems, deservedly so. Not only does it constitute the heart of J. E. Cairnes's (1888) *The Character and Logical Method of Political Economy*, but it is still the core of John Neville Keynes's authoritative summing up in 1890 in his *Scope and Method of Political Economy*. Keynes, who was an acquaintance of Marshall's, wrote in a very different intellectual environment than did Mill. Not only had economic theory changed considerably through the so-called neoclassical revolution that began in the 1870s, but a major methodological challenge to abstract theorizing in economics had been posed by members of the German historical school. Economists such as Roscher (1874), Knies (1853), and Schmoller (1888, 1897) had contended that economics must focus on the details of particular institutions and that it should exchange its pretenses of pure abstract science for concrete investigations with immediate normative consequences (see also Weber 1975). Neville Keynes grants to the historical school that the *application* of economic theory requires the sort of detailed institutional and historical knowledge that

they emphasized. But in this concession Keynes scarcely disagrees with Mill. And in any case, Keynes agrees with Mill completely concerning the nature of *theorizing* in economics.

The transition from classical to neoclassical economics brought both substantive changes in economic doctrine and changes in methodology. In its focus on individual decision making, neoclassical theory, particularly in its Austrian or Walrasian variations, is a more individualistic and subjective theory than was its classical predecessor, and the recognition and appreciation of this fact are the most significant contributions of early twentieth-century methodological writing. The major authors are Ludwig von Mises (1949, 1978, 1981), Frank Knight (1935, 1940), and Lionel Robbins (1935). Von Mises and the so-called Austrian economists laid particular emphasis on the individualism and subjectivism of economic theory. Frank Knight's distinctive methodological contribution was his stress on the importance of *uncertainty* and *error* in economics, which led him to agree with the Austrians that one loses sight of the central problems and concerns of economics as soon as one abandons the subjective point of view and attempts to think of economics as if it were a natural science.

Lionel Robbins wrote the classic defense of the individualist and subjectivist perspective in his *Essay on the Nature and Significance of Economic Science* (1935). His *Essay* is best known for its argument that interpersonal utility comparisons require value judgments and for the definition of economics that it presents. Robbins (1935:16) asserts that "economics is the science which studies human behaviour as a relationship between ends and scarce means which have alternative uses." According to this definition, economics is not especially concerned with any particular classes of social phenomena (e.g., the production, distribution, exchange, and consumption of goods and services). It is concerned instead with a particular *aspect* of human behavior. Abraham's decision to sacrifice Isaac or Anna Karenina's decision to elope with Vronsky are by this definition clearly part of the subject matter of economics (see Blinder 1974; Fair 1978; Becker 1981). Robbins is, in effect, attempting to *define* economics as the theory of constrained rational choice, which is what the modern neo-Walrasian version of neoclassical theory comes down to (Hausman 1981a:195–8). Such redefinitions are, as many philosophers of science have noted, characteristic of scientific development (Kuhn 1970:chs. 9, 10; Stegmüller 1976:93, 176–7). But although Robbins's definition has appealed to a great many economists, it has never com-

pletely captured the profession, for it rules out of economics theories, such as John Maynard Keynes's, which are of great practical importance. And even when they wanted to, economists have never been able to escape completely the practical demands that governments, businesses, and unions make of them.

Robbins, Knight, and the Austrians all stress the individualism and subjectivity of (neoclassical) economics, and they all emphasize the peculiarities of human action as an object of scientific investigation. But they still 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. Robbins (1935:121) explicitly notes the ancient lineage of the view he defends, and by exaggerating the weakness and obviousness of the postulates of economics, he provides one of the most persuasive formulations of the position:

The propositions of economic theory, like all scientific theory, are obviously deductions from a series of postulates. And the chief of these postulates are all assumptions involving in some way simple and indisputable facts of experience relating to the way in which the scarcity of goods which is the subject-matter of our science actually shows itself in the world of reality. The main postulate of the theory of value is the fact that individuals can arrange their preferences in an order, and in fact do so. The main postulate of the theory of production is the fact that there are more than one factor of production. The main postulate of the theory of dynamics is the fact that we are not certain regarding future scarcities. These are not postulates the existence of whose counterpart in reality admits of extensive dispute once their nature is fully realised. We do not need controlled experiments to establish their validity: they are so much the stuff of our everyday experience that they have only to be stated to be recognised as obvious. (Robbins 1935:78-9)

Only the Austrians go farther with their insistence that the basic premises are a priori truths (von Mises 1949:34, 1981).

### Logical positivism

The first and, in my view, only major change in economists' official position on the justification of economic theory came with the intrusion of the views of the logical positivists. In 1938 Terence Hutchison published *The Significance and Basic Postulates of Economic Theory*. In this important book, Hutchison offered a sweeping methodological indictment of what he called "pure theory" in economics. Drawing on the emerging logical positivist consensus concerning the nature of science, Hutchison maintained that statements of "pure theory" in economics are empty

definitional or logical truths. Claims in economics are so hedged with qualifications and *ceteris paribus* (other things being equal) clauses that they are untestable and uninformative. With the weight of contemporary logical positivism behind him, Hutchison insisted that it was time for economists to start behaving like responsible empirical scientists.

Hutchison was mounting a fundamental attack on the Millian tradition. As Mill recognized, neither the basic premises of economics nor their implications are universal laws. Instead, they are statements of tendencies. They say what would happen in the absence of disturbances. In more formal or linguistic terms, they are qualified claims of the form "In the absence of disturbances or interferences (or *ceteris paribus*) \_\_\_\_\_," where the statement of tendency fills in the blank (Hausman 1981b). But from the perspective of the logical positivists or from Karl Popper's perspective, such statements are apparently illegitimate in science. Because the content of the *ceteris paribus* clause is unspecified, these statements have no definite empirical meaning, and they appear to be unfalsifiable. Either things are as claimed by the tendency, or there is some disturbance. No outcomes are prohibited. Furthermore, given the positivist and Popperian emphasis on intersubjective testing, the subjectivism of economics was at the very least worrying.

As Hutchison (1938:ch. 2) himself partly recognized, this critique can be answered from within the Millian tradition, for one need not regard or employ *ceteris paribus* clauses as blanket excuses. I have elsewhere explored at length not only the truth conditions for such qualified claims, but also the conditions under which one may be justified in taking them to be true (Hausman 1981a:ch. 7), so I shall not explore these technical questions here. The basic point is, however, very simple: Rough generalizations such as those in economics or folk medicine or most other areas of human thought and action can have worth and content despite their vagueness and imprecision. I learned something useful when I was told that aspirin cures headaches, even though I have no illusions about the universal truth of this generalization.

Although Hutchison's attack thus need not have led to any methodological revolution, it was nonetheless a serious and disquieting challenge. Could the consensus that economics was just fine, despite its empirical shortcomings, be squared with the demand that good science be well confirmed? Those who first rose to answer Hutchison's challenge, such as Frank Knight (1940, 1941), explicitly repudiated the empiricist or positiv-

ist philosophy of science on which it rested. Younger and less philosophically ambitious members of the profession might well have wondered whether there was any way to respond to Hutchison without rejecting up-to-date philosophy of science. Perhaps the development of revealed preference theory, which appeared to provide a behaviorist reduction of talk of preferences or utility, coupled with what Paul Samuelson called "operationalism" (1947), showed how to change economic theory to bring it in line with positivist philosophy of science. But doubts remained. Did logical positivism make traditional neoclassical theory untenable?

In the forties, these qualms increased when some economists attempted to test fundamental propositions of the neoclassical theory of the firm. In a particularly notorious inquiry, Richard Lester (1946, 1947) tried to determine whether firms attempted to maximize expected returns, whether they faced rising cost curves, and whether they in fact adjusted production until marginal revenue equals marginal cost. Lester's tests, which consisted of surveys sent to various businesses, were not well designed. But they attracted considerable attention and provoked angry responses (especially Machlup 1946, 1947; Stigler 1947), partly because everybody knew in their hearts that Lester was right, that firms did not behave precisely as marginal productivity theory maintained. As Fritz Machlup (1956:488), one of the staunchest defenders of neoclassical theory and one of Lester's harshest critics, wrote, "Surely some businessmen do so some of the time; probably most businessmen do so most of the time. But we would certainly not find that all of the businessmen do so all of the time. Hence, the assumption of consistently profit-maximizing conduct is contrary to fact." But how could one thus, in effect, confess that Lester was right without being forced to confess that neoclassical theory makes false statements and is thus, on positivist standards, disconfirmed and inadequate? To criticize the details of Lester's surveys, while conceding the relevance of more adequate studies of the same kind, appeared to abandon the traditional neoclassical ship to the rising tide of logical positivism.<sup>3</sup>

Although some, such as Knight and the Austrians, were prepared to say that the standards of the natural sciences did not apply to economics, most writers on economic methodology attempted to show that economics does

<sup>3</sup> In *Rational Economic Man*, Martin Hollis and Edward Nell (1975) argue that the acceptance of neoclassical economics presupposes a commitment to a positivist epistemology. The truth is almost the opposite.

satisfy all reasonable empirical demands that one may make of any science. As I have already mentioned, Mill's views themselves are extremely useful in allaying such empiricist qualms (see Hausman 1981b). But instead of trying to make respectable sense of Mill's talk of tendencies and causal factors, writers on methodology looked to contemporary philosophy of science and attempted to show that economics satisfies the more sophisticated (and weaker) criteria to which the logical empiricists had already retreated. Fritz Machlup's (1955, 1960) essays, including his confrontation in the mid-fifties with Hutchison, give some idea of the nature of such attempts (Hutchison 1956; Machlup 1956). Machlup argues that Hutchison's criticisms of economics are mistaken and that economic practice can be reconciled to the demands of logical empiricism, once one takes into account more sophisticated analyses of scientific theories such as instrumentalism or "partial interpretation" views.

### **Friedman's methodology and its paradox**

In his famous essay "The Methodology of Positive Economics," Milton Friedman (1953) offered the apparent way out of the difficulties that has proved most popular with economists.<sup>4</sup> It is that apparent way out, not the possible intricacies of Friedman's views, with which I am concerned. Indeed, Friedman's essay is by far the most influential methodological statement of this century. It is the only essay on methodology that a large number, perhaps a majority, of economists have ever read. Although Friedman does not refer to contemporary philosophy of science, he, too, attempts to show that economics satisfies positivist standards.

Friedman's essay is problematic, and its ambiguities and inconsistencies have engendered a large literature.<sup>5</sup> Almost all of the many essays that have been written in response to Friedman's work have been critical, and

<sup>4</sup> I do not mean to maintain that all economists have adopted Friedman's way out. Interesting work has been done demonstrating that the alternatives proposed by Lester and others are in many cases equivalent to neoclassical theory. Important work has also been done that takes Lester's findings seriously and suggests conservative and sensible ways of modifying neoclassical economics to take the findings to heart. See, for example, Cyert and March (1963).

<sup>5</sup> For some of the better discussions see Koopmans (1956), Rotwein (1959), Archibald (1961), Winter (1962), Cyert and Grunberg (1963), Nagel (1963), Simon (1963), Melitz (1965), Bronfenbrenner (1966), Bear and Orr (1967), Brunner (1969), Coddington (1972), Rosenberg (1976), Bray (1977), Jones (1977), Blaug (1980), and Caldwell (1982).

although the quality of the responses has been uneven, the many serious problems in Friedman's claims have been clearly exposed. But decisive criticism has been largely impotent here. In Friedman's views the peculiarities of the divorce between methodological rule and behavior, the mediocrity of the methodological discussion, and the divorce between methodology and philosophy of science are all particularly evident.

After distinguishing positive and normative economics, Friedman (1953) begins his answer to critics of neoclassical economics by asserting that the goals of science are predictive and not at all explanatory (p. 7). "Predict" is a technical term for Friedman. A theory's predictions are those of its implications whose truth is not yet known, whether they concern the future, present, or past (p. 9). Since the goal of science is accurate prediction, Friedman concludes that a theory that enables one to make reliable predictions is a good theory. In case of a tie on the criterion of predictive success, theories of wider scope (provided that they are not inconvenient to use) are to be preferred to theories of narrower scope. Simpler theories are also preferable to more complicated ones (p. 10). Friedman stresses that there is no other test of a theory in terms of the realism of its "assumptions" (p. 14). When Friedman speaks of the "assumptions" of a theory, he includes both the fundamental assertions of the theory (such as the claim that consumers are utility maximizers) and the additional premises one relies on when drawing implications from a theory. It is not clear what Friedman or the critics he is responding to mean by the term "unrealistic." Friedman himself equivocates. Sometimes he means simply "abstract" or "not descriptively complete." But usually when he calls an assumption unrealistic, he means (as he must if he is to respond to Lester's challenge) that it is not true, perhaps not even approximately true, of the phenomena to which the theory is applied.

Given this view of scientific inquiry, Friedman answers Lester by arguing that Lester mistakenly attempts to assess the "assumptions" of neoclassical theory, when all that matters is the correctness of its predictions. In rejecting any assessment of the assumptions of a theory, Friedman is also responding to a critical tradition that extends back to the German historical school via the American Institutionalists. This critical tradition questions abstract theorizing in general and objects in particular to the purportedly unreasonably unrealistic assumptions of neoclassical theory (which is part of what Veblen criticizes in his classic 1898 work). Friedman seems to enable one to reject all such criticism as fundamentally confused.

As thus far stated, Friedman's position is untenable, because the assumptions of neoclassical theory, such as the claim that firms attempt to maximize profits, lead to false predictions concerning, for example, the results of surveys such as Lester's. Since neoclassical theory evidently has false predictions, it thus appears flawed, although possibly less flawed than any existing alternatives. But to concede that neoclassical theory is faulty in this way would be to grant Lester's criticism and would undermine Friedman's determined opposition to any attempt to develop more "realistic" alternatives.

Friedman seems to be aware of this obvious problem (pp. 26–7), and he responds to it by distinguishing those predictions in which economists are interested from those that they do not care about (pp. 20, 27–8). The boundaries need not be fixed once and for all, but may depend on the particular inquiry (pp. 27–8). All that matters, Friedman maintains, is how well a theory predicts the phenomena in which economists are (at least on a particular occasion) interested. Friedman's views are thus an odd variety of instrumentalism. Falsity, even falsity of predictions, is of no importance unless it detracts from a theory's performance in predicting the phenomena in which one is interested. A theory of the distribution of leaves on trees according to which it is *as if* leaves had the ability to move instantaneously from branch to branch is thus regarded by Friedman as perfectly "plausible" (p. 20), although of narrower scope than the accepted theory. In Friedman's view, if a theory predicts accurately what one wants to know, it is a good theory, otherwise not.

When Friedman says that it is *as if* leaves move or *as if* expert billiard players solve complicated equations (p. 21), what he means is that attributing movement to leaves or great calculating power to billiard players leads to correct predictions concerning the phenomena in which one is interested. And a theory that accomplishes this is a good theory, for a "theory is to be judged by its predictive power for the class of phenomena which it is intended to "explain" (p. 8). It may thus seem obvious that the realism of a theory's assumptions or the truth of its uninteresting or irrelevant implications is unimportant except insofar as either restricts the theory's scope. Since economists are not interested in what business people say, it makes no difference what Lester's surveys show about how people in business claim to behave.

But if one were to take seriously the injunction not to pay any attention to the realism of assumptions, science would grind to a halt. Even if one

fully grants Friedman's view of the goals of science, one still has to be concerned about the realism of assumptions. There is no good way to know what to try when a prediction fails or whether to make use of a theory in a new application without assessing the accuracy both of a theory's claims and of the statements one relies on in deriving an empirical implication from a theory. Without assessments of the realism (in the sense of approximate truth) of assumptions, the process of theory modification would be hopelessly inefficient and the application of theories to new circumstances founded on nothing but arbitrary guesswork. The point is simple: If one wants to use a machine in a new application or to build a new machine out of its components or to diagnose a malfunction, it helps to know something about the reliability of the parts of which it is made. So even if *all* one wants is valid predictions concerning some particular phenomena, one must still make assessments of whether the assumptions of a theory are reasonable approximations to the truth, and thus one must be concerned about the accuracy of irrelevant as well as relevant predictions.

It should go without saying that no serious economist, Friedman included, acts on the injunction to pay no attention to the realism of assumptions. In fairness to Friedman one should point out, first, that with an inconsistency that is admirable in this case, Friedman (1953:29, 31n) recognizes these points and, second, that the forms of instrumentalism espoused by philosophers in the late forties and early fifties had their own serious problems (Morgenthau 1969).

How could this methodological morass have been so influential? The answer lies, I think, in the opportunity it provides for equivocation. The trick is that Friedman's lack of clarity has enabled economists both (1) to dismiss all criticism of the basic assertions of neoclassical theory as methodologically misguided and (2) to believe that this dismissal follows from less controversial and more plausible methodological views than those that Friedman in fact endorses. Economists can easily read Friedman's essay as maintaining only that, if a particular theory "works," one should use it no matter what its inadequacies. But, of course, no sensible philosopher or scientist ever thought otherwise, and the issue is not whether to use neoclassical theory where it works, but whether to regard it as a good scientific theory. Moreover, economists can easily interpret Friedman as asserting only that the results of testing should be decisive in an empirical science. Thus Mark Blaug (1984:360), for example,

maintains that “Friedman is simply Popper-with-a-twist applied to economics.” But those who attribute such weaker and more plausible views to Friedman fail to appreciate what Friedman was trying to accomplish: To dismiss Lester out of hand, one needs Friedman’s unacceptable thesis that false predictions do not matter when they concern phenomena in which one is not interested. Either one can attribute plausible views to Friedman or one can use his views to reject all criticism of the axioms of neoclassical theory. One cannot do both – unless, that is, one is a little confused. One finds in Friedman’s essay splendid apologetics that, with a bit of confusion, can masquerade as up-to-date, respectable, quasi-positivist philosophy of science.

I have dwelled on Friedman’s views both because of their enormous influence and because they vividly illustrate the problem I raised at the beginning of the chapter. First of all, I have (perhaps unreasonably) enough confidence in philosophers to believe that Friedman’s essay could never have been so influential were philosophy and reflections on economic methodology not cut off from one another. But the moral of this history of economic methodology and particularly of this episode is more complex and interesting, for what Friedman argues is profoundly paradoxical. If one takes literally those aspects in Friedman’s essay that I have stressed, his confidence in “the maximization-of-returns hypothesis” and in neoclassical theory in general rests in “the repeated failure of its implications to be contradicted” (Friedman 1953:22), not in any judgment concerning the realism or plausibility of its assumptions (but see pp. 26–30 on indirect testing). But as Friedman and everyone else knows, the implications of neoclassical theory have certainly been contradicted on many occasions. This would be so even if the theory lived up to its highest praises. All it takes is some disturbance, such as a change in tastes, a technological innovation, an institutional change in property rights or contract law, a redistribution of income, or for that matter an earthquake or an invasion from Mars. Does *any* economist really accept neoclassical theory on the basis of “the repeated failure of its implications to be contradicted”? Is this not rather a doctrine piously mouthed in the presence of philosophers or of their economist fellow travelers, but completely cut off from the reality of economists’ methodological practice and attitudes? If discussions of economic methodology had been *completely* cut off from philosophy of science, economists could have been

more sensible. Following, but not really joining in the philosophical discussion concerning the nature of science, with a vivid appreciation of the practical consequences of particular philosophical models, writers on economic methodology have been in an almost impossible position.

Such a drastic contradiction between methodological dictum and practice shows only that both cannot be right. Most of those who have noticed the gulf have been too impressed with current philosophical models and have attacked the methodological practice. With some notable exceptions (e.g., Simon 1976), these attacks have not been well informed and well justified. Too many of those who have written on economic methodology during the past generation have failed to understand the methodological practice of standard economics and how it can be justified. In addition, methodological condemnations and defenses of economic theories have usually relied on some indefensible philosophical theory of the nature of science. Few economists realize how little philosophers have to offer by way of systematic theories of the scientific enterprise.

### Contemporary directions

Methodological writing is now pouring out at an increasing rate. In the past decade there have been at least forty books,<sup>6</sup> literally hundreds of articles, and even a new journal, *Economics and Philosophy*. Some of this current methodological literature exhibits increasing sophistication and much higher quality, but a large portion manifests the same inadequacies that one finds in the methodological writing of the 1950s and 1960s. This considerable contemporary methodological literature falls into four main classes.

First, there are criticisms of neoclassical theory, often from an institutionalist or radical perspective (many appearing in the *Journal of Economic Issues*). Even though neoclassical or neo-Walrasian theory is the most important contemporary school of economics, it is not and never has been without significant competitors and a mass of critics. This competition has in the past generated some classic methodological statements such

<sup>6</sup> The recent books and anthologies that have generated the most interest include Hollis and Nell (1975), Latsis (1976), Rosenberg (1976), Hutchison (1977, 1978, 1981), Stewart (1979), Hahn and Hollis (1979), Blaug (1980), Hausman (1981a, 1984), Pitt (1981), Boland (1982), Caldwell (1982, 1984), Stegmüller, Balzer, and Spohn (1982), Weintraub (1985), and McCloskey (1986).

as those of Marx (1973) and Veblen (1898, 1909). But it now calls forth rather repetitious complaints about the extent of abstraction in neoclassical theory, its empirical disconfirmation, or the official quasi-positivist defense of neoclassical theory (see, e.g., Samuels 1980; Eichner 1983). Many of these criticisms in effect seize upon the inconsistency between the espousal of positivistic or Popperian views by neoclassical economists and their Millian methodological practice. Note that my unkind words are directed only to the methodological complaints of some institutional and radical economists, not to the substance of the economic theories they defend.

Second, one finds still more refutations or rehabilitations of Milton Friedman (e.g., Boland 1979), which are often less distinguished than those that appeared in the immediate wake of his essay (the comments here excepted, of course). It will be a great step forward when economists come to regard Friedman's essay as no more than a historically interesting document.

Third, one finds many applications of current or not so current trends in philosophy of science, especially of work by Popper, Kuhn, Lakatos, and, most recently, even Feyerabend. Although those less familiar with systematic philosophy of science or less cynical about it might expect a great deal of such attempts to apply up-to-date philosophy of science, this literature is almost as disappointing as is the grumbling about neoclassical economics or the rehashing of Friedman. One large problem is that the predominant questions for most writers on economic methodology have been normative: Is some particular economic theory worthless or not? Since Kuhn and Feyerabend are evasive or dismissive concerning such normative issues, their work has been difficult to apply. By contrast, Popper has been only too easy to apply unreasonably. In Mark Blaug's 1980 text, *The Methodology of Economics*, for example, one finds whole branches of economics such as general equilibrium theory forcefully and often rather unfairly bashed with the falsificationist cudgel. The problem here is not mainly with Blaug's knowledge or judgment; the problem is with the cudgel. It is not surprising that a philosophy of science that, apart from its slogans, is almost completely worthless (Liebersohn 1982a,b; Hausman, in press) leads to unhelpful criticism. Although Lakatos has some useful things to say about the structure and autonomy of scientific theorizing (especially concerning the importance of heuristics), which

have been ably put to work by economists such as Roy Weintraub (1985), Lakatos's views on the assessment and modification of scientific theories incorporate Popper's central mistakes and are of little value.<sup>7</sup>

Finally, there have been various attempts to focus on the methodological peculiarities and difficulties presented by particular economic work. Several of the other chapters in this volume exemplify this work. Although not without their limitations, I find these attempts to make contact with real methodological problems and practices of economists most informative.

Many authors have taken such a tack. The best known among philosophers is Alexander Rosenberg (1976), whose *Microeconomic Laws: A Philosophical Analysis* is something of a watershed. In the decade since *Microeconomic Laws* was published, Rosenberg's views concerning the whole enterprise of neoclassical economics have shifted dramatically, but his analyses of particular aspects of economics such as the relations between micro- and macroeconomics (ch. 8) or the sense in which explanations in economics are both causal and teleological (ch. 5) have been more stable and are a useful and permanent contribution.

Among economists the best known authors in this more eclectic and empirical vein are probably Bruce Caldwell (1982) with his "methodological pluralism" and Donald McCloskey (1983, 1986) with his "rhetoric of economics" (see also Chapter 2, this volume).<sup>8</sup> I do not find Caldwell's methodological pluralism to be a clear philosophical position yet. Sometimes it seems to be intended as the thesis that different economic methodologies must be assessed entirely in their own terms and that each is valid if it is internally coherent. I see no justification for this view. I think that Caldwell should be interpreted, more charitably, as beginning with the recognition that philosophers of science have no complete recipe for scientific practice, so economic methodologists have no prepared

<sup>7</sup> The problem in a nutshell is that Lakatos, like Popper, rejects all forms of "justificationism" (see Lakatos's classic 1970 work). Thus both deny that evidence ever rationally increases our confidence that a theory is correct, close to correct (but see Lakatos 1974), or reliable in application. Without being able to make use of information concerning how well supported various claims are, we cannot have an efficient and rational policy of theory modification.

<sup>8</sup> By focusing only on Rosenberg, McCloskey, and Caldwell, I do not mean to suggest that no one else is doing interesting and valuable work on economic methodology. There are a large and growing number of economists and philosophers currently doing first-rate work on the philosophy of economics and economic methodology.

sermons to preach to the heathen economists. Cast among the heathen, bereft of revealed truth, methodologists must face the bewildering task of attempting to understand and assess the practices and products of economists. Before judging competing methodological views, one must patiently attempt to understand and to appreciate them. Swift dismissals are not warranted.

Donald McCloskey also begins with the recognition that systematic philosophy of science provides no well-justified, comprehensive code of scientific practice. He then proposes that the tools of classical rhetoric and literary criticism will provide better guidance in understanding economic practice than have the tools of philosophers. To the extent that McCloskey encourages careful study of economic argumentation, I think his views helpful. New perspectives can be liberating. But McCloskey offers little argument for the superiority of his favored literary tools, and having cast away philosophy of science, he has a hard time explaining how his proposed successor to economic methodology is supposed to retain any normative role, as of course it must.

There is also a separate technical literature on econometric methods that overlaps infrequently with the methodological mainstream. Although this unjustifiable gulf between discussions of economic and econometric methodology arises in large part because of differences between the technical expertise of economists and that of econometricians, I suggest that another reason is that econometrics does not perform the theory-testing role assigned to it in popular methodological fantasies. Once methodologists remove the distorting glasses of one or another simplistic philosophical theory of science, they may be better able to appreciate the role of econometrics in economics and the relevance of questions concerning econometric methods to questions concerning economic methodology.

Although most recent work on economic methodology has been performed by economists, the contribution of philosophers has been increasing. Not only does most current work bear the mark of contemporary philosophy of science, but philosophers have recently had more to say directly about economics. Actually, there have been only about a half-dozen philosophers with an active interest in economic methodology, but that is still many more than were previously interested. It is fair to say that the gap between philosophers and economists is narrowing. Not only are economists willing to study contemporary philosophy of science, but a few philosophers have also been willing to study economics.

### **Positivism and the boundaries between philosophy and economic methodology**

We can thus see that for generations many economists have been engaged in an activity that resembles philosophical inquiry but has remained separate from, although constantly influenced by, inquiry within the philosophy of science proper. The past generation has been exceptional in its break from traditional wisdom concerning justification in economics, in the mediocrity of its discussions, and in its implicit repudiation of methodological practice, yet typical in its combination of reliance on philosophy of science and separation from philosophical discourse. How can one explain these rather strange facts?

When I first started thinking about this question a couple of years ago, I was inclined to blame it all on the logical positivists. After all, for a truly up-to-date philosopher of science, they are the source of all error; so they must be the source of this error, too. From the perspective of logical positivism, reflections on economic methodology (when not nonsensical) fall into three categories. First, the general positivist revelation must be explained to economists. Those who have grasped the true philosophy and who know a little economics are thus called upon to spread the good empiricist news to benighted economists with their dreams of synthetic a priori truths. Second, economists and philosophers must engage in the sort of conceptual analysis and conceptual scrutiny that are supposedly exemplified by Einstein's analysis of the concept of simultaneity. Getting straight what a concept means – that is, for the positivists, getting straight about what the sensory circumstances are when one is justified in using a concept – can eliminate muddles and help keep a science from being sidetracked. Finally, there is the unphilosophical but possibly valuable empirical task of investigating what techniques of economic knowledge acquisition might be efficacious. So from the perspective of logical positivism, someone concerned with economic methodology is applying the general insights of the logical positivists, or analyzing particular economic concepts, or engaged in a sort of applied psychology or sociology. In fact, the first of these tasks attracted the most attention from both philosophers and scientists, including economists. No wonder, then, that reflection on economic methodology should be philosophically derivative and that it should not draw the sustained effort of the most talented and original economists.

Not only were the positivists in this way responsible for the mediocrity of methodological discourse, but in preaching an impossible methodology they (and Karl Popper) induced the methodological schizophrenia that we observed before. Unable to challenge the positivists on their own philosophical grounds, writers on economic methodology faced the impossible challenge of squaring methodological practice in economics with the unreasonable demands of positivist and Popperian philosophy of science. Thus we find a further reason for the weakness of recent methodological writings.

In my essay "How to Do Philosophy of Economics" (1980), I imagined that all of this would change with the passing of logical positivism. Instead of merely applying the preaching of philosophers concerning the goals of science, the nature of scientific explanation and confirmation, and so forth, economists and philosophers should join minds in a serious consideration of the peculiarities of economic theory and practice. Out of their triumphant collaboration would emerge major methodological advances that would aid the acquisition of economic knowledge. One might, for example, come up with a well-defined set of criteria for the legitimate and sensible use of the sort of extreme simplifications that are common in economics, such as attributing perfect knowledge of the future to economic agents.

So went my story. Although it still seems to me essentially correct as an explanation for the gulf between methodological preaching and practice and for the weakness of methodological writing, I think that it is only a small part of the explanation for the divorce between philosophy of science and reflection on economic methodology. The divide between philosophy and methodology preceded logical positivism and has, up to now at least, survived logical positivism's demise. Furthermore, even on the narrowest version of logical positivism, much interesting work was left for those interested in economic methodology. Once the analysis of general scientific concepts such as explanation or confirmation was complete, the only work remaining for those interested in philosophy of science would be to analyze concepts that figure in specific sciences such as economics. According to the positivists themselves, then, philosophy of science should dissolve into philosophy of the special sciences, and the only possible dividing line between philosophers and economists would lie between conceptual analysis and empirical study of research techniques. But as it turned out, of course, there was plenty for philosophers to do at

home to keep the general positivist program from collapsing and thus plenty of revised good news for philosophers and economic methodologists to spread among the heathen economists. The crucial point is, however, that even an unsophisticated positivist might have done interesting work concerning economic concepts. Revealed preference theory hardly exhausts the possibilities for empiricist analyses of economic concepts. The puzzle remains.

Of course, as any sociologist who studies the institutionalization of academic disciplines can point out, there are numerous sociological barriers to effective collaboration between philosophers and economists. Rhetoricians can also point to major differences in argumentative style. Just as one finds differences in, for example, the ways in which chemists and physicists study quantum mechanics, so one would expect to find differences in the way in which economists and philosophers reflect upon economic theory and upon the practice of economics.

### **The ineliminable boundaries between philosophy and economic methodology**

But is there anything more? Are the only barriers to collaboration sociological and rhetorical? Are philosophers of science and economists interested in methodology prevented from dancing together by anything more than their clay feet? Subject to the limitations imposed by various institutional barriers, can philosophers and economists now work hand in hand, analyzing and overcoming the diverse and detailed methodological difficulties confronted by the various kinds of economic inquiry?

I think not. There are difficulties inherent in such collaboration among philosophers and economists, for the aims and interests of philosophers and economists seem quite different. Philosophers want to understand knowledge acquisition in economics mainly because of their general interest in the possibilities and limits of human knowledge and because of their general interest in human agency. Economists want to understand knowledge acquisition in economics mainly because they want to understand and improve the process and to reveal the blunders of those who pigheadedly adhere to a different approach to economic theorizing. While Popper, for example, wanted to understand the difference between science and nonscience and to understand how knowledge can grow without the possibility of inductive proof, Popperian methodologists want clear-cut

rules for economic theory acceptance and rejection. Can any single enterprise satisfy both these goals? Can philosophy of science be applied to economics without becoming vulgarized and, to some extent, stripped of its worth as philosophy? Can methodological reflection on economics become philosophically sophisticated and subtle without losing its applicability and relevance to the practice of economics?

Obviously, the difficulty to which I have vaguely pointed is an instance of a more general problem concerning the relations between theory and practice and concerning the applicability of theoretical knowledge. One has here the same conflict of purposes and of criteria of success that one finds between scientists and engineers, between moral theorists and preachers or social critics, or between economists themselves and bankers or politicians. The goal of achieving knowledge of a certain domain, which dominates the work of theorists, is at best an instrumental, mediate goal or an incidental outcome for those who have some practical purpose they want to achieve. The Copernican Revolution and the formulation of Newton's theory of motion and gravitation could, for example, be epochal events in the development of science without, for a very long time, contributing anything significant to the practice of navigation. A hard-headed sailor might have regarded the whole issue as just so much nit-picking. Rawls's (1971) critique of utilitarianism could have been an important development in ethics even if it had no immediate application to questions of public policy. The philosophical merit of Quine's (1953) and White's (1956) critiques of the analytic-synthetic distinction is not to be measured by its implications for the practice of physics, economics, or basketball. The entrepreneur's challenge to economists, "If you're so smart, why aren't you rich?" mistakes the aims of economic theorizing. And so on. Even where the goal of theorizing is ultimately to assist practice, this conflict in immediate aims cannot be ignored.

The gap between theory and practice does not arise solely from this difference in aims, and it manifests itself in many ways. In some cases it may be completely unbridgeable because a particular theory simply has no current practical applications. In other cases it may be only shakily bridged because the theoretical solution is too subtle and complex to be applied directly or because the theoretical solution is too simple and global and requires too many qualifications before it can be applied. In either of these cases the approximations that drive practice may have only a tenuous link with fundamental theory. There are also cases in which a theory may

be applicable to a practical problem yet still not be applied. The theorist may not know of the practical problem or may not be interested in it, and those concerned with the problem may not understand the theory well enough to apply it.

One can find each of these kinds of gaps between philosophy of science and economic methodology. For example, the work of twentieth-century philosophers concerning the meaningfulness of terms that refer to or denote newly hypothesized and unobservable entities and properties is largely irrelevant to much of economics, whose basic terms, such as “consumer,” “commodity,” “price,” and “preference,” denote either observable things or properties, or “unobservable” ones that humans have been referring to for millennia. The account in my book on capital theory of when one is justified in making use of simplifications and *ceteris paribus* clauses (Hausman 1981a:129–33) provides an example of near inapplicability by reason of too much subtlety. Whatever virtue my account of such justification conditions may have, it does not provide the sort of easily grasped bludgeon that one needs in the construction and criticism of economic analyses. Many of the standard philosophical accounts of theories, explanation, confirmation, and so forth are nearly inapplicable as well, not because they are too subtle, but rather because they are too global and too simple. Consider, for example, the deductive-nomological model of explanatory arguments. One can, I have argued, often fit explanations in economics into the model (Hausman 1981a:148–50), but in doing so, one has said little of substance or interest about explanations in economics. Finally, recent work on causality (e.g., Mackie 1974), for example, has found little application to economics because it is not understood by economists.

### **Crossing the boundaries**

If there are, then, these gaps between the goals and interests of philosophers and of writers on economic methodology, how closely can their efforts be linked and how should the two inquiries be related? First, the general division of labor with philosophers as the pure theorists and methodologists as the “engineers” is basically sensible and is bound to continue. In maintaining that there is such a division of labor, I do not mean to assign philosophers a “higher” task than methodologists. In my view “pure theorizing” is not in any way superior to “engineering.” The

tasks are just different. It is utopian to expect more than a handful of philosophers interested in economics to become competent economists or for any appreciable number of economists interested in philosophy to become good philosophers. And in any case, I am arguing that philosophy and methodology are distinct tasks, not that philosophers and those interested in economic methodology must be distinct persons. One can, however, still reasonably hope for more feedback from practice to theory, with more involvement of economists with philosophy and more involvement of philosophers with questions of methodological application; for in general philosophical conclusions are not going to be well confirmed apart from their attempted applications in disciplines such as economics. Given the absence of independent confirmation, philosophers cannot simply disregard problems of application. So economists potentially have important philosophical tasks, and philosophers can also do more to make their views applicable and to criticize misapplications. Working economists are, of course, going to rely on approximations that will appear crude to philosophers. But these approximations should simplify philosophical truth rather than philosophical error.

Second, a recognition of both the fallibility of philosophers and of the gap between philosophy and methodological practice gives those interested in methodology more responsibility. They might, I suspect, have more to contribute if at least some of the time they tried to focus on details of particular economic theories or inquiries without necessarily linking their investigation or its conclusions to any general philosophical views. Obviously economists, no matter how greatly they despise philosophers, can never escape their influence entirely, but there is still an important difference between, on the one hand, attempting to look and see, however biased the results by expectations and prior commitments, and, on the other hand, trying to establish, refute, apply, or caricature a philosophical position.

Third, I am still enough of a conforming postpositivist philosopher of science to believe that one of the best ways for philosophy of science to progress is to pay closer attention to scientific practice. But insofar as one wants to contribute directly to philosophy of science, one should not think of one's task as immediately or directly helping scientists to go about their business. Some good philosophy of science will be helpful to scientists, but not all of it need be or will be. Someone who holds a position in a philosophy department and has an extensive education in philosophy may,

of course, devote himself or herself fruitfully to economic methodology, but there are, I think, real advantages to not identifying such work with work in philosophy of economics. To fail to draw the distinction is to fail to have clear goals and clear criteria for success.

So philosophy is to economic methodology as science is to engineering or as economics and ethics are to public policy. Acquiring knowledge and applying knowledge are closely related but nonetheless different tasks with different goals and different criteria for success, *and the latter is not just a trivial appendix to the former*. Even if those interested in economic methodology must look to philosophers for general insights concerning aspects of the sciences, they face serious and important problems of their own. And, of course, once one surrenders any hopes of finding an a priori path to philosophical truth, one must recognize that the general insights of philosophers are going to depend heavily on the applications and insights of methodologists.

### What is philosophy of economics?

Where, then, does the philosopher of economics stand (or fall)? Is there indeed any sense to such a beast? The question is as ambiguous as is the term “philosophy of economics,” for philosophers are also interested in economics for nonepistemological reasons. Philosophers may, for example, study decision theory not because they are interested in knowledge acquisition in economics, but simply because they are themselves interested in decision theory. “Philosophy of economics” includes parts of social and political philosophy, ethics, action theory, philosophy of mind, and metaphysics, in addition to studies of knowledge acquisition in economics. These other areas of overlap between economics and philosophy are not my concern here.

What, then, of the philosophy of economics that is my concern? One might think of the philosopher who is interested in knowledge acquisition, the methodologist and the economist as akin to the manufacturer, the retailer, and the consumer. Perhaps the philosopher of economics might fit in as some sort of philosophical insight wholesaler. But the analogy is askew, for the analogs to consumers, retailers, and wholesalers jointly do the “manufacturing” here. As I have already asserted, without arguing the point here, philosophical knowledge comes largely from a study of specific disciplines such as economics (the point is argued in Hausman

1980). It is thus better to think of philosophers of economics and their brother and sister philosophers of physics, biology, nursing, and anthropology as replacements for all-purpose philosophers of science. There may, of course, be interesting and important comparisons and contrasts to be drawn among the various aspects of knowledge acquisition in different disciplines, and there will thus still be room for general philosophy of science. But general philosophy of science is not related to philosophy of economics or philosophy of psychology as theory to application. In my postpositivistic view, general philosophy of science is and ought to be parasitic on philosophical study of specific disciplines and theories.

So philosophers of economics are philosophers of science who recognize that they had better know quite a lot about some specific science – in this case, economics – if they are to have any chance of contributing to solving the general philosophical problems concerning the possibilities of human knowledge and the means of acquiring it. Economic methodologists, though concerned with the same data, have a different set of questions and concerns. They are more interested in contributing to the acquisition of economic knowledge. The two groups are natural partners; both will do better work with the help of the other. But there are two sets of questions and two activities here, not just one.

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