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Introduction

> Those who can, do science; those who can't prattle about its methodology.
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> [Samuelson 1992, p. 240]

> If we were to believe many economic methodologists, particularly those attempting to impress philosophers of science, you would think that all methodologists sit around "appraising" the work of economists. I have a vision of these guys sitting around in priestly robes . . . passing judgment on people such as Becker, Arrow, Samuelson, Friedman, Keynes, etc. On what basis do they criticize such economists? Do they accuse economists of being unscientific? Who cares?
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> [Boland 1997, p. 152]

> Back in 1982, a brief but brusque exchange, touching on the relations between Philosophy and Economics, took place between James Tobin, the liberal, Nobel Laureate, Yale economist, and Robert Nozick, the conservative Harvard philosopher. In the course of a debate . . . Tobin exclaimed at Nozick: "There's nothing more dangerous than a philosopher who's learned a little bit of economics." To which Nozick immediately responded: "Unless it's an economist who hasn't learned any philosophy."
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> [Hutchison 1996, p. 187]

This book has three separate but interrelated goals. The first is to provide a *survey of recent developments in the field of economic methodology*.

The second goal is to *survey contemporary science theory* as it relates to economics and economic methodology. I say contemporary "science theory" in order to include fields like the sociology of scientific knowledge and the rhetoric of science as well as the more traditional fields of philosophy of science and epistemology. Both of these surveys are frankly interpretative, but the survey of economic methodology is perhaps less interpretative than the discussion of science theory. I have tried to touch on most of the major debates within economic methodology; I felt no such compulsion when it came to contemporary philosophy of science and science studies. Although these two surveys constitute the preponderance of the text, they really do not need much introduction; their execution requires a great amount of time and detail, but their motivation is pretty straightforward. This is not the case for the third aspect of the book.

The third goal is to convince the reader that we *should change the subject* (or perhaps I should say that we *should recognize the subject has changed*, since the change is already underway). The traditional subject of economic methodology has been *applied philosophy of science*; economists have simply borrowed various arguments from the philosophy of natural science and then applied (or tried to apply) those arguments to economics – most commonly focusing on the issue of whether economics is (or is not), or what it would need to do to become, a legitimate empirical science. This view – what I have elsewhere (Hands 1994a) called the "shelf of scientific philosophy" view of economic methodology – is, I hope to show the reader, no longer a good place to invest our intellectual resources. Note the word "show"; I will attempt to persuade the reader by titillation rather than regulation; I am not trying to outlaw the production of literature that takes the traditional approach to economic methodology (I have certainly been involved in such work myself). My effort will be on the demand side, an effort to tempt readers away from familiar habits of thought by pointing out the difficulties with those habits and also by letting them try out some of the other approaches that are (increasingly) available. Consistent with the naturalistic perspective I will be presenting, this is not an edict from above, but rather a simple attempt to get the reader into a new intellectual vehicle by giving them a "free" mechanical inspection of their old one, a list of many happy new owners (some of whom aren't aware they traded), and a test drive in a few of the new models with the most innovative features. I realize of course that most readers are just looking – reading this book as a survey of the literature (which most of it is) – and not seriously shopping for either model: new or old.

1.1 Economic Methodology

There are, of course, many ways to characterize the field of economic methodology. One way to think about methodology is to view it as the study of "methods": the practical techniques employed by successful economists in the execution of their day-to-day professional activities. This type of methodology has appropriately been called lower-case-m methodology (McCloskey 1985a, p. 25); it is essential to professional success, usually acquired tacitly, or by rote, in the context of actually working on specific economic research projects: initially under the guidance of one's research supervisor or thesis director, and then later through interaction with one's colleagues, department chair, and various journal editors. It is the source of answers to day-to-day questions like: Is an R^2 this low OK for this kind of model? Is it reasonable to assume the Jacobian matrix has this strange sign pattern? or, It's OK to drop all of the data from the first two quarters of 1929, right? As important as such questions might be, lower-case-m methodology is *not* what most economists mean when they use the term Economic Methodology. One will not find such discussions in existing surveys of the methodological literature such as Blaug (1980, 1992) or Caldwell (1982, 1994a); it is not generally what one will see published in journals like *Economics and Philosophy* or *The Journal of Economic Methodology*, which specialize in methodological research; and, whereas one might overhear such topics discussed by Nobel laureates, it is not what they write about when they write about "Methodology."

Methodology has traditionally focused on the issue of *scientific knowledge* and whether economics in general, or a particular economic theory, is or is not, scientific knowledge. Methodology has traditionally been about the methodological *appraisal* of economic theory: the job of deciding whether an economic theory is a success or failure with respect to the rigorous standards of the scientific method. This view of methodology, of course, carries us immediately into the field of the philosophy of natural science; if one wants to appraise an economic theory with respect to *the scientific method*, then one needs to know what the scientific method is (and is not) and that specification has traditionally been the responsibility of the philosophy of science. In the words of William Whewell over 150 years ago: "The Philosophy of Science . . . would imply nothing less than a complete insight into the essence and conditions of all real knowledge, and an exposition of the best methods for the discovery of new truths" (quoted by Hacking 1996, p. 38). This methodological perspective leads us directly into the traditional "shelf of scientific philosophy" view of economic methodology. We want to have *rules* for what is and is not good science so that we can methodologically

appraise economics (or some part of economics) and the best source for these rules is the philosophy of science. Now, it may be the case that the particular subject matter and/or character of economics requires (or allows) the rules to be tweaked a bit in order to accommodate the specific concerns of the economic scientist, but the point of departure clearly remains the shelf of (natural) scientific philosophy.

This way of thinking about economic methodology is certainly consistent with the Enlightenment view of scientific knowledge that has been handed down from Bacon, Descartes, and other philosophers. The view that knowledge of the causal structure of the world could be obtained with certainty if the proper method were followed, and even though philosophers have differed radically about what that proper method actually is, the idea that it – the scientific method – is the secret of epistemic success is common to all the various philosophical approaches. If a social science like economics is to be a science and have certain access to the causal structure of the social and economic world, then it, too, will need to abide by the rules of the proper scientific method. The argument is that science *progresses* in a way that no other human activity progresses, and if economics is to partake in such (even potential) progress, then it had better follow the scientific method. This is, of course, a very difficult assignment for a social science such as economics, which has traditionally been concerned with agency, subjective valuations, individual interests, and intentionality: but according to the traditional view this is a problem for economics and not a problem for the scientific method. The scientific gauntlet has been thrown down; it is up to the economics profession to show it can meet the cognitive challenge. Either economists need to demonstrate that their theoretical concepts pass rigorous scientific muster, or to make a convincing case for some kind of partial special-exemption that allows economics to *be* scientific while playing the epistemic game by slightly different rules. Almost all of the traditional work in economic methodology has fallen into one of these two general categories. Chapter 2 will discuss many of these traditional approaches to economic methodology, while Chapter 7 will examine the more recent literature (some of which is relatively traditional in this sense, and some of which is not).

1.2 Contemporary Science Theory

Science theory went through a major transformation during the latter half of the twentieth century. Perhaps "transformation" is not the best word to describe the process, as transformation suggests that the changes actually culminated in a new well-formed consensus about the structure and character of scientific knowledge. Not so. What

happened was a major upheaval with no clear victor emerging (as yet) from amongst the rubble. There was a mainstream view in (at least Anglo-American) philosophy of science during the middle of the twentieth century – as we will see, it was variously called the Received View, or Legend, or (less appropriately) Positivism – and it began to unravel during the 1960s and 1970s. There is not as yet a clear replacement for this mainstream view.

What has become increasingly clear during the last few years is that in order to even be included within the (large) set of contenders, a particular approach to scientific knowledge must be able to address a set of specific and fairly well-defined issues; these are essentially the issues that sunk the former consensus and, consequently, they need to be addressed by any competing approach. They are, in no particular order: underdetermination, theory-ladenness, the social nature of science, relativism, antifoundationalism, and naturalism (all defined and discussed in detail in the chapters that follow). Although it is possible for a particular approach to effectively avoid a direct assault on a few of these issues, such dodges will only be acceptable if they are counterbalanced by exceptional success with respect to most of the others. These issues and concerns constitute the *problem situation* for contemporary science theory. Explaining how these issues came to be the main issues, how various approaches have attempted to deal with them, and how all of this involves (and affects) economics, constitutes the main task of Chapters 3–6.

Because the following chapters examine these issues in what some readers (particularly economists) will consider to be excruciating detail, it does not seem to be particularly useful to jump into the philosophical debate in this brief introduction. This said, I would in fact like to make at least a clumsy pass at one of these particular issues: foundationalism (and antifoundationalism). My reasons for introducing this topic at this point are twofold. First, it is fairly easy to see what the issue is and how it relates to the standard *rules* approach to economic methodology. And, second, it is an issue that begins to play a role very early in the literature on economic methodology (early in Chapter 2).

One way to characterize the problem of the proper scientific method is to focus on the question of *justified* belief. If properly applied, what the scientific method should do is to guarantee that beliefs that have been dutifully processed via the method will be justified. *Foundationalism* is the traditional approach to such justification. Suppose we could identify a set of *basic* beliefs that were "directly" justified – they were self-justified, or incorrigible, and did not rely on any other beliefs for their justification – once we had such basic beliefs we could then

"indirectly" infer the justification of other, higher level, beliefs derived from these basic beliefs. These basic beliefs are the *foundations* of knowledge, and the various epistemological frameworks built on such incorrigible foundations are *foundationalist* approaches to knowledge. Of course, foundationalism comes in a variety of different hues; the two most influential in the history of philosophy are *empiricism* (where sense data serve as foundations) and *rationalism* (where reason serves as the foundation). We will find that empiricist foundationalism has constituted the epistemological backdrop for most of mainstream philosophy of science (until quite recently), and that wrestling with the tensions between empiricist foundationism and the practice of economic science has been one of the main concerns of those writing in the field of economic methodology. The unraveling of empiricist foundationalism is one of the main developments leading to the substantial changes that have taken place within contemporary science theory.

1.3 Changing the Subject

The disarray within contemporary science theory poses an interesting problem for the traditional shelf-of-philosophy approach to economic methodology. Although I will argue that the shelf-approach has always been problematic, one does not need to accept this conclusion in order to see that it is *particularly problematic today*, given the current malaise within the philosophy of science and science theory more generally. If philosophers and others within science theory can't agree about the constitution of the scientific method (or even whether asking about a scientific "method" makes any sense), doesn't it seem a little dubious for economists to continue blithely taking things off the shelf and attempting to apply them to economics? The people who have traditionally claimed to be most knowledgeable about the subject of knowledge are currently in disarray on almost every substantive issue; they no longer provide (assuming they ever did) a reliable tool for discussing the relationship between economics and scientific knowledge. The old view of economic methodology as a rule-giving meta-discourse derived from the philosophy of natural science, a methodology that sought to prescribe the correct scientific practice for economists, is entirely discredited by these philosophical (and other) developments. In this (narrow) sense I am in agreement with the recent pronouncements of the "death of economic methodology" by economists such as D. McCloskey (1985a/1998, 1994) and Roy Weintraub (1989).

The difference between me and the aforementioned critics is that I find the revolt against borrowed rules to be liberating, not a death knell: not the end of economic methodology but a chance to change the

subject, to reformat the debate, in a more viable and interesting direction. Although the narrow borrowed-rule-giving economic methodology is effectively dead, I believe this is a very fertile and productive period for work in a new more broadly defined field of economic methodology. If economic methodology is defined as the *interpenetration of economics and science theory*, then economic methodology is not only alive, but alive and well. The developments in contemporary science theory open the door not only to new ways of thinking about economics, and economics as knowledge, but also about how economic ideas might be used to help us understand scientific knowledge more generally. Economic methodology is dead; long live economic methodology.

It is important to note that I am not just making a normative claim – arguing that we should adopt a new broader definition of economic methodology – I also will be arguing, in fact mostly arguing, descriptively; the change has already taken place (although many of those who have changed the subject are not aware that they have done so). All I am really asking readers to do is to *recognize* the change that has taken place within the field (and within science theory more generally), and, once it is recognized, to perhaps allow their methodological imaginations to wander a bit more widely than they have in the past. I am advocating the displacement and redirection of the current methodological problematic, but much of my advocacy amounts to little more than pointing out the displacement and redirection that has already occurred. I spend very little time arguing directly against borrowed-rules economic methodology – although once in a while I do attack particular approaches to such rules (old habits die hard) – mostly I will just try to make a new way of thinking and a new vocabulary seem inviting by showing all of the things that have been, and might be, done with them. I will detail the problems within contemporary science theory, but even there I will focus more on redescription than direct attack.

One of my themes throughout is that economics was in some sense *always* involved in science theory – that there never was a pristine shelf of science theory that existed independently of what the contributors to the shelf thought about production, distribution, markets, economic life, and economic theory. I make an ongoing argument that our views about the epistemic order are (and have always been) inexorably intertwined with our views about the economic order. Although I think this is an extremely important point – and one that undermines the traditional view of economic methodology independently of the disarray within contemporary science theory – I also want to stress that even if one does not buy this part of my story, it seems undeniable (at least by the end of Chapter 8) that economics is inexorably intertwined with science theory

today. As we will see, the problems of contemporary science theory open the door to economics in significantly new ways; I would say to foreground that which had previously been backgrounded, but in significantly new ways in any case.

As a final point, I would like to note that none of this needs to bring discomfort to those who hold a relatively traditional view of scientific knowledge. One could choose a radical reading of the recent developments within science theory, but it is not necessary. Suppose science does have the unique key to discovering the way that nature really is; such a supposition doesn't alleviate the current problem within the philosophy of science. Science may very well be the unique path to objective truth about the world, but we do not currently know why that is the case or how in any detailed way to differentiate those activities that do the epistemic right thing from those that do not. Sure, we have some rough ideas – conduct empirical tests, be objective, control variables – but there no longer exists a generally accepted philosophical Received View that spells this out in any great detail. This could very well just be a problem *for the philosophy of science* and *not at all a problem for science*, but it still means we should be extremely skeptical about using philosopher's epistemic stories to "appraise" the scientific status of any field: natural or social. In fact, as we will see, the recent "naturalist" turn in science theory (discussed in detail in Chapter 4) considers contemporary *science* to be the starting point for the study of scientific knowledge. Antifoundationalism is not inconsistent with the basic Enlightenment commitment to science as a uniquely worthy form of life, and naturalist versions of antifoundationalism actually elevate science over the traditional foundationalist discourse of philosophers. Does this relief also apply to those with a relatively traditional view of economic knowledge? Perhaps, but it is not entirely clear; we shall see.

1.4 A Reader's Guide

I want to close this introduction with a chapter-by-chapter reader's guide, but before undertaking that task I would like to make a few general remarks about audience, tone, what is and is not included, and such. These remarks are not presented in any particular order of importance.

First, while I certainly hope this book is interesting and useful to philosophers and students of philosophy, people in science studies and students of science studies, and a wide range of general readers, the fact is that it is written primarily with economists and economics students in mind. Most of the people who read and write economic methodology – and, consequently, most of the people who would find a survey useful

(and those whom I would want to convince to change the subject) – are economists. They have been trained in economic theory and econometrics and they know what professional life in economics is all about (if they forget, it only takes a moment's conversation with their colleagues or teachers to refresh their memory). Most people interested in economic methodology (or students of economic methodology) do not need a survey of economic theory or a discussion of what economists do and think – they already know those things – what they need is a survey of what is happening elsewhere in intellectual life (particularly in science theory), which might help them in their reflections concerning economics. To this end, the book, although written primarily with economists in mind, does *not* contain very much economics. There are numerous examples scattered throughout the book, but there is not, as is often the case with methodology books written by philosophers, any detailed case studies or attempts to give an elaborate discussion of some particular aspect of economic theory. In other words, the book assumes the reader has more background in economics than in science theory. The book *concerns* economics; it *explains* science theory. The absence of detailed case studies is certainly not because I think such studies are uninteresting or not useful; I have written detailed case studies in the history of economic thought, including contemporary economic thought, and intend to produce many more in the future. It is just that the main purpose of this particular work is to discuss economic methodology and contemporary science theory in the way that best serves the interests of the representative reader: a reader who generally has a pretty good idea what economics is, but would like to know (a lot) more about science theory.

Second, the focus is on *disciplinary* economics – the economics of academic economists and those trained by them – and not ersatz economics, better business bureau economics, or folk economics. *It concerns what students think economics is after they take an economics class, not before.* This is certainly not to suggest that these other forms of economics are not interesting – in fact, I think the relationship between these other forms of economics and disciplinary economics is extremely interesting – it is just that work in economic methodology generally concerns disciplinary economics. It is important to note that, although the focus is on disciplinary economics, it is not exclusively on mainstream disciplinary economics. The book contains substantial discussion of various aspects of heterodox economics – Marxist, Institutionalist, Austrian, and others – as well as mainstream views that are no longer mainstream (say, Mill or Ricardo). Heterodox economists have generally been the disciplinary economists most interested in and most sensitive to methodological

issues, and the attention they receive in the following chapters reflects the attention they have given to methodological subjects.

Third, many worthy subjects do not get discussed in the following chapters. Most of the examples of economic theorizing come from microeconomics, general equilibrium theory, and macroeconomics, and while these are clearly the theoretical heart of the discipline, there are other relevant areas within contemporary economics that end up getting short shrift; econometrics and experimental economics come immediately to mind. Econometrics and experimental economics are areas where there has been increased methodological discussion during the last few years and perhaps the framework presented here will serve as an inspiration for additional work in these areas. Another missing subject is any discussion of the growing literature on "ethics and economics"; the book contains a lot of philosophy, but for the most part it is epistemology-based philosophy and not moral philosophy. This, too, is an important subject for additional research.

Fourth, the following chapters do not discuss many of the economists who have made substantial contributions to the methodological literature. Because the book is primarily concerned with recent developments, the discussion of the methodological classics in Chapter 2 focuses mainly on the big names – the "greatest hits" – and neglects many of the economists who had very interesting things to say on methodological topics. There is not any serious discussion of the methodological ideas of economists like Fritz Machlup, Joseph Schumpeter, Tjalling Koopmans, or Wesley C. Mitchell. All I can say is that space considerations and the focus on recent changes precluded the methodological work of such economists. If it is any consolation to readers troubled by the neglect of one (or all) these figures, I would like to note that the book also does not seriously discuss the methodological writings of Frank Knight, a figure that I personally find extremely interesting. I too share the pain of "space considerations."

Finally, there is always the question of when to stop. In writing a survey of a body of literature that continues to grow, one must at some point stop trying to include all of the relevant new material. In my case, that point was reached sometime during the first few months of 1999. Although a few things published after that date make their way into the following chapters, that was the point at which I stopped trying to include everything that I thought might be relevant. In particular, Cartwright (1999b); Favretti, Sandri, and Scazzieri (1999); Friedman (1999); Fuller (2000); Garnett (1999); Goldman (1999); Hacking (1999); and Motterlini (1999) are among the works that appeared too late to be integrated into the text.

With that bit of background out of the way, I now move to the chapter-by-chapter reader's guide.