Critical economic methodology

‘No methodology please, we’re economists.’ Economic methodologists frequently complain that they are ignored by the mainstream. Practicing economists claim that methodology has nothing useful to say to them. In this typically provocative book, Lawrence Boland takes issue with both sides, arguing that there has been too much ‘methodology for methodology’s sake and that mainstream economics might benefit by using methodology to take a critical look at economics theory.

Containing twenty essays, most of which are previously unpublished or extensively revised, the book discusses:

- Friedman’s famous essay on methodology
- the role of the assumption of maximization in neoclassical economics
- the limitations of traditional economic methodology
- the possibilities for more fruitful methodological criticism
- Karl Popper’s theory of science

Throughout, the essays emphasize the positive role of criticism as a central part of intellectual activity.

Lawrence A. Boland is Professor of Economics at Simon Fraser University. His previous publications include *The Foundations of Economic Methodology* (1982), *Methodology for a New Microeconomics: The Critical Foundations* (1986), *The Methodology of Economic Model Building: Methodology after Samuelson* (1989) and *The Principles of Economics: Some Lies My Teachers Told Me* (1992).

Critical economic methodology

A personal odyssey

Lawrence A. Boland

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Preface

One wonders who would ever choose to specialize in economic methodology when the economics discipline is so dominated by careerism, that is, by people who are more interested in furthering their careers than they are by either integrity or fortitude. Such a choice requires a minimum level of naivety. This is a requirement I easily met when I began an odyssey in the study of economic methodology.

My interests from the beginning concerned the methodological decisions one would have to make when building models in economics. I naively assumed that, of course, I would not be alone in such an essential area of study. Surely, every model builder has to think about how they are going to model economic theories. In retrospect I am reminded of a psychology professor I had at Bradley University, Dr Carl Smith. At the height of 1950s' prudishness, Dr Smith told us how he thought it was very strange that young women were expected to be expert cooks the day they got married yet they were being told they should not go into the kitchen or even think about cooking until they got married. Risky stuff, given the secret topic he was discussing. But it occurs to me now that his complaint applies equally to how we educate graduate students today. When they begin their careers as model builders, they must be experts concerning model-building methodology but they must never discuss methodology until they have tenure.

Thirty-two years ago, I was oblivious to careerism. When I faced my PhD thesis examining committee, the first question asked was, 'Is it true that if you are correct then all economic model building is on the wrong track?' I guess I was supposed to be humble and apologize for such an impression. Instead, my response was simply that if I was correct then it was not my problem. Similarly, when one of my Simon Fraser colleagues was needling me by noting that he was about to attend the committee meeting where my tenure case was to be considered and he was wondering whether I could pass the test, I told him that the question was whether the tenure committee could pass the test. Obviously, I was still oblivious to careerism.

After thirty-two years, I still find careerism unacceptable, but in these days of budget cuts and down-sizing I do have sympathy for people just entering our academic profession. And having seen referee reports that some of my younger colleagues have received recently, I think the economics discipline is in serious trouble. There is no accountability when it comes to referees' reports. There is no willingness on the part of journal editors to allow a broader approach to economics questions. If smart, intellectual graduate students had any idea of what they have to face in order to succeed as an academic economist, I am sure many would look for something else to do.

Even the notion of an academic discipline is doubtful. Economists have taken 'discipline' literally. Major journals will not entertain any paper that does not involve model building. Publishing today has become the research equivalent of painting by numbers. The reason is clear. Painting by numbers is safe for both the researcher and the referees. But that is today. Economics did not seem so narrow-minded when I began writing methodology papers. However, that perception may have been due to my naivety. Perhaps economics has always been narrow-minded.

Economics needs informed criticism. Particularly, it needs criticism of its narrow-minded approach to methodology. By informed criticism I mean criticism that appreciates what economic model builders do but at the same time maintains a detached perspective. Criticism has become an impossible task today if that criticism is directed at model-building techniques. If you wish to discuss the methodological ideas that must be addressed before one begins building a model (e.g. what theory of knowledge should one presume decision makers employ?), you will still face referees who will demand that you provide a model to discuss model building! Such formulas abound in the methods of economics but nobody is allowed to criticize them.

This apparent fear and loathing of critics is ironic for those like me who wish to see economics from the perspective of Karl Popper's view of science. Specifically, he said that what characterizes science is its emphasis on criticism. According to Popper, scientists go out of their way to foster criticism. Building barriers to criticism is unscientific behavior.

This leads me to note another symptom of my naivety. I seem to have thought that once I figured out some idea, it was reasonable for me to think that the idea would be obvious to everyone. I thought I had figured out Popper's view of science, so, when methodology seemed to achieve the status of a viable sub-discipline worthy of serious study in the 1980s and Popper's name kept appearing prominently in the literature, I thought

everyone understood Popper as I did. It seemed obvious to me that the beginners in economic methodology would want to join me in my efforts to provide informed criticism that could interest mainstream economists. Instead, there ensued numerous attacks on my work. At first, I dismissed these attacks as either youthful careerism or misdirected ideology. I responded too slowly to the realization that the Popper the beginners were talking about was not the same one I understood. My slowness caused a lot of unnecessary and irreparable rancor. Nevertheless, too many of that younger generation of methodologists still do not understand Popper or the importance of informed criticism.

In this book I will address this schism between those who understand Popper's view of science as I do and those who think that to succeed in methodology we must adopt a stance that makes criticism a spicy sauce rather than the meat of the discussion. Those of us who see criticism as the main dish spend our research on critical studies of theoretical and methodological questions that ought to be of interest to mainstream economists. Leaving aside the difficulties concerning the 'ought' of this statement, the immediate problem is that critical studies are not easy to categorize. This has been the problem for all of us who try to practice what, in Chapter 20, I call the Socratic-Popper view of intellectual activity. Personally, I find intellectual debates to be far more interesting than bland and boring model-building exercises. So in this book I address many of the debates and arguments I have had with traditional methodologists over the last twenty years. My purpose is to promote what I think are potentially fruitful lines of criticism. I have done this by gathering together some of my essays in critical methodology to demonstrate this approach.

Since each chapter is concerned with some aspect of criticism, the reader should note that every chapter begins with one or more quotations. In most cases these are essential parts of the debates as they present the essence of my opponents' views in the best light possible. In other cases, the quotations provide evidence of the relevant atmosphere, or of the behavior, that I discuss in the chapter.

Several people have read parts of this book and I thank them for their patience and their criticism. The provocative things that I may have said will undoubtedly yield criticisms from my friends and foes alike. I welcome the criticism. I hope we all learn by it.

L.A.B.
Burnaby, British Columbia
17 March 1996

Prologue

Criticism vs titillating methodology

Methodological writings in economics ... exhibit considerable diversity. There are the pronouncements of practitioners about how economics is done. ... Methodological argumentation is also used ... by critics of 'mainstream economic theory'. ... A third approach to methodology is much more explicit in the importance it accords to philosophy ... Practitioners of this approach include both philosophers of science who are interested in economics as an example of a science and economists who look towards the philosophy of science for insights about how science works. ...

In the last decade or so, work within the third tradition has grown dramatically. ... A recent bibliography of the field contains over 2,000 entries, and most of these were published within the last fifteen years.

Bruce Caldwell [1991a, pp. 95–6]

It is not ... contentious to observe that explicit methodological analysis and commentary are widely frowned upon in contemporary economics, especially by those working in the mainstream. ... An effective restraint on methodology ... is the clear reluctance of mainstream journals to publish much of it. ...

Of course, the phenomenon of the discouragement of methodology is not a uniform one; there are exceptions even within the contemporary mainstream. ... Not only do those who oppose methodology most vociferously unavoidably fall back upon it, albeit often only implicitly, in their own substantive contributions, but every now and again, even if mainly in coffee-room discussions or local seminars, they can be found putting forward arguments and defending positions that can only be described as overtly methodological/philosophical. ... [F]or example, Frank Hahn has both published on methodology ... and frequently entered into local methodological debate. Yet, his official position has long been one of opposition to training or study of such matters. ... [O]n the occasion of his retirement ... Hahn offers various 'reflections' which take the form of advice to young economists. Notably these include the recommendation to 'avoid discussion of "mathematics in economics" like the plague', and to 'give no thought at all to methodology'.

Tony Lawson [1994, p. 106]

In the 1970s I attended a conference where the organizers unknowingly assigned a methodology paper to a session that also was assigned a paper by a highly regarded mainstream economic theorist. After both papers were presented to the standing-room-only audience, virtually every question and comment was directed to the author of the methodology paper. The highly regarded mainstream economic theorist was visibly upset. And it was no wonder. Methodology as a subject of discussion can always be more interesting than dull, boring papers about the latest fads in mathematics-based model building techniques. The highly regarded mainstream economic theorist complained that methodology was mere titillation lacking substance. Until the 1980s, mainstream economists treated methodology as prudes treat sex. During the 1980s, methodology was allowed out of the closet. Methodology articles could be found occasionally in many leading journals. But, perhaps because of this exposure, methodology has lost its titillating appeal. Judging by recent complaints, the official line that captures the mainstream economist's attitude to methodology is 'no methodology please, we're economists'.

In the 1990s, methodologists are again complaining about not being openly appreciated by mainstream economists. According to the methodologists, they have important ideas that, if listened to, the mainstream economists would find useful in their everyday activities. But what are these useful ideas? Examining the many recent methodology articles published in the three or four journals devoted to economic methodology, I have found it difficult to find any methodological prescriptions or proscriptions – useful or otherwise. If mainstream economists are rejecting the study of methodology, it cannot be because methodologists are making outrageous prescriptions or proscriptions. Maybe it is simply that methodologists have nothing useful to say to mainstream non-methodologists. Maybe methodology is considered to be intellectual pornography and thus its widespread exposure in the 1980s has shown how dull and boring it can be, particularly when it is thought to be mere rhetoric, as some critics have recently claimed. Nevertheless, if methodology is ever to be useful, it will have to be seen to be useful by mainstream economists.

Methodology no longer has a titillating appeal among mainstream economists. Serious methodologists, of course, would never wish to be seen to be producers of intellectual pornography. Serious methodologists still think there are good, wholesome reasons to study methodology. Despite the good intentions of the serious methodologists, mainstream economists still reject any need to encourage the study of methodology. For methodologists to understand why the study of methodology is rejected, methodology's lack of titillating appeal will not suffice. So, what other

reasons might mainstream economists give for their rejection of any need to study methodology? In this book I will explore some of the possible reasons and then provide some suggestions for how methodology might be made more useful to mainstream economists.

METHODOLOGY'S DEMAND AND SUPPLY

As a teacher of mainstream microeconomic theory, I can understand why mainstream economists reject methodology studies. There is very little in methodology journal articles that one could say is obviously useful in the classroom. But just as obviously there must be a market for all of those 2,000 published methodology articles that Bruce Caldwell refers to. For this market, however, methodologists supply articles *about* methodology. Since the 1970s, the demanders in this market have been primarily other methodologists. For the most part, it has been methodology for methodology's sake.

The methodology market is dominated by three major suppliers, but not the three that Bruce has in mind. There are historians of economic thought who write about the methodological views of dead economists. There are philosophers of economics who worry about whether economics is a science. And there are the traditional economic methodologists who argue about whether assumptions always need to be realistic and, if not, how we should choose the best theory that uses unrealistic assumptions. Little of this is interesting to mainstream economists or even to some methodologists like me. Before discussing why mainstream economists might not find the product of these three suppliers interesting, let me state my objections to mainstream methodology.

All three groups share an undesirable characteristic – namely, intolerance toward other views. They each hold invited conferences – sometimes jointly, sometimes not, but either way they only invite those who will conform to the approved viewpoint (even though they will often deny the existence of a common viewpoint). For example, some historians of thought are most happy with methodologists who talk about whether there has been progress in economics or, if so, why. Philosophers are usually interested in questions concerning whether economic theoretical propositions are 'law-like', that is, similar to the law of gravity. Traditional methodologists seem willing to talk or listen to other methodologists only with regard to the various criteria proposed to solve the 'theory-choice' problem. Over the recent years, many of their meetings have been concerned with the question of whether the minimum condition for the 'best' theory is falsifiability. These kinds of meetings can be very boring. Few of these papers contain anything original. Too often, they are promoting their favorite philosopher of science as an authority for their methodological pronouncements.

Surely, one of the main reasons that mainstream economists deny any interest in such methodology articles is that traditional methodologists today address questions that only methodologists find interesting. Contrary to what I have said so far, it is always possible that the mainstream economists' rejection of methodology study is mere posturing. It could really be that mainstream economists are afraid of some conceivable methodology questions that might be asked other than ones that appeal to just methodologists. Let me illustrate.

In my theory seminars, where students present papers they have written, at the outset I always ask them, 'What problem do you think your paper will solve (or what question it will answer)?'. And after they have answered that question to the satisfaction of the members of the seminar, I ask them, 'Why do you think it is an interesting and worthy problem for the whole seminar to consider?'. Unavoidably answers to these questions are either purely methodological or can be easily shown by follow-up questions to be purely methodological. For economists in the mainstream, these two questions can be very troubling. The problem with Frank Hahn and other leaders of the mainstream is that they are afraid of where such questions will lead – such questions *might* show that their accumulated works are empty or a waste of time.

Of course, the customer is always right. So methodologists should stop complaining about the mainstream's rejection of the study of methodology and instead try to determine what kind of product the mainstream wants to buy. After all, pure methodology articles have been published in mainstream journals. One obvious example is my 1979 *Journal of Economic Literature* article about Friedman's famous methodology essay [1953]. Another is Bruce Caldwell's 1991 article in the same journal which was about understanding some of the views of a philosopher of science. And there is my 1981 *American Economic Review* article that was chastising critics of neoclassical theory who do not understand the role of the assumption of maximization. While it may be too early to assess the impact of Bruce's article, my articles seem to have changed how mainstream economists treat those two methodological issues. People no longer discuss neoclassical maximization by dismissing it as a mere untestable tautology. Similarly, people no longer dismiss Friedman's methodology as being antiquated 1930s logical positivism but instead tread very carefully. I think this is because mainstream economists who previously dismissed Friedman's methodology now recognize that there is a lot more in common between what he said and what modern econometrics-based economists do.

OUTLINE OF THE BOOK

Sorry to say, there is little of the three major suppliers of economic methodology that interests either me or most mainstream economists. This is not to say that methodology is inherently uninteresting. To the contrary, methodology can be very interesting if it is directed to helping those mainstream economists who are willing to take a critical look at economic theory. Taking a critical look does not necessarily entail the immediate necessity of rejecting all mainstream propositions. Taking a critical look at theory is central to my alternative to traditional methodology.

In this book I will demonstrate my alternative to traditional methodology with twenty critical essays. The primary intended audience is those would-be methodologists who, like me, wish to discuss questions that matter to economic theorists as well as teachers and researchers. Of course, I welcome mainstream economists who might like to see what substantive methodology can be.

These twenty essays include the two essays I mentioned above. Those two will be presented virtually as they originally appeared, with the exception of minor editorial modifications. All but one of the other essays are previously unpublished or, if published in some form before, have been rewritten to form a more coherent whole. However, complete coherence has not been the objective since many papers discuss similar ideas but from different perspectives. Differing perspectives usually contribute to an improved understanding. The essays are organized into three parts. Part I deals with my 1979 *Journal of Economic Literature* (*JEL*) article which is presented in Chapter 2. Chapter 1 discusses the environment in which my essay was launched. Chapters 3 and 4 discuss its aftermath. Part II deals with my 1981 *American Economic Review* (*AER*) article which is presented in Chapter 6. Chapter 5 discusses the environment in which this article was launched. Chapter 7 discusses its aftermath.

In Part III, I present my criticisms of traditional methodology. My criticisms will be not just that traditional methodology is mere titillation but that it is useless beyond its role in economic rhetoric. Chapter 8 sets the scene for the critical essays in Chapters 9 through 12. Part IV is my primary demonstration of ways that one can engage in methodological criticism that mainstream economists should be able to appreciate – even if they do not agree. Part V contains two articles that were the result of my frustrations of dealing with traditional methodologists who incorrectly identify Karl Popper's theory of science with so-called falsificationism. Chapter 19 discusses the outcome of a conference devoted to finding a Popperian legacy in economics. Chapter 20 attempts to educate traditional methodologists and convince them to abandon their 'falsificationist' view of Popper's theory of science. I close with an Epilogue that summarizes what I have learned over the last thirty years about the sociology of the sub-discipline of economic methodology.

Part I

Friedman's methodology essay

1 Economic methodology prior to 1979

> Pangloss taught metaphysico-theologo-cosmolo-nigology. He proved incontestably that there is no effect without a cause, and that in this best of all possible worlds, his lordship's country seat was the most beautiful of mansions and her ladyship the best of all possible ladyships. ...
>
> One day Cunégonde was walking near the house in a little coppice, called 'the park', when she saw Dr Pangloss behind some bushes giving a lesson in experimental philosophy to her mother's waiting-woman, a pretty little brunette who seemed eminently teachable. Since Lady Cunégonde took a great interest in science, she watched the experiments being repeated with breathless fascination. She saw clearly the Doctor's 'sufficient reason', and took note of cause and effect. Then, in a disturbed and thoughtful state of mind, she returned home filled with a desire for learning, and fancied that she could reason equally well with young Candide and he with her.
>
> Voltaire [*Candide*, Chapter 1]

When I began studying methodology in the early 1960s, there was little to read. There was, of course, the ubiquitous 1953 article by Milton Friedman and the ubiquitous textbook references to it. Being an aspiring methodologist, it is reasonable to think that my reading would have begun with this article, but for two reasons it did not. First, I had read Paul Samuelson's critique which convinced me that Friedman's methodology was 'wrong'. Second, coincidentally, a fellow graduate student told me of his experience with the *Journal of Political Economy* (*JPE*), where he had recently submitted a paper on methodology. He was told that without including a reference to Friedman's article, there was little chance of publication. In an immature, petulant state of mind, I vowed never to read Friedman's article – a vow that was kept for over ten years.

During those ten years my reading was devoted almost entirely to philosophy of science literature – not just any philosophy of science literature but exclusively that devoted to Karl Popper's views. My PhD thesis was an application of Popper's view to economic model building with virtually no reference to methodology of economics literature [see Boland 1989, Chapters 2 and 3]. My only motivation for the study of methodology was that I saw it as an avenue to the advancement of economic theory and model building. Beyond articles by disciples of Popper [e.g. Klappholz and Agassi 1959; and Agassi 1971a], there are two possible exceptions to the limited scope of my reading. One might be that I had a copy of Sherman Krupp's 1966 collection of essays on economic methodology, but since there was only one mention of Popper in this book, I basically dismissed it. Another exception might be my cursory examination of Fritz Machlup's 'The problem of verification in economics' [1955], but since it was concerned solely with verification and Popper had exposed the irrelevance of verification, I dismissed it too.

Petulance aside, if one was not interested in the grumblings and gossip surrounding Friedman's essay, there really was little to read about economic methodology in the 1960s and early 1970s. It was clear to me that everything substantive that could be said about methodology was clearly laid out in the opening chapters of most intermediate theory textbooks. Unless one could add to these, there was nothing more to say. All textbooks fell into two categories: in one group were those that made reference to Friedman.[1] A typical example – but the most muddled example – was Ferguson [1966/69, p. 6], who, using Machlup's article, identified Friedman, Samuelson and Maclup as 'logical positivists'. The other group includes those textbooks that did not mention methodology at all.[2] Moreover, the only thing in common with those that did mention methodology was a concern for the mainstay, the ubiquitous positive vs normative distinction.

Methodology in the 1960s and 1970s can easily be characterized by one question: whose side are you on, Friedman's or Samuelson's? Clearly, I was on Samuelson's side and, like so many others on this side, I always dismissed Friedman's view as some form of positivism or logical positivism.[3] To readers of Popper, the rejection of Friedman's view on these grounds was always easy since Popper had convincingly criticized logical positivism. Also, Popper's identification of positivism with failed attempts to solve the problem of induction meant that, methodologically speaking, Friedman was clearly on the wrong track.

It is easy to understand why someone might have thought that Friedman was advocating some form of positivism (logical or otherwise) – particularly easy for me given that I had not read Friedman's essay. After all, 'positive' was the most significant word in the titles of both his essay and his book. Identifying Friedman as a positivist was encouraged by the one chapter of Krupp's book that I did read, namely the essay by Martin Bron-

fenbrenner, who refers to 'Friedman and his fellow positivists' [1966, p. 14]. Following this party line, I continued to refer to Friedman as a logical positivist even though I used his reported statement about the 'realism of assumptions' as an example of 'instrumentalism'.[4] The issue, I always thought, was the alleged dichotomy between 'applied' and 'pure' theory. I had clearly identified Friedman with 'applied theory' and even used the television repairman as an example of an applied theorist (someone who might believe there are little men in the tubes or transistors), such that the truth of the repairman's understanding does not matter so long as he fixes the broken television. Obviously I had made the connection between Friedman's views and instrumentalism but, since I still had not read his essay, I held to the party line. In the fall of 1971 while visiting Cambridge, England, I tried to help one of my students, Stanley Wong, who was working on a paper about Samuelson's views of methodology (which was subsequently published in the 1973 *American Economic Review*). Prompted by my discussion with Stan concerning his proposed explanation of Friedman's essay, I finally made the explicit connection that Friedman's essay could be interpreted as an exact form of the instrumentalism that Popper had often criticized. Nevertheless, sticking with my vow, I still had not bothered to read Friedman's essay.

THE SAGA OF MY 1979 *JEL* PAPER

So, how did I come to write a paper about Friedman's famous essay? Well, Cliff Lloyd, my friend and colleague at Simon Fraser, taught a graduate theory class and often left the door open. In the summer of 1975 while walking by his class I overheard him explaining *his* view of why Friedman's methodology was all wrong. Like me, Cliff did not read a lot – his excuse was that his finger did not move fast enough. What I heard Cliff present to his students was merely the critique that Samuelson had published twelve years earlier. For some reason, this inspired me to think I could teach Cliff some methodology. I began by reading Friedman's essay. What I found was shocking. Apart from a vague reference to John Neville Keynes' distinguishing between positive and normative economics, there was nothing in Friedman's essay that could be considered a clear version of positivism or even logical positivism. Actually, Friedman's essay was more an argument *against* positivist methodologists.

I wrote up my paper and presented it to my methodology seminar that semester. On two occasions in the next two years I attempted to get it on the program of the meetings of the Canadian Economics Association in order to get some feedback and criticism. Both times it was rejected. Rarely are methodology papers accepted for the CEA meetings – and surely never would one be accepted that might be seen to defend Friedman in any way. So in March 1978 I decided to submit my paper to the home of Friedman's methodology, the Chicago School's *Journal of Political Economy*. Simultaneously, I sent a copy to Professor Friedman. By the end of April, I had received a long letter from Professor Friedman dated April 14 which began:

> Needless to say I was delighted to receive the paper that you sent me along with your letter of March 6th, 1978. I should add that I have done no systematic work on methodology since I wrote that essay. I have read all of the various critiques you referred to but never thought it appropriate to reply to them primarily because I really had nothing to add to what I had said in the essay and felt as you did that the criticisms derived from a misunderstanding of what it was I was trying to say and hence that readers could judge for themselves. I must admit that I was also deterred from doing so by the observation which impressed itself on me that there was essentially no relationship between an author's methodological views and his actual scientific work. As an instrumentalist, which you are entirely correct in describing me as, that suggests that investigations in methodology are not themselves a clearly useful activity for the purpose of affecting scientific conclusions, however useful they may be for other purposes. Nonetheless, my vanity is certainly delighted at having someone with your obvious ability and command of the subject write an answer to the various criticisms that have accumulated.

Two days later I received a letter from George Stigler who, as editor of the *JPE*, enclosed what he called 'a highly informal referee's report' which began with the following:

> Large chunks of [Boland's] verbiage strike me as empty of content. Not until his page 10 does he even get around to trying to spell out the 'instrumentalist' position that he imputes to Friedman and proposes to defend. I doubt that Friedman would welcome Boland's aid.

The anonymous referee was obviously not satisfied and thus concluded with the following:

> After forming my own judgment of Boland's paper but without expressing that judgment, I asked ***** to have a look at the paper. (***** is a graduate student who – *not* at my instigation – is writing an M.A. thesis on some aspects of methodology and has been studying Friedman's article and much of the other literature that Boland concerns himself with.) ... Boland, he says, misreads Friedman; it is far from clear that Friedman is an 'instrumentalist' in Boland's sense.

Without complaining about the meanness expressed in such a referee's report, I wrote back to Stigler enclosing a copy of the letter from Friedman and suggesting that my paper might be worthy of reconsideration. With no mention of Friedman's letter, Stigler replied: 'I have no difficulty in arriving again at my previous decision that the JPE is not interested in this paper or any reasonable revision of it.' So there!

While this was going on, Bob Clower came to give a seminar at Simon Fraser. I asked him to read my paper – which he did while waiting to give his seminar. Bob said he was interested in publishing it if I cut out the first ten pages. So there was still some hope. One of my colleagues, Don Gordon, suggested that I might send my paper to Mark Perlman, the editor of the *Journal of Economic Literature*. Don said that Mark was once a student of Stigler's at Columbia and might understand. I sent my paper with a copy of Friedman's letter to Mark, explaining the *Journal of Political Economy* saga. Eventually, Mark reported that his editorial board was split. One board member from an Ivy League school was opposed but a couple others were in favor. Subsequently, during the fall of 1978, Mark Blaug was asked to give his opinion regarding the split and he was mildly in favor and so Perlman decided to publish my paper in the next June issue.

It is interesting to note that of the seven critics of Friedman's essay discussed in my paper only one of them bothered to respond, namely Gene Rotwein. Nevertheless, there were many bystanders who were eager to respond. Most of them, however, were interested only in perpetuating the Friedman bashing that turned me off of methodology literature in the 1960s. In Chapter 2, I will present the entire 1979 *JEL* article. I have made only one small substantive adjustment to the text which was prompted by a letter from Rotwein complaining that I misrepresented his view at least in one small regard. In Chapter 3, I will discuss some of the published responses, including the ones which tried to elevate the discussion above the usual dull, but apparently titillating, game of Friedman bashing.

NOTES

1 For examples, see Leftwich 1966; Ferguson 1966/69; Mansfield 1970; Clower and Due 1972; Bilas 1967/71.
2 For examples, see Stigler 1966 and Gisser 1969.
3 See Boland 1969 and 1970.
4 See Boland 1971, p. 112.

2 Criticizing the critiques of Friedman's 1953 essay

> let us now turn towards an examination of methodological writings. ... [F]irst we have *grand methodology*, which takes place when a practicing economist, usually a prominent one, lays out a few key methodological principles ... and then shows that following these principles leads one to accept a certain group of theoretical constructs and to reject its rivals. The most famous example is Friedman (1953)...
> The second category for methodology, analogous to normal science, is the large *secondary literature* that has sprung up in response to the seminal works in methodology. To make a contribution here, one takes a particular position and criticizes it. ... The literature here is large; indeed, writing a commentary on Friedman or Samuelson is almost a rite of passage for those interested in making a contribution in this area. Just as normal science is derived from grand science, this secondary literature in methodology is also derivative. The difference between them is that while normal science seeks to extend the work of grand science, this secondary literature is almost always *critical* of the grand methodological pronouncements it takes as it subject. This leads to some strange results. For example, one reason that Larry Boland's (1979) paper ... aroused such passion is that he reversed the usual procedure. When he declared that '*Every* critic of Friedman's essay has been wrong', ... he was attacking the secondary literature, which was unprecedented. Many of his readers mistakenly transposed Boland's argument: they drew the faulty inference that a critique of Friedman's critics must also be a defense of Friedman. Actually, Boland's paper is one of the most subtle attacks on Friedman in the literature.
>
> Bruce Caldwell [1989, pp. 11–12]

Milton Friedman's essay 'The methodology of positive economics' [1953] is considered authoritative by almost every textbook writer who wishes to discuss the methodology of economics. Nevertheless, virtually all the journal articles that have been written about that essay have been very critical. This is a rather unusual situation. The critics condemn Friedman's

essay, but virtually all the textbooks praise it. Why should honest textbook writers ignore the critics? It will be argued here that the reason is quite clear. *Every* critic of Friedman's essay has been wrong. The fundamental reason why all of the critics are wrong is that their criticisms are not based on a clear, correct or even fair understanding of his essay. Friedman simply does not make the mistakes he is accused of making. His methodological position is both logically sound and unambiguously based on a coherent philosophy of science – instrumentalism.

In order to defend Friedman from his critics, I shall outline some necessary background knowledge – a clear understanding of the nature of logic and the philosophy of instrumentalism – and then present a reader's guide to his essay. Based on this background knowledge and the reader's guide, I shall survey and comment upon the major critics of Friedman's methodology. I shall conclude with a suggestion as to how a fair criticism would proceed.

THE USEFULNESS OF LOGIC

***Modus ponens*: logic's only useful property**

Artistotle was probably the first to systemize the principles of logic; most of them were common knowledge in his time. Logic has not changed much since then, although some presentations lead one to think that our logic is different. Modern writers too often discuss logic as if it had nothing to do with truth. But such a view of logic is an error. In Aristotle's view logic was the study of the principles of true and *successful* argument.[1]

Recognizing that arguments consist only of individual statements joined together with an 'and' or an 'or', Aristotle was concerned with determining what kinds of statements are admissible into logical arguments. He posited some rules that are in effect necessary conditions for the admissibility of statements into a logical argument. These rules, which later became known as the axioms or canons of logic, cannot be used to justify an argument; they can only be used to criticize or reject an argument on the grounds of inadmissibility.[2]

The only purpose for requiring arguments to be logical is to connect the truth of the premises or assumptions to the truth of the conclusions. Merely joining together a set of admissible statements does not necessarily form a logical argument; the only criterion for whether an admissible argument is logical is whether it is a sufficient argument in favor of its conclusions in the following sense. *If* your argument is logical, then whenever *all* of your assumptions (or premises) are true *all* of your conclusions will be true as well.

To prove that an argument is logical, one must be able to demonstrate its sufficiency. Whenever one establishes the logical sufficiency of a formal (or abstract) argument, one can use that formal argument as a part of a larger empirical (or contingent) argument that is *in favor* of the truth of any particular conclusion of the formal argument.[3] That is to say, whenever you offer an empirical argument in favor of some proposition, you are purporting both that the form of the argument is logically valid *and* that your assumptions are true. In this sense, logical validity is a necessary (but not sufficient) condition for an empirical argument to be true.

Using a formal argument in favor of the truth of any of its conclusions by arguing from the truth of its assumptions is said to be using the argument in the affirmative mode – or, more formally, in *modus ponens*. The ability to use any argument successfully in *modus ponens* is the primary necessary condition for the argument's logical validity or consistency (or, for short, its 'logicality'). However, this is not the only necessary condition for an argument's logicality. Whenever *modus ponens* is assured for a given argument, that argument can always be used in a denial or criticism of the truth of its assumptions. Specifically, *if* your argument is logical, then any time *any one* conclusion is false *not all* of your assumptions can be true (i.e. at least one assumption must be false).[4] Using this mode of argument against the truth of one's assumptions by arguing from the falsity of a conclusion is called *modus tollens*. Whenever one successfully criticizes an argument by using *modus tollens*, one can conclude that either an assumption is false *or* the argument is not logical (or both).

Beyond *modus ponens*

In order to distinguish *modus ponens* from its corollary *modus tollens*, not only must we explicitly refer to truth and falsity, but we must also specify the direction of the argument. Heuristically speaking, *modus ponens* 'passes' the truth *forward* from the assumptions to the conclusions.[5] *Modus tollens*, on the other hand, 'passes' the falsity *backward* from the conclusions to one or more of the assumptions.[6] The important point here, which I shall argue is implicitly recognized by Friedman in his essay, is that if one changes the direction (forward or backward) of either valid mode of using a logical argument, then the logicality of one's argument ceases to be useful or methodologically significant. Specifically, any use of *modus ponens* in *reverse* is an example of what logic textbooks call 'the Fallacy of Affirming the Consequent'. Similarly, any use of *modus tollens* in *reverse* is an example of what is called 'the Fallacy of Denying the Antecedent'. It is especially important to note that truth cannot be 'passed' backward nor can falsity be 'passed' forward.[7]

The major point to be emphasized here is that while the truth of assumptions and conclusions is connected in the use of a logical argument in *modus ponens*, the truth of the same assumptions and conclusions is not connected if they are used in *reverse modus ponens*. Similarly, their falsity is not connected when used in *reverse modus tollens*.

I think an explicit recognition of the two *reverse* modes of argument is essential for a clear understanding of Friedman's essay. Any methodological criticism which presumes that any formal argument that can be used in *modus tollens* can also be validly used in *reverse modus ponens* involves a serious methodological error. Recognition of this methodological error, an error which Friedman successfully avoids, is essential for an appreciation of his rejection of the necessity of testing (as I will show in the third section).

Objectives of an argument: necessity vs sufficiency

Finally, there is another aspect of the logicality of an argument that is reflected in Friedman's essay. It has to do with the 'necessity' and the 'sufficiency' of statements or groups of statements. In some cases one is more concerned with the sufficiency of an argument; in other cases one is more concerned with the necessity of its assumptions. To illustrate, consider the following *extreme* dichotomization. There are basically two different affirmative types of argument: the conjunctive and the disjunctive.

> *Conjunctive type of argument*: Because statement A_1 is true, *and* A_2 is true, *and* A_3 is true, *and* ..., one can conclude that the statement C_1 is true.

Axiomatic consumer theory might be an example of such an argument where the *A*s include statements about the utility function and the existence of maximization is the conclusion. On the other hand,

> *Disjunctive type of argument*: Because statement R_1 is true, *or* R_2 is true, *or* R_3 is true, *or* ..., one can conclude that the statement C_2 is true.

A politician's reasons for why he or she is the best candidate might be an example of this type of argument. These two ways of arguing can be most clearly distinguished in terms of what is required for a *successful refutation* of each type of argument. The conjunctive type of argument is the easiest to refute or criticize. Ideally, a pure conjunctive argument consists of assumptions *each of which is offered as a necessary condition*. It is the conjunction of *all* of them that is *just* sufficient for the conclusion to follow. If any one of the assumptions were false, then the sufficiency of the argument would be lost. To refute a pure conjunctive argument, one needs only to refute *one* assumption. The disjunctive argument, on the other hand, is very difficult to refute. Because in the extreme case such an argument, in effect, offers every assumption as a *solitarily sufficient condition* for the conclusion to follow, none of the assumptions are necessary. If someone were to refute only *one* of the assumptions, the argument would not be lost. In order to defeat a pure disjunctive argument, one must refute *every* assumption – clearly a monumental task.[8]

'INSTRUMENTALISM' AND THE RELATIONSHIP BETWEEN LOGIC, TRUTH AND THEORIES

The problem of induction

The discussion so far has not worried about how one knows the truth of the assumptions (or conclusions). Unfortunately, logic is of little help in determining the truth of a statement. Logic can only help by 'passing' along known truths. This limitation of traditional logic leads to a consideration of the so-called *problem of induction*: the problem of finding a *form* of logical argument where (a) its conclusion is a *general* statement, such as one of the true 'laws' of economics (or nature), or its conclusion is the choice of the true theory (or model) from among various competitors; and (b) its assumptions include *only* singular statements of *particulars* (such as observation reports). With an argument of this form one is said to be arguing inductively from the truth of particulars to the truth of generals. (On the other hand, a deductive form of argument proceeds from the truth of generals to the truth of particulars.) If one could solve the problem of induction, the true 'laws' or general theories of economics could then be said to be induced logically from the particulars. But not only must one solve the problem of induction, one must also acquire access to all the particulars needed for the application of the solution. Any 'solution' that requires an infinity of particulars is at best impractical and at worst an illusion. The requirement of an infinity of true particulars in order to provide the needed true assumptions for the application of *modus ponens* means in effect that such an inductive argument would not carry the force of *modus ponens*.

One might ask, just what determines whether or not a form of argument is logical? But I have already discussed this question above. As noted in the first section, the criterion or necessary condition for any logical argument is that it must be capable of fulfilling the promise of *modus ponens*. However, as far as anyone knows *modus ponens* is assured only by a 'deductive' form of argument.

'Inductivism'

One can identify (at least) three different views of the relationship between logic, truth and theories. The 'inductivists' say that theories can be true and all true theories (or assumptions) are the result of applying inductive logic to observations. 'Conventionalists' deny that a theory can be inductively proven, and they furthermore consider it improper to discuss the truth status of a theory. 'Instrumentalists', such as Friedman, are only concerned with the usefulness of the conclusions derived from any theory. Unlike conventionalists, instrumentalists may allow that theories or assumptions can be true but argue that it does not matter with regard to the usefulness of the conclusions.

A clear understanding of inductivism, I think, is essential for the appreciation of every modern methodological point of view. Even when economists only argue deductively (that is, by using *modus ponens* and including assumptions that are necessarily in the form of general statements), it might still be asked, how do they know that the 'laws' or other general statements used are true? The inductivist philosophers have always taken the position that there is a way to prove the truth of the needed general statements (as conclusions) using only assumptions of the form of singular statements (e.g. observations). Such inductivists often think the only problem is to specify which kinds of singular statements will do the job, that is, those which are unambiguously true and capable of forming a sufficient argument for the truth of a given statement or conclusion.

What kinds of statements must economists rely on? Clearly, biased personal reports will not do even if their conjunction could be made to be sufficient. For this reason inductivist philosophers and many well-known economists (following John Neville Keynes) distinguish between 'positive' statements, which can be unambiguously true, and 'normative' ones, which cannot. Singular positive statements would supposedly work because they can be objectively true. But normative statements are necessarily subjective, hence they would not carry the same logical guarantee of unambiguous truth.

Contrary to the hopes of the inductivists, even though one can distinguish between positive and normative statements, there is no inductive logic that will guarantee the sufficiency of any finite set of singular statements. There is no type of argument that will validly proceed from assumptions that are singular to conclusions that are general statements. Specifically, there is no conjunction of a *finite* number of true singular statements from which unambiguously true general statements will validly follow with the assurance of *modus ponens*. Thus, distinguishing between positive and normative statements (as most economists do today) will not by itself solve the problem of induction;[9] and for this reason Friedman tries to go *beyond* this distinction.

The 'conventionalist' alternative to inductivism

Since no one has yet solved the problem of induction, one is always required to assume the truth of his or her premises or assumptions. In response to the failure to solve the problem of induction, some philosophers and economists go as far as to avoid using the word 'truth' at all. They may, however, attempt to determine the 'validity' of a theory or argument, since logic can (at least) help in that determination. Too often, many economists who are unaware of these methodological problems create much confusion by using the word 'validity' when they mean 'truth' [e.g. see Friedman 1953, pp. 10ff.]. Their formal alternative to avoiding the word 'truth' is to take the position that 'truth' is a matter of convention; philosophers who take such a position are thus called 'conventionalists'. They view theories as being convenient catalogues or 'filing systems' for positive reports. Of course, catalogues cannot be properly called true or false. They are to be judged or compared only by criteria of convenience such as simplicity or degrees of approximation or closeness of 'fit', etc.

Conventionalism forms the foundation for most methodological discussions in economics today (e.g. which criterion is best, simplicity or generality?). It is also the primary source of methodological problems because its usual application is built upon a fundamental contradiction. Conventionalists presume that it is possible to discuss logical validity without reference to truth or falsity. Yet, as noted above, the fundamental aspect of logic that defines 'validity' (namely, the assurance of *modus ponens* or *modus tollens*) requires an explicit recognition of (a concept of) truth or falsity.[10] Conventionalism does not offer a solution to the problem of induction; it only offers a way to avoid discussing such philosophical obstacles. Although Friedman accepts and employs several conventionalist concepts, to his credit he constructs a methodological approach that goes beyond the sterile philosophy of conventionalism.

Instrumentalism and the usefulness of logic

For the purposes of discussing Friedman's point of view, one can consider any theory to be an argument in favor of some given propositions or towards specific predictions. As such a theory can be considered to consist only of a conjunction of assumption statements, that is, statements, each of which is *assumed* (or asserted) to be true. In order for the argument to be sufficient it must be a deductive argument, which means that at least some

of the assumptions must be in the form of general statements. But, without an inductive logic, this latter requirement seems to raise in a modified form the methodological problems discussed above. When can one assume a theory is true? It is such difficulties that Friedman's essay attempts to overcome.

So long as a theory does its intended job, there is no apparent need to argue in its favor (or in favor of any of its constituent parts). For some policy-oriented economists, the intended job is the generation of true or successful predictions. In this case a theory's predictive success is always a sufficient argument in its favor. This view of the *role* of theories is called 'instrumentalism'. It says that theories are convenient and useful ways of (logically) generating what have turned out to be true (or successful) predictions or conclusions. Instrumentalism is the primary methodological point of view expressed in Friedman's essay.

For those economists who see the object of science as finding the *one* true theory of the economy, their task cannot be simple. However, if the object of building or choosing theories (or models of theories) is only to have a theory or model that provides true predictions or conclusions, *a priori* truth of the assumptions is not required *if* it is already known that the conclusions are true or acceptable by some conventionalist criterion.[11] Thus, theories do not have to be considered true statements about the nature of the world, but only convenient ways of systematically generating the already known 'true' conclusions.

In this manner instrumentalists offer an alternative to the conventionalist's response to the problem of induction. Instrumentalists consider the truth status of theories, hypotheses or assumptions to be irrelevant for any practical purposes so long as the conclusions logically derived from them are successful. Although conventionalists may argue about the nature or the possibility of determining the truth status of theories, instrumentalists simply do not care. Some instrumentalists may personally care or even believe in the powers of induction, but such concern or belief is considered to be separate from their view of the role of theories in science.

For the instrumentalists, who think they have solved the problem of induction by ignoring truth, *modus ponens* will necessarily be seen to be irrelevant. This is because they do not begin their analysis with a search for the true assumptions but rather for true or useful (i.e. successful) conclusions. *Modus tollens* is likewise irrelevant because its use can only begin with false conclusions. This also means that like the pure disjunctive argument, the instrumentalist's argument is concerned more with the sufficiency of any assumptions than with their necessity. This is because any analysis of the sufficiency of a set of assumptions begins by assuming the conclusion is true and then asks what set of assumptions will do the logical job of yielding that conclusion. Furthermore, any valid or fair criticism of an instrumentalist can only be about the sufficiency of his or her argument. The only direct refutation allowable is one that shows that a theory is insufficient, that is, inapplicable. Failing that, the critic must alternatively provide his or her own sufficient argument, which does the same job.

By identifying three distinct philosophical views of theories, I am not trying to suggest that one must choose one (that would merely be reintroducing the problem of induction at a new level). Few writers have ever thought it necessary to adhere to just one view. Most writers on methodology in economics make some use of each view. For this reason it is sometimes necessary to sort out these views in order to make sense of methodological essays. I hope to show that even a superficial understanding of these philosophical views will help form a clear understanding of Friedman's 1953 essay.

A READER'S GUIDE TO FRIEDMAN'S ESSAY

An overview

Friedman's 1953 essay is rather long and rambling. However, he does manage to state his position regarding all of the issues I have discussed so far. Because the essay is long, it is hard to focus on its exact purpose, but I think it can best be understood as an instrumentalist's argument for instrumentalism. As such it tries to give a series of sufficient reasons for the acceptance of instrumentalism. And furthermore, it can be fairly judged only on the basis of the adequacy or sufficiency of each reason for that purpose. We are told that the essay's motivation is to give us a way to overcome obstacles to the construction of a 'distinct positive science' centering on the problem of 'how to decide whether a suggested hypothesis or theory should be tentatively accepted as part of the "body of systemized knowledge [of] ... what is"' [p. 3]. The 'distinct positive science', we are told, is essential for a policy science [pp. 5–7]. This methodological decision problem is, in fact, an inductivist's problem.[12] Implicitly Friedman recognizes that we do not have an inductive logic [p. 9], and he offers what he considers to be an acceptable alternative. Basically Friedman's solution (to the problem of induction) is that our acceptance of a hypothesis for the purposes of policy application should be made a matter of 'judgement'. Judgements, he says, cannot be made *a priori* in the absence of a true inductive science.

'Positive vs normative economics': the problem of induction in instrumentalist terms

In the introduction Friedman expresses his interest in the problem of induction and then, in Section I, he restates the problem in instrumentalist terms. He says the task of positive economics is to

> provide a system of generalizations that can be used to make correct predictions about the consequences of any change in circumstances. Its performance is to be judged by the precision, scope, and conformity with experience of the predictions it yields. [p. 4]

The inductivist's distinction between positive and normative statements is the most important part of inductivism that is retained by Friedman. And he brings with that distinction the inductivist's claim that normative economics depends on positive economics, but positive economics does not necessarily depend on the normative [p. 5]. In this light he notes that even methodological judgements about policy are also positive statements to be accepted on the basis of empirical evidence [pp. 6–7].

'Positive economics': conventionalist criteria used with an instrumentalist purpose

Friedman begins Section II with a mild version of conventionalism by saying that a theory (i.e. a set of assumptions) can be viewed as a language whose

> function is to serve as a filing system for organizing empirical material ... and the criteria by which it is to be judged are those appropriate to a filing system. [p. 7]

But his viewing a theory as a language has its limitations. I would think that a distinguishing feature of all languages is that they are intended to be both consistent and complete (e.g. there should be nothing that cannot be named or completely described); and this would preclude empirical applications as the theory would, in effect, yield only tautologies. To avoid this he adopts the now popular opinion that we must add 'substantive hypotheses' [p. 8]. But here he again raises an inductivist's problem: how do we choose the substantive hypotheses? Friedman answers that positive statements ('factual evidence') can determine acceptance. He clearly indicates that he does understand the fundamentals of logic by implicitly using *modus tollens*. He says that a 'hypothesis is rejected if its predictions are contradicted' [p. 9]. But what about *modus ponens*? Well, that is considered inapplicable because there is no inductive logic. Friedman, using the word 'validity' when he means 'not inconsistent with facts' (which happens to be a necessary condition of true hypotheses), says:

> The validity of a hypothesis in this sense is not by itself a sufficient criterion for choosing among alternative hypotheses. Observed facts are necessarily finite in number; possible hypotheses, infinite. [p. 9]

In other words, one cannot directly solve the problem of induction.

All this means that the main task of a positive economics is left unfulfilled. At this point Friedman says that we need additional criteria (beyond consistency with the facts) if we are going to be able to choose [p. 9]. Here he poses the problem of choosing between *competing* hypotheses or theories, *all* of which have already been shown to be consistent with available positive evidence (that is, none of them have been shown to be false using *modus tollens*). The criteria with which he claims there is 'general agreement' are the 'simplicity' and the 'fruitfulness' of the substantive hypotheses [p. 10].[13] However, these are not considered to be abstract philosophical (i.e. conventionalist) criteria but rather they, too, are empirically based, hence can be expressed in instrumentalist terms: 'simpler' means requires less empirical 'initial knowledge' (the word 'initial' refers here to the process of generating predictions with something like *modus ponens*). 'More fruitful' means more applicable and more precise [p. 10]. The possibility of a tradeoff is not discussed.

Friedman explicitly rejects the necessity of requiring the 'testing' of substantive hypotheses before they are used simply because it is not possible. But here it should be noted that his rejection of testing is partly a consequence of his use of the word 'testing'. Throughout his essay 'testing' always means 'testing for truth (in some sense)'. It never means 'testing in order to reject' as most of his critics seem to presume. That is, for Friedman a *successful* test is one which shows a statement (e.g. an assumption, hypothesis or theory) to be true; and, of course, a minimum condition for a successful test is that the statement not be inconsistent with empirical evidence [see pp. 33–4].[14]

Appreciating the success orientation of Friedman's view is essential to an understanding of his methodological judgements. For Friedman, an instrumentalist, hypotheses are chosen because they are successful in yielding true predictions. In other words, hypotheses and theories are viewed as instruments for successful predictions. It is his assumption that there has been a prior application of *modus tollens* (by evolution, see [p. 22]), which eliminates unsuccessful hypotheses (ones that yield false predictions), and which allows one to face only the problem of choosing between successful hypotheses. *In this sense*, his concentrating on successful predictions precludes any further application of *modus tollens*.

And similarly, any possible falsity of the assumptions is thereby considered irrelevant. Such a consideration is merely an appreciation of the logical limitations of what I above called *reverse modus tollens*. And since he has thus assumed that we are dealing exclusively with successful predictions (i.e. true conclusions), nothing would be gained by applying *modus ponens* either. This is a straightforward appreciation of the limitations of what I called *reverse modus ponens*. Knowing for sure that the hypotheses (or assumptions) are true is essential for a practical application of *modus ponens*, but such knowledge, he implies, is precluded by the absence of an inductive logic [pp. 12–14].

By focusing only on successful hypotheses, Friedman correctly reaches the conclusion that the application of the criterion of 'simplicity' is relevant. He says there is virtue in a simple hypothesis *if* its application requires less empirical information. One reason a simple hypothesis can require less information, Friedman says, is that it is descriptively false [pp. 14–15]. (For example, a linear function requires fewer observations for a fit than does a quadratic function.) This raises the question of 'unrealistic' descriptions versus 'necessary' abstractions. Friedman explicitly recognizes that some economists (presumably, followers of Lionel Robbins) hold a view contrary to his. For them the 'significance' of a theory is considered to be a direct result of the descriptive 'realism' of the assumptions. But Friedman claims that

> the relation between the significance of a theory and the 'realism' of its 'assumptions' is almost the opposite. ... Truly important and significant hypotheses will be found to have 'assumptions' that are wildly inaccurate descriptive representations of reality, and, in general, the more significant the theory, the more unrealistic the assumptions (in this sense). [p. 14]

Clearly, this latter judgement is based on the additional criteria of importance and significance that presume a purpose for theorizing: namely, that theories are only constructed to be instruments of policy. Those economists who do not see policy application as the only purpose of theorizing can clearly argue with that judgement. But nevertheless, in terms of the economy of information, his conclusion is still correct with respect to choosing between *successful* hypotheses that are used as policy instruments.

'Realism of assumptions' vs the convenience of instrumentalist methodology

In his Section III, Friedman continues to view successful 'testing' to be 'confirming', and for this reason he concludes that testing of assumptions is irrelevant for true conclusions (since *modus ponens* cannot be used in reverse). Having rejected the necessity of testing for the truth of assumptions, Friedman examines the question of the relevance of the falsity of assumptions for the various uses of theories. That is, what if one could show that an assumption is false? Does it matter? Friedman argues again [p. 18] that the falsity of the assumptions does not matter *if the conclusions are true*. He correctly says: one can say there must be an assumption that is false *whenever* some particular conclusion is false (*modus tollens*), but one cannot say any assumptions are true *because* any conclusion is true (*reverse modus ponens*, again) [p. 19].

This leads Friedman to discuss the possibility that a false assumption might be applied as part of an explanation of some observed phenomenon. Here he introduces his famous version of the 'as if' theory of explanation. He says that as long as the observed phenomenon can be considered to be a logical conclusion from the argument containing the false assumption in question, the use of that assumption should be acceptable. In particular, if we are trying to explain the *effect* of the assumed behavior of some individuals (e.g. the demand curve derived with the assumption of maximizing behavior), *so long as the effect is in fact observed and it would be the effect if they were in fact to behave as we assume*, we can use our behavioral assumption even when the assumption is false. That is, we can continue to claim the observed effect of the individuals' (unknown but assumed) behavior is *as if* they behaved as we assume. Note carefully, the individuals' *behavior* is not claimed to be *as if* they behaved as we assume, but rather it is the *effect* of their behavior that is claimed to be *as if* they behaved according to our assumption. Failure to distinguish between the effect and the behavior itself has led many critics to misread Friedman's view. His view does not violate any logical principles in this matter.

So far the choice between competing hypotheses or assumptions has been discussed with regard to currently available observations, that is, to existing evidence. But a more interesting question is the usefulness of any hypothesis in the future; past success will not guarantee future success. This presents a problem for the methodological conclusions that Friedman has, for the most part, presented correctly up to this point. He offers some weak arguments to deal with this problem. The first is an adaptation of a Social-Darwinist view that repeated success in the face of competition temporarily implies satisfaction of 'the conditions for survival' [p. 22]. Unfortunately, he does not indicate whether these are necessary conditions, which they must be if his argument is to be complete. He adopts another Social-Darwinist view, which claims that past success of our theory is relative to other competitors, thereby claiming a revealed superiority of our theory. This unfortunately presumes either that the other theories have not

survived as well or that the comparative advantage cannot change. The former presupposition, however, would be ruled out by his prior commitment to discussing the problem of choosing between successful theories [p. 23]. The latter merely begs the question. Finally he unnecessarily adds the false conventionalist theory of confirmation that says the absence of refutation supports the (future) truth of a statement [pp. 22–3].

The 'positive aspects of assumptions' are positive aspects of instrumentalist methodology

If assumptions do not need to be true, why would one bother worrying about them? Or, in other words, what role do assumptions play? Friedman says their role is positive [p. 23]. Assumptions: (a) are useful as an 'economical mode' of expressing and determining the state of the 'givens' of a theory – that is, the relevant facts – in order to provide an empirical basis for the predictions; (b) 'facilitate an indirect test' of a hypothesis of a theory by consideration of other hypotheses that are also implied; and (c) are a 'convenient means of specifying the condition under which the theory is expected' to be applicable.

Friedman is not very careful about distinguishing between assumptions, hypotheses and theories, and to make matters worse, in his Section IV he introduces the concept of a model. This can present some difficulty for the careful reader. Inductivist methodology posits significant differences between assumptions, hypotheses, theories and some other things that are called 'laws'. The inductivist's distinctions are based on an alleged difference in the levels of inductive proofs of their truth. Assumptions are the least established and laws are the most. Without committing oneself to this inductivist tradition, one can easily see hypotheses as intermediate conjunctions formed by using only part of the assumptions of a theory. For example, the theory of the consumer entails certain hypotheses about the slope of the demand curve, but the assumptions of the theory of the consumer are only part of our market theory of prices. Moreover, the assumptions and hypotheses of consumer theory are independent of the theory of the firm.

Discussing models raises totally new issues. A model of a theory is a conventionalist concept. As Friedman correctly puts it, 'the model is the logical embodiment of the half-truth' [p. 25]. Models in his sense correspond to the concept of models used in engineering. When one builds a model of something, one must simplify in order to emphasize the essential or significant features. Such simplification can always be seen to involve extra assumptions about the irrelevance of certain empirical considerations. These extra assumptions are usually descriptively false.

Most simplifying assumptions are designed to exclude certain real-world complications or variables. Such exclusion also reduces the need for information concerning those variables when one wishes to apply the model. In this sense, assumptions are economical in terms of the amount of prior information required for empirical application.

Friedman notes that the problem of choosing models can be seen as a problem of explaining when the model is applicable. To solve the latter version of this problem, he says that to any model of a theory or hypothesis one must add 'rules for using the model' [p. 25]. These required rules, however, are not mechanical. He says that 'no matter how successful [one is in explicitly stating the rules] ... there inevitably will remain room for judgement in applying the rules' [p. 25]. Unfortunately, the 'capacity to judge' cannot be *taught*, as each case is different (another instance of the problem of induction). However, it can be *learned*, 'but only by experience and exposure in the "right" scientific atmosphere' [p. 25] (this is a version of conventionalism). This seems to bring us back to the inductive problem that his version of instrumentalism was intended to solve.

In spite of all the discussion about 'assumptions', Friedman cautions us not to put too much emphasis on that word. By saying there are problems concerning judgements about the applicability of certain assumptions of particular hypotheses or theories, we are not to be misled into thinking there is some special meaning to the term 'assumption'. The assumptions of one hypothesis may be the conclusions of a (logically) prior set of assumptions. In other words, when one says a statement is an assumption, one is not referring to any intrinsic property. A statement is called an assumption because that is how one chooses to use it. There is nothing that prevents one from attempting to explain the assumed 'truth' of one's assumption by considering it to be a conclusion of another argument, which consists of yet another set of assumptions.[15] Moreover, the popular notion of a 'crucial assumption' is likewise relative to the particular model in which it is being used.

In the last part of his Section IV, Friedman faces an alleged problem that may be created by the dismissal of the testability (i.e. confirmability) of assumptions. The set of conclusions of any argument must contain the assumptions themselves. In some cases, within some subsets of assumptions and conclusions of a given theory there is interchangeability. In these cases dismissing testability of assumptions can seem to mean that the testability of some conclusions has been dismissed as well. Recall, however, that testing for Friedman still means confirming. Thus, if one considers the testing of an assumption one can, in effect, be seen to be considering merely the confirming of one of the conclusions. Friedman's emphasis on true (successful) conclusions is seen to be playing a role here, too. Of

course, there are other conclusions besides the assumptions themselves. However, someone may propose a set of assumptions only because *one* of the (true or observed) conclusions of interest is a logical consequence of that set. If one bothers to use the proposed assumptions to derive other conclusions from these assumptions, one can try to confirm the additional conclusions. In this sense, the assumptions used to derive one conclusion or hypothesis can be used to 'indirectly test' the conclusion of interest. Nevertheless, logic does not permit one to see the confirmation of the secondary conclusion as a direct confirmation of the conclusion of interest. The significance of such an indirect test is also a matter of judgement [p. 18].

'Economic issues' or some examples of instrumentalist successes

Finally, in his Section V, Friedman applies his methodological judgements to some specific examples, but here he does not raise any new questions of methodology. His objective seems to be merely to provide a demonstration of the success of instrumentalist methodology with several illustrations. Note that such a line of argument is quite consistent with instrumentalism and its compatibility with the disjunctive form of argument.

THE CRITICS

Friedman's paper elicited a long series of critiques, none of which dealt with every aspect of his essay. The primary motivation for all of the critics seems to be that they disagree with particular things Friedman said. I will argue here that the basis for each of the critiques is a misunderstanding and hence each involves a false accusation.

Testability vs refutability: Koopmans

Most misunderstandings are the result of Friedman's 'Introduction', where he seems to be saying that he is about to give another contribution to the traditional discussion about the methodology of inductivism and conventionalism. Such a discussion would usually be about issues such as the verifiability or refutability of truly scientific theories. What Friedman actually gives is an alternative to that type of discussion. Unfortunately, most critics miss this point.

In regard to the traditional discussion, Tjalling Koopmans says that the object of our attempts to develop or analyze the 'postulational structure of economic theory' is to obtain 'those implications that are verifiable or otherwise interesting' [Koopmans 1957, p. 133]. In this light, Koopmans says that one must distinguish between the logical structure of a theory and the 'interpretation' of its terms. He says that the logical structure's validity is considered to be independent of the interpretations (Koopmans is using the term 'validity' correctly, but it does not correspond to Friedman's usage). He says, 'from the point of view of the logic of the reasoning, the interpretations are detachable. Only the logical contents of the postulates matter' [p. 233]. When any argument is logically valid, no interpretation can lead to a contradiction. (This is one interpretation of *modus ponens*.) One way to view the testing of an argument is to see a test as one interpretation of the terms such that a conjunction of the argument and the specific interpretation in question forms an empirical proposition about the real world, which does or does not correspond to our observations.

Koopmans also says a 'distinction needs to be made here between *explanatory* and *normative* analysis' [p. 134]. Here Koopmans explicitly equates *positive* with *explanatory*. He adds that

> these two types of analysis do not necessarily differ in the interpretations placed on the terms. They differ *only* in the motivation of the search for conclusions. ... In explanatory analysis, what one looks for in a conclusion or prediction is the possibility of testing, that is, of verification or refutation by observation. Of course, the interpretations of the terms used in the postulates form the connecting link through which observation is brought to bear on the statements that represent conclusions. Verification, or absence of refutation, lends support to the set of postulates taken as a whole. [ibid., emphasis added]

Now Friedman clearly does not agree with this distinction since he argues that how one views the parts of a theory depends on its use and that a theory cannot be analyzed independently of its use. Also, Koopmans' statement seems to suggest that priority should be given to testing conclusions. Friedman need not agree. Since Friedman's analysis begins with *successful* conclusions, testing is precluded because it is automatically implied by the usefulness and the logicality of the explanation.

Starting with a different concept of theorizing – that is, that theories are directly analyzable independently of their uses – Koopmans proceeds to criticize Friedman by restating Lionel Robbins' methodological position [Robbins 1935]. The basic concern for Koopmans (but not Friedman) is the sources of the basic premises or assumptions of economic theory. For the followers of Robbins, the assumptions of economic analysis are promulgated and used *because* they are (obviously) true. The truth of the assumptions is never in doubt. The only complaint Koopmans brings against Robbins is that his assumptions were a bit vague – a problem that Koopmans

thinks can be solved with the use of sophisticated mathematics. The primary virtue of Koopmans' work is that it does try to solve that problem. Implicitly, both Robbins and Koopmans see the process of economic theorizing as merely the task of applying exclusively *modus ponens* and *modus tollens*. In particular, the sole purpose of developing a theory is so that one can 'pass' the obvious truth of the assumptions on to some conclusions.

Koopmans seems to object to Friedman's dismissal of the problem of clarifying the truth of the premises – the problem that Koopmans wishes to solve. Friedman's view is that (*a priori*) 'realism' of assumptions does not matter (i.e. *modus ponens* is not applicable). The source of the disagreement is Koopmans' confusion of *explanatory* with *positive*. Koopmans is an inductivist, who defines successful explanation as being logically based on observably true premises, that is, ones that are in turn (inductively) based on observation. Friedman does not consider assumptions or theories to be the embodiment of truth but only as instruments for the generation of useful (because successful) predictions. Thus, for Friedman *positive* is not equivalent to *explanatory* because he does not use *modus ponens*. Explanation in Koopmans' sense is irrelevant in Friedman's instrumentalism.

In order to criticize Friedman's argument against the concern for the 'realism' of assumptions, Koopmans offers an *interpretation* of his own theory of the logical structure of Friedman's view. Koopmans says:

> Since any statement is implied by itself, one *could* interpret Professor Friedman's position to mean that the validity or usefulness of any set of postulates depends on observations that confirm or at least fail to contradict (although they could have) *all* their implications, immediate and derived. [1957, p. 138, first emphasis added]

He then goes on to claim that this interpretation of Friedman's argument leads to some objectionable conclusions and thus claims to destroy Friedman's argument. The details of this line of argument do not matter here, since Koopmans' argument itself can be shown to be irrelevant and thus of no logical value.

Koopmans' interpretation contradicts Friedman's purpose (that *some* conclusions be successful – not necessarily *all*). Remember that Friedman is only concerned with the *sufficiency* of a theory or set of assumptions. He would allow any theory to be even more than 'just' sufficient[16] so long as it is sufficient for the successful predictions at issue. On the other hand, Koopmans' interpretation falsely presumes a concern for *necessity*. In other words, Koopmans' theory of Friedman's view is itself void because (by his own rules) at least one of its assumptions is false. Or, also by Koopmans' own rules (*modus tollens*), his own theory of Friedman's view must be considered refuted, since the false assumption is also one of the conclusions. His theory is not 'realistic' even though some of his conclusions may be. There is nothing in the application of *modus tollens* to a specific interpretation (which necessarily involves additional assumptions – e.g. rules of correspondence) that would require the rejection of Friedman's view itself.[17]

Necessity of verifying assumptions: Rotwein

Some economists would accept the obviousness of the premises of economic theory. In this group would fall the self-proclaimed 'empiricists'. The basis of their philosophy is the view that the truth of one's conclusions (or predictions) rests *solely* (and firmly) on the demonstrable truth of the premises; and the prescription that one *must* so justify every claim for the truth of one's conclusions or predictions. Needless to say, empiricists do not see a problem of induction. Friedman clearly does, and in this sense he is not an orthodox empiricist (even though the term 'positive' usually means 'empirical'). According to Eugene Rotwein, Friedman criticizes their view by claiming that it represents 'a form of naive and misguided empiricism' [Rotwein 1959, p. 555]. Actually, Rotwein sees his criticism as a family dispute amongst empiricists. What is questioned is

> Friedman's contention ... that the 'validity' of a 'theory' is to be tested *solely* by its 'predictions' with respect to a given class of phenomena, or that the question of whether or to what extent the assumptions of the 'theory' are 'unreal' (i.e. falsify reality) is of no relevance to such a test. [p. 556]

(Note that Friedman was not discussing the 'validity of theories' but rather the validity of 'hypotheses' used in a model of a theory.)[18]

Now it seems to me there is 'good' and 'bad' naivety. Good naivety is exemplified by the little boy in Andersen's story 'The Emperor's New Clothes'. Good naivety exposes the dishonesty or ignorance of others. Friedman simply refuses to join in the pretense that there is an inductive logic, one that would serve as a foundation for Rotwein's verificationist-empiricism. Rotwein attempts to twist the meaning of 'validity' into a matter of probabilities so that he can use something like *modus ponens* [p. 558]. But *modus ponens* will not work with statements whose truth status is a matter of probabilities, and thus Friedman is correct in rejecting this approach to empiricism. Rotwein's arguments are on a far weaker foundation than are Friedman's. It is, in fact, Rotwein's view that is naive, since it is based on an unfounded belief that science is the embodiment of truths based (inductively) on true observations, which are beyond doubt, or on true hypotheses, which can be inductively proven.

Testability as refutability: Bear and Orr and Melitz

Some sophisticated and friendly critics of Friedman's methodology choose to criticize only certain aspects while accepting others. This can lead to criticisms that are necessarily invalid. For example, Donald Bear and Daniel Orr dismiss Friedman's instrumentalism, yet they recommend what they call his 'as if' principle [Bear and Orr 1967]. They recommend 'as if' because they too accept the view that the problem of induction is still unsolved. They are correct in appreciating that the principle is an adequate means of dealing with the problem of induction.

That it is possible to accept one part of Friedman's methodology while rejecting another does not necessarily create a contradiction. The appreciation of such a possibility is facilitated by recalling that each part of Friedman's argument is designed to be sufficient. In this vein, Bear and Orr claim that Friedman's arguments against the necessity of testing and against the necessity of 'realism' of assumptions are both wrong. Bear and Orr (agreeing with Jack Melitz) say that Friedman erred by 'confounding ... abstractness and unrealism' [1967, p. 188, fn. 3]. And they further claim, 'all commentators except Friedman seem to agree that the testing of the whole theory (and not just the predictions of theory) is a constructive activity' [p. 194, fn. 15].

These criticisms are somewhat misleading because Friedman's concept of testing (sc. verifying) does not correspond to theirs. It is not always clear what various writers mean by 'testing', mostly because its meaning is too often taken for granted. One can identify implicitly three distinct meanings as used by the authors under consideration. Where Friedman sees testing only in terms of verification or 'confirmation', Bear and Orr adopt Karl Popper's view that a successful test is a refutation [Bear and Orr 1967, pp. 189ff.]. But Melitz sees testing as confirmation or disconfirmation [Melitz 1965, pp. 48ff.]. Unfortunately, one can only arrive at these distinctions by inference. Bear and Orr present, in one section, the logic of refuting theories, followed by a lengthy discussion of tests and the logic of testing. Melitz is more difficult to read. The word 'testing', which figures prominently in the article's title, never appears anywhere in the introduction. Melitz never does directly discuss his own concept of testing.

In both critiques, the logic of their criticisms is an allegation of an inconsistency between *their* concepts of testing and Friedman's rejection of the necessity of testing assumptions. The logic of their critiques may be valid, but in each case it presumes a rejection of instrumentalism. But instrumentalism, I argue, is an absolutely essential part of Friedman's point of view. Consequently, contrary to the critics' views, the alleged inconsistency does not exist *within* Friedman's instrumentalist methodology.

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As was argued above, Friedman's concept of testing is quite consistent with his instrumentalism and *his* judgements about testing. Viewed from the standpoint of Friedman's concept of testing, Melitz and Bear and Orr present criticisms that are thus logically inadequate. This situation shows, I think, that one cannot understand the particular methodological judgements of Friedman unless one accepts or at least understands his instrumentalism.

Their suggestion that Friedman's view is based on an error of logic is simply wrong. And furthermore, it is unfair to make that suggestion only on the basis of an inconsistency between *their* concept of testing and his judgements, which were based on *his* concept. There is no reason why Friedman's view should be expected to be consistent with their view of what constitutes science or of what others think testability or testing really is.

Errors of omission: De Alessi

Another even more friendly criticism is offered by Louis De Alessi. He meekly criticizes Friedman for seeing only *two* attributes of theories – namely, a theory can be viewed as a language and as a set of substantive hypotheses. On the other hand, De Alessi seems to think Friedman should have included a set of rules of correspondence or rules of interpretation. His criticism of Friedman is in the spirit that such rules of interpretation are necessary for a positive theory. He says, 'Unfortunately, Friedman's analysis has proved to be amenable to quite contradictory interpretations' [De Alessi 1965, p. 477]. But as I said before, this is not necessarily a criticism for an instrumentalist who has rejected further applications of *modus tollens*.

De Alessi later raises another minor criticism [De Alessi 1971]. He says Friedman leaves room for error by telling us that some assumptions and conclusions are 'interchangeable'. De Alessi correctly notes that such 'reversibility' of an argument may imply that the argument is tautological. When an argument is tautological, it cannot also be empirical, that is, positive. The logic of De Alessi's argument is correct. However, it is not clear that with Friedman's use of 'interchangeable' he was indicating 'reversibility' of (entire) arguments. The only point Friedman was attempting to make was that the status of being an 'assumption' is not necessarily automatic. In any case, just because some of the conditions and assumptions are interchangeable does not necessarily mean that the theory as a whole is tautological. If Friedman were viewing assumptions as 'necessary' conditions, then the problem that De Alessi raises would be more serious. But Friedman's instrumentalism does not require such a role for assumptions.

Both of De Alessi's criticisms are founded on the view that *modus tollens* can be applied to Friedman's view. In particular, it is the view that was asserted by Koopmans, namely that if *any interpretation* of a view (or argument) is considered false then the view itself must be false. But this presumes that the assumptions were necessary conditions. As I have said, that is not the case with instrumentalism. Hence De Alessi's criticisms are irrelevant, even though one might find merit in the details of his argument.

The 'F-Twist': Samuelson

The most celebrated criticism of Friedman's methodology was presented by Paul Samuelson [1963] in his discussion of a 1963 paper by Ernest Nagel.[19] Samuelson explicitly attributes the following proposition to Friedman.

> A theory is vindicable if (some of) its consequences are empirically valid to a useful degree of approximation; the (empirical) unrealism of the theory 'itself', or of its 'assumptions', is quite irrelevant to its validity and worth. [Samuelson 1963, p. 232]

Samuelson calls this the 'F-Twist'. And about this he says, it is

> fundamentally wrong in thinking that unrealism in the sense of factual inaccuracy even to a tolerable degree of approximation is anything but a demerit for a theory or hypothesis (or set of hypotheses). [p. 233]

However, Samuelson admits that his representation of Friedman's view may be 'inaccurate' (that is supposedly why he called it the 'F-Twist' rather than the 'Friedman-Twist'). Nevertheless, Samuelson is willing to apply his potentially false assumption about Friedman to explain (should one say describe?) Friedman's view. His justification for using a false assumption is Friedman's own allegedly valid 'as if' principle. Samuelson argues in this way on the basis of the theory that if he can discredit or otherwise refute Friedman's view by using Friedman's view, then followers of Friedman's methodology must concede defeat.

Samuelson's argument goes as follows. First he says:

> The motivation for the F-Twist, critics say, is to help the case for (1) the perfectly competitive laissez faire model of economics, ... and (2), but of lesser moment, the 'maximization-of-profit' hypotheses [p. 233].

Then he says:

> If Dr Friedman tells us this was not so; if his psychoanalyst assures us that his testimony in this case is not vitiated by subconscious motive-

tions; ... – still it would seem a fair use of the F-Twist itself to say: 'Our theory about the origin and purpose of the F-Twist may be "unrealistic" ... but what of that? The consequence of our theory agrees with the fact that Chicagoans use the methodology to explain away objections to their assertions.' [p. 233]

Samuelson admits that there is an element of 'cheap humor' in this line of argument. But nevertheless, it is an attempt to criticize Friedman by using Friedman's own methodology.

I will argue here that Samuelson does not appear to understand the 'as if' principle. I argued above that when using the 'as if' principle, one must distinguish between the empirical *truth* of a behavioral assumption and the *validity of using* that assumption, and I noted that the latter does not imply the former.

Perhaps Samuelson is correct in attributing a pattern of behavior to the followers of Friedman and that such a pattern can be shown to follow logically from his assumption concerning their motivation, but the 'as if' principle still does not warrant the empirical claim that his assumption about Friedman's or his followers' motivation is true. More important, the 'as if' principle is validly used *only* when explaining *true* conclusions. That is, one cannot validly use such an 'as if' argument as a critical device similar to *modus tollens*. If the implications of using Samuelson's false assumption are undesirable, one cannot pass the undesirableness back to the assumption. Furthermore, there are infinitely many false arguments that can imply any given (true) conclusion. The question is whether Samuelson's assumption is necessary for his conclusion. Of course, it is not, and that is because Samuelson is imitating Friedman's mode of argument using sufficient assumptions.

The mode of argument in which Friedman accepts the 'as if' principle is neither a case of *modus ponens* nor one of *modus tollens*. Yet when Samuelson proceeds to give a serious criticism of the 'as if' principle, he assumes that both of them apply. But even worse, by Samuelson's own mode of argument, his assumption that attributes the F-Twist to Friedman is false and his attempts to apply this by means of *modus ponens* are thus invalid.

ON CRITICIZING INSTRUMENTALISM

It would seem to me that it is pointless (and illogical) to criticize someone's view with an argument that gives different meanings to the essential terms.[20] Yet this is just what most of the critics do. Similarly, using assumptions that are allowed to be false while relying on *modus*

ponens, as Samuelson does, is also pointless. Any effective criticism must deal properly with Friedman's instrumentalism. Presenting a criticism that ignores his instrumentalism will always lead to irrelevant critiques such as those of Koopmans, Rotwein and De Alessi. None of these critics seems willing to straightforwardly criticize instrumentalism.

Instrumentalism presents certain obstacles to every critic. When instrumentalists argue by offering a long series of reasons, each of which is sufficient for their conclusions, it puts the entire onus on the critic to refute each and every reason. Friedman makes this all the more difficult by giving us, likewise, an instrumentalist argument in support of instrumentalism itself. Thus, refuting or otherwise successfully criticizing only some of Friedman's reasons will never defeat his view. Since Friedman never explicitly claims that his argument is intended to be a logically sufficient defense of instrumentalism, one cannot expect to gain even by refuting its 'sufficiency'. Yet it would be fair to do so, since 'sufficiency' is the only logical idea that instrumentalism uses. Such a refutation, however, is unlikely, since it would seem to require a solution to the problem of induction.

Finally, and most importantly, I think it essential to realize that instrumenttalism is solely concerned with (immediate) practical success. In this light, one should ask, 'What are the criteria of success? Who decides what they are?' Questions of this type, I think, must also be dealt with before one can ever begin – constructively or destructively – to criticize effectively the instrumentalism that constitutes the foundation of Friedman's methodology.

What then must one do to form an effective but fair and logical critique of Friedman's methodology? Whatever one does, one cannot violate the axioms of logic. It does not matter to instrumentalists if others have different definitions of the words 'validity', 'testing', 'hypothesis', 'assumptions', etc. When criticizing an argument in which reasons are offered as sufficient conditions, it should be recognized that *modus tollens* is useless. And when *modus tollens* is useless, there is no way one can directly criticize.

Since, as I have argued here, the internal construction of Friedman's instrumentalism is logically sound, in any effective criticism of his view the only issue possibly at stake is the truth or falsity of instrumentalism itself. But no one has been able to criticize or refute instrumentalism. That no one has yet refuted it does not prove that instrumentalism is universally correct. To claim that it does is to argue (invalidly) from *reverse modus ponens*. Again, this is a matter of logic.

Any effective criticism of instrumentalism must at least explain the absence of refutations. There are, I think, three possible ways any given argument may avoid refutations. First, as a matter of logical form, an argument may merely be irrefutable.[21] Second, if an argument is of a logical form that is conceivably refutable, it may simply be that it is true, hence no one will ever find refutations because they will never exist. Third, the absence of refutations may not be the result of an intrinsic property of the argument itself, but the consequence of how one deals with all potential refutations. That is, the defense may be either circular or infinitely regressive.[22]

As a matter of logic alone, instrumentalism need not be irrefutable. So, as an argument about how one should treat economic analysis, either instrumentalism is true or its proponents have been supporting it with a circularity or an infinite regress. And thus the first question is, is instrumentalism true? Repeated successes (or failed refutations) of instrumentalism are logically equivalent to repeated successful predictions or true conclusions. We still cannot conclude logically that the assumptions, that is, the bases of instrumentalism itself, are true. They could very well be false, and in the future someone may be able to find a refutation.

It has been argued in this paper that Friedman's essay is an instrumentalist defense of instrumentalism. That may be interpreted to mean that Friedman's methodology is based on an infinite regress, but if it is then at least it is not internally inconsistent or otherwise illogical. His success is still open to question. The repeated attempts to refute Friedman's methodology have failed, I think, because instrumentalism is its own defense and its *only* defense.

NOTES

1 However, he also explained how one can win an argument by cheating – for example, by concealing the direction of the argument – see Kneale and Kneale 1962, p. 33.

2 Specifically, Aristotle said that in order for an argument to be logical, *the premises must not violate any of the following axioms*: first is the *axiom of identity*, viz different statements cannot use different definitions of the same words; second is the *axiom of the excluded middle*, viz statements that cannot be true or false, or can be something else, are prohibited; and finally, the *axiom of non-contradiction*, viz statements cannot be allowed to be both true and false. Thus, any argument that contains such prohibited statements cannot qualify as a *logical* argument.

3 Previously proven mathematical theorems are the major source of the formal proofs used in economics.

4 These logical conditions are not independent of the axioms of logic. Each condition presumes that the statements of the argument are admissible. For example, each condition presumes that if a statement is not true it must be false.

5 I say 'heuristically' because otherwise it is quite incorrect to consider 'truth' to

be some*thing* that can be passed around. Properly speaking, 'truth' is a property of statements only; that is, there is no 'truth' without a statement that is true. And the verb 'to pass' suggests the passage of time as well as the involvement of direction, but the intention is to avoid the time aspects. The verb 'to connect' preserves the timelessness, but it does not suggest direction.

6 But usually when there are many assumptions, one does not know which assumption 'caused' the false conclusion.

7 To illustrate, since this may seem counterintuitive to someone unfamiliar with formal logic, let us consider a simple example of an argument, the statements of which individually do not violate the axioms of logic. Let the assumptions be:

A_1: 'All males have negatively sloped demand curves.'
A_2: 'Only males have negatively sloped demand curves.'
A_3: 'All my demand curves are negatively sloped.'

And let the conclusion that would follow as a matter of logic alone be:

C: 'I am a male.'

Now let us say we do not know whether the assumptions are true or false. But let us say we know that the conclusion is true. Does knowing that the conclusion of a logical argument is true enable us to say that we also know that any of the assumptions are true? Unfortunately not. As the above illustrative argument demonstrates, even if the conclusion is true all the assumptions can be false! In other words, although one's argument is logical, one still cannot use its logicality to assert that the assumptions are true on the basis of a known true conclusion. Note also that this example shows that the falsity of any assumption is not necessarily 'passed' on to the individual conclusions.

8 This is even more important if we distinguish between the two different purposes for building arguments. A disjunctive argument might be used by pure politicians who wish to convince us to vote for them or their policies. A conjunctive argument might be the objective of pure theorists who offer their arguments as tests of their understanding of the world or the economy. If the theorists' understanding of the world is correct, they should be able to explain or predict certain relevant phenomena; the assumptions used will represent their understanding (for example, the so-called 'laws' of economics, physics, etc.). If a prediction turns out wrong, with the use of *modus tollens* one can say there is something wrong with their understanding of the world. Pure politicians, contrarily, may not care *why* someone votes for them or their policies so long as the vote is in their favor. *Success* is the politicians' primary objective.

9 Few economists today are serious inductivists; yet most follow Friedman's lead by stressing the importance of distinguishing between normative and positive statements. It might be argued that for some economists the use of this distinction is merely an unexamined inductivist ritual.

10 Truth substitutes, such as probabilities, will not do. Stochastic models, in which the assumptions are in the form of probability distribution statements, usually cannot provide the logical force of either *modus ponens* or *modus tollens*. This point was stressed by early econometricians, but is usually ignored in most econometrics textbooks [see Boland 1989, Chapter 7].

11 This was seen above as the limitation of *reverse modus ponens* in the illustrative argument about males and negatively sloped demand curves.

12 Which would easily be solved if we only had an inductive logic.

13 Note here, although Friedman uses conventionalist criteria, it is for a different purpose. For a conventionalist the criteria are used as truth status substitutes; in conventionalism one finds that theories are either better or worse. In this sense, Friedman can be seen to pose the problem of choosing among theories already classified as 'better' in his sense (successful predictions).

14 I stress, this is the view Friedman used *in his essay*. In correspondence Professor Friedman has indicated to me his more general views of testing in which success might be either a confirmation or a disconfirmation. But he still would question the meaningfulness of 'testing in order to reject'.

Although Friedman seldom used the word 'truth', it should be noted that throughout he consistently uses the word 'validity' (by which he always means at least 'not inconsistent with the available facts') in the same sense that 'truth' plays in *modus ponens* seemingly while also recognizing that *modus ponens* is assured only when applied to 'truth' in the absolute or universal sense (i.e. without exceptions). Technically speaking his use of the word 'validity' may lead one to the incorrect identification of 'truth' with 'logical validity'. In this regard, applications of Friedman's methodology are often confused with orthodox conventionalism. This confusion can be avoided by remembering that 'validity' is a necessary (but not sufficient) condition of empirical 'truth' – hence, validity and truth are not identical – and by recognizing that we can believe our theory is true, even though we know we cannot prove that it is true.

15 For example, the assumption of a negatively sloped demand curve may be an assumption for the market determination of price, but it is the conclusion of the theory of the consumer.

16 That is, Friedman might argue that 'Occam's Razor' need not be used, as it is a pure intellectual exercise which serves no useful purpose.

17 Specifically, with an argument consisting of a conjunction of many interdependent assumptions, a false conclusion does not necessarily implicate any particular assumption but only the conjunction of all of them.

18 Nor does he say 'solely'.

19 Nagel's paper [1963] is often alleged to be a criticism of Friedman's essay. But Nagel's paper only tries to show that some of Friedman's definitions may not be universally accepted. Furthermore, a close reading will show that Nagel explicitly agrees with Friedman's methodological position. It is for this latter reason that Samuelson responds *to Nagel* by offering a criticism of Friedman's position. Also, it might be noted that Stanley Wong's paper [1973] is likewise not very critical of Friedman's methodology, although Wong, like Nagel, does note that Friedman's methodology is an example of instrumentalism.

20 Such an argument would at least involve a violation of the axiom of identity.

21 Statements of the form 'there will be a revolution' can never be proven false *even if they are false*. And tautological statements are true by virtue of their logical form alone, hence they cannot be refuted simply because one cannot conceive how they might be false.

22 For example, if one were to argue that revolutions are never successful, and one supported this with the evidence that every revolution has failed, the revolutionary might respond by saying that those were not 'genuine' revolutions.

3 Criticizing satisficing, empiricism and formalism in methodology

> I'm not surprised to learn of Stigler's response to your piece on Friedman, in the sense that when I began questioning Stigler about the methodology essay he came out and said that he had always 'disagreed strongly' with Friedman on the issue of the domain of a theory. This caused me to wonder whether he had actually understood what Friedman was about; certainly Stigler's own preparedness to bring back assumptions to clarify this point suggests to me that he simply didn't see how radical was Friedman's case against assumptions.
>
> Neil de Marchi [1981 correspondence]

> Boland notes that Friedman's approach to economics has no validity with regard to the 'prediction' of future events. It seems to me that Boland makes a rather devastating point here – he charges Friedman with invoking a sort of 'social Darwinism' to bolster up this evident weakness in the usefulness of positive economics. ... Publishing Boland's piece would probably stir up some responses.
>
> member, *JEL* Board of Editors (1978)

> I have been fascinated over the years by the number of commentaries that have arisen in respect of my original article on methodology. Since I have not been working intensively in any way on the problem in the interim years I have never thought it proper to reply to them. Hence I was pleased when Dr Boland did reply and I fully support what he said in that article.
>
> Milton Friedman [1982 correspondence]

Many years ago in a philosophy journal I noted that 'all economists can be divided into two groups – those who agree with Milton Friedman ... and those who do not' [Boland 1970]. I was attempting to capture the culture surrounding the usual methodology discussions of the 1960s. That culture, in which I was a participant-observer, was one that could only sneer at Friedman's methodology and rejoice in Samuelson's efforts to make jokes at Friedman's expense. At the time I accepted this as good 'college humor'.

What I did not understand was that this humor was merely a polite symptom of a more deep-seated hostility founded on perceived ideological difference between the so-called Chicago school and everyone else.

As a close follower of Popper, I knew that fair criticism must always begin with a clear understanding of what one wishes to criticize. Examining the major critiques I was quite disappointed. A careful reading of Friedman's essay from the perspective of Karl Popper's philosophy of science convinced me that none of the critics presented a fair, logically adequate criticism. They merely criticized various invented caricatures of Friedman's essay.

In retrospect it is obvious that I was quite naive in thinking that if Friedman's essay was important to textbook writers, it was worthy of competent and above all fair criticism. How could so many otherwise competent critics be so universally wrong? This question posed an interesting puzzle. My response was to understand both Friedman and the critics on their own terms. The solution to the puzzle was to recognize that in all cases the critics' terms of reference were not the same as Friedman's. I presented my evidence in my 1979 article (see Chapter 2). Simply stated, using Popper's terminology my paper showed that Friedman's essay was a logically consistent version of 'instrumentalism'. On the basis of a fair and non-hysterical examination of Friedman's essay, I was able to examine the adequacy of each of the major critiques.

It is still not widely appreciated that Friedman's essay, while an expression of instrumentalism, is thereby a rejection of an influential and excessively philosophical viewpoint – the one which Bruce Caldwell calls 'positivism' or which Popper called 'conventionalism'.[1] For that matter, it should now be recognized that Friedman rejected all views of methodology that are intended to provide an algorithm or formula for finding the one true theory of economics. Regardless of how many readers misinterpret Friedman's references to John Neville Keynes' discussion of 'positive science', Friedman's essay is not very philosophically minded – it is a rejection of any need to deal with most of the questions with which philosophically minded economists have been so concerned.

My 1979 article closed with a statement of requirements for any effective critique of Friedman's 1953 essay. To convince Friedman's many followers, such a critique must be in terms of the objectives of that essay. It is pointless to criticize Friedman's essay for not achieving goals which he has never endorsed or goals which he has even denied. My article appeared in the June 1979 *JEL* issue and since then it seemed that almost everyone accepted my argument that Friedman's essay is nothing more than an instrumentalist defense of instrumentalism – even Mark Blaug, who had said in his 1978 book that Friedman is not guilty of 'instrumentalism'.

Apparently, after refereeing the manuscript of my article, he now talks about 'the methodology of instrumentalism espoused by Friedman' [Blaug 1980, 1992].

Many papers have been written in response to my 1979 article. Most of them claim that I did not understand Friedman's argument. Given Friedman's defense of my understanding, I think they have to devise a much more fruitful line of attack. Unfortunately, most of the critics appear to think that if one does not criticize Friedman's essay then one must be supporting Friedman. I reject this dichotomy. Nowhere in my 1979 article is there any implication of support for Friedman's essay. The question now is, why do so many writers think that any defense of Friedman against unfair criticism constitutes support of Friedman's methodology?

THE SOCIOLOGY OF ECONOMICS VS DEFEATIST CONVENTIONALISM

The answer may be contained in my original 1970 observation quoted above. Most of the critiques come down to one sociological question: 'Which side are you on?', thereby leaving no room for those of us who choose neither side if the choice is between Friedman's essay and conventionalist methodology. One does not have to pick sides in order to understand the widespread influence of or support for Friedman's essay.

Methodologists surely must meet the challenge of specifying a non-Friedman alternative. Samuelson's dictums stand out as an obvious alternative. Indeed, most economists probably hold Samuelson's views to be the only alternative. Unaligned methodologists like me find it interesting that, while there are numerous critiques of Friedman's 1953 methodological views, there is virtually no criticism of Samuelson's methodological views in the leading journals (except two short articles[2]). Since I think I have already shown that Friedman's methodology is logically consistent, acceptance of Samuelson's methodology cannot be based on any logical inconsistency of Friedman's essay.

It is perhaps even more puzzling to me that the methodological foundation of the widespread reliance on econometric model building is difficult to distinguish from methodological views presented in Friedman's essay. Econometric model building routinely accepts assumptions (equations) which are known not to be quite empirically true on the grounds that the true ones would be too complicated. As a consequence, proper econometric practice[3] can hardly be viewed as anything methodologically different from Friedman's 'as if' approach.

EVIDENCE OF THE HOSTILE ATMOSPHERE

In the remainder of this chapter I am going to present my responses to some of the best-known published comments on my 1979 article. But before doing so, it is important to have a clear picture of the hostilities that surfaced after the publication of my paper. If there was a saga before my paper was published, there has been a circus since its publication.

The circus began in November 1979 during the meetings of the Southern Economic Association in Atlanta. Bruce Caldwell was chairing a session on methodology that was being held in a large room where there was standing room only. At some point Dudley Dillard was discussing Bruce's paper (which was a critique of my 1979 article) and giving the large gathering his negative views of Friedman's methodology. Bruce remembers the event as follows:

> it was my first professional paper, and the guy I was nominally attacking who had published in the prestigious *JEL* was going to be there! But luck was with me. My discussant, Dudley Dillard, attacked me but said that at least my paper was not as bad as the one that I examined, Larry Boland's. A polite way to characterize Boland's response is to say that it was acerbic. [Caldwell 1988a, p. 3]

My impression is only slightly different. Where in the past Friedman would be the target of the college humor, now I would be the target. And, as I recall, Dudley's comments which attacked me personally yielded much laughter from the audience. Dudley, whom I had briefly met many years before, did not know I was in the audience. I stood up and interrupted him, indicating that I did not appreciate his unfair and erroneous personal attack. For the remainder of the meetings, Dudley followed me around trying to apologize. Now, I liked Dudley. He was a very nice man and he did not mean any harm to me. But it became clear that I was now to be the economics profession's stand-in for Friedman in matters of methodology. The future would be Boland bashing rather than Friedman bashing.

The next evidence of a circus occurred when Rendigs Fels, using the nom-de-plume 'Tilton Cerf of the University of Paris (Tennessee)', sent a two-page comment to the editor of the *JEL*. This subtle humor was again intended to express disrespect for Friedman's methodology as well as me. The angrier reply by Gene Rotwein followed soon after. The complaint of Fels, which will be discussed in the next section, was published in March 1981. Rotwein's was published in December 1980 and will be discussed in the fourth section. But the circus was only beginning to roll as I was invited to present a paper on methodology to the History of Economics Society at the University of Virginia in May 1983. Rotwein was to be a discussant. As

soon as I finished presenting my paper,[4] the next circus act began with Rotwein standing up at the back of the room and shouting at me, asking if I now recanted my 'defense' of Friedman. Much laughter, again.

In December 1984, the editor of the *AER*, Bob Clower, published two comments on a paper I helped William Frazer write.[5] I will discuss these two comments in the fifth and sixth sections. At the end of the following year I attended the 1985 Dallas meetings of the American Economic Association, where the next circus act took place. During his presentation at one session, Roy Weintraub stated that Friedman was obviously silly since, in addition to the letter he sent me where he agreed with my characterization of his essay, he was now telling other people a contrary story, namely, that he now thinks the essay is an example of Dewey's and not Popper's instrumentalism, and to others he was saying that he thinks his methodology paper is consistent with Popper's philosophy of science. Much laughter, again. As Dudley had assumed before, Roy did not know I was in the room. After offering his humorous sneer at Friedman and me, he soon became aware of my presence.

In December 1986, without my being warned, an elaborate critique of my 1979 article was published in the *Journal of Economic Issues*. This response has its own mini-circus, which I will discuss in the final section. The big circus continues even today with efforts to get Friedman to say that his methodology essay is not as I characterized it. This part of the overall saga will be discussed in Chapter 4.

SATISFICING IN METHODOLOGY

> Boland's 'Critique of Friedman's Critics' never mentions Simon as one of the critics.
>
> Rendigs Fels [1981, p. 83]

> I cannot in this brief space mention, much less discuss, all of the numerous logical fallacies that can be found in Friedman's 40-page essay.
>
> Herbert Simon [1979a, p. 495n.]

> Let me propose a methodological principle to replace [Friedman's] principle of unreality. I should like to call it 'the principle of continuity of approximation'. It asserts: if the conditions of the real world *approximate* sufficiently well the assumptions of an *ideal type*, the derivations from these assumptions will be *approximately* correct. ... Unreality of premises is not a virtue in scientific theory; it is a necessary evil – a concession to the finite computing capacity of the scientist that is made tolerable by the principle of continuity of approximation.
>
> Simon [1963, pp. 230–1, emphasis added]

In addition to complaining that I neglected Herbert Simon's 1963 critique in my survey of the *major* critics of Friedman's essay, Rendigs Fels says that I should have noted the superiority of Simon's alternative to the claimed nature of Friedman's methodology.

My reasons for excluding Simon's 'Discussion' from my survey were quite simple. I did not consider it to be either different from other critiques or without its own logical flaws.[6] However, in view of the widespread interest in Simon's methodological comments, in my reply to Fels I took the opportunity to explain why I think Simon's critique is not successful – as an addendum to my previous paper.[7]

My critique of Simon's critique of Friedman's essay

I have two criticisms of Simon's 1963 'Discussion'. First, Simon began by identifying a theory Z (profits are being maximized at the observed prices and quantities), which was based on two assumptions, X (maximization is desired by the maximizer) and Y (deliberate maximization is possible). In this regard, he claimed, 'If, under these circumstances, Z is a *valid* theory, it *must* be because it follows from empirically *valid* assumptions about actors together with empirically *valid* composition laws' [1963, pp. 230–1, emphasis added – I assume that by 'valid' he meant 'true']. This quotation is evidence of an elementary logical error. It is an instance of what logicians call 'the Fallacy of Affirming the Consequent' or what I called *reverse modus ponens*. Perhaps he has an escape-clause in what he meant by 'under these circumstances'. But I am not sure what he meant. It may have been that X and Y are necessary *and* sufficient for Z. However, if this is what he meant, I think my criticism still holds.

My second criticism concerns the claimed superiority of what Fels terms Simon's neglected contribution, the new principle – 'the principle of continuity of approximation'. As quoted above, Simon's 'principle' says in effect that if the assumptions of an *ideal type* are *approximately* true then the derivations from these assumptions will be *approximately* true.[8] This principle is nothing more than a sophisticated version of the inductive principle often used by mathematicians to avoid the irresolvable complications caused by the absence of an inductive logic. Formally, Simon's principle would appear to be a restatement of *modus ponens*, but unfortunately there is no valid *approximate modus ponens*. *Modus ponens* is not valid for arguments consisting of statements that are only 'approximately correct'[9] since approximately true statements are not admissible into logical arguments (as they violate the Axiom of the Excluded Middle). Thus, contrary to Fels' opinion, I do not find any virtue in Simon's new principle.

Satisficing and the acceptance of unsuccessful methodological critiques

In his 1978 Nobel lecture, Simon claimed to find 'numerous logical fallacies' in Friedman's essay. After reading his lecture [1979a], I wrote to Professor Simon about his claim. His response [1979b] was that the first part of his footnote 1 [1979a] contained his criticism – namely, that distinguishing between 'workability' and 'realism' was inconsistent. This *new* criticism may be correct, but it is much too vague for me to judge and I still await his more detailed critique.

I enjoyed very much one of Simon's comments, namely '[economists] believe that businessmen maximize, but they know that economic theorists satisfice' [1979a, p. 495]. I would think it is also true for most economists' views of methodology regardless of whether they agree with Friedman's essay. All the critiques of Friedman's essay are wrong, yet those who rely on one or more of them in order to reject the essay are in effect satisficing.[10] It could equally be argued that virtually all those who instead accept Friedman's essay as adequate for *all* 'purposes at hand' are also satisficing with respect to their methodology. Does this mean that there are correct and incorrect forms of satisficing? Or, can one be satisficing when one chooses between Friedman's essay and its critiques?

Whether Friedman's instrumentalist methodology is satisfactory depends on one's objectives or the 'purposes at hand'. Of course, recognition of such a dependency is the cornerstone of Friedman's essay. It could be argued that if there is a methodological problem in economics, it is with *followers* of Friedman's essay – specifically, with their excessive concern for *immediate* practical success. Advances in theory usually require much more patience – satisficing with regard to methodological strategy may not always 'work'. 'Businessmen' may be more interested in satisficing because of a competitive urgency, but satisficing when it comes to methodology *and* theory will be counterproductive if our purpose is to advance economic theory – that is to say, if our purpose is to *improve* our fundamental understanding of the real world. So long as economists continue to emphasize policy questions and immediate practical success to the exclusion of developing new theories, Friedman's essay will continue to be considered a satisfactory methodology by many economists – whether we like it or not.

FRIEDMAN'S METHODOLOGY VS CONVENTIONAL EMPIRICISM

> [Boland's] position is indeed logically impeccable. But it would be difficult to imagine a more classic case of a scenario of Hamlet without the Prince of Denmark. ... Modern philosophical empiricism

> was born out of a recognition of the distinction between empirical and logical relations; and it was the empiricist David Hume who first pointed out that there was no reasoning that could justify (inductive) expectations that past regularities would be repeated in the future. Hume, however, held that such expectations were to be accepted *because*, given the kinds of creatures we are, or the manner in which we form our beliefs, we had no alternative to their acceptance; and this view has been central to the empirical tradition ever since his time. What then, it may be asked, *is* the alternative? Friedman ... wishes to have the cake of 'valid' false assumptions and everything that goes with induction too. And Boland, having disposed of induction on the ground that it lacks the sanction of 'logic', leaves us with no basis for empirical science whatever.
>
> Eugene Rotwein [1980, pp. 1553–5, first emphasis added]

Despite Rotwein's willingness to consider Hume an authority with whom one should never argue, some philosophers have argued against Hume's view that, since we have no alternative to inductive expectations, we must therefore accept such expectations [e.g. see Agassi 1966b and Popper 1972]. Professor Rotwein should probably have been arguing with those philosophers. Instead, he scolded me for suggesting that there is no inductive logic and for criticizing the common pretense that there are adequate philosophical substitutes. In my reply to Professor Rotwein, I attempted to explain why I think the philosophical issues he has resurrected cannot be used to construct a successful critique of Friedman's essay.

In my 1979 defense of Friedman's essay against what I considered to be unfair critiques, I stressed the importance of distinguishing Friedman's instrumentalism from the philosophers' alternatives that are more concerned with methods of establishing the universal truth (or probable truth) of scientific theories. The key issue is the separation of purposes, that is, the separation of immediate practical problems from long-term philosophical questions. Although instrumentalism may be appropriate only for the former, the view that conventional empiricism is a superior alternative is at least open to question. In the following I will once again present a critical examination of the logic of conventionalism and its relationship to Friedman's instrumentalism.[11]

Instrumentalism and the 'realism' of assumptions

In the short run or for most practical problems, one's theories do not have to be true to be successful. I shall illustrate this with a pedestrian example. When we take our television to a repairman, we do not usually think it necessary to quiz the repairman about his understanding of electromagnetics or quantum physics. For our purposes, it is usually quite adequate for

him to believe there are little green men in those tubes or transistors and that the only problem was that one of the little green men died. So long as the tube or transistor with the little green dead man is replaced *and* our television subsequently works, all is well.

This is the essence of instrumentalism. If emphasis is being placed on short-run success *and there are no doubts about one's success* – for example, the television set does in fact now function properly – there is no immediate need for a philosophical substitute for inductivism. As I argued in my 1979 article (see Chapter 2, above), logically the truth (or probable truth) of one's assumptions is not necessary. To say that it is necessary is the 'Fallacy of Affirming the Consequent'.

Conventionalism and the 'basis for empirical science'

One can easily agree with Rotwein's claim that '[Hume's] view has been central to the empirical tradition ever since his time'. Indeed, it is true that many philosophers and philosophically minded economists still think we need a substitute for inductive proofs. The alternative provided is usually one of many conceivable rules of confirmation. But it should be noted that rules of confirmation are accepted only as a matter of convention (since their validity cannot be proven by induction). Professor Rotwein's recommendation is a very common philosophical alternative to orthodox inductivism. In his 1959 article on empiricism, he recommends the probabilistic version of conventionalism. Although he does not specify the logic of his probabilism, one can identify in it the most common line of argument. The major problem is that probabilistic conventionalists, by using their substitute criteria for inductive proofs, assume what they intend to establish or 'prove'.

Probabilism and degrees of 'confidence'

Since the use of probabilistic conventionalism is quite common in economics and econometrics, I should explain my criticism. Following Hume, many conventionalists claim that although inductive proofs may not be possible (in the short run), it is still possible to argue inductively, and the outcome of such an argument will be a 'degree of probability of truth'. Such a 'degree' concept presumes that a greater quantity of positive evidence implies a higher degree of probability of truth. This basic presumption does not withstand critical examination, as we shall see.

Recall that an inductive argument proceeds from particular positive statements, such as 'I observed a white swan today in British Columbia', to a general statement, such as 'all swans in BC today are white'. In the absence of refuting observations, the general statement's probability of truth (or our degree of confidence) is measured by the ratio of the number of confirming observations to the unknown but finite number of possible observations, such as the ratio of observed white swans (without double-counting) to the number of all swans in BC today. So long as we specify which day 'today' is, this general statement is both verifiable and refutable.

The only question of empirical significance here is whether subsequent observations of confirming evidence necessarily increase the degree of confidence in the general statement *as opposed to its denial* (viz the statement that there is at least one non-white swan in BC today). As Rotwein recognizes, this is a question of predicting future observations. For example, based on the quantity of evidence available, what degree of confidence does one have that the next swan observed will be white? Empiricists would have us believe that each past observation of a white swan necessarily increases the probability that the next swan observed will be white. This alleged necessity is in fact based on a prior (and unsupported!) assumption that the general statement is true (or its ultimate probability is 1).

This point may be surprising to many econometricians, so I would like to offer an explanation. If you think the general statement 'all swans in BC today are white' is *false*, your confidence in the denial will also be increased by the observation of each *white* swan. In other words, the probability that the next swan observed will be non-white (hence proving the falsity of the general statement in question) will increase as each white swan is observed (and tagged to avoid double-counting); that is, the ratio of the number of as yet unobserved non-white swans to the number of all unobserved swans also increases as each white swan is counted. Thus, I think we can conclude that the significance of one's 'empirical predictions' is based solely on one's *prior* assumptions. You will see confirming evidence for a given empirical generalization only because you have already assumed that the statement is true! So much for Rotwein's probabilism and his empirical basis for scientific confidence.[12]

Friedman's instrumentalism through conventionalists' eyes

The common error of seeing a necessary superiority of conventionalism over instrumentalism is the result of falsely assuming that one's own objectives are shared by everyone. If Friedman's methodology were intended to be an all-encompassing philosophy of science, as Rotwein seems to think, any modern philosopher could easily be dissatisfied. But, contrary to Rotwein's protestations, in my 1979 article I argued that although Friedman gives the appropriate bow to John Neville Keynes, Friedman's approach is to drop the traditional problem posed by Keynes because its solution would require an inductive logic. Friedman's method of dealing with the question of a 'positive science' is to limit the domain of the question in the case of economics to only that which is appropriate for a practical policy science. Limiting the domain of applicability for any method or technique is a rather obvious instrumentalist ploy – one which can easily be justified on instrumentalist terms.

Philosophical comparisons of instrumentalism with conventionalism are not uncommon; but I think they can be misleading if they are only presented on conventionalist terms. The late Imre Lakatos is noted for considering instrumentalism to be 'a degenerate version of [conventionalism], based on a mere philosophical muddle caused by lack of elementary logical competence' [1971, p. 95]. But his judgement is based on whether instrumentalism is a means of achieving the objectives of most conventionalist philosophers of science, and not whether it is a useful guide for dealing with practical policy problems. In terms of instrumentalist objectives, any instrumentalist could argue that conventionalist philosophy of science is obviously useless. Moreover, as I think my 1979 article shows, Lakatos is wrong; instrumentalism on its own terms is devoid of the alleged elementary logical errors.

'SOUND METHODOLOGY' VS 'LOGICALLY SOUND' ARGUMENT

> Most logicians distinguish logical validity (a formal property of an argument) from soundness (a substantive property). An argument with false antecedents may be valid without being sound. My position is that in the only cases in which instrumentalist arguments are sound, they are also jejune. The only sense in which restricting our attention to short-run, practical problems might make instrumentalism more logically warranted is if '*practical* problems' is construed to mean '*past* problems'. For really practical problems this is useless.
>
> Kevin Hoover [1984, p. 792]

Like many critics before, Hoover argues correctly that Friedman's essay is a poor substitute for a good, 'sound methodology' – that is, one that would be acceptable to the average logical positivist philosopher. As many before him, he misses the point. By rejecting the problems and concerns of those philosophers, Friedman's essay is not intended to be a good substitute. Hoover misses the point of my 1979 contribution in two ways.[13] Not only does he unfairly read my paper as '[my] support of instrumentalism', he erroneously reads the term 'sound' to mean something more than 'logically sound'. Nowhere in my 1979 article was the term 'sound' used to mean 'the premises were true' as Hoover seems to require. Nowhere in my 1983 paper with Frazer was it claimed that Friedman's essay is anything more than logically sound. Nowhere was any meaning attached to the term 'sound' other than 'logically sound'. The only claim made for Friedman's essay was that it was without logical errors. Hoover, it would seem, criticizes only his own caricatures of my 1979 article and Friedman's essay.

POPPER VS CONVENTIONALIST-POPPER

> Boland sees Friedman's as a 'practical' approach. But does practicalism necessarily go with instrumentalism? Surely not the instrumentalism of Berkeley, Mach, and the rest whom Popper points to as instrumentalists. Thus, while Boland in this instance has certainly put his finger on an important aspect of Friedman's thinking, he has failed to connect it to his instrumentalism thesis. We need the Dewey-type of (pragmatic) instrumentalism to enable us to make the connection. ...
>
> It is worth pausing to remind ourselves of what it is that Friedman and Popper are trying to do, each in his own work. Friedman wants useable predictions of the consequences of changes in the economic environment ... Popper's work has been an extended attempt to formulate a logic for science as the pursuit of truth which avoids the pitfalls of Inductivism and Conventionalism.
>
> Abraham Hirsch and Neil de Marchi [1984, pp. 783–4]

While Hirsch and de Marchi present some interesting ideas about Dewey's version of instrumentalism, they have little to say about the view of instrumentalism which, as I noted above, Friedman accepts as a correct representation of his view. Unfortunately most of their critical comment concerns Popper's views of science and learning, about which they are simply far off the mark. They continually interpret Popper as if he were a paradigm conventionalist. In particular, their arguing that Popper had attempted to come up with a logic for science could be no better evidence that they do not understand Popper.[14] Moreover, they did not try to understand Friedman – except to see him as only a variant of Dewey.

More distressing about the criticism of Hirsch and de Marchi (as well as Hoover's) is its implicit view of methodology that presumes there must be a correct algorithm or recipe for constructing short-run practical policy or for choosing the 'best theory' in the long run. Friedman rejects such concerns. So does Popper! It is probably the one point on which Popper and Friedman are in complete agreement. Methodology for Popper,[15] and perhaps even Friedman, is a matter of understanding *why* economists do what they do – it certainly is not a set of formula-type prescriptions. Anyone studying methodology in order to obtain a methodological receipe is bound to be disappointed by Friedman's essay or any careful reading of Popper.

DO INSTITUTIONALIST ECONOMISTS REALLY BELIEVE IN FORMALISM?

> I shall defend an incoherence thesis – namely, that both Friedman's essay and Boland's instrumentalist interpretation of it are incoherent, logically inconsistent, and logically unsound. Before turning to that thesis, however, I would like to enter a disclaimer of sorts. ...
>
> I would like to head off any misconstrual of my rebuttal of Friedman and Boland as criticism of instrumentalism. As James Wible has already shown in the pages of this journal [viz the *Journal of Economic Issues*], the version of instrumentalism attributed by Boland to Friedman is not the original version propounded by John Dewey (Wible 1984, pp. 1049–54). As far as I know, John Dewey was the first to make use of the term 'instrumentalism' to distinguish his philosophy ... from the pragmatism of William JamesS. Schiller and of C.S. Pierce's pragmatic theory of meaning...
>
> It was Karl Popper who introduced a new, and now commonly accepted, meaning for 'instrumentalism' in a 1956 paper to describe the otherwise named phenomenalism of Berkeley. ... Since it is this latterday, bastardized version of instrumentalism ... that Boland attributes to Friedman, and not Dewey's original version, we need to be very careful in distinguishing them.
>
> Ken Dennis [1986, pp. 633–4]

> In formal logic, the conditional form of statement, 'if *P*, then *Q*', is treated as a *material conditional*, '*P*of conditionality does not conform to our intuitions about the ordinary indicative and subjunctive conditionals of common usage, it has worked well in formal logic and is the only interpretation of conditionality now in use by formal logicians.
>
> Dennis [1986, p. 646]

In this final and largest section I wish to discuss the methodology of criticizing Friedman's methodology. Methodologists tend to talk only to other methodologists (partly because no one else thinks they have anything to say). For methodologists, the issue of Friedman's methodology was closed because thirty years ago most methodologists felt that Friedman's methodology was dead and buried after Samuelson presented his now famous 'F-twist' joke. Unfortunately for them, as I keep pointing out, there is a very large proportion of the economics profession which still thinks Friedman's methodology is alive and well. If Friedman's methodology were dead, it would not be so widely practiced. Methodologists simply must face the facts and explain the continued widespread acceptance of Friedman's methodology.

When I wrote my 1979 article on Friedman's methodology essay, I wrote it with these facts in mind rather than with the self-serving myopia of many methodologists. My 1979 explanation proceeded from the presumption that the acceptance of Friedman's methodology is so widespread that by now it would be unlikely for anyone to find that this methodology is internally inconsistent. Moreover, Friedman's methodology is not necessarily the correct methodology for all occasions (despite what many of his followers think) even if everyone were to agree that Friedman's methodology is internally consistent.

As we have already seen, many writers have responded to my 1979 article. Only a small proportion of the written reponses were published in major journals. With one exception, to be discussed below, I was always given the opportunity to reply in the same issue in which the response appeared, as is the usual practice of major journals. With few exceptions, the purposes of these responses to my 1979 article have not been clear. The styles of the responses ranged from the very civilized complaints such as those by Fels and Hoover to those with varying degrees of hysteria. Obviously, most of these writers have very strong ideological objections to Friedman's economics and thus feel duty-bound to attack Friedman at every opportunity. People have been attacking Friedman and his methodology for over forty years and, as I continue to claim, nothing much has been accomplished. In the 1979 article my intention was to deliver the message that if one wishes to criticize Friedman's methodology, one is more likely to succeed when the criticism is based on one's clear-headed understanding of the aim and purpose of Friedman's essay than on one's ideological biases. With each successive attack on my 1979 article one gets the impression that the critics are ever more interested in attacking the messenger rather than coming to grips with the fact that Friedman's methodology still lives in the hearts and minds of many, if not most, mainstream economists.

The most hysterical attack on the messenger was delivered by Ken Dennis, a relatively unknown economics teacher at the University of Manitoba. This attack not only lacked humor but was probably the most disgraceful of all. This circus act began in November 1986 when I was invited to the University of Manitoba to participate in a weekend seminar that had as its theme 'methodological pluralism'. The main meeting was held in a large lodge with a big fireplace. I was to be the main speaker. At the opening reception I was introduced to Mr Dennis but he had little to say to me. During the seminar he sat at the far end of the room, as far away from me as possible. I knew of his interest in methodology and thus was surprised that he had no interest in speaking to me. Since everyone else was so nice to me, I did not give his slight any further thought.

It was a few weeks later when I opened a copy of the December 1986 issue of the *Journal of Economic Issues*, the leading journal devoted to institutionalist economics, to find what amounts to a sneak attack [Dennis 1986]. I say sneak attack because Mr Dennis could have easily warned me during the November seminar that such an attack was about to be published – as a simple matter of civilized courtesy. Moreover, the journal's editor should have given me the opportunity to respond in the same issue or volume, as is customary. But clearly, the level of hostility engendered by any discussion of Friedman's methodology seems to be ample justification for uncivilized, disgraceful and unscholarly behavior. I called the editor to complain and to ask if I might be given the opportunity to respond. He said I could but, of course, my response would not appear until the next year's volume – which meant that many readers of the sneak attack would not be aware that there ever was a response. And to top this off, without telling me, the editor extended to Dennis the courtesy of an opportunity to reply to me in the same issue – the same courtesy I was denied!

Dennis' attack is the most vitriolic criticism I have ever read in a leading journal. It reads like a case written by a Crown Prosecutor, detailing what is purported to be indisputable evidence that I must be hopelessly incompetent when it comes to reading the printed word and discussing the fine points of Aristotelian logic. Actually, there is much more low-level name calling than is appropriate for a scholarly journal. Certainly, most readers will agree, vitriolics and name calling will never be considered a good substitute for a logically sound argument or criticism. Despite my misgivings, I accepted the offer by the editor of the *Journal of Economic Issues* to reply and thus presented my defense in the March 1987 issue. In my reply, I invited the readers of that journal to join my jury. The following is my defense, my 'Apology'.[16]

The evidence

My learned colleague from Manitoba has charged me with (1) not understanding the essence of instrumentalism, (2) not representing Friedman's essay correctly, (3) not understanding the history of logic, (4) making grievous logical errors and, above all, (5) not understanding the essence of 'modern formal logic'. I plead Not Guilty to all charges.

My learned friend began his prosecution with an impressive display of serious concern for proper terminology. It would, of course, be wise for any prosecutor to make clear to a jury which includes readers of the *Journal of Economic Issues* that he understands the essence of Dewey's use of the term 'instrumentalism' since these readers are by and large very familiar with the writings of the philosopher John Dewey. Furthermore, if he can do

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this with a lot of puffery and indignation showing, all the better. But, while this may be a very clever rhetorical stance, I still do not understand what a discussion of Dewey's instrumentalism has to do with my 1979 article. Obviously, I carefully defined how I was to use the term 'instrumentalism' in my article and I adhered to my definition throughout – in a most consistent fashion. Nowhere in my article did I mention Dewey or Dewey's instrumentalism. Nowhere did I say that Friedman's instrumentalism is in any way related to Dewey's instrumentalism.

Perhaps my learned friend presumed that if Dewey was the first to use the term 'instrumentalism' then no one else should use that term in any other way. If we accept this presumption and this type of rhetoric, then it should also be true that whenever my learned friend uses the term 'logic' he should always mean Aristotelian logic since Aristotle came before all the modern formalist logicians. More on this later. I asked the jury to ignore my learned colleague's untimely outburst on the essence of instrumentalism since it was introduced only to deaden the jury's senses concerning fair play and justice.

Also, I asked the jury to ignore my learned friend's extensive arguments for his claim that the 'text of [Friedman's] essay scarcely supports the interpretation Boland tries to place on it'. This is not an argument I have to defend. As I have been continually pointing out, Professor Friedman has told me and several others that he completely accepts my 1979 interpretation of his essay. Given such expert testimony, there is nothing more for me to do. My learned friend will have to figure out how to bring Milton to court to face cross-examination.

My learned friend claimed I said 'it is difficult to find the "focus" on the "exact purpose" of Friedman's essay' [Dennis 1986, p. 653] and thereby claimed I said Friedman's essay needed clarification. This claim is both misleading and unfair. What I actually said was, 'Because the essay is long, it is hard to focus on its exact purpose' [p. 509]. I am not going to spend much time trying to understand why my learned friend would resort to such poor scholarship. Unfortunately, he went even further by suggesting that Friedman believes his famous essay needed clarification. But I cannot see how Professor Friedman's explicit statement that I correctly represented his essay implies it needed clarification. Surely, by correctly stating something one does not always perform an act of clarification.

Let us now turn to the methodology of my learned colleague's attack on my 1979 article. Why, it might be asked, did my learned colleague carry on for almost six pages [pp. 648–53] trying to show that I do not understand the essence of Aristotle's logic? Surprisingly, he provided an answer to this question. He claimed he is offering evidence that I 'lack a command of elementary principles of formal logic'. And thus he wanted the jury to

conclude that my 'discussion of hypothesis-testing is ... [no] less suspect' [p. 650]. I urged them not to be fooled. Again, my learned colleague was trying to cloud the jury's vision – the nature of Aristotle's logic played no role in my 1979 article. Since he seemed to think my few references to Aristotle are very revealing, let me be more specific.

Let the record of evidence show the following. In my 1979 article I mentioned Aristotle in two paragraphs and one footnote. The point of the first mention was that we should follow Aristotle and not separate questions of truth from logical validity (as many modern formalist logicians are wont to do). The second mention (including the footnote) was to explain that 'Aristotle was concerned with determining what kinds of statements are admissible into logical arguments'. Nowhere in my references to Aristotle did I connect him to the more modern terminology about how logic is used (viz *modus ponens* and *modus tollens*). Yet my learned colleague would have the jury believe that I was claiming that Aristotle lectured about such things as *modus ponens* and 'the conditional form of statement and argument'. Nowhere did I do any such thing. This kind of accusation and misrepresentation of my paper is both unfair and misleading.

My learned colleague from Manitoba wanted the jury to believe that, unlike him, I was unable to read the well-known history of logic by William and Martha Kneale [1962]. The irony is that this book is precisely the one I used as a reference to write the sections on *modus ponens* as well as my later discussion of the 'material conditional'. Moreover, it was on the basis of this book that in my first mention of Aristotle's logic I said: 'Logic has not changed much since [Aristotle's time], although some presentations lead one to think that our logic is different'. My very next sentence was: 'Modern writers too often discuss logic as if it had nothing to do with truth.' Now let me be absolutely clear here. The logic that Aristotle discussed has nothing whatsoever to do with 'modern formal logic'. The logic that Aristotle discussed, and the one which I reviewed in my 1979 article, is the one ordinary people use every day. Ordinary logic has not changed since Aristotle's day. Ordinary individuals never find a need to use 'modern formal logic' in their everyday arguments. I have never heard of a lawyer using 'modern formal logic' to present his or her case to a judge or jury. Despite my learned colleague's obvious attempt to mislead the jury into thinking that I was totally ignorant of modern formalist logic when I claimed logic has not changed much, he let the cat out of the bag by admitting that 'true connoisseurs of formal logic will be aware of recent attempts to modernize Aristotelian logic ... but so far these efforts have had little success'. I think he must concede that I have won this point.

To press his personal attack, my learned Manitoban colleague referred to my 1982 book and accused me of making a logical error on page 138.

He said that there I 'reduce the material conditional ... to a conjunction' [Dennis 1986, pp. 648–9]. My learned friend is wrong. I have not done what he says. But more importantly, he has taken my discussion out of context. The question raised in my book is whether material conditionals are admissible into logical arguments about empirical facts. Even more important, in my book the discussion is about how and why modern formal logic is misleading. By taking truth tables for granted, formalist logicians claim that the statement 'if *P* then *Q*' must be considered true whenever *P* is false [e.g. see the quotation from his page 646 at the top of this section]. My learned friend claimed that I must accept this interpretation of conditional statements since 'it has worked well in formal logic and is the only interpretation ... in use by formal logicians'. I do not care how many 'formal logicians' accept this nonsense; I certainly do not accept it, nor do I feel any need to accommodate these formalists. Somehow, I feel my friend would not accept Friedman's argument that we should all accept neoclassical economics simply on Friedman's claim that it has worked well in the minds of many economists.

My Manitoban friend wanted the jury to believe that my article is riddled with logical errors. He gave only one example taken from a footnote – namely, my illustration of how a logically valid argument can be misused. Now despite the fact that this example has nothing to do with Aristotle, he persisted in claiming it shows how ignorant I am in not realizing that the example is impossible 'in traditional Aristotelian terms'. Since I never claimed it was possible in 'Aristotelian terms', what did his discussion supposedly establish? Well, in this case he said 'modern formal logic' comes to the rescue by showing that my example is not logically valid by the rules of 'modern formal logic'. This is not only unfair, it is quite bizarre. Since he referred to my 1982 book, I know he was aware that I explicitly reject what he called 'modern formal logic' as well as all such self-serving exercises in formalism-for-formalism's-sake.

This brings us then to the keystone of my learned colleague's case against me – my alleged lack of an understanding of 'modern formal logic'. Let me state my position for the record. I am very much opposed to unnecessary and self-serving formalism. For the record, it should be noted that formalism in mathematics was primarily an early-twentieth-century phenomenon where the methodological program of formalism was promoted as the means of formalizing the proof procedure.[17] The foundation of this early program of formalism was to be mathematical logic or what my learned friend called 'modern formal logic'. Formalism in economics has also been active for several decades, although its growth was greatest in the 1970s. The excessive formalism of recent mathematical economics has become an immediate concern to many economists today.[18]

Much of mainstream economics has been taken over by formalists who are quite willing to assume anything to make their models formally complete.[19] Realism and relevance are virtually of no concern in the many journals which devote most of their space to mathematical economics.

It is all too easy for us to see that 'modern formal logic' is to ordinary logic what mathematical economics is to ordinary economics. Despite all the resources devoted to the self-serving games of mathematical economics, hardly anything useful has been learned over the last fifty years of its development. Mathematical economists will likely counter that mathematical economics gave us linear programming and input/output analysis but these examples are misleading. These techniques have their origins in attempts to solve practical problems rather than in the fortunate outcomes of a few mathematical economists entertaining themselves by assuming whatever happens to meet their fancy. On the basis of its track record, the practice of formalism-for-formalism's-sake, whether in mathematical economics or in formal logic, has not proven to be a very useful methdological exercise.

The summation

I have presented enough evidence to summarize my defense. I am not guilty of misrepresenting instrumentalism. I am not guilty of the logical errors my learned colleague attributed to me. I am not guilty of misusing Aristotelian logic. However, I am most definitely guilty of not respecting the wishes of modern formalists, be they logicians, mathematicians or mathematical economists.

Much of my learned colleague's paper presented his formalist critique of Friedman's essay with specific reference to Friedman's views of assumptions, predictions and hypothesis testing. I have made no attempt here (or elsewhere, for that matter) to *defend* Milton Friedman or his essay beyond the specific critics of his essay that I listed. Nor have I tried to defend the well-known philosopher of science Karl Popper, who also received some scorn from my learned Manitoban friend. To the contrary, neither Friedman nor Popper needs any help from me.

My learned colleague from Manitoba has presented a long and intense paper that purports to put me and Professor Friedman in our place – supposedly somewhere among the 'crackpots' and 'quacks'. I asked the jury to ignore these youthful excesses of intensity and focus instead on the logic of his case. His paper began and ended with a discussion of Dewey's instrumentalism. At the beginning, his discussion was intended to lead the audience astray by claiming that the term 'instrumentalism' used throughout my 1979 article is not the instrumentalism that Dewey discussed. Of course, I accept this claim since I never said Friedman's instrumentalism was the same as Dewey's. At the end, his discussion was intended to meet a challenge I presented in my 1979 article. Namely, given that Friedman's instrumentalist methodology is the centerpiece of his famous methodological essay, an effective criticism of that essay must be internal and thus somehow deal with Friedman's instrumentalism. Unfortunately, my learned friend did not deal with Friedman's instrumentalist methodology but instead with Dewey's instrumentalism. Since my friend has already argued that Friedman's instrumentalism is definitely not Dewey's, he cannot now use Dewey's version to form an internal criticism.

Most of my learned colleague's criticisms of my views of testing would have the jury think that I have only superficially considered the question of hypothesis testing in economics. He wished them to believe my views of testing are thus suspect. To achieve this end, however, he concealed some relevant information from their view. He never dealt with the dozen or so of my widely available papers where I discuss testing in economics.[20] Instead of dealing with my published views of testing, he attempted to discredit my alleged expertise in logic in order to argue on the basis of 'guilt by association'.

The bulk of my learned friend's critique of my views of logic rested on two things – my opening reference to Aristotle and my innocent attempt to assist those readers 'unfamiliar with formal logic'. My use of the word 'formal' seems to have violated a cardinal rule. Of course, I nowhere presented a formalist version of logic, nor did I suggest anywhere that I had. Moreover, I have never claimed to be an expert on questions of logic yet everything my learned colleague said seems directed at proving to the jury that I am not an expert. Now, I cheerfully admit my lack of expertise in logic. But my saying that I am not an expert should not be interpreted as my signing away any right to think for myself. I am certainly not going to be convinced by formalist logicians, formalist economists or mathematicians who argue that I should reject Aristotle's logic in favor of demonstrably empty formalism. Nor can I imagine that Friedman would be willing to give up his view in favor of formalism-for-formalism's-sake.

NOTES

1 See Caldwell 1982 and Boland 1979a, 1980, 1982.

2 The exceptions are, specifically, Gordon 1955 and Wong 1973.

3 But not econometric *theory*, which is usually based on conventionalism: see Boland 1982, Chapter 7; 1989, Chapters 7 and 8.

4 My paper was about methodology in neoclassical theory of decision making [Boland 1986a].

5 I will not discuss this paper [Frazer and Boland 1983] here because at the galley stage Frazer changed the conclusions of the paper to ones with which I do not agree. And for this reason, when it came time to respond to the comments, I wrote mine separately from Frazer's.

6 It should be noted that many reported critiques of Friedman's essay only discuss Friedman's essay because the authors really wish to surreptitiously examine Samuelson's methodological arguments [e.g. McClelland 1975]. Contrarily, Stanley Wong does not present a critique of Friedman's essay. He provides an interpretation of Friedman's view only in order to criticize Samuelson's critique [Wong 1973]. Wong's criticism is more fully developed in his book [1978].

7 The remainder of this section is my reply. Permission has been granted by the American Economics Association to use those parts that were published in Boland 1981a.

8 Elsewhere I have already criticized the ideal-type methodology [1978, pp. 256–7] and the methodological reliance on stochasticism or approximationism [1969, 1977a, 1986b and 1989].

9 For example, for arguments involving stochastic 'estimates' – see Haavelmo 1944, p. 56.

10 With this in mind, it is difficult to distinguish between 'satisficing' and Friedman's 'as-if' methodology.

11 In the remainder of this section I will present my reply to Rotwein. Permission has been granted by the American Economics Association to use those parts that were published in Boland 1980.

12 For even more serious problems of empiricism, see Agassi 1966a, Gardner 1976, and Boland 1968 and 1989.

13 Also, there are technical errors concerning logic and there are confusions of my views with my representation of Friedman's views. What is worse is that Hoover often presents my views as if they were his own!

14 There are two reasons why I think they do not understand Popper. First, Popper's major work [1934/59] (the English version was not published until 1959) simply began with an alternative 'logic of science' – that is, he definitely has not been attempting to come up with one since his first work! Second, when Popper began his work, the terms 'logic of science' and 'scientific methodology' usually referred to inductive logic and methodology. In my 1982 book (Chapter 1) – which was available to Abe and Neil – I carefully explained that Popper has always criticized the widespread use of those concepts in ordinary philosophy of science literature because they embody 'inductivism'. Modern 'conventionalism', which is the viewpoint Abe and Neil present, is but a sophisticated variant of the 'inductivism' which Popper has always explicitly rejected.

15 I discuss various misconceptions of Popper's view of methodology below in Chapters 19 and 20.

16 Permission has been granted by the publisher of the *Journal of Economic Issues* to use those parts of the remainder that were published in Boland 1987a.

17 See Meschkowski 1965, Chapter 10.

18 See Grubel and Boland 1986.

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19 See also Boland 1986b.

20 Specifically, see Boland 1968, 1969, 1970, 1971, 1974, 1975, 1977a, 1977b, 1977c, 1977d, 1981b and 1985a. See also Chapter 8 of my 1989 book as well as Chapter 20 below.

4 On the methodology of the history of contemporary economic thought

J.D. Hammond I notice that in the *New Palgrave* Alan Walters says that in your 1953 methodology essay you introduced Popper's philosophy of science to economics. Would that be an overstatement ... ?
M. Friedman No. ... I didn't read his *Logik der Forschung*, but I knew the basic ideas from my contact with him [in 1947], and I have no doubt that that contact ... did have a good deal of influence on me.
J.D.H. In that light it's rather strange that your methodology has been labelled instrumentalism, which is a view that Popper was very critical of.
M.F. Much later. Popper has changed ... I haven't kept up with his methodology. I only know about this attack on instrumentalism from people like you ... telling me about it. His book on *Conjectures and Refutations* doesn't contain – maybe I am wrong... Does it contain any ...?
J.D.H. I'm not sure.
M.F. I'm not sure either.

J. Daniel Hammond [1993, pp. 223–4]

In comparing Dewey and Friedman, the differences between instrumentalism and realism arise ... in a completely unexpected fashion. Investigation into Dewey's philosophy reveals that it gradually evolved into a variant of the realist position and was not an 'instrumentalist' view as this term is currently used in philosophy of science. ... Friedman's instrumentalism can be taken as a narrow, methodological special case of John Dewey's instrumentalism.

James Wible [1984, pp. 1054 and 1065]

Two questions that flow from the saga over my 1979 article seem to remain. First, there is the persistent worry over who was the first to identify Friedman as an instrumentalist. Second, there is the misdirected worry over just what philosophy or methodology of science Friedman truly advocates. One would think that these two questions would be easily answered. The first one is a matter of painstakingly searching the published record. The second, it would seem, could be easily decided. It is interesting to note that almost all history-of-thought research concerns dead economists. Surely, someone might suggest, if there is any dispute concerning whether or not Friedman is a positivist, a logical positivist, a Popper-type or Dewey-type instrumentalist, or a pragmatist, it could simply be decided by cross-examining Friedman. Ironically, this method is the least likely way to determine the truth of the matter.

Nowhere have I ever claimed that I was the first to identify Friedman's essay as an argument for instrumentalism. Nevertheless, correspondents and many commentators continue to tell me that I was not the first and to tell me who they think was the first to mention that Friedman's methodology is instrumentalist. The irony here is that if, as many have been claiming, I am so wrong to claim that Friedman's methodology essay constitutes instrumentalism, why are so many people arguing over who said it first? Since I helped Wong write his 1973 article that made this identification, I know first-hand that I was not the first but it must be recognized that until my 1979 article was published the common view was that Friedman was a logical positivist. So the question historians of thought should be concerned with is *why* the abrupt change in the common view? Surely my article played a significant role. Could it simply be that I made a convincing argument?

To a certain extent I find the question of what Friedman's true position is regarding methodology to be a waste of time. Mostly, this is because my essay was not about Friedman the man, but just the nature of his 1953 essay. Nevertheless, several writers think it is an important question. I will discuss the views of three of them here. First, Abraham Hirsch (a close friend of Gene Rotwein) seeks to undermine Friedman's support for the argument in my 1979 article by presenting his own contrary views to Friedman. Second, William Frazer, who is a strong advocate of Friedman's views of monetary economics, searched through Friedman's files of correspondence in order to find scientific significance in Friedman's views. And third, there is Daniel Hammond who, like Frazer, has had access to Friedman's files, and has interviewed Friedman the man. I will discuss these in turn so that I can ultimately see what prospect there is for doing real-time history of economic thought by asking the man-himself.

Hirsch makes his strongest stand against my 1979 article in a book he wrote with Neil de Marchi [1990]. I reviewed this book for the *Journal of the History of Economic Thought* [Boland 1991a]. Despite what they say at the beginning, the book is really two books in one, with the first part being Hirsch's view that Friedman is not an instrumentalist as defined by Karl Popper but is an instrumentalist as defined by John Dewey. Unfortunately, Hirsch makes too much of Friedman's personal reports that my 1979 article

about the 1953 methodology essay is 'entirely correct'. Such reports are troubling for Hirsch since what my article explained is that Friedman's essay can be clearly understood as an instrumentalist argument in favor of instrumentalism – and nothing more! Perhaps Hirsch would have been more pleased had I been referring to Dewey's notion of instrumentalism instead of Popper's. It would seem that Hirsch feels that the record must be set straight.

Supposedly, after reading early drafts of some of the chapters of their book, Friedman is now convinced that his 'own methodological views are almost identical with those of John Dewey'. This sounds quite inconsistent – but is it really? Elements of Dewey's pragmatism are so commonplace in American academic circles that hardly anyone will find themselves in disagreement. Even Samuelson in 1963 noted that anyone who claims that any hypothesis or theory should be judged on its consequences is merely repeating what we would all know if we read early-twentieth-century philosophical writers such as Dewey, Charles Peirce or William James.

It is doubtful whether Friedman the man would want to be narrowly categorized as an instrumentalist – even if he really agrees that his essay is nothing more than an instrumentalist defense of instrumentalism. Nor would he wish to be narrowly categorized as either a pragmatist or a falsificationist. Unfortunately, Hirsch is more concerned with the pulse of Friedman the man. As Samuelson noted in 1963, even if we could conceive of contrary testimony of Friedman's hypothesized psychoanalyst as to Friedman's subconscious motivation, we all can see how people use Friedman's essay to explain away objections to their assertions. The real utility of Friedman's essay is just that; it is a useful instrument to deflect methodological criticism. I think more is to be learned from examining how other people have invoked Friedman's 1953 methodology essay than will ever be learned by using a magnifying glass to examine each word in his essay or in his other writings.

William Frazer is a professor from Florida whom I met at the 1979 Southern Economic Association meetings after my infamous confrontation with Dudley Dillard. Bill showed me copies of referee reports and correspondence which Friedman had before his 1953 essay was published. He asked me if I would help him write a paper about Friedman's essay. I said I would try. The end result was our 1983 *American Economic Review* article. Unfortunately, Bill handled all the correspondence with the *Review* and it was not until the managing editor called me to get some documentation for some claims made at the end of the article that I realized he had changed the conclusion. Bill wanted to argue that Friedman was a devotee of Popper's so-called philosophy of science. The reason was that in his conversations with Friedman, Milton had made such a claim. Given that

Friedman said he agreed with my characterization of his essay, I found this a rather surprising claim. I could see why he might identify with the rather conservative views of social policy that Popper advocated in *The Open Society and its Enemies* [1945/63], but Popper in his *Conjectures and Refutations* [1965] clearly rejected the kind of instrumentalism that I used to describe Friedman's essay. But since I said I would help, I tried to see to what extent Friedman could make his claim. And, except for the concluding paragraphs of our article, I think only a *very* limited support could be established and even that support is available only so long as one does not read too much into it. Unfortunately, Bill wanted to read a lot into it and did so without my approval.

Hammond attempted to decide the issue of Friedman's true methodology in a very straightforward manner. He simply asked Friedman during a 1988 interview [Hammond 1993]. As the quotation at the beginning of this chapter shows, it was not a very fruitful interview. Friedman may wish to think he is a follower of Popper, but it is evident that he has not read much of Popper's philosophy of science work and the little that he did read seemed to stick. He clearly does not remember what Popper said in his *Conjectures and Refutations*. To me, this exercise raises some questions concerning the methodology of doing the history of economic thought on living economists.

FRIEDMAN'S ALLEGED INCONSISTENCIES IN CORRESPONDENCE

Before discussing these questions, I want to address the only interesting problem that has come from all these efforts to undermine Friedman's support for my 1979 article. Namely, Hirsch and de Marchi argue that Friedman's support for the correctness of my description of this 1953 essay is inconsistent with other things he has said. Specifically, they note Friedman's statement to me that he thought my description of his essay was 'entirely correct'. They complain that 'this has led others ... to suggest that nothing more remains to be said on the subject' [Hirsch and de Marchi 1990, p. 6]. They think there is evidence that contradicts this. They refer to two other letters in which they think Friedman has made contrary statements. Specifically, in 1984, Friedman sent a letter to Donald McCloskey. In that letter Friedman says: 'some recent papers I have read [namely, early drafts of papers by Hirsch and de Marchi] have persuaded me that my own methodological views are almost identical with those of John Dewey' [reported in Hirsch and de Marchi 1990, p. 6]. But Hirsch and de Marchi report that in a letter to Hirsch in 1983 Friedman wrote that he regarded his own views of methodology to be 'entirely consistent with

Popper's'. Armed with these utterances, Hirsch and de Marchi spend six chapters trying to prove I was wrong.

This is a perfect case of much ado about nothing. Friedman's notion that his views are consistent with Popper is easily understood but only so long as one does not try to read too much into it. Surely Friedman does not see himself as a disciple of Popper. All that Friedman is claiming is that he and Popper both argue that theories should be judged by their consequences. While Friedman may wish to see this as a basis for consistency, he can do so only if he ignores most of Popper's writing on the subject. And, as is evident in Hammond's interview quoted above, since he clearly has not read much of Popper's writing on the subject, this is easy for him to do.

What Friedman probably thinks is 'entirely correct' in my 1979 essay is my argument that all the critiques which I discussed there were wrong. Moreover, instrumentalism does not worry over whether my assumptions concerning the nature of his essay are true but only whether they obtain the desired result, namely, the refutation of all the critiques of his essay. But again, the central issue is that my article was about the essay. I made no claims about Friedman the man. Friedman the man is free to say all sorts of things that are inconsistent with his essay.

LIMITS TO THE HISTORY OF CONTEMPORARY THOUGHT

At the 1993 Anaheim meetings of the American Economic Association, I discussed one of Hammond's papers where he again reported about his interview with Friedman and how it showed that I was wrong because Friedman is really a Dewey-type instrumentalist, not a Popper-type as I am alleged to have claimed. In my discussion I tried to explain why I think a living source can be unreliable. I offered two anecdotes to demonstrate the limitations of original living sources. One concerns Paul Samuelson and the other concerns myself.

In the fall of 1975, Stan Wong met Samuelson at a conference. They discussed Stan's PhD thesis that was entirely devoted to Samuelson's revealed preference theory. In particular, they discussed Stan's interpretation of Samuelson's seminal 1938 article concerning whether it was an attempt to dispense with any assumption of non-observational concepts such as utility and preference. In their discussion Samuelson made a couple of claims that seemed to refute some of Stan's work. In one case, Samuelson claimed that he never was interested in the so-called non-integrable case. In another, Samuelson denied Stan's claim that the term 'revealed preference' did not appear until his 1948 paper and, as Stan claimed, represented a problem shift from the one addressed in the 1938 paper. Stan returned home and re-examined his research. In a letter to Samuelson, Stan was able to quote from page 68 of the 1938 article and thereby counter Samuelson's claim concerning the problem of integrability. Similarly, he was able to show Samuelson that he did not use the term 'revealed preference' until his *Foundations* book [1947/65], and even there it was not used in the context of consumer theory. So the point of this anecdote is simply that it is a good thing Stan did not rely on Samuelson's memory as a basis for his research.

The other anecdote that I mentioned concerns my 1992 book, where I went back to several of my articles published in the early 1970s. What I reported was my shock at trying to figure out what I had said in a 1970 article about axiomatic methods. The problem was that I could not figure out what *I* was trying to say. Some paragraphs now made no sense whatsoever. If I could not understand what I was doing in my own 1970 article, why would anyone think Friedman or Samuelson would have perfect recall for articles that were written forty or more years before?

Methodology is a difficult field to study. We cannot just ask someone forty years after his or her published paper what they were doing. Ideally, as an alternative, in the spirit of this age of computers, we could do 'real-time' history of thought. But when it comes to methodology, this, too, is very difficult. It would be easy only if writers and researchers were self-conscious – even dialectical. For example, rarely do we find someone saying 'I learned that my previous view or argument was in error'. More likely, one might expect the source to try to influence how the historian of thought will portray his or her image. Given all of the controversy surrounding Friedman's methodology, surely it is not inconceivable that Friedman might like to see a good spin put on how both his famous essay and his methodology will be viewed. When it comes to his view of methodology, it should be clear even with the interview quoted above that, judging by his recent claims, Friedman's memory is no more reliable than Samuelson's was twenty years ago.

Part II

Methodological criticism and neoclassical economics

5 Tautology vs testability in economic methodology

> An approach sometimes used explicitly is the tautological interpretation of utility maximization. ... From this standpoint not only is everyone assumed to maximize all the time, but non-maximization is *assumed* to be out of the question.
>
> Harvey Leibenstein [1979, p. 495]

> The point that 'maximization is not necessarily a tautology' is odd. No one argues it is necessarily a tautology. But it can be used (or interpreted) by some as a tautology. It can also be used by some authors as a factual assertion. It does not help to call it a metaphysical assertion; it need not be. However, it can be less than adequately specified for some purposes.
>
> anonymous *QJE* referee (February 1980)

> This is a 'philosophy of science' kind of article. It suggests that critics of the maximization hypothesis are doomed to futility because there is no way of knowing what is to be maximized.
>
> anonymous *AER* referee (September 1980)

In November 1979 Harvey Leibenstein of Harvard University gave a seminar at Simon Fraser University titled 'Relaxing the maximization assumption in microeconomic theory'. Harvey said that he *chose* not to interpret the neoclassical maximization assumption as a tautology. I challenged Harvey by asserting that he did not know what a tautology is. Specifically, either a statement is a tautology or it is not. It is not a matter of one's chosen interpretation. Tautologies are statements for which it is not logically possible to even conceive of a counterexample. In his June 1979 article, Leibenstein openly recognizes that 'The main argument against the maximization postulate is an empirical one – namely, people frequently do not maximize' [p. 494]. Thus, by recognizing that the failure to maximize is obviously conceivable, Leibenstein must admit that the maximization hypothesis cannot be a tautology.

One problem here is that economists have for a long time misused the term ‘tautology’. Their intention is to refer to statements that are true solely by virtue of how one defines the key (non-logical) words. For example, the statement ‘all swans are white’ could be considered true if we define swans as white birds or we define white as the color of swans. There are other definitions of ‘white’ and ‘swans’ for which the statement is conceivably false. Some philosophers might say that what economists mean by ‘tautology’ is an analytically true statement.

Another problem is that economists think any statement which is not testable must be a tautology. Even allowing for the translation from a tautology to an analytically true statement, they are wrong. There are at least three options, not just two. This is the elementary central point made convincingly by Klappholz and Agassi [1959]. Specifically, they say a statement can be tautological, testable or metaphysical. Thus, contrary to what Hutchison [1938] had implied, it is not true that a statement must be tautological if it is not testable. There are non-tautological statements which are true but untestable – which statements qualify depend on what one means by testing. Since the 1930s, alternative necessary conditions have been used to define testability: verifiability and falsifiability. As a matter of quantificational logic, those that think testing is verification will have to recognize that all non-tautological strictly universal statements (e.g. ‘all swans in the universe are white’) are unverifiable (*even when true*). Those, like followers of Popper, who think testing is falsification willingly recognize that all non-tautological strictly existential statements (e.g. ‘there is at least one unicorn in the universe’) are not testable or refutable (*even when true*) simply because it would be impossible to perform a test in real time that could disprove them. Either way, it should be obvious that it is logically possible for non-testable statements to be non-tautological.

There is a special sense in which one can think of the statement ‘all consumers are maximizers’ as being always true. That is by expanding the statement to include assumed specifics that make it so. Of course, one can just as easily expand the statement to make it testable. In a 1969 article I introduced into economics what the philosopher John Watkins calls ‘All-and-Some’ statements. For example, one might say that ‘for every consumer there exists at least one definable utility function by which he or she maximizes utility such that the Slutsky equation is true’. If one does not specify a ‘such that’ clause, the statement is neither verifiable (because of the universal ‘every’) nor refutable (because of the ‘there exists at least one’). It is the ‘such that’ clause that makes the statement conceivably falsifiable. Moreover, being conceivably falsifiable means that the statement is neither tautological nor analytically true. In my 1969 article, I was trying to say that one can consider Samuelson’s work in the history of

consumer theory to be that of offering specifications of the ‘such that’ clause that are ‘operationally meaningful’ (i.e. falsifiable).

The important point is the following. Seeing ‘scientific’ consumer theory as a process of trying to specify the ‘such that’ clause clearly presents the process as one involving intent on the part of the theorist. Whether one is successful depends on one’s attitude to fostering criticism. One can be very specific regarding the ‘such that’ clause and make the statement easily refuted. If, however, one is vague in the specification, the opposite can result. But those who think careful specification can always make the original statement testable must be cautioned about the so-called Duhem–Quine problem. Even if one could specify the ‘such that’ clause to make the expanded statement testable, any claimed refutation could be easily explained away. For example, one might ask, has the ‘such that’ clause been properly specified? If not, one can easily explain the refutation away as involving only the part constituting the expansion so that the original unexpanded statement is not refuted, only the expanded version.

Despite people often referring to this situation as the Duhem–Quine *problem*, it is really not a problem in the usual sense. It is an unavoidable matter of logic. Instead, the problematic issue is a matter of attitude, namely, a critical attitude. One can always avoid refutations when building models (which amounts to specifying the expansion of the basic behavioral assumptions used in the model). In order to focus on the critical attitude, the question I ask my colleagues who ever ‘dare’ take a stand in favor of the truth of a behavioral assumption (e.g. ‘all consumers are utility maximizers’) is, what would *you* accept as refuting evidence? It is difficult for me to see how a true-believing neoclassical economist would ever deny the unexpanded maximization hypothesis; the ‘such that’ clause will always be blamed. As will be explained in the next chapter, if there is a problem with neoclassical economics, it will not be revealed by criticizing the logic of the maximization hypothesis.

The next chapter is my 1981 article with only minor editorial adjustments. In February 1980 I submitted the original version to the *Quarterly Journal of Economics* since that journal was edited at Harvey’s department. My paper was quickly rejected. Next, I submitted it to the *American Economic Review*. Again my paper was quickly rejected. But there was a change of editors and my paper was evenually accepted for publication. For entertainment’s sake, note that the above-quoted referee reports were directed at the original version of that article.

6 Criticizing the neoclassical maximization hypothesis is futile

10 March 1986

Dear Mr Mongin,

Thank you for the copy of your [1986] paper. Herbert Simon mentioned your paper last October and said that you had refuted something in my 1981 paper. After reading your paper I am disappointed. While your paper has interesting things to say about 'All-and-Some' statements, it has little to do with what I argued in my 1981 paper. Let me explain. ...

My 1981 paper is about *how* the neoclassical maximization hypothesis is used. ... The point of my argument is that the proponent of neoclassical economics can always avoid any claimed refutation (the existence of a function V which does violate at least one of the axioms of consumer theory) by asserting the existence of some other function V which does not violate the axioms. This *strategy* immunizes the neoclassical hypothesis from refutation even when it is false – and regardless of whether the axiom is falsifiable. The question is not whether the statement of the maximization hypothesis is falsifiable but whether the immunization *strategy* can ever be defeated!

Now you will say that if an axiom (such as transitivity) is refuted then any theory based on it is false. You would be correct but only if it could be proven that such an axiom is both independent and necessary. None of the so-called axioms of consumer theory have ever been established as necessary (despite what you claim) – they are always argued to be *necessary for the sufficiency* of the particular axiomatic system proposed. ...

You will undoubtedly still claim that if you have evidence that indicates a violation of an axiom, say transitivity, then no function V that is based on that axiom can be true. This would be so, but only for static utility theory. And worse, the evidence necessary to establish the violation of the axiom must of necessity be non-static (because it requires choices at more than one point in time – remember the choice axiom requires only one choice at a time). The neoclassical believers can always escape by claiming the utility function changes ... but even when they do not, your evidence is *ceteris paribus* and some (Becker and Stigler, for example) can claim that some as yet unknown givens

have changed hence the static conditions are not met in your claimed refuting evidence. In other words, what *you* accept as refuting evidence may not be accepted by the neoclassical true-believer. ...

For reasons that are beyond me, Herbert seems to think that I consider the neoclassical maximization hypothesis as obviously true. Nowhere do I claim that anything of neoclassical theory is true. My task is to expose neoclassical methodology's capability of allowing the unscientific immunization of neoclassical theory from empirical criticism. My view is that any theorist who has to resort to immunizing strategies to avoid obvious criticism should not be taken seriously. But as long as neoclassical economists accept such immunized applications of neoclassical theory, no amount of argument or criticism (such as Herbert's) will ever be successful. Before Herbert's criticisms can be appreciated, economists must reject any immunizing strategies such as those that I have examined in my 1979 JEL and 1981 AER papers...

Sincerely,

L.B.

Your letter of 10 March [to Philippe Mongin] is astonishing. You must be the world's champion keeper of secrets. For the views you express in that letter were certainly hidden successfully in your 1981 AER article. No reader could possibly have guessed that your views are those expressed in the letter to Mongin.

I am highly gratified that you now think (or always thought) that neoclassical theorists should not be taken seriously. We can join hands on that. I think you should announce that to Friedman and Lucas, who believe you are on their side. But you cannot fault Mongin for taking issue with what you actually said in your AER article ... He interpreted your article exactly as everyone else has.

Herbert Simon (1986 correspondence)

The last couple of decades have seen an intensification of methodological criticism of the foundations of neoclassical theory and in particular of the maximization hypothesis. Harvey Leibenstein argued for a 'Micro-Micro Theory' on the grounds that profit maximization is not necessarily the objective of the actual decision makers in a firm and that a complete explanation would require an explanation of intrafirm behavior. He also gave arguments for why maximization of anything may not be realistic or is at best a special case. Herbert Simon's Nobel lecture argued that individuals do not actually maximize anything – they 'satisfice'. And of course, George Shackle had for many years argued that maximization is not even possible.

Some anti-neoclassical economists are very encouraged by these arguments, but I think these arguments are unsuccessful. For anyone opposed to neoclassical theory, a misdirected criticism which by its failure only adds apparent credibility to neoclassical theory will be worse than the absence of criticism. The purpose of this paper is to explain why, although the neoclassical hypothesis is *not* a tautology, no criticism of that hypothesis will ever be successful. My arguments will be based first on the possible types of theoretical criticism and the logic of those criticisms, and second on the methodological status of the maximization hypothesis in neoclassical explanations.

TYPES OF CRITICISM AND THE MAXIMIZATION HYPOTHESIS

There are only two types of criticism of any behavioral hypothesis once one has established its logical validity. One can argue against the *possibility* of the hypothesized behavior or one can argue against the empirical truth of the premise of the hypothesis. In the case of the neoclassical maximization hypothesis, virtually everyone accepts the logical validity of the hypothesis. For example, everyone can accept that *if* the consumer is a utility maximizer, then for the particular bundle of goods chosen: (a) the marginal utility is zero, and (b) the slope of the marginal utility curve at the point representing the chosen bundle is non-positive and usually negative.[1] That is to say, necessarily the marginal increment to the objective must be zero and falling (or not rising) whenever (i.e. without exception) the maximization premise is actually true. Of course, one could substitute the word 'profit' for the word 'utility' and the logic of the hypothesis still holds. In either form, (a) and (b) are the 'necessary conditions' for maximization. Note that there are no 'sufficient conditions' for maximization. Rather, the maximization premise is the sufficient condition for (a) and (b).

Parenthetically, I should note that economists often refer to (b), or more properly to the conjunction of (a) and (b), as a sufficient condition for maximization. This is a common error. Even if (a) and (b) are both true, only *local* maximization is assured. However, maximization in general (i.e. *global*) is what the premise explicitly asserts and that is not assured by (a) and (b) alone. I will return to this in the last section when I discuss the methodological uses of the maximization hypothesis.

THE LOGICAL BASIS FOR CRITICISM

As stated above, there are two types of criticism of the maximization hypothesis: the *possibilities* criticism and the *empirical* criticism. In this section I will examine the logical bases of these critiques, namely of the possibilities argument which concerns only the *necessary conditions* and of

the empirical argument which concerns only the *sufficient conditions*. In each case I will also discuss the possible logical defense for these criticisms.

The possibilities critique: can the necessary conditions be fulfilled?

The possibilities critique builds on the difference between necessary and sufficient conditions. Specifically, what is criticized is the possibility of fulfilling *all* of the necessary conditions for maximization. Of course, this type of critique begs the question as to what are all the necessary conditions. Are there more conditions than the (a) and (b) listed above? Shackle, following Friedrich Hayek and John Maynard Keynes, argued that maximization also presumes that the knowledge necessary for the process of choosing the 'best' alternative has been acquired.[2] That is to say, as a behavioral hypothesis (i.e. about the behavior of decision makers), if maximization is a deliberate act, Shackle argued that the actor must have acquired all of the information necessary to determine or calculate which alternative maximizes utility (or profit, etc.) and he argues that such acquisition is impossible hence deliberate maximization is an impossible act.

This argument appears to be quite strong although it is rather elementary. A closer examination will show it to be overly optimistic because it is epistemologically presumptive. One needs to ask: *why* is the possession of the necessary knowledge impossible? This question clearly involves one's epistemology – that is, one's theory of knowledge. The answer, I think, is quite simple. Shackle's argument (also Hayek's and Keynes') presumes that the truth of one's knowledge requires an inductive proof.[3] And as everyone knows today, there is no inductive logic which could supply a proof whenever the amount of information is finite or it is otherwise incomplete (for example, about the future).

The strength of the Shackle–Hayek–Keynes argument is actually rather vulnerable. Inductive proofs (and hence inductive logic) are not necessary for true knowledge. One's knowledge (i.e. one's theory) can be true even though one does not know it to be true – that is, even if one does not have proof. But I think there is an even stronger objection to the 'true knowledge is necessary for maximization' argument. *True* knowledge is not necessary for maximization! As I have argued elsewhere, the consumer, for example, only has to think that his or her theory of what is the shape of his or her utility function is true. Once the consumer picks the 'best' option there is no reason to deviate or engage in 'disequilibrium behavior' unless he or she is prone to testing his or her own theories.[4]

In summary, the Shackle–Hayek–Keynes inductivist argument against the possibility of a true maximization hypothesis is a failure. Inductive proofs are not necessary for true knowledge and true knowledge (by any means) is not necessary for successful or determinate decision making. Maximization behavior cannot be ruled out as a logical impossibility.

The empirical critiques: is the sufficient premise true?

Simon and Leibenstein argue against the maximization hypothesis in a more straightforward way. While accepting the logical validity of the hypothesis, they simply deny the truth of the premise of the hypothesis. They would allow that *if* the consumer is actually a maximizer, the hypothesis would be a true explanation of the consumer's behavior. But they say the premise is false; consumers are not necessarily maximizers hence their behavior (for example, their demand) would not necessarily be determinable on that basis. Leibenstein may allow that the consumer's behavior can be determined, but it is an open question as to what is the determining factor – utility, prestige, social convention, etc.? Simon seems to reject as well the necessity of determinate explanation although he does discuss alternate decision rules to substitute for the maximization rule.[5]

A denial of the maximization hypothesis on empirical grounds raises the obvious question: how do they know the premise is false? Certain methodological considerations would seem to give an advantage to the critics over those who argue in its favor. Recall that we distinguish between those statements which are verifiable (i.e. can be proven true) and those which are refutable (i.e. can be proven false) on purely logical grounds. As we know, (strictly) universal statements – those of the form '*all X*'s have property *Y*' – are refutable (if false) but not verifiable (even if true). On the other hand, (strictly) existential statements – those of the form 'there are *some X*'s which have property *Y*' – are verifiable (if true) but not refutable (even if false). At first glance it would seem that the maximization hypothesis – 'all decision makers are maximizers' – is straightforwardly a universal statement and hence is refutable but not verifiable. But the statistical problems of empirical refutation present many difficulties. Some of them are well known but, as I shall show a little later, the logical problems are insurmountable.

The methodological problems of empirical refutations of economic theories are widely accepted. In the case of utility maximization we realize that survey reports are suspect and direct observations of the decision-making process are difficult or impossible. In this sense behavioral maximization is not directly testable. The only objective part of the maximization hypothesis is the set of logical consequences such as the uniquely determinate choices. One might thus attempt an indirect test of maximization by examining the outcomes of maximization, namely, the

implied pattern of observable choices based on a presumption that there is a utility function and that utility is being maximized by the choices made.

If one wishes to avoid errors in logic, an indirect test of any behavioral hypothesis which is based on a direct examination of its logical consequences must be limited to attempting refutations of one or more of the necessary conditions for the truth of the hypothesis. For example, in the case of consumer theory, whenever utility maximization is the basis of observed choices, a necessary condition is that for any given pattern of choices the 'Slutsky Theorem' must hold.[6] It might appear then that the above methodological problems of observation could be easily overcome, since the Slutsky Theorem can in principle be made to involve only observable quantities and prices. And if one could refute the Slutsky Theorem then one could indirectly refute the maximization hypothesis.[7] Unfortunately, even if from this perspective such an indirect refutation cannot be ruled out on logical grounds alone, the methodological problems concerning observations will remain.

The fundamental methodological problem of refuting any behavioral hypothesis indirectly is that of constructing a convincing refutation. Any indirect test of the utility maximization hypothesis will be futile if it is to be based on a test of any logically derived implication (such as the Slutsky Theorem). On the one hand, everyone – even critics of maximization – will accept the theorem's logical validity. On the other hand, given the numerous constraints involved in any concrete situation, the problems of observation will be far more complex than those outlined by the standard theory. Thus, it is not difficult to see that there are numerous obstacles in the way of constructing any convincing refutation of maximization, one which would be beyond question.

I now wish to offer some new considerations about the potential refutations of the neoclassical behavioral hypothesis. I will argue here that even if one could prove that a consumer is not maximizing utility or a producer is not maximizing profit, this would not constitute a refutation of the neoclassical hypothesis. The reason why is that the actual form of the neoclassical premise is not a strictly universal statement. Properly stated, the neoclassical premise is: 'For all decision makers there is something they maximize'. This statement has the form which is called an 'all-and-some statement'. All-and-some statements are neither verifiable nor refutable! As a universal statement claiming to be true for all decision makers, it is unverifiable. But, although it is a universal statement and it should be logically possible to prove it is false when it is false (viz by providing a counterexample), this form of universal statement cannot be so easily rejected. Any alleged counterexample is unverifiable *even if the counterexample is true*!

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Let me be specific. Given the premise – 'All consumers maximize something' – the critic can claim he has found a consumer who is not maximizing anything. The person who assumed the premise is true can respond: 'You claim you have found a consumer who is not a maximizer but how do you know there is not something which he is maximizing?' In other words, the verification of the counterexample requires the refutation of a strictly existential statement; and, as stated above, we all agree that one cannot refute existential statements.

In summary, empirical arguments such as Simon's or Leibenstein's that deny the truth of the maximization hypothesis are no more testable than the hypothesis itself. Note well, the logical impossibility of proving or disproving the truth of any statement does not indicate anything about the truth of that statement. The neoclassical assumption of universal maximization could very well be false, but as a matter of logic we cannot expect ever to be able to prove that it is.

THE IMPORTANCE OF DISTINGUISHING BETWEEN TAUTOLOGIES AND METAPHYSICS

Some economists have charged that the maximization hypothesis should be rejected because, they argue, since the hypothesis is not testable it must then be a tautology hence it is 'meaningless' or 'unscientific'. Although they may be correct about its testability, they are wrong about its being necessarily a tautology. Statements which are untestable are not necessarily tautologies because they may merely be metaphysical.

Distinguishing between tautologies and metaphysics

Tautologies are statements which are true by virtue of their logical form alone – that is, one cannot even conceive of how they could ever be false. For example, the statement 'I am here or I am not here' is true regardless of the meaning of the non-logical words 'I' or 'here'. There is no conceivable counterexample for this tautological statement. But the maximization hypothesis is not a tautology. It is conceivably false. Its truth or falsity is not a matter of logical form. The problem with the hypothesis is that it is metaphysical.

A statement which is a tautology is intrinsically a tautology. One cannot make it a non-tautology merely by being careful about how it is being used. A statement which is metaphysical is *not* intrinsically metaphysical. Its metaphysical status is a result of *how* it is used in a research program. Metaphysical statements can be false but we may never know because they are the assumptions of a research program which are *deliberately put*

beyond question. Of course, a metaphysical assumption may be a tautology but that is not a necessity.

Typically, a metaphysical statement has the form of an existential statement (for example, 'there is class conflict'; 'there is a price system'; 'there is an invisible hand'; 'there will be a revolution'; etc.). It would be an error to think that because a metaphysical existential statement is irrefutable it must also be a tautology. More importantly, a unanimous acceptance of the truth of any existential statement still does not mean it is a tautology.

Some theorists inadvertently create tautologies with their *ad hoc* attempts to overcome any possible informational incompleteness of their theories. For example, as an explanation, global maximization implies the adequacy of the consumer's preferences or his or her theory of all conceivable bundles, which in turn implies his or her acceptance of an unverifiable universal statement. Some theorists thus find global maximization uncomfortable as it expects too much of any decision maker – but the usual reaction only makes matters worse. The maximization hypothesis is easily transformed into a tautology by limiting the premise to local maximization. Specifically, while the necessary conditions (a) and (b) are not sufficient for global maximization, they are sufficient for local maximization. If one then changes the premise to read, 'if the consumer is maximizing over the neighborhood of the chosen bundle', one is only begging the question as to how the neighborhood was chosen. If the neighborhood is defined as that domain over which the rate of change of the slope of the marginal utility curve is monotonically increasing or decreasing, then at best the hypothesis is circular. But, what is more important here, if one limits the premise to local maximization, one would severely limit the explanatory power or generality of the allegedly explained behavior.[8] One would be better off maintaining one's metaphysics rather than creating tautologies to seal their defense.

Metaphysics vs methodology

Fifty years ago metaphysics was considered a dirty word but today most people realize that every explanation has its metaphysics. Every model or theory is merely another attempted test of the 'robustness' of a given metaphysics. Every research program has a foundation of given behavioral or structural assumptions. Those assumptions are implicitly ranked according to their questionability. The last assumptions on such a rank-ordered list are the metaphysics of that research program. They can even be used to define that research program. In the case of neoclassical economics, the maximization hypothesis plays this methodological role. Maximization is considered fundamental to everything; even an assumed equilibrium need not actually be put beyond question, as disequilibrium in a market is merely a consequence of the failure of all decision makers to maximize. Thus, those economists who put maximization beyond question cannot 'see' any disequilibria (for example, as with some uses of the Coase theorem).

The research program of neoclassical economics is the challenge of finding a neoclassical explanation for any given phenomenon – that is, whether it is possible to show that the phenomenon can be seen as a logical consequence of maximizing behavior – thus, maximization is beyond question for the purpose of accepting the challenge.[9] The only question of substance is whether a theorist is willing to say what it would take to convince him or her that the metaphysics used failed the test. For the reasons I have given in the previous section, no logical criticism of maximization can ever convince a neoclassical theorist that there is something intrinsically wrong with the maximization hypothesis.

Whether maximization should be part of anyone's metaphysics is a methodological problem. Since maximization is part of the metaphysics, neoclassical theorists too often employ *ad hoc* methodology in order to deflect possible criticism; thus any criticism or defense of neoclassical maximization must deal with neoclassical methodology rather than the truth of the assumption. Specifically, when criticizing any given assumption of maximization it would seem that critics need only be careful to determine whether or not the truth of the assumption matters. It is true that for followers of Friedman's instrumentalism, the truth of the assumption does not matter hence for strictly methodological reasons it is futile to criticize maximization. And the reasons are quite simple. Practical success does not require true knowledge and instrumentalism presumes that the sole objective of research in economic theory is immediate solutions to practical problems. The truth of assumptions supposedly matters to those economists who reject Friedman's instrumentalism, but for those economists interested in developing economic theory for its own sake, I have argued here that it is still futile to criticize the maximization hypothesis. There is nothing intrinsically wrong with the maximization hypothesis. The only problem, if there is a problem, resides in the methodological attitude of most neoclassical economists.

NOTES

1 Note that any hypothesized utility function may already have the effects of constraints built in, as is the case with the Lagrange multiplier technique.

2 Although the Shackle–Hayek–Keynes argument applies to the assumption of

either local or global maximization, it is most telling in the case of global maximization.

3 More will be said about this in Chapter 15 below.

4 Again this raises the question of the intended meaning of the maximization premise. If global maximization is the intended meaning, then the consumer must have a (theory of his or her) preference ordering over all conceivable bundles. At a very minimum, he or she must be able to distinguish between local maxima all of which satisfy both necessary conditions, (a) and (b).

5 Some people have interpreted Simon's view to be saying that the reason why decision makers merely satisfice is that it would be 'too costly' to collect all the necessary information to determine the unique maximum. But this interpretation is inconsistent if it is a justification of assuming only 'satisficing' as it would imply cost minimization, which of course is just the dual of utility maximization!

6 For an elementary explanation of the mechanics of the Slutsky Theorem, see Varian 1993, Chapter 8.

7 For example, if one could show that when the income effect is positive but the demand curve is positively sloped, then the Slutsky Theorem would be false or there is no utility maximization [see Lloyd 1965].

8 See n. 4 above. If one interprets maximization to mean only local maximization, then the question is begged as to how a consumer has chosen between competing local maxima.

9 For these reasons the maximization hypothesis might be called the 'paradigm' according to Thomas Kuhn's view of science [1970]. But note that the existence of a paradigm or of a metaphysical statement in any research program is not a psychological quirk of the researcher. Metaphysical statements are necessary because we cannot simultaneously explain everything. There must be some exogenous variables or some assumptions (for example, universal statements) in every explanation whether it is scientific or not.

7 Appraisal vs criticism in economics

> Boland's assertion that there are only two forms of criticism of a logically valid behavioral hypothesis is true if one counts only logically compelling criticism as legitimate or important. But such a narrow definition of criticism is of little use when one considers the appraisal of scientific theories ... In the evaluation and criticism of scientific theories, a number of criteria of appraisal may be employed, depending on the purposes of the theory in question: predictive accuracy, simplicity, generality, heuristic value, mathematical elegance, plausibility, and extensibility ... The definitions of, and the relative weights that should be attached to, such criteria have provided the grist for numerous debates in economic methodology. Criticism can also take place on another level – Karl Popper's distinction between internal and external criticism comes to mind. In short, there are many routes to criticism in the appraisal of scientific theories; Boland's definition of criticism is overly narrow. His restricted definition causes Boland to misinterpret the writings of other economists...
>
> Boland's argument provides a textbook example of why logical empiricists since the mid-1930s have avoided discussing scientific theories in terms of the cognitive significance of individual sentences. Their predecessors, the logical positivists, tried to rid scientific discourse of the metaphysical ... [I]t was necessary to find some criterion by which legitimate [empirical] statements could be separated from metaphysical ones. Testability was the criterion chosen, but making that notion concrete proved difficult.
>
> Bruce Caldwell [1983, pp. 825–6]

In his 1983 comment on my 1981 article Professor Caldwell scolds me for ignoring the contributions of modern philosophers. Supposedly, if I were to appreciate the contributions of the 'logical empiricists since the mid-1930s' I would see the error of my ways. Caldwell chooses to focus on my view of criticism – namely, that for any criticism to be successful it must be decisive or, as he says, 'logically compelling'. He feels this concept is 'overly narrow'. Now this kind of discussion can get quite awkward. Is his

criticism of my 1981 article intended to be logically compelling, or something else? For my purposes I will assume that he is intending to convince us of something – in this case it is that my concept of criticism is too narrow. Specifically, I understand him to be saying that there are other concepts which are less narrow and thus my concept is only a special case.

My reply is that he is exactly wrong.[1] The idea that criticism must be logically compelling is not narrow but instead the broadest possible. And worse, Caldwell's concept of criticism is completely inadequate. The implication that we should avoid 'logically compelling' criticism in favor of his recommended weaker line of argument (which he calls 'appraisal') is merely an expression of his advocacy of the commonly promoted, but logically inadequate, methodology which followers of Popper today call 'conventionalism'.

APPRAISAL AS CRITICISM

From the standpoint of logic, it would seem to many of us that there is only one form of 'logically compelling' criticism – namely the demonstration that the argument being criticized leads to a contradiction and thus is logically invalid. Note that this was not the basis of my argument in my 1981 article. My article was about arguments about the neoclassical maximization hypothesis that arise *after* 'one has established its logical validity'. That is, the maximization hypothesis, like any hypothesis, asserts that if certain prior conditions are met then *necessarily* particular subsequent conditions will be met. In short, if the prior conditions are all true then the subsequent conditions will also be true. Logical validity concerns the term 'necessarily', and thus if logical validity has been established the hypothesis can only fail to explain the truth of the subsequent condition because one or more of the prior conditions cannot be or are not met. The question of logical validity is about the possibility of the hypothesis being employed in a successful explanation and the question of meeting prior conditions is about the empirical truth of an explanation based on the hypothesis.

Now I will have to agree that even these two forms of criticism can be seen to be invocations of logically compelling criticism. Any condition is truly impossible only if it leads to a necessary contradiction (a classic economics example is Arrow's 'possiblity theorem') and a condition is empirically false whenever the conjunction of it with an observation statement necessarily yields a contradiction. So, if Caldwell is correct there must be some other form of criticism which can never be reduced to a claim that there exists a contradiction.

What alternative does Caldwell offer? First he gives us a list of conventionalist criteria – simplicity, generality, mathematical elegance, plausibility, etc. Presumably, such criteria are to be used to 'appraise' economic explanations in the same way a welfare function is used to appraise alternative economic policy recommendations. In effect, the methodology of appraisal is a variation on neoclassical analysis.[2] The difficulty is that the use of such criteria in the appraisal of theories or models fails to fulfill its objective for the very same reason that plagues welfare economics – there does not exist a universal criterion that will work in all cases. Nevertheless, how are we supposed to use such criteria to form a criticism? Perhaps one is supposed to adopt a criterion and use it to measure a given model or theory. Now the only possibility of a criticism that can be advanced against the given theory is that *if* one's aim is to maximize according to the accepted criterion then the criticized theory somehow fails to achieve the maximum. But failure to achieve one's aim can easily be reduced to a failure to avoid a contradiction between one's aim and one's achievement. In other words, Caldwell's first alternative form of criticism, the use of conventionalist criteria, does not avoid the use of 'logically compelling criticism'.

His second alternative, 'Popper's distinction between internal and external criticism', meets the same fate. Internal criticism is merely based on the theorist's acceptance of his or her aims and failure to achieve them.[3] External criticism centers on criticism of the theorist's aim by measuring it against some externally given criterion. In either case my above discussion of aims applies but with a more general view of what constitutes an aim. In short, Caldwell's second alternative involves criticism that a given explanation fails to solve some particular theoretical problem – thus it does not avoid being reducible to 'logically compelling criticism'.

All forms of criticism depend on aims, criteria, testing conventions, etc. which are put beyond question *for the purposes of the criticism*. Any criticism succeeds only by showing that to remain consistent the only alternative is to give up one's aims, criteria, etc. So, contrary to Caldwell's arguments, my concept of criticism is not narrow but rather it is the most general since all forms of criticism can be reduced to matters of logical consistency.

THE POVERTY OF CONVENTIONALIST METHODOLOGY IN ECONOMICS

The only contribution of 'logical empiricists since the mid-1930s' has been to deflect interest from the difficult question of the empirical truth (or falsity) of an explanation to the more convenient question of the logical validity of the argument formed by that explanation. Rather than arguing

about whether a theory is true or false we are supposed to choose between the available theories using some criterion such as 'simplicity' or 'mathematical elegance'. Instead of claiming that a theory is true or false, we are supposed to judge it as being better or worse than any other theory according to the accepted criterion. Obviously, this approach to methodology leads to an infinite regress.[4] By what super-criterion do we choose the best criterion?

Each of the criteria listed by Caldwell has its advocates and critics. No logical empiricist would ever claim that his or her employment of a super-criterion to choose a particular theory constitutes a proof that the theory is actually true. But if it is not intended to be a proof, what is accomplished? Or, better still, how do we know when any critical 'appraisal' is successful and when it is not?

The idea that we should appraise rather than criticize the theories of economists is promoted because most people feel that decisive criticism fosters unproductive fights and controversies over the truth or falsity of theories. Supposedly, reasonable people would see that theories cannot be proven absolutely true or false. And thus, to be productive we should be more tolerant. The difficulty with this 'reasonable' view is that it fails on its own terms. The assertion that all theories are neither true nor false is itself merely another theory – one which is claimed to be true!

Some economists may feel better about advocating appraisal rather than advocating 'logically compelling' criticism, but it only postpones the arguments as we would still have to decide which appraisal criteria are true in order to make our appraisals. The room for fights and controversy is even greater when it comes to choosing one's criteria.

NOTES

1 The remainder of this chapter is based on my 1983 reply to Caldwell's comment. Those parts that appeared in my published reply are used here with the permission of the publishers of the *American Economic Review*.

2 It is for this reason that I examined methodology in these terms [Boland 1971]. A more general criticism of conventionalist theory-choice criteria – namely, that conventionalism is self-contradictory – is offered in my 1982 book as well as Chapter 8 below.

3 See, for an example of internal criticism, Wong 1978, pp. 23-4, which is about criticizing Samuelson's revealed preference theory in terms of Samuelson's declared aims.

4 See further my discussion of the methodological problems of multiple criteria in Boland 1974 and 1992a, Chapter 12.

Part III

Criticizing the methods of economic methodology

8 The theory and practice of economic methodology

Truth becomes fiction when the fiction's true;
Real becomes not-real where the unreal's real.
Cao Xueqin [1791]

Man must strive, and striving he must err.
Goethe [1808]

In March 1985 Donald McCloskey and I were invited to Laval University in Québec to deliver papers on methodology. When we got there we discovered that the hosts had billed our papers as a debate. Since McCloskey and I agreed on many points, the idea of a debate was amazing. In particular, we totally agreed that usual methodology is authoritarian and thus a waste of time.

Prior to arriving in Québec I gave considerable thought to what I would present to a group consisting of faculty and graduate students in Laval's Administrative Science program. In advance I knew that French-speaking scholars who study methodology usually do so in the European tradition. That is, they usually start from a Cartesian perspective where any thinker can be located as holding a 'position'. As noted before, where one is located in economic methodology has been determined by whether or not one agrees with Friedman's 'as if' instrumentalism. Since my perspective on methodology starts with a rejection of any position concerning the correct or best 'scientific method' and instead I consistently promote methodology as a program of systematic criticism (the program demonstrated repeatedly by Popper and Agassi), I thought I would take the opportunity to explain why my research on methodology is unlike that of any other methodologist in economics. My presentation at Laval is one of the few times I have talked about methodology in the traditional sense. This seemed necessary because in my periodic dealings with traditional methodologists at various conferences, I felt that we were always arguing at cross-purposes since almost everyone I argued with expected me to be taking a 'position' on traditional issues in methodology, issues which, as I explained in detail in my 1982 book, I dismiss as artifacts of inductivism. My primary objective was to explain why I do not take a position on the traditional issues.[1]

In this chapter I will present my non-inductivist views concerning the theory *and* practice of economic methodology.[2] It will not be about the usual worn-out issue of theory *versus* practice. Identifying the methodology which economists actually practice is more interesting than asking if they practice what they preach. To a certain extent this is an empirical question, and, like all empirical questions, we need a theoretical framework for the examination of the empirical detail. For this purpose I will present the theory of methodology that I have been using for the last thirty years or so. Armed with this theory of methodology I will discuss some of the ways methodology is practiced in economics today.

KNOWLEDGE AND TRUTH STATUS: HISTORICALLY SPEAKING

Traditionally, methodology is considered to be about the identification of 'correct' answers to important questions. Whenever someone claims their answer is correct, the question methodologists might ask is, 'How do you know your answer is correct?' Needless to say, the question has been asked countless times. Many people today view 'science' as the embodiment of 'correct answers', and 'scientific method' as *the* only sure way to demonstrate that one's answer is 'correct'. Of course, methodology has been discussed for centuries. The currently popular belief in Science and Scientific Method is based on a 350-year-old methodology that was refuted 200 years ago.[3]

Since our modern view of methodology has its roots in philosophical problems, a good starting point for the study of methodology is history itself. But 350 years of history is surely filled with an excessive amount of detail. So I will have to simplify the historical detail by presenting a 'theoretical history' concerning the common interest in correct answers and in the methods alleged to yield correct answers. My objective is to explain 'historically' why there has been a concern for a method of knowing the 'correct answers'. From this we may learn why we find popular methodology frozen at a point just one step beyond its refutation in the eighteenth century.

Thinking and 'correct answers'

I suppose I should begin my 'theoretical history' with a disclaimer like one of those found at the beginning of some movies: 'All characters in this story are fictional; any resemblance to real persons is purely *intentional*.'

Students today are too often taught that the primary objective of learning, or even thinking, is finding the correct answers. The basic presumption is that 'knowing is knowing the truth'. It has *not* always been that way. Before the time in which Socrates is supposed to have lived (say, prior to 450 BC) many people considered thinking to be a process of discovering or inventing all of the possible or conceivable answers to any given question. That is, thinking people did not necessarily begin with a burning desire to know the correct answers.

Among the so-called Pre-Socratics were some fellows whom I shall call Sophists. These fellows maintained that there just had to be correct answers. But whenever a Sophist thought he knew the correct answer he could not always prove it to be correct merely by arguing directly in its favor – that is, by simply giving reasons to prove the truth of the answer. Some of these Sophists devised an indirect way to argue in favor of their chosen answer. This Sophist's method, which is still followed today by some members of the so-called Chicago school of economics, proceeds as follows.

First, the Sophist must claim (or presume) that there is a finite number of conceivable answers to any given question. For example, for some questions there are only two possible answers – 'yes' or 'no' (a response such as 'who cares?' is not an answer). The second step is for the Sophist to attempt to refute *all other* answers. If the first step was successful – that is, if all possible answers have actually been listed – then the refutation of all answers other than the one thought to be true would mean that the favored answer is revealed to be the correct one.

The success of this Sophist argument depends primarily on there being a *finite* (and mutually exclusive) set of possible answers. Very often, Sophists argue without always being sure they have identified *all* of the answers. They might not have identified all answers if a complete search takes a long time. In general, the Sophist argues by criticizing competing answers in hopes of convincing everyone that the Sophist's favored answer is the correct one. But the Sophist's argument can work only when *all* of the possible answers have indeed been identified and *all* of the *competing* answers have been refuted.

Knowledge, authority and method

Unfortunately, the legacy of the Sophists is an excessive concern for (quickly) finding *the* correct answer – rather than for (slowly and carefully) identifying *all* the possible answers. For many questions it would be difficult even to list all the answers let alone determine *which* one is correct. But people demand (correct) answers. Politicians and kings demand answers, governmental agencies demand answers, and even corporation directors demand answers. Given these demands, it is easy to understand how the institution of 'authority' might be seen to be able to overcome the insufficiencies of logic – authority gives people answers quickly.

Galileo and the authorities

For hundreds of years the Church was the institutionalization of authority. Its College of Cardinals would decide what we were to consider true knowledge. It is this tradition that faced Galileo (1564–1642). Galileo believed that the truth of one's knowledge could not be decided with a vote by a group of individuals – even a group of cardinals. Rather, the truth of one's knowledge would have to be decided by the real world. Galileo is said to have climbed to the top of the Tower of Pisa to demonstrate the truth of his knowledge of falling bodies. This was particularly challenging to the 'authorities' and thus Galileo was not very popular with them.

As is well known, Galileo ran into difficulty with the Church 'authorities' because he taught his students about a theory of heavenly bodies authored by Copernicus (1473–1543). Galileo's problem was that the authorities had given their approval to the competing theory of Ptolemy (AD 100–170). As the simple story usually goes, the approved Ptolemaic theory was that the earth is the center of the universe and all the planets and stars revolve in circles around the earth. In a more complicated form the Ptolemaic theory allowed for epicycles (the path of a point on a rolling circle) in place of perfect circles.

Galileo chose to discuss the Copernican theory which put the sun rather than the earth at the center of rotation. The Copernican theory was a direct challenge to the authorized Ptolemaic theory. To maintain the authority of the Church, Galileo was told to stop teaching his students about Copernicus. But Galileo responded that *people* cannot dictate which answer is true, nor is the truth of one's knowledge a matter of authoritative opinion. The truth of one's knowledge is a matter of its objective relationship to the 'real world'. If you think you know something about falling bodies, you can climb with Galileo to the top of the tower and test your knowledge.

But the Church authorities replied, so my story goes, that Galileo simply

had no authority to challenge the authorities or even authoritarianism. Furthermore, the Church *did* have the authority and the overwhelming power to prevent Galileo from challenging it. With a simple show of their immense power, Galileo was forced to give in. He was banished to southern Italy and no longer taught his students about the Copernican view.

The humanists' challenge and their social contract

Another reaction to the authoritative Ptolemaic view that 'earth is the center' was the claim that by accepting this view we are actually led to further considerations which might also contradict the authority of the Church. Specifically, it was argued by some of those who witnessed the Church's victory over Galileo that if the earth is the center of the rotation of heavenly bodies then potentially Man or humanity is the center of rotation. I shall call this interpretation of the Ptolemaic view *humanism*. Although there were many different aspects to this extension of the Ptolemaic view (e.g. the rise of Protestantism), I will be concerned only with what it means for our modern view of knowledge. The humanist's argument was, in effect, that if Man can be the center of everything, then all knowledge can reside in the minds of humans.

My concern here will not be with whether the humanist's view of the possibility of human knowledge is a logically sound view or even an acceptable view on its own. Rather, I will be concerned only with how it challenges the authority of the Church in all matters and in particular in matters of knowledge. Since the Church accepted the responsibility of determining what is (or is not) correct knowledge, there would seem to be little room left for independent human knowledge. No individual person was allowed to claim that his or her knowledge was true without the authoritative approval of the Church. But the humanists claimed that one's knowledge could be true regardless of the opinion of Church authorities.

The Church authorities were unable to fight back as effectively as they did in Galileo's case. For one thing, all overwhelming or excessively powerful victories have a common problem – the victors tend to be discredited in the eyes of the spectators and critics. Such was the case with the victory over Galileo. Thus, the Church authorities had to be more careful with the humanists. The tactic adopted by the Church was to offer the humanist challengers a 'deal' – namely, a specific social contract.

Now, my story of an explicit confrontation between the Church authorities and the humanists may very well be entirely fictional – I was not there. I can only propose the following heuristic viewpoint. While the Church authorities wanted to defeat the challenge of the humanists, the humanists wanted to establish that humans could possess correct or true knowledge.

The authorities offered the following contract: Any individual can claim to have knowledge *only if* he or she can prove or 'justify' its truth.

The humanists eagerly accepted and signed the offered contract. I shall henceforth call this hypothetical contract the *Social Contract of Justification*. Although the humanists did not realize it, by signing they had agreed to play a 'no-win' game with the authorities – which of course is exactly why the authorities wanted to play (from the authorities' standpoint, it was a 'no-loss' game). But before I explain this, let me first consider why the humanists were so eager to agree to play.

The authority of justification

The reason why the humanists were willing to sign the Social Contract of Justification was simply that they thought there would never be a problem proving one's knowledge to be true whenever it is true. Today it is difficult to see why they could have thought that it would be so easy. If we try not to be wise in retrospect, we can see that the reasons were easy. Far from the direct power of the Church in southern Europe, there was one thinker – Francis Bacon (1561–1626) – who was arguing that if one was 'scientific', one could always provide rational arguments for the truth of one's knowledge. Thus, Bacon was the humanists' 'secret weapon'. Bacon's inductive Science would be their alternative to the Church's authority.

Before examining the nature of Bacon's Scientific Method of proving the truth of one's knowledge, we should ask why this Scientific Method might be of interest to the humanists or anyone else. I think the reason is simple. By justifying one's knowledge using the Scientific Method, the Method itself replaces the authority of the Church. The Scientific Method is not a challenge to authoritarian*ism*. Rather, it is merely a challenge to those who play the social role of authorities.

The Scientific Method

The Scientific Method of Bacon promised that whenever your knowledge is true, you could always prove the truth of your knowledge by following his method. The promise of the Scientific Method is founded on the following doctrines: (1) *Truth is Manifest* in Nature (i.e. the truth of anyone's knowledge of the real world is manifest and thus discoverable in the real world); and (2) *To Err is Sin* (thus, error must be avoided). An appreciation of these two doctrines is essential for a clear understanding of Bacon's Scientific Method. So let us examine his doctrines.

These two doctrines are not independent. If 'truth is manifest', truth is there to be seen. Only people who blind themselves to manifest truth would

ever make false claims – that is, make claims that their (false) knowledge is true. But would anyone ever be so blind? Bacon argued that blindness to the truth is a symptom of prejudice and impatience for success and fame, and both are consequences of greedy self-interest. Since greedy self-interest is often considered a Sin, it is a Sin to make a false claim about the truth of one's knowledge. To avoid Sin, one must not make any claim *until* one has gathered the facts to prove it true. Only a greedy, impatient, self-interested person would commit the error of jumping to a conclusion without first collecting all the facts.

The warning 'do not jump to conclusions' is both the key to Bacon's Scientific Method and its primary legacy. When following his Method, one must always be careful, patient, unprejudiced, open-minded, diligent, etc., and if one works hard and long enough (i.e. collects enough facts) then one *cannot* commit an error. Bacon's Scientific Method then is a recipe. Every scientific investigation begins with an unbiased collection of data, followed immediately by a logical demonstration (i.e. 'proof') of any knowledge derived from the collected data. Thus, Bacon's Scientific Method is both a method of assuring that the collected facts are beyond question since the *collector* was scientific (i.e. unbiased, unprejudiced, etc.) and a method of justifying claims to true knowledge.

The scientific facts are accordingly the primary basis for any rational argument in favor of one's knowledge – one's *human knowledge*, that is. Thus, we see why the humanists saw Bacon's Scientific Method as their secret weapon. The humanists saw no risk in putting their signatures on the Social Contract of Justification since Bacon's Scientific Method assured them that there existed a way to prove one's knowledge true whenever it is true. And most important, the proof, the rational argument consisting only of the unbiased scientific facts, would never require the authority of the Church.

The success of the Scientific Method

It was often claimed that there were many examples of successful applications of Bacon's method. The most famous is Newton's physics. Isaac Newton (1642–1727) claimed to have arrived at his 'Laws of Physics' by using the Scientific Method. With Bacon's Scientific Method, a proposition about the nature of the real world can be called a 'Law' only after it has been proven beyond a shadow of a doubt. Can one ever argue with someone who claims that his knowledge has been arrived at by the Scientific Method?

The promises of the Scientific Method even go so far as to suggest that all knowledge of the world can be shown to be based on real-world experience – that is, on empirical data. It promises that it is possible to show that our knowledge is based only on facts since the logical demonstration of the truth of one's human knowledge will be based only on the scientific collection of empirical facts – gathered, so to speak, by experience.

Knowledge versus psychologism

The problematic 'no-win' contract: the Social Contract of Justification

For a long time Bacon's Scientific Method reigned as the solution to the problem of providing the rational basis for human knowledge. In short, all human (i.e. all subjective) knowledge could be shown to follow *logically* from objective facts or experience. In this light, there are only two elements that constitute human knowledge: (i) facts or experience, and (ii) logical proofs. But this also means that the humanists, by relying on Bacon's Scientific Method, signed a contract which had a built-in contradiction. Let me explain.

Specifically, if human knowledge must be justified by logical proofs using only empirical facts, where is the *humanity* in human knowledge? Clearly, if facts must be found in the objective real world, they are not human. This leaves only the logic of the argument in favor of one's knowledge. If there is humanity in human knowledge, as the humanists hoped, it must reside in the logic of argument.

Now, it should be easy for anyone living today to see that this is a problem. Consider the use of computers and consider that there are satellites circling the earth and others travelling by Jupiter and Saturn. These are merely logical machines and some of them just collect facts, without the hand of any human. It is not difficult for us to see that today there is no humanity in being logical. Logical decisions can be represented by a machine without any human having to make real-time decisions. In fact, the entire essence of logical proofs is their universality – *anyone* can understand them. The inventor of the proof does not have to be present to explain the proof.

Whenever the humanist *is* successful in justifying the truth of his or her knowledge with a logical proof using only empirical (objective) facts, he or she has produced something which is necessarily *not human*! Thus, *there is no humanity in (justified) human knowledge*. This means that the Church has defeated the challenge of the humanists on at least one count. The legacy of this apparent defeat is simply the common view that rationality or logic is itself the humanity in human knowledge. After all, as it has been often argued: How do we distinguish humans from mere animals? – Well, of course, animals cannot reason!

The problem of the infinite regress

There were more serious problems for the humanists. The adequacy of logical proofs was always suspect. For a logical proof to be a justification, it must be possible to demonstrate the proof for all to see. Failure to do so is evidence of an error. An example of a failure to demonstrate is the so-called 'infinite regress'. If we give reasons for *why* some particular statement is true, we might be asked to show why we think our reasons are true. Following the Social Contract of Justification, we must step backward and provide another set of reasons to prove the truth of the first set of 'reasons'. But if that is possible, then any subsequent reasons can also be questioned. This requires still another backward step and another set of reasons. There is no limit to the number of required sets. Hence, we have an infinite regress. Such a possibility means that one could never provide a complete (and thus finite) proof of one's knowledge.

This is precisely the challenge of David Hume (1711–76). He argued that there did not exist any objective logic that could do the job of providing a logical proof of one's knowledge based *only* on experience. This is a serious indictment of Bacon's Scientific Method. It means that one cannot even get started. For example, whenever one claims to have collected the facts to prove one's knowledge is true, someone else can ask for an additional proof showing that one's facts are true as well as logically sufficient. In face of these difficulties, nineteenth-century romantics would have us consider relaxing the doctrine that to err is sinful. So, today most people would instead accept Goethe's claim that to err is human.

The real source of the problem for the believers in the Scientific Method is that the Method depends on the existence of an inductive logic – a logic which can proceed from the truth of particulars (of experience) to the truth of general statements such as those which comprise anyone's knowledge. Although Hume may have recognized that such an inductive logic does not objectively exist, he argued that people still claim that, on the basis of *their* experience, they know that particular statements are true and those 'knowers' are often correct. Hume concluded that they therefore must have a workable inductive logic in their heads. Thus, we see how the study of knowing becomes the study of the *mind of the knower* – that is, of the psychology of *knowing*. If there is no objective rational proof of one's knowledge, then there can only be subjective proofs of one's knowledge. In this case, every rational proof of knowledge reduces to a study of the psychology of the knower.

Romanticism and neo-romanticism

The consequence of Hume's argument that knowledge exists in the minds of people, rather than in objective proofs which might please the Church, is that the minds of humans matter more than 'the facts' since the facts themselves must exist in the minds of humans. For many people today things have not progressed beyond Hume's observations. Most of the romantic literature of the early nineteenth century is merely examining the ultimate in truth – everything is centered in the human mind rather than in objective rationality. Even the existentialists (or neo-romantics) of the early twentieth century adopted the view that everything may be a product of the mind – hence everything may be arbitrary. In either case, the justification of human knowledge is supposed to be based on the rationality of the human mind and thus justified knowledge is a product of Human Nature.

An ultimate reliance on Human Nature as the foundation of explanations is precisely what some philosophers today call psychologism. It is this type of explanation which was rejected in my 1982 book and, of course, in Karl Popper's writings. But, as can be seen from my heuristic history of human knowledge, psychologism is only a symptom of a more serious problem – namely, the signing of the 'no-win' Social Contract of Justification by the eager and optimistic humanists.

Anti-justificationism

There is no reason why anyone today should consider themselves bound to abide by a contract they did not sign. Thus, everyone is quite free to make any claims they wish. That anyone *thinks* his or her theory is true does not guarantee the truth of that theory. Conversely, not knowing the truth of one's theory does not guarantee that the theory is not true. Likewise, the truth of one's theory or knowledge cannot be decided by a vote – simply because, even when the vote is unanimous, the voters could be unanimously wrong!

EPISTEMOLOGY VS METHODOLOGY: THE THEORETICAL PERSPECTIVE

The primary object of my heuristic story was to identify three elementary notions: (1) the doctrine of Manifest Truth; (2) the doctrine that To Err is Sin and thus error must be avoided; and (3) what I called the Social Contract of Justification. I turn now to examine one particular theoretical legacy of that contract – namely, the historic fusion of questions of epistemology with questions of methodology. The distinction between epistemology and methodology can be simply stated. Epistemology is concerned with the *nature* of knowledge (i.e. with *what* is knowledge) and methodology is concerned with *how* knowledge is acquired. In other words, epistemology is like a restaurant's menu whereas methodology is more like a street map showing how to get to the restaurant.

Sensationalism, methodology and epistemology

In addition to these three elements of theories of knowledge and methods of knowing, I wish to make explicit the common-sense notion about learning that says all knowledge comes by way of the senses. This view, called sensationalism, is the foundation of virtually all views of methodology and epistemology and is responsible for the fusion between epistemology and methodology. Here, I want to focus on the two major views which are based on sensationalism – inductivism and conventionalism – because, as I have been saying, they are found at the root of all methodological controversies and prescriptions in economics today.

One way to understand any theory is to understand the intellectual problem at issue. One can always take a retrospective view of any theory by conjecturing what problem is solved (intentionally or not) by that theory. This will be my program here for the study of methodology. Specifically, I will conjecture a problem situation in order to explain the existing views of methodology.

Throughout its long history, methodology has served to solve both epistemological and sociological problems. That is, methodologies have existed to deal with knowledge itself and with society's view of knowledge. Before discussing the specific matter of methodology in economics, I will attempt to formulate a general theory of methodology by discussing some of the philosophical and social problems that methodology has been, at times, thought to solve.

The primary philosophical problem that methodology has been said to solve arises out of various theories of knowledge which are based on the aforementioned Manifest Truth doctrine – namely, the doctrine that truth is there to be seen or discovered. The problem is: 'How do we mere humans *uncover* the truth without making errors if "to err is human"?' The 'how' will depend on the details of one's theory of knowledge, that is, on one's epistemology.

From the standpoint of sensationalism, the epistemological question ('What is knowledge?') is answered when one answers the methodological question ('How do I know?'). According to sensationalism, the answer to the second question is: '*I* know only by having either "observable facts" or "demonstrable truths"'; hence, 'knowledge is essentially factual or demonstrable'. This latter conclusion precludes the existence of theoretical knowledge, that is, of knowledge which is not based on sense observations or demonstrable 'truths' alone. The next question is: '*How* does one *have* the "facts" or "demonstrable truths"?' Is this methodological question separate from epistemological questions ('What are "facts"?' and 'What are "demonstrable truths"?')? The question of how one knows is not separable from specifying what the facts are or what is provable. The result is that methodology traditionally deals with the epistemological questions 'What are facts?' and 'What are demonstrable truths?'. If one followed Hume (as discussed in Chapter 3 above), the question of *how* I know would be considered a psychological phenomenon.

Inductivism

One variant of sensationalism which has been attributed to Bacon is what I have been calling inductivism. Inductivism needs to be further examined because it has been institutionalized. Its institutionalization has overcome its weak foundation, namely, the belief in the existence of an inductive logic. Inductivism attempts to answer simultaneously the methodological question 'How do I know?' and the epistemological question 'What is knowledge?'. It does this by attempting to objectify knowledge – that is, by making the logical basis of knowing non-psychological.

Bacon's inductivism objectifies knowledge by eliminating subjective influences in the process of establishing the 'facts'. Once the 'facts' are established the mental process becomes irrelevant since it can be replaced by a non-subjective inductive logic. To do this the *existence* of an inductive logic is simply assumed. Truth then will be manifest in the 'facts' if the facts and the logic are independent of human influence. For inductivist-sensationalism, methodology is thus a procedure which eliminates human influences and thereby minimizes error.

There are two important and well-known variants of inductivist-sensationalism. One is the verificationism associated with the twentieth-century 'logical positivists' and the other is classical empiricism. Both were mentioned in Chapter 3 above and are well known to economists. Both have to do with the status of theories in the nature of knowledge. All that inductivism says is that if theories exist they must have followed *inductively* from the existing facts (hence cannot go *beyond* the facts). Verificationism allows for hypothetical leaps beyond the available facts so long as one goes back later and verifies the hypotheses with facts. It is in this spirit that we are urged to say something is 'hypothetical' if not known to be true. For classical empiricism all theories must always be directly related to *existing* facts. That is, no theory can go beyond experience –

theories only *represent* our experience.

Most details of any inductivist methodology are concerned specifically with the question 'What are "facts"?' (e.g. distinguishing between positive and normative statements). This question needs to be answered in order to answer the primary methodological question 'How do *I* know?'. The question 'What are "facts"?' is dealt with by explaining *how* one should collect them. The quality of the facts is supposed to be related to the personal competence of fact collectors (e.g. collectors must be unbiased, unprejudiced, clear-thinking, etc.). From this perspective methodology is seen to be concerned with the personal mode of behavior of the 'fact collector'. In particular, can just any ordinary individual's observation report be accepted as a 'fact' worth noting or using? Obviously not.

Despite all its philosophical problems and controversial aspects, inductivist methodology lives on as ritual. Textbooks are written to satisfy inductivist principles, curricula are organized according to inductivist learning principles (viz learning from examples, no speculation before data collection, practical questions before theoretical ones, etc.).

The *combination* of the doctrine of Manifest Truth and the doctrine of sensationalism fails without something like an inductive logic. Although the combination has been institutionalized in academic economics through curriculum and textbook rituals, it is striking that it is no longer *openly* adhered to among economic methodologists. How does one abandon this combination of doctrines? There are three options available – abandon sensationalism, abandon Manifest Truth, or abandon both.

The view which results when denying sensationalism while still maintaining Manifest Truth is merely the well-known and oft-despised 'apriorism'. If we were instead to drop the doctrine of Manifest Truth but retain sensationalism we would construct the foundations of the philosophy I have been calling conventionalism. If we drop both doctrines we obtain the basis of Popper's views of methodology.

With apriorism all methodological matters reduce to matters of deductive logic (i.e. ordinary logic), hence reference to the real world is unnecessary. We need not discuss this further since there are so very few apriorists today. By denying Manifest Truth, conventionalism suggests that our senses need help – that is, that the facts we collect are always 'theory-laden' since factual reports contain theoretical elements which cannot be separated out. Conventionalism is the methodology which McCloskey [1983] calls 'Modernism'. Conventionalism needs to be clearly understood because it is both the methodology advocated today and the basis of most methodological arguments in economics.

Conventionalism

Given the hypothesized Social Contract of Justification, should *all* facts be theory-laden, the basis of knowledge would still need to be objectively justified yet this would in turn lead to an infinite regress. The combination of the failure to provide an inductive logic to make inductivism work with the failure to justify (rationally) any knowledge within the doctrine of sensationalism has always been the basis for many bitter disputes within the sciences and between scientists and non-scientists. How congenial the world would be if an inductive logic could be found. Almost all disputes could be rationally resolved since everyone could appreciate the logic. Another way to avoid disputes over whose theories are supported by facts, and thereby shown to be true, would be to relinquish the idea that theories can be either true or false.

Giving up truth and falsity does not avoid a primary sensationalist problem – that is, the avoidance of controversies and disputes over whose senses have produced knowledge. Many think that what is still needed is an objective authority – something to substitute for the previous combination of inductive logic and Manifest Truth. It might be said that without an objective authority we would have mere ‘existentialism’. The solution to the implied problem is rather easy, it would seem. We can still rely on rationality itself (i.e. deductive logic and mathematics) to be the needed objective authority. This is just the program of a conventionalist alternative to inductivism, namely to rely on universal rationality without giving up sensationalism.

Conventionalist methodology is concerned also with the question ‘What are demonstrable truths?’. Like inductivism, this question needs to be answered in order to answer the primary, but now modified, question ‘How do *we* know?’. Without Manifest Truth, conventionalist methodology consists of a set of (social) conventions or decision rules for accepting a given theory or for choosing one theory from a set of competing theories. The need for a (rational) choice exists because (the retained) sensationalism denies the existence of informative theories (i.e. information beyond the facts or known truths). The appearance of informative theoretical knowledge must be explained away if sensationalism is to be retained. By using non-theoretical criteria, possibly involving independent observations, we can choose to accept a theory. The standard means of making a choice is to view all theories as catalogues of ‘facts’, classification systems or even languages and then apply some criteria such as simplicity, generality, or minimization of statistical error with respect to observations. In other words, choose the ‘best’ approximation where the definition of ‘best’ is based on *explicit rational* criteria.

The ‘explicit rational’ criteria simply do the job that the doctrine of Manifest Truth was supposed to do when applying inductive logic. Their use avoids pure subjectivism in the process or state of knowing. Thus to complete the conventionalist version of sensationalist methodology, we need one more assumption which will ensure objectivity. That assumption is about the existence of universal rationality, namely, the view that if *everyone* begins with the same mutually consistent premises (or criteria) everyone will necessarily reach the same conclusions. Here it is the common acceptance of the criteria by rational (hence ‘objective’) people that is the basis of all knowledge. Facts are demonstrable truths. Facts, by being logically derivable from accepted theories, are thus defined by those theories used to demonstrate the truth of the ‘facts’. By defining facts, theories have no epistemological status. It is the logically derived (i.e. ‘valid’) facts (hence demonstrable truths) which are the sought-after goal (viz knowledge). With conventionalism it is said that *we* ‘know’ when *we* accept particular theories. The only possible errors one could make within this conventionalist view of knowledge (which combines sensationalism with the *denial* of Manifest Truth) are those which result from being irrational; hence if one is rational then errors will be avoided. In short, conventionalist methodology, by choosing the ‘best theory’ to define the ‘facts’, solves the problem of establishing a factual basis for rational (social) agreement over what is knowledge.

Anti-sensationalism as a social theory of knowledge

I have been arguing that traditional philosophy has dealt historically with the question ‘What is knowledge?’ within the confines of the Social Contract of Justification and thus that knowledge can never be explained *without* explaining ‘knowing’.

Although the origins of psychology may be found in the history of the problems of fulfilling the Social Contract, the everyday, commonplace solutions are more sociological. In simple terms, *knowledge is whatever a knower knows*. The only social problem then would seem to be about how to determine who the ‘knowers’ are. There are two extant solutions, which I will call the ‘role theory of knowledge’ and the ‘status theory of knowledge’.

The *role theory* says that a knower is anyone who plays the role of a knower in society – the most obvious example is the ‘expert witness’. In general, the role theory implies that ‘it is not what you say, but how you say it’ – but of course, how you say it may depend on what you want to say. Role playing with regard to knowledge is rather vague and uncertain. The *status theory* is much less ambiguous – it implies that ‘it is not what you say, but who you are'. There are many obvious examples. Knowers usually hold university degrees or professional licenses.

Although role or status gives the appearance of solving the problem of determining who is a knower and hence what is knowledge, few philosophers or methodologists would ever be impressed. What can be noted, however, is that both theories are non-sensationalist. But of course, philosophers are generally more impressed by sensationalist theories of knowledge or method. The (sensationalist) view that knowledge is obtained through our senses can clearly be seen as a way of fulfilling the Social Contract of Justification. Inductivist-sensationalism is an attempted explanation of subjective knowledge (*I* know...) of the objective world. Conventionalist-sensationalism is an attempted explanation of group-subjective knowledge (*we* know...) of the objective world. At this stage let us consider a new question: 'Is it possible to explain knowledge *without* explaining the *process* of knowing?' An affirmative answer to this question is a denial of the Social Contract. Such a denial also makes it possible to reject sensationalism and instead adopt the view that all knowledge contains essential theoretical elements.

THE PRACTICE OF ECONOMIC METHODOLOGY

Conventionalist methodology in economics

When discussing their philosophy of science, most economists advocate inductivism in the long run and conventionalism in the short run. Of course, if one had an infinity of time, then one could always make induction work *in the long run*. Most economists who advocate conventionalism will readily admit that there is a problem *with* induction in the short run. These economists will be concerned with a different problem – namely, the conventionalist choice problem: 'How can we choose the "best" theory when there is no inductive logic?' This would seem to be a simple matter of economic analysis where the only question concerns our objective function – that is, our choice criterion.

The conventionalist's choice problem

Most methodological debates in economics are about the criterion to be used to choose between competing theories. I will list a few of the most commonly discussed criteria. Concerning the choice of one theory over another, conventionalism admonishes us to choose the theory which is one of the following: (i) more simple, (ii) more general, (iii) more verifiable, (iv) more falsifiable, (v) more confirmed or (vi) less disconfirmed. For the

followers of Friedman's instrumentalism, that is, the economists interested only in solving practical problems, the confirmation criterion, (v), should probably be more important, but usually instrumentalism would have us just try each theory until one is found which *works* regardless of these criteria.

Criticizing conventionalist methodology in economics

While I never wish to prescribe methodology to anyone, I do think economists who wish to propound their versions of conventionalism ought to consider two elementary criticisms of conventionalism.

The first concerns the irrelevance of the conventionalist choice problem. Once one drops the Social Contract of Justification, choosing a 'best' theory would no longer seem to be essential. Of course, there may be sociological needs for choosing one theory. For example, textbooks are easier to write when there is only one theory to be described. Also, a certified 'best' theory provides a shibboleth which can be used to determine who are the 'good guys' and who are the 'bad guys'. The choice of one theory among competitors might be appropriate for practical or policy concerns – since only one can be applied at a time – but the choice cannot solve any intellectual problems. Without the Social Contract of Justification, the onus is on anyone practicing conventionalism to show why we should even have to choose *one* theory.

The second criticism is quite simple. It concerns the circularity of conventionalist criteria. Although economic methodologists who practice conventionalism usually deny that a theory is true or false (a theory is either 'better' or 'worse'), they presume their criteria can be true. Each of the criteria listed above presumes something about *the* true theory of the real world. For example, saying the 'best' theory is one which is most simple presumes that the real world is essentially simple. In other words, whenever economic methodologists propose any particular criterion for choosing the 'best' theory, we can always ask, 'How do they know that is the "best" criterion?'. Of course, such a question can lead to an infinite regress. If instead economic methodologists argue that their proposed criterion is 'best' because by using it one can show that the chosen theory is 'best', then conventionalism is reduced to circularity.

Conventionalism and the sociology of economics

My many criticisms of conventionalism are sometimes acknowledged by economic methodologists but seldom heeded since economic methodologists regularly claim that they long ago rejected conventionalism. They often claim to have rejected the explicit criteria listed above since these criteria no longer seem to hold promise – even Popper's criterion of falsifiability. Some methodologists claim to have gone beyond conventionalism and even beyond Popper. But if a methodologist walks like a duck and quacks like a duck, then he or she is a duck. In a fundamental way it does not matter what methodologists claim they are doing. Of more concern is what economists do that depends on accepted methodology. In the remainder of this chapter I shall dig deeper to show how conventionalist methodology permeates the economics profession and its practiced methodology.

By rejecting the Manifest Truth doctrine but accepting the romantic's doctrine that 'to err is human', practitioners of conventionalism would have us think that the fundamental social problem concerning knowledge is: 'How does our society, now and in the future, avoid mistakes with respect to understanding the world around us?' One of society's many social institutions is the economics profession itself. As such it produces economic knowledge which *represents* acceptable knowledge based on a rational minimization of error. The standard ways of making this representation concrete are the particular institutions of textbooks, professional meetings and, above all, academic departments and curricula. To understand more clearly how conventionalism permeates economics, I will now attempt to analyze each of these 'concrete' institutions to show that conventionalism is the methodology practiced among economists.

Textbooks

Standard textbooks are deliberate attempts to represent the consensus concerning accepted facts (and theories) in a given area of study. The logic of the textbook business is that a book can only become one of the standard textbooks if it does in fact represent the consensus in terms of both content and form. What the standard textbook contains is the latest accepted work on what are the accepted theories in a given area of study. Any would-be textbook whose contents deviate from this will fail as a textbook since it will not be generally used. The form in which textbooks are written is as important as their contents. Any attempt to deviate here may also be doomed. For example, in the area of elementary economics, where the consensus is very strong, one finds that virtually all textbooks about 'principles' contain only minor variations in their tables of contents from that of the leading textbook; for years it was the one written by Paul Samuelson, today it is more likely Richard Lipsey's. Such mimicry is often true in more advanced areas such as microeconomic theory; for years, all accepted textbooks were variants of one of the older leading textbooks

(perhaps one written by Richard Leftwich, C.E. Ferguson or George Stigler). Furthermore, most of the standard textbooks that do have an introductory chapter on methodology provide nothing more than a statement of some variant of conventionalism – even though they still give references to Friedman's 1953 instrumentalist essay, of course. The philosophical aspects of economic theories are confined entirely to that chapter – otherwise one might be suggesting that there could be some controversy over a particular theory.

The problem that is solved by such an institutionalized consensus (concerning the proper form and content of any textbook) is not clear. It might only be that it permits teachers to estimate what any rational student or colleague expects of them when teaching courses in a given area of study. Or it might help to assure that students are getting their money's worth. Most likely, it minimizes the obvious mistakes one might make in thinking about the given area of study.

Professional meetings

Specialized professional meetings are organized much like standard textbooks. Opening addresses (typically like after-dinner speeches) are usually the depository of all philosophical matters while the meetings themselves (i.e. lectures, symposia, etc.), that follow contain the non-philosophical matters. (Of course, meetings among methodologists can easily be exceptions.)

Ideally, the lectures, symposia, etc. would contain the latest attempts at solving new problems or the latest findings concerning some old problems, thereby solving the social problem of keeping the profession aware of new developments. Unfortunately such meetings are very difficult to organize. In reality the meetings are characterized either by 'cronyism' or by 'anti-cronyism' – either one invites papers only from friends or one does not invite papers from any friends. Cronyism is most prevalent today.

To organize a large professional meeting, a select group, supposedly representing the consensus concerning the proper areas of interest, delegates the job of organizing sessions in chosen areas. Usually, the criteria applied to choosing papers for presentation would be irrelevant for an ideal meeting. In particular, large meetings today usually serve the purposes both of a social gathering and a market for recruiting and employment. Although the purpose of ideal meetings can be used to explain why smaller professional meetings continue to be held, if the social aspects were recognized as the real purpose, the universities or companies that pay the expenses of holding the meetings would be unwilling to finance the attendance of an ordinary member. But they are quite willing to finance the intended consequences because these promote the progress of science through timely communication of the latest developments, findings, etc.

Departments and curricula

Despite what some economists might think, the administration of academic economics is quite similar to that of other academic disciplines. By far the most interesting social phenomena of the scientific community are the academic institutions of departments and curricula. Let us consider some problems that might be solved by having separate departments of Economics, Physics, Sociology, Philosophy, etc. Since the conventionalist view is that scientists do not get involved in arguments over truth, one way to make sure that this view is correct is to separate those 'schools of thought' administratively such that there is little contact, hence overcoming the social problem of having scientists 'fighting it out'. In other words, separating departments within a university or partitioning a department into such sub-disciplines as microeconomics, macroeconomics, international trade, industrial organization, managerial economics, finance, accounting, etc., makes the practice of conventionalism possible. By grouping together those scientists who speak the same 'language', it makes agreement more possible since if they speak the same language they will be able to concentrate on the logic of the discussion. Similarly, since all rational people will ultimately agree if they start from the same premises, if we group together scientists who use the same premises we minimize the possible disagreement. Moreover, since those in one group (by definition) will accept the same theories, they will agree on what are to be the accepted facts in their area. This makes it possible to write textbooks, hold meetings, etc. Above all, agreement on facts makes it possible to agree on what students must learn.

Since the entire fabric of the academic scientific community is organized to prevent (embarrassing) disagreement from breaking out and thereby organized to make the ordinary economists' conventionalist methodology work or seem to be true, we cannot risk allowing students to be a source of disagreement. Thus, students must be socialized as soon as possible. The primary technique of socializing them is to have a set pattern of prerequisite courses that they must take *before* we allow them to think on their own about any particular area. If such an organization is successful, again one can show that conventionalism today is true *by construction*. (Such a proof would be very popular among mathematicians and other advocates of conventionalism.)

The methodology of mathematical economics

In my 1982 book I explicitly examined the two ways in which the economic researcher practices conventionalism. One of my chapters presented the view that all of analytical economics is 'defeatist conventionalism'. Analytical economics retreats to dealing with analytical truths that are not dependent on empirical statements about the real world rather than dealing with the difficult problems of determining the truth of statements about the real world. Another chapter presented the view that all of positive economics is 'optimistic conventionalism'. Specifically, I argued that positive economics is nothing but repeated attempts to prove inductively that neoclassical economics is true. I said it does this by showing that neoclassical economics can be successfully used to explain ordinary behavior. A few years later I gave more thought to just what is positive economics. These further considerations will be presented in the next chapter.

For many years, the existence of these two competing views of the appropriate methodology for economics (which is really a family dispute) fostered considerable tension in many Economics departments. In recent years, things seem much less tense. There are two possible reasons for the reduced tension. First, many optimistic proponents of conventionalism have retreated to departments of Applied Economics that are located within business schools. And second, those optimistic economists who have remained have found ways to co-exist without surrendering to extreme hard-core mathematical economists, that is, to those who are interested in formalism-for-formalism's-sake. The result is an economics discipline that can appeal to most positive economists and to most theorists interested in mathematics-based model-building techniques. Today, nobody would ever feel that they have to choose between positivism and mathematical economics. Even journal editors and referees will accept both types of papers. However, to be accepted, a paper must obviously either involve a logically rigorous model or provide empirical evidence about a model. This limited compromise has allowed positive economics to acquire a dominant methodological position in the economics profession, as is evident in almost any generalist journal (i.e. non-hard-core mathematical economics journals). By all means, having achieved a successful détente, it would be unwise to allow anyone to rock the boat.

Despite the monumental growth of mathematics-based economics, there seems to be no public discussion of the use of mathematics in economics. Ten years ago, my colleague Herbert Grubel and I surveyed opinions concerning the economics of mathematical economics [see Grubel and Boland 1986]. That is, we asked prominent economists whether they think there are any net benefits to encouraging more mathematical economics at the expense of more modest literary and applied economics. The idea of even asking about net benefits caused much wailing and abuse from those of our colleagues who spend most of their time manipulating mathematical models. But just what *are* the net benefits?

Let us look at the commonly stated benefits. The most common claims are that mathematics ensures a high degree of 'rigor' and promotes 'economy of thought'. This latter is related to mathematics being a 'common language'. Without arguing whether mathematical economics is rigorous, or whether also non-mathematical economics is incapable of rigor, it is interesting to note that whenever we make our theories and models more dependent on mathematical analysis neither of these supposed attributes of mathematical economics ensures that we will thereby be able to make better predictions or that our models will be true or better able to explain economic phenomena. The ostentatious use of mathematics-based models is only a matter of 'proper scientific form' rather than substance. The emphasis on form rather than substance is a characteristic of conventionalism. Since conventionalism denies that theories can be true or false, what can be of concern other than form?

The question to ask believers in mathematics-based positive economics is, just what has been accomplished in the last fifty years? While the believers will be quickly getting their list ready, a better question is, what has been accomplished with mathematical model building that could not have been accomplished without mathematical model building? The honest answer to the second question is that nothing has been accomplished that could not be done without sophisticated mathematics. And whatever is listed for the first question will be seen to be an accomplishment only by believers.

I think I have said enough to indicate that the methodology practiced in economics is what I have been calling conventionalism. The firmly established acceptance of mathematical economics even among those interested in positive economics is the most convincing evidence. Form is more important than substance, and logical validity by itself is considered more important than difficult questions of empirical relevance. Today if you wish to show you are a 'knower', you had better express your thoughts using mathematics-based model building – even if you are interested in so-called positive economics. Make sure you have used only acceptable techniques of analysis. In the 1990s, some form of game theory seems to be the most promising strategy. And, if you want tenure or promotion, you would be wise to try to publish your papers in journals with status, that is, in those that give prominence to mathematics-based economics. But most important, never be caught worrying about the truth of your analytical models or how you might learn whether your model is actually true or false.

NOTES

1 Ironically, the first question asked of me after delivering my lecture was: 'So, what is your position?'

2 The original version of this chapter was published as 'Economic methodology: theory and practice' in *La production des connaissances scientifiques de l'administration / The Generation of Scientific Administrative Knowledge* edited by Michel Audet and Jean-Louis Malouin (Québec, Canada: Les Presses de l'Université Laval). The remainder of this chapter is a revised version of the paper I delivered during our 'debate'. Its use in this chapter is with the permission of Les Presses de l'Université Laval. Unfortunately, McCloskey's paper was not published.

3 As is the tradition in discussions of economic methodology since the publication of McCloskey's first paper on economic rhetoric [McCloskey 1983], everyone is careful to distinguish between big-M Methodology and small-m methodology. The former involves the philosophers' Big Questions and the latter concerns only the everyday business of practicing economists. In this spirit I am distinguishing here between big-S and small-s science (and scientific method). The big letter Science is built upon beliefs and promises that go beyond what is possible. The small letter science is about the unassuming business of everyday science.

9 Criticizing economic positivism

> John Neville Keynes distinguished usefully, not just between a positive science and a normative art, as his forebears had done, but between (1) a 'positive science', (2) a 'normative or regulative science' and (3) an 'art', that is, a system of rules for the attainment of given ends.
>
> Mark Blaug [1992, p. 122]

> The object of a positive science is the establishment of *uniformities*, of a normative science the determination of *ideals*, of an art the formulation of *precepts*.
>
> John Neville Keynes [1917, p. 35]

Given the comfortable compromise between those interested in positive economics and those interested in the mathematics-based model-building techniques, it is not surprising that few economists today will be found waving the banner of 'economic positivism' or 'positive economics' or even 'positive science'. Thus the absence of flag-waving does not mean that economic positivism is dead. Positive economics is now so pervasive that virtually all competing methodological views (except the most defeatist hard-core mathematical economics) have been eclipsed. The absence of methodology flag-waving is thus easy to understand. There is no territory to dispute and thus no need to wave one's flag.

The dominance of economic positivism is abundantly evident in current textbooks. As I noted in Chapter 1, almost every introductory textbook explains the difference between 'positive' and 'normative' economics and tries to make it clear that economists are interested in positive economics and capable of fulfilling the demands of economic positivism. Why should economists be interested in positive economics? And has economics fulfilled the demands of economic positivism? These two questions will be the focus of this chapter.[1]

POSITIVE ECONOMICS VS WHAT?

While every textbook clearly distinguishes 'positive' from 'normative' questions by characterizing the distinction with an 'is/ought' dichotomy, it is not clear that the history of the distinction supports such a dichotomy. Neville Keynes is most often quoted to support the dichotomy despite the fact that, as the quotations above point out, he advocated a trichotomous classification. Unfortunately, the widespread reliance on the 'is/ought' dichotomy has nullified Neville Keynes' best efforts to improve our understanding of positive economics.

While promoting 'positive methodology' in his famous 1953 essay, Milton Friedman tried to deny the 'is/ought' dichotomy by arguing that answers to 'ought' questions necessarily depend on a prior establishment of 'what is'. Nevertheless, most critics of Friedman's methodology think he was arguing against normative economics and thus assume that he was only arguing in favor of positive economics (see Chapter 2 above) Koopmans 1957, Rotwein 1959, Samuelson 1963 and Simon 1963]. The presumption seems to be that one must always choose between 'is' and 'ought' questions as if they were inherently mutually exclusive.

To be fair, there is a good reason to presume that 'is' and 'ought' questions are mutually exclusive. David Hume long ago argued that 'ought' statements cannot be deduced from 'is' statements and vice versa [see Blaug 1992, pp. 112–13]. The mere mention of 'is' and 'ought' in the definition of positive economics thus seems to demand a sharp dichotomy such as the one between positive and normative economics as defined in the textbooks.

In addition to the is/ought distinction, there are other dichotomies that seem to support the separation between positive and normative economics. There is the philosopher's distinction between analytic and synthetic truths – the former being ones that do not depend on empirical questions while the latter do. There is the science-vs-art distinction which motivated early economic methodologists (such as Nassau Senior) – while 'science' was alleged to be about material truths, 'art' was considered to be about normative rules [Blaug 1992, p. 54]. More recent dichotomies are the objective/subjective, descriptive/prescriptive and rational/irrational, which are often considered direct correlates with the positive/normative distinction. And, of course, there is the more commonplace distinction between theoretical and applied economics that prevails in most Economics departments today.

To this list I wish to add one more distinction – namely, the romantic/classical distinction often found in discussions of nineteenth-century British literature. Specifically, I think one can recognize a distinction between 'romantic' and 'classical' postures concerning the realism of assumptions. While it might be considered romantic to assume the world is the way one would like it to be, it would be classical to dispassionately try to make one's assumptions correspond to the way the world really is. For example, while a romantic egalitarian might wish that wealth be evenly distributed, a classical realist would contend that one should not assume distributional uniformities unless there are good empirical reasons to do so.

So, given all of these various dichotomies, how does one understand the nature of positive economics and why should one ever want to promote it? I think the reason why there are so many different distinctions raised in the discussion of positive economics is that each of them represents something that positive economics is claimed *not* to be. That is, most people understand positive economics more by what it is argued not to be than by what it is argued to be in fact. Briefly stated, we have only a negative understanding of economic positivism!

POSITIVISM AS RHETORIC

There is a sense in which the distinction between positive and normative is completely confused. Positive policy advisors are in effect always recommending that their policy is the *best* way to achieve the given ends. This is evident even in John Neville Keynes' original discussion. It is difficult to conceive of a way one could ever avoid normative judgements. So, what is it that one is truly accomplishing when demanding that one's economic research or advice conform to the dictates of positivism?

The idea of 'positive' economics is mostly a matter of rhetoric. The rhetorical purpose is also evident in the use of some of the other dichotomies. One can find books titled System of *Synthetic Philosophy* [Herbert Spencer 1896], *Positive Philosophy* [Auguste Comte 1855/1974], *Scientific Management* [Drury 1922/68], *Objective Psychology of Music* [Lundin 1967], *Rational Economics* [Jackson 1988], *Descriptive Economics* [Harbury 1981], and so on. Whenever an author is extolling the virtues of a theory by claiming it is a positive theory, he or she is usually asserting that it is not something of a scientifically unacceptable nature. What is acceptable in these matters is usually dictated by the prevailing view of 'scientific method'. But it is not often clear why the term 'positive' must always indicate something acceptable or desirable. Perhaps considering again the historical discussion of Chapter 8 might help to clarify this.

Up to the time of Hume (late eighteenth century), most thinkers seemed to believe in the power of rational or logical thought and especially in its embodiment in science. And the term 'science' usually implied, following Francis Bacon's seventeenth-century view, that all science can be reduced to *positive evidence* from which in turn all systematic knowledge could be

shown to follow by the logic of *induction*. This, I think, gives us a clue to why the accolade of 'positive' has for so long implied something good. Any theory which offers or is based on positive evidence – that is, on observations or hypotheses which make *positive contributions* toward an inductive proof of one's systematic knowledge – is worthy of the title 'positive'. And, given the common nineteenth-century belief in the viability of inductive science, 'positive' implied 'scientific', 'rational' and even 'objective'. The implication of objectivity follows from Bacon's promotion of inductivism as antidote to self-interested or prejudicial claims of knowledge.[2] To be scientific, inductive proofs were to be based only on objective observations. Whether one's theory makes a positive contribution to scientific knowledge is solely a question of one's personal research skills. A true scientific researcher is objective, unprejudiced, unbiased to the point that any reported data will be beyond question. The remainder of science is simply a matter of objectively based inductive logic. As a corollary, if anyone errs in their scientific claims to knowledge, it could only be due to introduced biases, prejudice or injecting one's subjective values.

In economics, the association between 'positive' and 'descriptive' seems to be a direct consequence of the reliance on Hume's view of the is/ought dichotomy. One describes 'what is' and prescribes 'what ought to be'. The association between 'positive' and 'applied' economics and between 'positive' and 'synthetic' statements is rather confusing. While it is easy to claim that one's theory is 'positive', it is more often thought that pure theory is not empirical[3] and thus applied economics must be 'positive'. So, what did Böhm-Bawerk mean by the title of his 1889 book *Positive Theory of Capital*? While it might be easy to see a connection between 'positive' and 'synthetic', their opposites do not seem connected. Hardly anyone would connect 'normative' with 'analytical' – except from the perspective that a normative conclusion is a logically contingent truth that depends on the acceptance of presumed values. But if analytical truths must be tautologies then, technically, the connection is rather weak.[4]

The post-war influence of the logical positivists and the retrospective influence of Max Weber have combined to make the rhetoric of positivism even more confused. In Chapter 1, I noted that the logical positivists were those analytical philosophers who thought verifiable scientific knowledge is distinguishable from unverifiable 'metaphysics'. The turn-of-the-century social scientist Max Weber is now credited as being a leader in developing the idea that scientific knowledge could be 'value-free'. And, to confuse things still more, Karl Popper presented a critique of logical positivism based on the logical grounds that one's theory makes a positive contribution to scientific knowledge only if it is falsifiable (which, as I discussed in Chapters 5 and 6, most commentators seem to think means only that it is not a tautology). With all this confusion in mind, it may be difficult for us to determine even what 'positive' is not.

WHAT EVERYONE SEEMS TO THINK 'POSITIVE' IS

Economic positivism as it is currently practiced seems to be available in four different flavors. The first and most optimistic version is what I will call *Harvard positivism*. It is represented by the recent attempts to develop 'experimental' economics and has its origins in the early teaching of Edward Chamberlin. At the other extreme is the weak minimalist version which I will call *MIT positivism*. Its weakness is due to the methodological view that says that to be of interest a theory need only be *potentially* refutable – there is no additional requirement that says it needs to be supported or tested by empirical evidence. In between these two extremes there are two more modest versions. One is what I will call *LSE positivism*, which does not require controlled experiments but does see economics as a scientific endeavor that emphasizes a necessary role for empirical, quantitative data. The other one is *Chicago positivism*, which includes both the simplistic instrumentalism of Friedman and the more complex confirmationism of Becker and Stigler.

Harvard positivism

Those positivists who advocate 'experimental economics' still comprise a very small segment of mainstream economics. The current movement seems to have its origin in the experiments that Chamberlin often inflicted on his students at Harvard University. A well-known leader of this group is Vernon Smith.

The motivation for experimental economics is to overcome the obvious fact that most mainstream neoclassical models are self-professed abstractions which employ simplifying assumptions whose realism is always open to question. Given that any typical economic explanation is of the form 'if the world is of form *X* and people behave according to proposition *Y*, then we will observe phenomenon *Z*', the obvious questions facing any economist who claims to offer a positive explanation of economic phenomenon *Z* are: Is the world of form *X*? Do people in fact behave according to proposition *Y*? And do we observe phenomenon *Z*?

Since it is usually difficult to determine whether people actually behave according to proposition *Y*, almost all empirical research is concerned with world *X* and phenomenon *Z*. The usual approach is to build a model of the economy based on proposition *Y* and try to determine whether or not the model can be confirmed when confronted by the data *available* after the

event. Unfortunately, the available data are seldom decisive in any direct way. Instead, many additional assumptions must be made and thus any conclusions reached are always conditional.

Harvard positivism offers a different approach. Rather than accept the limitation of available data (which are usually aggregative and thus open to many methodological questions), experimental economics proposes to create a real-world situation in which the assumptions of the typical neoclassical model are true with respect to the claimed form of world *X*. Specifically, the experimental economists attempt to construct a world which is in fact of form *X* and then determine whether the behavior implied by proposition *Y* is logically consistent with the experimentally observed phenomenon *Z*. The extent of the laboratory skill of the experimenter is always the sole determinant of whether the experiment represents a successful exercise in economic positivism.

MIT positivism

The followers of Paul Samuelson's methodology adopt a much less fundamentalist view of economic positivism. Samuelson is the famous Massachusetts Institute of Technology (MIT) economist who published his views about methodology in his PhD thesis [1947/65]. Those following his methodology, yet seeking to assure the optimistic promises of positivism, argue that the minimum condition for a positive contribution to economic understanding is that anyone's positive theory must be capable of yielding to refutations based on positive evidence. In short, all truly positive theories are empirically refutable *in principle*. All that can be assured by such a weak requirement is that the proposed positive theory is not a tautology – as Hutchison [1938] recognized in the late 1930s. It should be clear that this minimalist version of positivism is serving more the interests of mathematical model builders, who wish to avoid all of the menial unpleasantness of dealing with complex real-world empirical data, than the interests of those who are concerned with promoting truly positive economics. For many mathematical economists, the elegance of one's model is always much more important than whether the model's assumptions are empirically realistic or whether the model's implications are useful with respect to economic policy.

Chicago positivism

Usefulness is the keystone of the positivism promoted by the followers of so-called Chicago school economics. However, there are two aspects of usefulness. On the one hand, providing positive theories that can be used as instruments by policy makers is one concern. On the other hand, being useful for promoting neoclassical economics in general, and confirming beliefs in the omnipotence of the market system in particular, is another concern of the Chicago school.

In his 1953 essay, Friedman gives a compelling argument for why anyone who is only interested in providing useful theories for policy makers ought to eschew the typical philosophical prejudices associated with the group of analytical philosophers often called 'logical positivists' and instead recognize that questions concerning the verifiability, falsifiability, or even *a priori* realism of the behavioral assumptions of economic models are of much less concern than the usefulness of their results. As I explained in my 1979 article (Chapter 2 above), it is easy to see that such an argument is really one favoring an instrumentalist methodology. The interesting question is, why would Friedman or anyone else see his argument as one promoting some form of positivism?

Friedman's essay was not an argument against positivism but only one against the more sophisticated logical positivism. Positive evidence still matters for Friedman. His only restriction is to limit the evidence to that of results or predictions and thereby exclude *a priori* or logical analysis of models, assumptions and theories *as a determinant of the usefulness* of positive theories. Positive data obviously play an essential role in Friedman's methodology. But for Friedman the only relevant positive data will be successful predictions which assure the usefulness of one's model or theory. There is nothing inherent in Friedman's methodological essay that would prevent his form of instrumentalism from being used by Post-Keynesians or even Marxists.

When it comes to ideological questions, however, other members of the Chicago school are much more prominent. In 1977, George Stigler and Gary Becker offered a manifesto for those who believe in neoclassical economics. Their argument, simply stated, was that they as Chicago school economists will offer models of the economy (i.e. of world *X*) which do not engage in analysis of the psychological (subjective) makeup of individual decision makers but instead offer analyses of the objective (positive) cost situations facing the individual decision makers and thereby explain any observable, positive behavioral evidence in question (i.e. phenomenon *Z*) – all observed changes in behavior will be explained as consequences of observable and objective cost situations.

Each positive economic model which succeeds (they never seem to report any failures) is offered as yet more confirming evidence that one can explain any social or behavioral phenomenon with an appropriately constructed neoclassical model (i.e. where proposition *Y* incorporates assumed maximization behavior in a free-market system). For this branch of the

Chicago school, the real purpose of neoclassical model building is once again to confirm the truth of a market-based system of social coordination [Boland 1982].

LSE positivism

Stigler and Becker may be correct in promoting neoclassical economics as the only true explanation of social and individual behavior but, if so, it ought to be tested in a more critical manner. At the end of the 1950s, a group of London School of Economics (LSE) economists proposed a more critical approach to economic model building. While it is easy to find positive evidence to confirm anyone's favorite model, the 'scientific' issue is one of approaching the evidence in a less predisposed manner. Such an approach does not preclude *a priori* beliefs; it merely cautions one to let the positive evidence do the talking.

The LSE approach to positivism was the self-conscious product of a group of young economists led by Richard Lipsey who formed what was called the 'LSE Staff Seminar in Methodology, Measurement and Testing'. The seminar was to some extent inspired by Popper's presence at LSE and his emphasis on criticism and empirical testing as the true basis for science.[5] The message of the seminar was captured in Lipsey's well-known 1960s textbook, *Introduction to Positive Economics*. The main thrust for Lipsey was the advocacy of developing an appreciation for real-world empirical data. His textbook became the major platform for all of modern economic positivism.

The combination of testing and measurement is the hallmark of LSE positivism. It is thus not surprising to find that econometrics plays a prominent role. But, unlike the instrumentalist tendency found among American econometric model builders,[6] LSE econometrics is supposed to be helping us to assess any economic proposition that might arise. The positive/normative distinction was to play a central role since it was thought that all normative statements are untestable and thus 'unscientific'.

MODERN ECONOMIC POSITIVISM IS PROFOUNDLY CONFUSED

As we have learned from historians of science (such as Thomas S. Kuhn and Joseph Agassi), most disciplines can be defined by their leading textbooks. The foundation of modern economic positivism continues to be Lipsey's textbook, *Introduction to Positive Economics*. The evolution of this book closely reflects how the practice of positivism has developed over the last thirty years. However, if one examined the introductory 'scope and method' part of the *first* edition of Lipsey's famous textbook, it would be difficult to understand how this book has become the foundation for modern economic positivism. Lipsey proudly announces that his book is about 'POSITIVE ECONOMIC SCIENCE'. The North American editions of his book play down the emphasis on 'science' (presumably because in North America such emphasis is considered pretentious) but then continue to share his emphasis on 'positive'. Yet a careful examination of his 1963 book shows that empirical evidence can be decisive *only in a negative way*. Specifically, Lipsey parrots the part of Popper's philosophy of science that claims that truly scientific theories can be refuted by empirical evidence but can never be verified by empirical evidence. In effect, then, according to Lipsey *circa* 1963, his book is really about NEGATIVE economic science!

This apparent inconsistency is abruptly corrected in his second edition, where he says he has

> abandoned the Popperian notion of refutation and [has] ... gone over to a statistical view of testing that accepts that neither refutation nor confirmation can ever be final, and that all we can hope to do is discover on the basis of finite amounts of imperfect knowledge what is the balance of probabilities between competing hypotheses. [Lipsey 1966, p. xx]

While this may accord better with common notions of science, it is not clear that there is anything positive (or negative!) left in the LSE version of positivism.

In the sixth edition we are told that only positive statements are testable. Normative statements are not testable because they depend on value judgements. Moreover, 'statements that could conceivably be refuted by evidence if they are wrong are a subclass of positive statements' [1983, p. 6]. So practitioners of positive economics 'are concerned with developing propositions that fall into the positive, testable class' [p. 7]. But, looking closer, on page 5 it is asserted that a statement is called 'testable' if it can 'be proved wrong by empirical evidence' and then turning to page 13 we are told it is 'impossible to refute any theory conclusively'! Unless Lipsey meant something different from what appears on page 5, it would seem that the class of positive economic statements is empty and thus positive economics is impossible. If there is any doubt about whether the advocates of LSE positivism are profoundly confused about methodology, the 1988 Canadian edition of Lipsey's book provides the proof: we are boldly told, 'There is no absolute certainty in any knowledge' [Lipsey, Purvis and Steiner 1988, p. 24]. I ask, how can one claim to know with absolute certainty that one cannot know with absolute certainty?

Their bold statement is self-contradictory and yet it appears to be the

foundation of modern economic positivism. As is well known, anything can be proven with a foundation containing contradictions (e.g. 2 equals 1, black is white, etc.), and whenever it is possible to prove contradictory things the proofs are meaningless. Thus, we would have to conclude that nothing can be accomplished with the modern positivist's methodology if that methodology is the one described in the various versions of Lipsey's famous book. I think Lipsey should not have simply dropped Popper in order to avoid some 'problems that seem intractable to a believer in single-observation refutations' [1966, p. xx]. While his move will please those philosophers of science who are all too eager to dismiss Popper's challenges to logical positivism, I think that Lipsey should have tried to critically examine those 'intractable' problems.

POSITIVE SCIENCE OR POSITIVE ENGINEERING?

Even though the philosophy of economic positivism has not been well thought out by its main proponents, it still captures all the satisfying notions that most mainstream economists seem to desire. On the one hand, it appears to support the commonly accepted view of explanatory science. On the other hand, it appears to support the appropriate cautions for a socially acceptable practice of social engineering. Specifically, both perspectives are served by the common view that positivism represents the avoidance of value judgements.

Explanatory science

Those economists today (including those from the Massachusetts Institute of Technology or the London School of Economics) who see themselves as scientists offering explanations of economic phenomena will be pleased to find that adherence to positivism only requires assurances that the assumptions of one's model are falsifiable. Falsifiability of one's assumptions merely assures that the conclusions and explanations provided by the model will not be what economists call tautologies. To be careful here, it should be recognized that what economists mean by the term 'tautology' is not always what philosophers or logicians mean by that term. As I discussed in Part II, economists too often think that if it is impossible to conceive of how a given statement could be false, then that statement is a tautology. Actually, what they mean by tautologies includes both what philosophers call tautologies (statements that are true by virtue of the logical form alone) and quasi-tautological statements that are true by definition or depend on definition-like statements such as value judgements. And again it is the latter form of statements which is usually what economists mean by the term 'tautology'.

But why are economists so concerned with avoiding tautologies? The only methodological problem solved by avoiding tautologies is the one facing economists who wish to claim that their empirical tests of their models or theories represent positive contributions on the basis that their empirical evidence verifies or confirms their models.[7] The problem is that there are some statements that are of the form that economists call a tautology, yet that can also appear to be confirmed. The most obvious example is the 'quantity theory of money'. That 'theory' is represented by the equation $MV = PT$. On close examination it turns out that the two sides of this equation are merely what you get by reversing the order of summation for a double summation over commodities and transactions [see Agassi 1971a]. Surely, confirming a statement which cannot conceivably be false cannot really contribute anything positive to economic science.

Social engineering

Those economists today who see themselves as providers of policy advice will be pleased to learn that adherence to positivism will assure them that their recommendations will not be easily dismissed. Policy makers seldom are concerned with whether the consulting economists are dealing with tautological models or whether any theory is falsifiable. What is important is the assurance that the advice given is not just a reflection of the biases of the consulting economists.

So, what methodological problem is solved by expecting policy advisors to be practitioners of economic positivism? Given all of the equivocation incorporated in the presentations of modern economic positivism (e.g. Lipsey's textbook), there is no reason for a policy maker to expect that the economist's advice will be firmly supported by empirical evidence. It all comes down to the economic researcher making judgements about whether the available evidence should be sufficient reason to support or reject a given theory that was used to form the advice given. In most cases, it is the personal demeanor of the researcher that gives his or her research credibility. Note well, by stressing the importance of the personal demeanor of the researcher it is evident that positive economic engineering is merely a version of Bacon's inductivism.

If economists who provide policy advice could get by with wearing white lab coats, I am sure they would parade before television cameras so attired. But again the demeanor of the practicing economic positivist is more understood by what it is not. Nobody will believe an economist who claims to know the truth and refuses even to look at data. Nobody will believe an economist who is interested only in publicly promoting his or

her personal value judgements. Nobody will believe the research done by someone who behaves like Goethe's young Werther. In other words, true-believers, zealots and romantics need not apply for the job of economic advisor. And it seems firmly believed that adherence to economic positivism precludes such objectionable demeanor.

POSITIVE EVIDENCE ABOUT POSITIVE ECONOMICS

Having discussed the nature of the economic positivism explicitly discussed in positivist textbooks, our next consideration ought to be about how positivism is actually practiced in positive economic analysis. The salient feature of all examples of 'positive' economic analysis is their conformity to just one format. Specifically, after the introductory section of a typical positive economics article there is a section titled 'The model' or some variation of this. This is followed by a section titled 'Empirical results' or something similar, and a final section summarizing the 'Conclusions'. The question that should be considered is: why do virtually all positivist papers conform to this one format? Is the dominance of this uniformity the *only* success of modern economic positivism?

A superficially true explanation for why a specific format is universally used is that it is a matter of rhetoric [see McCloskey 1989]. A trivial explanation of the widespead use of a specific format would be that all journal editors require that format, but surely they are only responding to what they think the market demands. The concern here is not just why any particular individual might decide to organize a research paper according to the accepted format. Instead, the concern is why this particular format is so widely demanded.

I do not see any reason why the same principles of understanding embodied in the current practice of economic positivism – namely, model building – would not also be applicable for the economic methodologist attempting to explain the empirical uniformity evident in the widespread practice of model building itself. So, in order to explain or describe the practice of economic positivism, let me attempt to build a 'model' of the format of a typical article in the literature of positive neoclassical economics. Judging by what is often identified as a 'model' in positive economics, virtually every formal statement is considered a model. Nevertheless, there are some basic requirements.

In order to build my model of positive or empirical analysis, as with any model, the assumptions need to be explicitly stated. Let me begin by stating the obvious assumptions which form the visible core of the research program of neoclassical economics. My first and most fundamental assumption is that every neoclassical model must have behavioral assumptions regarding maximization and market equilibrium. Furthermore, the results of the model must depend crucially on these assumptions.

The remaining assumptions are less fundamental to neoclassical economics but are required to provide the rhetoric of modern economic positivism. To provide the main needed ingredient of modern economic positivism, my second assumption is that every empirical model must yield at least one equation which can be 'tested' by statistically estimating its parametric coefficients.

My third assumption (which is required for the implementation of the second assumption) is that every empirical paper must presume specific criteria of 'truthlikeness' – so-called statistical testing conventions. For example, one must consider such statistical parameters as means and standard deviations, R^2s, t-statistics, etc. That is, every equation is a statement which is either true or false. However, when applying an equation to empirical data we supposedly know that the fit will not usually be perfect even if the statement (i.e. the equation) is true. So the question is: in what circumstances will the fitted equation be considered 'true'? The use of the testing conventions implies that the investigator is not attempting to determine the absolute truth of his or her model. Rather, the objective is to establish its acceptability or unacceptability according to standard testing conventions of one's chosen form of economic positivism.

My last assumption is that in order to be published, every empirical paper must have contributed something to the advancement of 'scientific' knowledge. That is, it must establish some new 'facts' – namely, ones which were previously unknown – by providing either new data or new analysis of old data.

In order to test this model of the methodology of neoclassical positive economics, the available data must be considered. First I must decide on where to look for mainstream 'positive economics'. Obviously, one should expect to find it in the pages of the leading economics journals. So, to test this model, I should be able to open any leading mainstream journal such as the *American Economic Review* or the *Economic Journal* and examine the contents of a few issues. To be relevant, the examination of the data should be restricted to those articles intended to be positive analysis. That is, avoid those articles considered to be avant-garde theories or concerned with the more technical (mathematical) aspects of 'economic theory'. Of course, one should also ignore topics such as 'history of thought' or 'methodology' if they can be found.

Actually, I performed this test for the *American Economic Review* for the year 1980 [see Boland 1982, Chapter 7]. My examination of the articles selected as stated seemed to me to indicate that all of them conformed to the format specified by this model of positive neoclassical analysis. The

only empirical question implied by this positive model is whether there are any exceptions to what I have claimed will be found in the mainstream journals. As expected I was able to report that there were none in the data considered. My model of positive analysis did fit the available data.

EXPLAINING THE USE OF THE STANDARD ARTICLE FORMAT

Despite the ease of confirming such a positive model of economic positivism, there is apparently no discussion of *why* papers *should* be written according to the observed format – apart from the recent discussion limited to the *rhetoric* of economic positivism [see McCloskey 1989]. Of course, there is no need to discuss the standard format if everyone agrees that it presents no problem and it is doing its required job. My general theory is that the reason why the format is not discussed is that its purpose is simply taken for granted. Taking things for granted is a major source of methodological problems and inconsistencies in economics, although the problems are not always appreciated. This is the case with the widespread use of one common format for neoclassical empirical research papers. Perhaps there is no discussion because the job performed is merely one of an elementary filter, one which presumes that only papers that can be expressed in the standard format could ever make a positive contribution to positive economics. This presumption is also not discussed anywhere. So, just what is the purpose of the standard format?

While there need not be anything inherent in positivism that would connect its practice with the development of neoclassical economics, the two are closely related. The purpose of the standard format for those articles which purport to provide positive neoclassical economic analysis is exactly the purpose of promoting positivism in the first place. The purpose is the facilitation of a long-run inductive verification of knowledge even though the format is promoted by people who would see themselves practicing a more modest view of knowledge and method, a view which supposedly denies induction. At the root of this view is the conviction of Manifest Truth that I discussed in Chapter 8. More specifically, it is the conviction of neoclassical economists that neoclassical economics represents a true theory of society – that the real world is manifestly what neoclassical economists claim it is – and thus any model based *only* on facts generated in the real world will in the long run lead one to see the Manifest Truth which in this case is believed to be the veracity of neoclassical economics. Basing models only on facts generated in the real world is, of course, the claimed purpose of positivism.

To understand the relationship between the standard format and the research program to verify neoclassical theory, we need to consider the following questions. What constitutes a successful positive analysis? What would be a failure? And, in order to determine what constitutes a success, it would seem that we ought to consider a more fundamental question: what is the objective of neoclassical model building?

If the usual published positive neoclassical articles are actually considered contributions to 'scientific knowledge', then it can only be the case that the hidden objective of such positive economics is the one of Chicago positivism, namely, a long-term verification of neoclassical economics. Specifically, each paper which offers a confirmation of the applicability of neoclassical economics to 'real-world' problems must be viewed as one more positive contribution towards an ultimate inductive proof of the truth of neoclassical theory. My reason for concluding this is merely that logically all that can be accomplished by the typical application of neoclassical theory to 'real-world' phenomena is a proof that it is *possible* to fit at least one neoclassical model to the available data. Critics can always say that a model's fit may be successful in the reported case but it does not prove that it will be successful in every case.[8] I would argue that the agenda of positive neoclassical research programs presumes that if we can continue to contribute more confirming examples of the applicability of neoclassical economics, then eventually we will prove that it is the only true theory of the economy.

POSITIVE SUCCESS OR POSITIVE FAILURE?

Clearly an examination of the format of a typical positivist economic analysis reveals that, *as a form of rhetoric*, economic positivism has been very successful. But has it been successful at fulfilling the broader promises of positivism? This is a particularly important question for those of us who reject the possibility of an inductive proof for any theory such as neoclassical economics.

While many of the proponents of the market system of prices in general and of privatization in particular are also proponents of positive economic analysis to support their views, it is seldom recognized that advocacy of either view is inconsistent with a non-romantic practice of positivism. It is not difficult to imagine the positivist economist's response to a simple observation that, while the positivist economists base their analysis of economic phenomena on the presumed existence of a perfectly functioning market system of prices, the world outside our windows is not such a perfectly functioning system.

For example, if the world were governed by a market system of prices without governmental interference or private collusion, then eventually society's resources would be optimally allocated according to the desires of

all individual consumers. And, we are told that the world outside our window is in a state of equilibrium and, specifically, all prices are equilibrium prices. For this reason, any subsequent introduction of governments into the model will usually be seen to result in sub-optimal allocations of resources. Thus it is argued that privatization and the reliance on prices (as the only information appropriate for social coordination) is to be recommended.

To be fair, it should be recognized that the advocacy of privatization is a relatively recent phenomenon and not all advocates consider themselves to be positivists. Moreover, not all positivists advocate privatization despite what may seem to be the case today. During the 1950s and 1960s, most of the positivists were engaged in the advocacy of government interference in everyday economic affairs on the basis of what they called Keynesian economics. To these positivists it was enough to look outside our windows and see that the world is characterized by cyclical high unemployment and various levels of instability. Much of the academic effort in that period resulted in the development of the econometric approach to economic positivism which was intended to assist governments in the process of managing and 'fine-tuning' the economy.

It would seem that truly positive economists would shun such advocacy and simply and dispassionately explain the world the way it is. Namely, they should explain how phenomena are generated in a world where governments and collusion are commonplace. The obvious fact that many proponents of economic positivism are almost always engaged in the advocacy of simplistic engineering views such as either global privatization or governmental macroeconomic management should lead one to recognize that too often today economic positivism is mostly, and perhaps only, rhetoric.

NOTES

1 The remainder of this chapter is a revised version of my article 'Current views on economic positivism', which appeared in the 1991 *Companion to Contemporary Economic Thought*, edited by David Greenaway, Michael Bleaney and Ian Stewart (London: Routledge). It is used here with the permission of the publisher.

2 How Bacon's inductivism has colored our view of the history of science is explained in Agassi 1963.

3 This seems to be the view of both Hayek [1945] and Hutchison [1938] Aspects of this view were discussed in Chapter 5.

4 Its weakness is explained in Quine 1953/61, Chapter 2.

5 This seminar is explained in the article by Neil de Marchi [1988a] which I will discuss in Chapter 19.

6 The relationship between instrumentalism and applied econometrics is discussed in my 1986 book, Chapter 8.

7 I do not think this limited aspect of avoiding tautologies is widely appreciated. I explored this in more detail in my 1989 book, Chapter 7.

8 The logical problems of testing theories with models is explored in my 1989 book, Chapter 8.

10 Criticizing philosophy of economics

> My method demands that in conducting a philosophical inquiry into economics one consider some specific aspect in detail. The reasons for choosing the theory of capital and interest instead of some other topic are twofold. First, that theory is of considerable theoretical importance. As I shall explain ... one's views concerning capital and interest are intimately tied to one's general perspective on economics. Second, the issues in capital and interest theory are emotionally charged. People have passionate views, for example, on why the rate of interest or profit is normally positive. Capital and interest theory is thus especially suitable for studying how descriptive and normative issues interact and how ideology matters to economic theory.
>
> Daniel Hausman [1981, pp. 1–2]

In 1981, when he was a beginning philosopher of economics, Daniel Hausman's award-winning PhD thesis was published by the Columbia University Press.[1] With it he offered a very new approach to writing in the philosophy of economics. It attempted to break new ground in two directions, in philosophical analysis and in economic theory. This was a rather ambitious approach and at the same time a very risky one. Failure in either direction would jeopardize both objectives.

As many philosophers of science have undoubtedly discovered already, an analysis which would satisfy philosophers is too often also one which would not impress the scientists in question.[2] This is not to mention the additional obstacle presented by disagreements amongst the philosophers which may mean that even some of them will not be satisfied. If one's objective is to impress philosophers then failure to satisfy the scientists may not be a serious drawback. I say 'may not be' because there is one case of unimpressed scientists which should be of concern to the philosopher – namely, the case where the scientists are dissatisfied because the philosopher has not accurately represented the scientists' ideas or theories. Most of the recent literature about the philosophy of physics or of chemistry probably would satisfy the average physicist or chemist. However, the recently growing literature on the philosophy of economics written by philosophers has not been sufficiently accurate to satisfy most economists – and for this reason most of this literature has not made a measurable impact on the economics profession.

On the question of accuracy, Hausman's 1981 book stands in contrast to recent philosophy of economics literature. Unfortunately, accuracy is not enough since an equally important problem facing anyone writing in the philosophy of economics is one that concerns the appropriate strategy for achieving one's objectives. If one wishes to convince economists that there are significant philosophical problems within economics that need to be faced or solved, then it is wise to center the discussion on the mainstream of economics so that economists will be willing to consider those problems. If one wishes to convince philosophers that there are some interesting philosophical problems in economics then one should try to follow closely the currents of mainstream philosophy. In both cases the presumption is that the respective mainstreams are well defined. In the case of economics, the mainstream is the overwhelmingly dominant and easy to define 'neoclassical theory'. As for the philosophy of science, analytical philosophy still has a strong foothold although it is now quite standard to have it strongly colored by various aspects of the contributions of Popper or one of his various followers.

In his preface Hausman said that he wished to show 'what contemporary philosophy of science can contribute to understanding and solving the puzzles' presented by the economists' theories of capital and interest. And he also wished to show 'what understanding capital and interest theory can contribute to philosophy of science'. So, from the very beginning Hausman has set out a task which could succeed only if he followed both mainstream courses. Unfortunately, in this book at least, he did not. By focusing on capital and interest theory he ventured away from the mainstream of neoclassical economics and thus has not succeeded in convincing mainstream economists that there are philosophical tools that they may want to employ.[3] His primary purpose, however, was to convince philosophers of science that they need to create new philosophical tools to investigate the philosophy of economics – and, most importantly, that economics is worthy of philosophical analysis.

Since he seems to have fashioned his book in a manner that is appropriate only for an audience of philosophers of science interested in studying economics, we might be able to understand why he introduced capital and interest theory as a mere case study to demonstrate the virtues of his new tools. The question to be examined below is whether one can accomplish much by demonstrating a new philosophical tool using a topic

of little contemporary interest to the theorists in question. It will be argued that, despite Hausman's generally competent representation of the state of capital and interest theory, not much was accomplished.

Hausman's 1981 book began by presenting three different views of capital and interest: 'Austrian theories', 'general equilibrium theories', and 'Sraffa's work'. By examining these three views he wished to convince us that economists 'possess no good theory of capital or interest, or of their relations to equilibrium prices' [p. 191]. Of course, the key element here is what he thinks constitutes a 'good theory'.

By 'good theory' Hausman did not mean 'true theory' since he told us that (in the case of the available theories of capital) we are 'not in any position now to decide what is the truth' [p. 193]. For him, then, the key question was how theories consisting of apparently false premises can be explanatory. To answer this question he (I think quite correctly) avoided developing a model of 'messy' explanation (e.g. involving stochastic decision theories). Instead, he endorsed the 'standard deductive-nomological model of nonstatistical explanatory arguments' [p. 203]. In avoiding 'messy' explanations he recognized that one must come to grips with some means of assessing 'simplifications', 'idealizations' and 'inexact laws'. He proposed four specific criteria to determine when simplifications or idealizations are 'justified': (1) confirmation condition (simplification must lead to confirmable consequences); (2) no-accident condition (every confirmation is non-accidental); (3) sensitivity condition (relaxing any simplification will lead to more accuracy); and (4) convergence condition (whenever a simplification is a better approximation, one's explanation is more accurate). Obviously, one cannot expect Hausman to justify these criteria since that might lead to circularities or an infinite regress. Nevertheless, he claimed that they 'seem consistent with the judgements scientists make' [p. 205].

Since he employed the deductive-nomological model he had to be continually concerned with 'laws' or 'lawlike' assumptions. For example, what if the 'apparently false premise' in question happens to be one of the 'laws' needed in the model of explanation? To deal with this problem he introduced four more criteria to justify when an apparently false generalization can be used as a 'law': (5) lawlikeness; (6) reliability (disconfirmations are rare); (7) refinability (capability to produce more confirmations and less disconfirmations); and (8) excusability (disconfirmations can be explained away). Even equipped with all of these criteria one still needs to face the fact that the deductive-nomological model cannot deal with the alleged real intent of many economic model builders – namely, to identify the 'causes' of particular economic events. So equipped he concluded that 'economists lack rational principles upon which to make [causal] judgements' [p. 207].

Although in his 1981 book Hausman claimed only to be putting the worth of these philosophical criteria to the test by examining various limited theories of capital and interest, he did draw some conclusions about economics in general. The general conclusions were based on his posited axiomatic analysis of what he considered to be the core of neoclassical economics – on what he called 'equilibrium theory'.

Before I turn to an evaluation of Hausman's representation of standard economics, let me again state the fundamentals of neoclassical economics. The research program of neoclassical economics is fairly easy to specify. The task for any theorist is to construct an acceptable explanation of all non-natural and non-psychological phenomena as the consequences of (universal) maximization. The obvious things to be explained are the level of supply of any good and the level of its price. Some economists even try to explain such diverse events as marriage, charity or capital punishment – that is, institutional rules and arrangements of all types. As I explained in Chapter 6 above, to be acceptable every explanation must include one particular behavioral assumption – namely, the infamous neoclassical maximization hypothesis which asserts that all decisions are made in order to maximize according to a given objective function such as a profit or a utility function. There are, however, some hidden clauses here. Only individuals can make decisions or choices. If a price is to be explained as the consequence of clearing a market – that is, as an equilibrium price – then the market process must also be explained as the consequence of individual decision making. Any market is in equilibrium only if all participants are maximizing their individual objective functions. In other words, the state of equilibrium is an epiphenomenon since it is an 'unintended consequence' of the self-interested acts of individual decision makers. If any individual is not maximizing his or her objective function then the market cannot be in a state of equilibrium since either demand exceeds supply or the reverse. Whenever the demand does not equal supply at least one individual is either not buying or not selling the quantity that he or she should have been in order to be maximizing his or her objective function. This is simply because the phenomena of demand and supply, like all other social phenomena, are explained as the consequences of maximization.

Economists call the things they wish to explain 'endogenous variables' and distinguish them from the natural or psychological givens (or constraints) which they call 'exogenous variables'. One might (erroneously) say that economists claim that the states of the endogenous variables are 'caused' by the state of the exogenous givens. This would be misleading. Actually, whenever the givens change, the states of the endogenous variables change as logical consequences of the revised maximization decision due to the revised logic of the entire situation facing each individual. In

economics, the common idea of a causal explanation is purely a thought experiment in which it is presumed that the state of only one exogenous variable changes while all others are artificially held fixed (economists call this 'comparative static analysis'). But in reality the effect of changing one exogenous variable cannot be completely attributed to that one variable since the nature of the effect itself depends on the state of all other exogenous variables. Had the other variables been fixed in a different state then it is possible that the effect of the given change in the specific exogenous variable might have been quite different. What this means is that in the typical neoclassical explanation there usually is not a single 'cause'. Instead, we would have to say that all exogenous variables 'influence' the effect in question. In economics then, the philosophical idea of a single 'cause' is usually out of place.[4]

What makes an economics explanation interesting is that economic theorists would have us believe that, while generally observable phenomena such as prices are not caused by any particular individual, all individuals influence the level of all prices. Also, since every individual depends on some given prices (to calculate the benefits or costs of various possible actions), the specific decisions of every individual are indirectly influenced by the decisions of all other individuals. Such indirect influence is due entirely to the impersonal markets in which all individuals are thought to participate. In this neoclassical conception of the world, general equilibrium (i.e. a situation where all markets are simultaneously in a state of equilibrium) is possible only if all individuals are actually maximizing their respective objective functions. This means that for neoclassical economists the two observable variables on which Hausman focuses his attention – capital and its price (viz the interest rate) – must be explained as the consequences of maximizing choices or decisions.

For the most part Hausman's presentations of the above fundamentals and the various versions of capital theory are quite competent, indeed. Unfortunately, unless the reader is already quite familiar with the deeper theoretical problems involved it is unlikely that the reader will appreciate the high quality of his survey of capital and interest theories. This is not to say that his presentations are perfect; in many cases they are not, as he misses the point of some of the more important models in the literature (such as the excellent paper by Oscar Lange [1935/36]). He claims that Lange, like all equilibrium theorists, does not succeed in explaining interest, but this claim is based on a presumption that Lange was trying to explain the interest rate as only a phenomenon of a hypothetical state of equilibrium. Actually, Lange explains the level of the interest rate as being a measure of the extent of a given disequilibrium and he also explains why there is a disequilibrium. Similarly, Hausman claims that Piero Sraffa's

[1960/73] explanation of prices (that they are logical consequences of an exogenously given income distribution) is somehow 'too limited' on the grounds that the basis of the income distribution is treated as 'exogenous'. This is considerably misleading since, as Hausman seems to appreciate, all economics models must have at least one exogenous variable. What Hausman fails to explain is why Sraffa's exogenous variables are any less legitimate than those presupposed by neoclassical economists.

One of the difficulties with most philosophers of economics is that they tend to dwell on historical relics rather than on active concepts in contemporary economics. The study of the history of economic thought (alas) is no longer an essential part of any economics curriculum. For this reason it is difficult to understand how the ideas of such writers as John Stuart Mill could be seen to have direct relevance. Nevertheless, Hausman is fond of quoting Mill concerning questions of scientific methodology. Unfortunately he failed to stress the one lasting legacy that can be directly attributed to Mill, namely, the view that all successful explanations in social science must be reducible to psychological laws. In effect, Mill's methodological prescription says that the only acceptable exogenous variables are the psychological states of the individual decision makers. I think this prescription, which is too often still taken for granted, by itself accounts for virtually all of the significant philosophical problems in economics.[5]

Another difficulty with many philosophers of economics is that, as critics of mainstream economics, they often do not recognize that economists have many more clever ways to accomplish their research program than they are given credit for. Virtually all equilibrium models in neoclassical economics presume (as did Hausman) that the production process exhibits 'constant returns to scale' (which only means that whenever all inputs increase by a fixed proportion, the output of that process increases by the same proportion). In all such models, as a matter of elementary mathematics, the equilibrium theorists are always right – capital can be treated as just another input whenever all other inputs are being used efficiently (i.e. profit or surplus is maximized with respect to each input). This is obviously a question that cannot be decided here (but more will be said in Chapter 14 below). Nevertheless, it should at least be noted that many neoclassical economists will not be impressed with Hausman's critical conclusions about the prevalence of inherent problems with capital and interest theory. This is simply becasue mainstream economists generally do not see any obstacle to providing a long-run explanation of the supply of capital and its price (the interest rate).

If the philosophy of economics is in any way an attempt to explain why mainstream economic theorists do what they do, then it is unlikely that Hausman's case study of capital and interest theory will have accomplished

much. Economists today are not concerned about formal questions of 'lawlikeness'. The question of whether or not the assumption of universal maximization is formally 'lawlike' is of no significance to understanding the concerns of economic theorists.[6] Today many economic theorists are more concerned with constructing an individualist explanation of the persistence of 'disequilibrium' without in principle denying the truth of the presumption of universal maximization. This requires some very clever moves to avoid self-contradictory explanations. The usual way of reducing every explanation to exogenous psychological states is accomplished by seeing every decision as one made from a perspective of a 'long-run' equilibrium where everything but the psychological states are thought to be endogenous variables. Unfortunately, if one is concerned with 'disequilibria' it is easy to see that such 'long-run' explanations are irrelevant (since whenever so much time is allowed there is nothing exogenous to prevent the achievement of equilibrium).

The primary methodological question facing equilibrium theorists is: which variables can we accept as legitimate exogenous variables in any economics explanation? What bothers neoclassical economists about the work of Sraffa is that his exogenous variable (either the given income distribution or the wage–profit ratio) is not a psychological state of nature – it is a sociological event. What Austrian economics has put into question is the typical neoclassical presumption that the basis of the decision maker's knowledge is psychologically exogenous. Austrian economists such as Hayek would have us be more concerned with substantive questions about what is assumed about the decision process of the individual decision makers. To the extent that every decision made depends on the expectations of the decision maker, economists today are far more concerned with whether or not the formation of one's expectations can be explained in the same way that one's decisions are explained.[7]

If mainstream economists do not see a problem with capital or interest then the questions of capital or interest cannot be central to economics. If the questions of capital or interest are be central, why should philosophers of economics be worried about such questions? If one is going to explain the workings of mainstream economics then one must first understand the methodology of mainstream economists as they understand it. If one does then one will begin by examining the problems that concern mainstream economists today.

NOTES

1 All undated page references in this chapter are to this book, which I reviewed for the *British Journal of Philosophy of Science*.

2 A perfect illustration of this was an event at the 1992 History of Economics Society meetings at George Mason University where there was a panel discussion of Hausman's 1992 book on the philosophy of economics. The panel consisted of one philosopher and two economists. The philosopher said he loved the book. The two economists thought the book was terrible.

3 Interestingly, my first attempt at a PhD thesis was also centered on capital theory. Hausman's was much better than mine.

4 Unfortunately, Hausman's strategy of dealing with this problem by using terms such as 'causal factor', 'causal influence' and 'causal condition' only creates another confusion by using the word 'causal' to identify something which is supposed to be a 'non-cause'.

5 See my 1982 book, Chapters 2 and 3.

6 Hausman did recognize that most economists do not talk in terms of 'laws' or 'lawlikeness' but he seemed to think that the widespread use of 'models' amounts to the same thing. If this is why he spent so much time discussing equilibrium models, then this is a mistake since most economists are using models in the engineering sense rather than the analytical philosophy sense.

7 See Boland 1986a. I will discuss this in a different way in Chapters 13 and 14.

11 Reflections on Blaug's *Methodology of Economics*

> The first edition of this book was published in 1980. Since then we have seen seven major textbooks, three books of readings, an annotated bibliography, and of course hundreds of articles, all focused on economic methodology – not bad going for a mere decade of intellectual activity in a relatively minor branch of economics.
>
> This explosion of the literature in the methodology of economics would alone have warranted a second edition, in order to take account of new developments in the field. Moreover, my central message has sometimes been misunderstood. ...
>
> At first, I had ambitions to double the length of the original book ... But in the final analysis, intellectual laziness and a disinclination to rush in where even angels fear to tread have produced a second edition which is only marginally longer than and different from the first. ... In the main ... the new edition is substantially the same book as the old.
>
> Mark Blaug [1992, pp. xi–xii]

When I first read Mark Blaug's 1980 book on economic methodology, I was elated. After a famine of almost fifteen years, there was now a major publication by a prominent economist and from a respected publisher. For years publishers and editors rejected manuscripts on methodology, saying that, while they might be well written and often very interesting, there was no market for methodology. But in one strong stroke, Blaug proved them wrong. And, more importantly, his 1980 book was reprinted each year after its initial publication, culminating in the second edition which appeared in 1992. Without any doubt, those of us interested in an open discussion and study of methodology will be in debt to Blaug for a long time. Without wishing to devalue my appreciation of this book, I must admit that my second reading was not so inspiring. There are major problems with this book which need attention. While a second edition was published in 1992, as the quotation above explains, it was essentially the 1980 book. The only changes were a new preface, a new concluding chapter, and updating of the discussion of the maximization hypothesis (his new Chapter 15).

SUGGESTIONS FOR ANOTHER REVISED EDITION

It is disappointing that Blaug chose not to do a major revision as I had suggested in my review article [1985b]. I think my suggestions still need to be addressed, so in the remainder of this chapter I will present again my suggestions for a much needed (and deserved) completely revised version of the original 1980 book and the 1992 second edition.

Before getting into the small but significant details, there is one major difficulty in the organization of this book that needs to be noted. The book is divided into four parts as follows. Part I presents a post-positivist view of the philosophy of science within which Karl Popper's views are alleged to play a central role. Part II presents a very interesting history of economic methodology covering all the familiar topics from Adam Smith to present-day discussions. Part III applies the discussion of the first two parts to a series of chapters demonstrating a 'practical use' of methodology. Part IV briefly summarizes what was accomplished.

The major difficulty is Blaug's entire Part III. It intends to be a 'methodological appraisal' of the neoclassical research program but it would lead many readers to think that methodology matters very little. It is not clear whether this part's impotence is due to the so-called 'Popperian methodology' advocated throughout the book or to the mere idea of 'methodological appraisal' itself. I will discuss these two sources of difficulty in turn.[1]

WILL THE REAL POPPER STAND UP, PLEASE!

While Blaug wishes to convince us that Karl Popper's philosophy of science is the one we should adopt, it is difficult for some of us to reconcile what is reported to be 'Popperian methodology' with what Popper actually said and did. The problem, it would seem, is that Blaug has based his understanding of Popper primarily on the self-serving opinions of Imre Lakatos. I think Blaug would make a much greater contribution if he were to present Popper's views rather than the cartoon strip created by Lakatos.

To a certain extent Blaug can be excused for not quite appreciating Popper's message. It is always difficult to read the classic Popper writings because of his chosen methodological strategy – he always attempts to practice very deliberately what he preaches. That is, Popper has always preached that criticism is the basis for learning. For him sound criticism must always be based on clear understanding of the theory being criticized, and to be effective criticism it must be on the terms of the advocates of the theory that is being criticized. This last point is often overlooked and thus readers often misread Popper. The question is whether one wants to con-

vince the advocates of a particular theory or other critics of that theory. On the one hand, it is certainly easier to convince other critics but of course the critics learn very little from another successful criticism. On the other hand, if the criticism is correct, advocates have a lot to learn. Thus, if we are to make a significant contribution to learning, we should try to convince the advocates. Beginning in the 1930s, this approach to learning through criticism is what Popper, with his *Logic of Scientific Discovery*, consistently attempted to employ. He attempted to convince the Vienna Circle of logical positivists (or whatever they are called) that *if* there is a methodological means of demarcating science from metaphysics, for purely logical reasons it would have to be falsifiability rather than verifiability. Similarly, *if* observations matter, it would be refuting observations rather than confirming observations. Again, the argument is a matter of logic. The questions of demarcation and whether observations are the foundation of knowledge were matters of concern for the Vienna Circle and thus were the objects of Popper's criticism. They were not necessarily a major issue for Popper's understanding of science or methodology [see Bartley 1968].

What is important to appreciate is that Popper's criticism of the Vienna Circle's methodology is presented on their terms. This approach to learning and criticism makes Popper's works very difficult to read and leads many readers to attribute viewpoints to him which are actually those of the advocates being criticized. The best example of this is his book *Poverty of Historicism* [1944/61] in which he presents several views of history. After each presentation he offers his criticism. I have seen quotations taken from this book, supposedly stating Popper's view, which in fact were taken from one of his presentations and thus were actually representing a view with which he disagreed. Of course, the source of this problem is Popper's chosen approach. While his approach may involve logically sound criticisms, it is too often an excessively risky form of rhetoric.

Throughout his book Blaug refers to what he calls a Popperian methodology of falsificationism. I cannot figure out just what this so-called methodology is. Popper repeatedly criticized the view that there is some method to invoke which will assure a certain outcome no matter who uses it. What is the purpose of such a methodology? What does it supposedly accomplish? Years ago I tried to explain why it is easy to be misled by the methodological requirement of falsifiability [Boland 1977b]. Paradoxically, verificationists are more interested than anyone else in falsifiability. The reason is that it is all too easy to accuse verificationists of being satisfied with verifications of tautologies since a tautology will always fit the facts. Thus, in order to avoid the embarrassment of claiming one's theory has been verified only to find out that it was a tautology, verificationists now require every theory to be falsifiable. This is a very useful methodological rule since tautologies are never falsifiable – i.e. as a matter of logic they cannot be false. In other words, the requirement of falsifiability is the solution to a verificationist's methodological problem. I still cannot see any other reason to advocate a 'methodology of falsificationism'.

The idea of a Popperian methodology based on falsifiability is more attributable to Lakatos than to Popper. Popper did not offer an alternative method of providing a sound foundation for our knowledge – to the contrary, he repeatedly criticized the view that we need such a foundation. A foundation consisting of all the confirming evidence or refuting evidence in the practical world will never prove the truth or falsity of our knowledge. This does *not* mean that our knowledge cannot be true or false *nor* that we should not be concerned about the truth or falsity of our knowledge. A true theory may very well be less falsifiable than a competing false theory. And the more falsifiable false theory may be difficult to falsify for practical reasons no matter how much we attempt to do so. Thus, appraising a theory on the basis of its falsifiability can be very misleading [see Wisdom 1963]. If by advocating a 'Popperian methodology' Blaug is urging us to put questions of falsifiability ahead of any other concern, then such a methodology may turn out to be very unproductive.

What is important to recognize here is that the view which emphasizes the progressive role of falsification is nothing but a sophisticated version of the belief that there is a mechanical (and objective) method which will always bring us eventually closer to the true or correct theory we want. That mechanical method is the inductivism that Popper so consistently criticized.

The problem that Popper was concerned with was the view that there exists some mechanical tool with which we can measure the structure of a theory to appraise its truth status. There is no such tool; that is, no methodological criterion exists that can be used to distinguish the true theories from the false ones.[2] This is *our* problem but it is not necessarily a problem of the structure of our theories. Attempting to overcome this problem by fooling ourselves with invented criteria such as verifiability or falsifiability is just an exercise in the conventionalism that Popper warned us not to follow. And implying that the truth status of a theory can only be a matter of convention (supposedly since it can only be appraised as the best according to the accepted criteria) may make us feel better but it accomplishes nothing that could be considered intellectually interesting.

As I read Popper, starting with the English edition of his 1934 book and continuing through his 1972 *Objective Knowledge*, he was always against what he called 'conventionalism'. It is difficult to find this view of Popper as an anti-conventionalist in Blaug's presentations in Part I. Instead we encounter a Popper who supposedly says something to the effect that for

scientists 'all "true" theories are merely provisionally true' [Blaug 1980, p. 17]. Either a theory is true or it is false, so what does it mean to be 'provisionally true'? Is it being alleged that Popper endorsed the conventionalist concept of 'provisionally true' or that he rejected it?

In one sense Blaug may be correct in presenting such a confused view of Popper since the confused view is the one most often entertained by economic methodologists who refer to Popper.[3] That is, if Blaug is only attempting to explain how economists understand methodology, then he is correct. But if he is trying to teach or advocate a consistent Popperian view of methodology, he should clear up these questions for the benefit of the general reader.

ON THE UTILITY OF 'METHODOLOGICAL APPRAISAL'

I think the only reason Blaug wishes to characterize the appropriate view of the philosophy of science as a methodology based on the conventionalist criterion of falsifiability is that he wishes to employ such a tool in the appraisal of economic research. The question of interest is not whether there is a better tool (this question leads only to an even more sophisticated conventionalism). Rather we should ask why we need a 'methodological appraisal' of economic theory. To put this question into Popperian terms, what problem is solved by a methodological appraisal?

It is all too easy to form a vision of the methodologist as a Supreme Court judge sitting high on the bench wearing a long, flowing black robe and, perhaps, even a white wig. The methodologist's job in this vision is to pass judgement on economic theories. According to Blaug the judge is not allowed to assess a theory in isolation but must decide between competing theories just as the courtroom judge must decide between the prosecution and the defense. It is just this view of methodology that Galileo challenged when the College of Cardinals (acting as the judge) told him that for methodological reasons he had to teach Ptolemaic astronomy rather than introduce his students to the competing view of Copernicus. I agree with Galileo – methodologists are no better qualified than anyone else to assess the truth or falsity of a theory about the nature of the real world.

Of course, there is a definite merit in requiring comparisons rather than singular judgements. To make a comparison one must recognize the existence of other viewpoints. At least to a certain extent, this avoids narrow-mindedness. But open-mindedness would have us go further to consider many theories and viewpoints and let the advocates make their cases. We may want to choose one theory but only when we need to. Of course, historians of economic thought always think they need to choose one theory every time they put their pens to paper. And worse, textbook writers always find it easier to write about a single theory since students too often object to being put into a position where they have to think for themselves.

SOME SUGGESTIONS

This brings us back to the problem with Blaug's Part III, where he applies his view of methodology to economics. It might be better to relegate this 'methodological appraisal' to an appendix or eliminate it entirely. While it provides excellent summaries of many parts of the neoclassical research program as well as some competing programs, it adds nothing to our understanding of methodology. With very few exceptions, methodology in these demonstrations never seems to matter to the proponents of the individual viewpoints discussed. If it does not matter to them, how can it matter to us? The only major exception is, of course, Paul Samuelson, who explicitly proclaimed his methodological purposes. Perhaps instead of the 1980 Part III, Blaug could give us a reader's guide to Samuelson's *Foundations* [1947/65]. Perhaps he could also review the questionable methodological basis for the overwhelming dominance of mathematical model building in mainstream economics. He could, perhaps, give us a first-hand report on why in the 1978 edition of his *Economic Theory in Retrospect* he said 'Friedman is not guilty of "instrumentalism"' but just two years later in the 1980 book he told us about 'the methodology of instrumentalism espoused by Friedman'. Without explanation, students of methodology must find this confusing. Fortunately, it is a matter which Blaug is in a good position to clarify.

If Blaug would follow my suggestions, I have no doubt that he would provide an invaluable textbook on economic methodology. If he does not, I think students of methodology would spend their time more fruitfully by studying the equally penetrating history of methodology provided by Bruce Caldwell [1982, 1994].

NOTES

1 The remainder of this chapter is based on my review article [1985b], which is used here with the permission of the publishers of the *Eastern Economic Journal*.

2 See Chapter 8 above as well as my 1989 book, Chapter 5.

3 I will present my detailed criticism of this confusion in Chapter 20 below.

12 Criticizing 'pluralism' and other conventionalist ploys

methodological falsificationism is not the last word in economics; but perhaps *methodological pluralism* is. ... [I]t must be coupled with the imperative that, just as there are many paths to knowledge, there are many forms of *criticism*, and the more that are heard, the better.

Boland is correct in asserting that the [theory-choice] problem is unsolvable, but he errs in thinking that it is therefore uninteresting.

Bruce Caldwell [1982, pp. 128 and 246]

the critical appraisal of theories plays an essential role in methodological pluralism. ... Pluralist methodologists do not embrace a particular tradition; their goal is the evaluation of all traditions. In a sense, pluralist methodologists attempt to practice *value-free evaluations*. Their assessments are critical, but they do not presuppose some ultimate universal grounds for criticism. ... Methodological pluralism makes no epistemological claims; it is not grounded in any theory of truth.

The goals of pluralism *are* modest. Methodologists are not set up as experts offering advice to economists on how to do their science. Methodologists do not try to solve the demarcation problem, or the theory choice problem, or the problem of truth. Rather, methodologists try, together with their colleagues in the history, sociology, and rhetoric of science, to enable us to reach a better understanding of the science of economics.

Caldwell [1988b, pp. 235, 241 and 243]

I have [made] some suggestions for how to do methodological work, a meta-methodological program which I originally called methodolgical pluralism but which is probably better dubbed critical pluralism ... I will not repeat this account, except to make the following two points. First, critical pluralism broadens the definition of methodology to include the study of things like rhetoric, sociology and history. Some may quibble with me over terminology, but I hope that all will agree that such fields are best viewed as complements rather than substitutes in the quest to understand economics better. Second, to see that studying methodology is *not* the same thing as studying economics, one can compare the travails of a methodologist to those of, say, an

> academic theologian or a sociologist of religion. The analogy is a simple one. One does not study theology or the sociology of religion to become more religious. One does it to understand religious phenomena better.
>
> Caldwell [1990, pp. 65–6]

> there is [an] alternative ... which I view as a middle ground between relativism and the quest for universal criteria. The position that gradually emerged in a series of works ... is one that I labelled critical pluralism. ... The major tenets include: ... A disavowal of quests for a single demarcation criterion or for universally applicable criteria of theory appraisal. ... An emphasis on criticism: the role of the methodologist is to assess the strengths and weaknesses of various research programmes. ... An objectivity constraint: in reconstructing a programme and its methodological content, one should try to give it its strongest possible portrayal. ... An insistence on viewing all criticism as problem-dependent. The content of the criticism will depend on the sorts of problems the programme seeks to answer. A programme could be found to be adequate for the solution of certain problems and inadequate for the solution of others. ... Finally, the critical pluralist values novelty. Though criticism is a key, new programmes should be encouraged to flourish and permitted a grace period in which they are not severely criticized. Once a programme is established, though, the critical process begins.
>
> Caldwell [1991a, p. 104]

Looking back over methodology literature that began with John Neville Keynes, it is difficult to find anything that has any direct bearing on economic literature in general. From the perspective of a practicing economist or economic theorist, until quite recently methodology has been completely useless. The only possible exception is the very limited role methodology plays in our understanding of the history of economic thought.

The primary reason for such a bleak picture is that economics, like every other intellectual enterprise during the last sixty years, has been dominated by the positivism that I discussed in Chapter 9. In particular, economics has been dominated by positivism's philosophical prejudice which considers only scientific activity to be 'meaningful' and all else (including methodology and philosophy itself) to be 'meaningless'. Against this background it has always been difficult for anyone to justify spending time studying methodology.

Nevertheless, prior to 1979 there were some contributions to methodology literature. Notably, Hutchison, Machlup, Friedman and Samuelson have made the most impact on the image of methodology in economics. Judging by what has been the course of economics over the last thirty years, it certainly appears that none of these writers have had

anything to say that would in any way affect the ordinary machinations of the everyday economist other than spicing up some of our rhetoric. Today, most methodologists (ever the optimists) will probably disagree with the accuracy of this dismal picture. But, alas, there is no evidence to support their contrary view. Instead, we see constant pressure to make methodology 'value-free evaluation' as Bruce Caldwell advocated in 1982. As meek as that sounds, things are getting worse. Today, there is ever more pressure to drop the evaluation part and instead stress the goal of understanding, or 'recovering', the actual practice of economics. Passive neutrality seems to be the norm. This has prompted Mark Blaug to openly criticize this movement by saying that it

> would open the door to any and all economics: in refusing to prescribe they end up with economics just as it is. 'Economics is what economists do', Jacob Viner once said. This ironic definition of the science of economics could well serve as the rallying cry of the anti-Popperians. 'Recovering practice' is what they call it but it is not much more than accepting economics as it is, for better or worse. [Blaug 1994, p. 129]

Without disagreeing with Blaug, I think the problem is more fundamental than just the sanguine wishes of some 'anti-Popperians'. The problem is that economic methodologists refuse to abandon their faith in the good-hearted conventionalism that I have criticized many times. The most persistent form of conventionalism is 'methodological pluralism'. The earliest advocates seem to be Sheila Dow [1980] and Bruce Caldwell [1982]. However, it should be recognized that methodological pluralism is merely conventionalism raised to a meta-theoretical level. Recall that with conventionalism one is not supposed to claim their theory is true (or false) since without induction we could never prove any theory's truth status [see Boland 1979, 1982]. At the meta-theoretical level the methodological pluralists claim that we cannot prove that any methodology is the 'best' hence none is best. Thus we see that methodological pluralism is just another intellectually defeatist stance – one which will have great appeal to liberal-minded intellectuals who are afraid to argue or to take a strong stand.

Bruce Caldwell has been the strongest advocate of pluralism, although he claims that his position has changed over time such that he now sees himself as an advocate of Popper's anti-conventionalist 'critical rationalism' under the banner that Bruce calls 'critical pluralism'. In this chapter I am going to discuss some of my criticisms of pluralism that I presented at the 1986 University of Manitoba workshop which I discussed in Chapter 3. The announced topic for that workshop was 'methodological pluralism'. In the process I will critically examine Caldwell's transformation as well as explain why pluralism is just another conventionalist ploy.

METHODOLOGICAL PLURALISM VS PROBLEM-DEPENDENT METHODOLOGY

Let me begin with a comparison of the young Caldwell's 'methodological pluralism', which was presented in his 1982 book, with my 'problem-dependent methodology' that I presented in my 1982 book. In the process of making this comparison, I wish to discuss the young Caldwell's emphasis on external criticism in his 1980 criticism of Friedman's methodology. I will contrast this with my emphasis on internal criticism. We will see that, despite its liberal-sounding name, methodological pluralism is much less tolerant than my problem-dependent methodology. This observation may seem rather perverse since the young Caldwell's concept of criticism is much weaker than mine. Specifically, the young Caldwell tells us that the main characteristic of methodological pluralism is that it encourages all forms of criticism and, in particular, while allowing for internal criticism as one of the many forms of criticism, it encourages external criticism, while I reject as useless any form of external criticism.

The logic of the situation

The key to distinguishing the young Caldwell's views from mine is to recognize that, despite his protestations [see his 1980 article, fn. 8], his espousal of pluralism is based on an implicit acceptance of what I have been calling conventionalism. Specifically, whenever we are speaking of theories being true, the young Caldwell bases his views on a presumption that we are always speaking of the truth relative to acceptable conventions which define truth, while I reject conventional truths and any concern for such conventional criteria. It is important to keep in mind that conventionalism is not a version of instrumentalism, nor does the reverse relationship hold. Conventionalism and instrumentalism are two responses to two different questions. On the one hand, if one asks about the *role* of theories in science, instrumentalism responds by claiming that the proper role is only to be useful in generating predictions or solutions to practical problems. Conventionalism responds by claiming that the role of theories is to provide convenient catalogues or filing systems for scientific evidence and observations. On the other hand, if one asks about the *status* of theories, conventionalism adamantly responds by saying that it is improper to consider theories true or false as there is neither foolproof evidence nor a logic that would provide proofs of the truth status even if the observations were foolproof. Instrumentalism claims that the question of status is irrelevant – theories can be true or they can be false but it does not matter so long as they are useful. Conventionalism claims that all we can say is

that the truth status of any theory is a matter of whether one can give a logical proof based on conventional criteria of acceptence (R^2 values, for example).

To differentiate my product, let me say that I am concerned whether theories are true by their reference to nature rather than to conventional notions about nature or about what is acceptable evidence. With this distinction in mind and the aforementioned differences in my emphasis on internal criticism rather than external criticism, the logic of the situation is that shown in the following options box:

Criticism	Theories: *True by convention*	Theories: *True by nature*
Internal	existentialism or neo-romanticism (post-1980 Caldwell)	Boland 1982 to present (post-1991 Caldwell?)
External	Caldwell 1980	The old Scientific Method (pre-1800)

Figure 12.1 Types of criticism

Internal vs external criticism

The young Caldwell's 1982 book (which was based on his PhD thesis) was primarily an argument for why we should be concerned with the opinions of philosophers of science. For example, Chapter 9 (which is based on his 1980 article) includes a section on an alleged 'philosophical rejection of instrumentalism' and another section presents a 'methodological critique of instrumentalism'. His only argument given in opposition to instrumentalism is that philosophers such as Karl Popper, Peter Achinstein, Grove Maxwell, Paul Feyerabend, Carl Hempel, Paul Oppenheim, etc. all reject instrumentalism or any form of emphasis on predictions over explanations. I think this line of argument borders on outright authoritarianism. The young Caldwell presents these 'external criticisms' of Friedman's instrumentalism as a critique of my *Journal of Economic Literature* article [1979a] where I argued that critics of Friedman's methodology have all failed because they failed to offer internal criticisms. Unlike many critics of my *JEL* article, both the young Caldwell and the middle-aged Bruce understand that I was not defending Friedman but rather I was showing what it would take to effectively criticize Friedman's 1953 essay. In parts of his 1982 book, he seems to agree that internal criticism is more effective than external criticism – at least when it comes to criticizing Austrian economics [Caldwell 1982, pp. 248–50]. We differ here only because the young Caldwell thinks external criticism is relevant and I continue to claim it is ineffective and erroneously presumes that all followers of Friedman's essay would bow to the authority of philosophers.

In the young Caldwell's 1983 comment in the *American Economic Review* he mildly appeals to the works of the philosophers Carnap, Hempel and Nagel who developed theories of confirmation. As an alternative, I insist that the only effective criticism will be based on finding logical contradictions. The young Caldwell here is promoting appraisal instead of criticism, as does Blaug [1980, 1992]. But the basis for appraisal is again the conventional criteria promoted by philosophers. Once more, it is an appeal to external authorities and as such it is unacceptable as far as I am concerned.

I suppose whether external criticism is relevant or not depends on what one wants to accomplish with one's criticism. For me, the purpose of criticism is to learn – more specifically, to improve *my* understanding. While this may suggest a one-way linear relationship between criticism and understanding, the relationship is a duality. While one might criticize in order to understand, to be effective criticism must begin with a prior understanding that is both fair and clear. One implication of this view of criticism is that I am not interested in understanding for understanding's sake. It is the process – not the achievement – that matters. When I offer a criticism of someone's theory it is for the purpose of testing my understanding of that theory. If my criticism is correct I should be able to convince the theory's advocates.

In order to convince the advocates I think it is important that the advocates be convinced in terms that they would accept and that would be consistent with their reasons for forming the theory in question. External criticism is not relevant here except when the advocates form the theory to impress outsiders (as was common among mathematical economists thirty years ago). Of course, I would reject outright any attempt to impress outsiders and thus it is easy to see why I am not interested in external criticism. So the question for advocates of external criticism is: just who are they trying to impress when they promote external criticism?

Against conventional criteria of truth

Now the young Caldwell repeatedly told me that he was not a conventionalist. But, I repeatedly asked, why are you concerned with 'theory-choice'? The problem of theory-choice which in 1982 he implied he found interesting [1982, p. 246] is nothing but a conventionalist problem. Perhaps the young Caldwell's methodological pluralism was invented to overcome difficulties with making a theory-choice. Starting in 1985 at an Amsterdam conference on testability in economics, he began to exhibit signs that he might have been listening to my complaints. But, if so, he was still not getting the point of my complaints. As of 1985 his methodological problem was to be based negatively on the so-called 'demarcation problem'. Methodological pluralism is claimed to be a way of avoiding this so-called problem. With this move the young Caldwell seemed to think we should deal with a different problem associated with conventionalist methodology. I think we can ignore conventionalism, and thus any methodological pluralism based on desires to avoid the conventionalist problem of demarcation can only be seen in a lesser light.

Whether one's theory of the economy or the natural world is true or false is not usually something for us to decide. As I have explained in Chapter 8, no method is sufficient. What methodological stance one adopts depends on the problem in hand. For some short-run practical problems, some people adopt Friedman's instrumentalism. As long as one is not really concerned whether one's theories are true, then one can adopt some form of conventionalism to explain away one's failures to completely understand phenomena. If one thinks one can learn only by experience, then some form of inductivism is probably the best choice if one needs to justify tedious data collection. To the extent that the truth status of one's theories matters, conventionalism and instrumentalism will never do. What methodological stance will do depends on what problem one is trying to solve, and thus in my 1982 book I called this 'problem-dependent' methodology.

Intolerant pluralism

By basing methodological pluralism on the acceptance of conventionalist methodological problems and occasionally on the associated appeals to external authorities, I think the young Caldwell's recommendations led to undesirable situations. In contrast, my problem-dependent methodology said that we should not criticize someone for not achieving ends which were not intended. When a researcher or writer sets out to solve one problem, failure to solve it may be criticized but failure to solve other problems is not necessarily a shortcoming.

Conventionalism-based methodological pluralism can too easily be used to criticize a researcher, a writer or even a methodologist for not attempting to solve the conventionally accepted problems. For example, the young Caldwell took it as a given that we must either solve the problem of determining an acceptable method for choosing the best theory or solve the problem of stating a criterion which can be used to demarcate the acceptable ones from the unacceptable ones. I asked, what problem is solved with such a method or such a criterion? As I explained in Chapter 8, an invocation of such a choice criterion is nothing more than an attempt to live up to the conditions of the hypothesized 350-year-old Social Contract of Justification.

Simply stated, I did not sign that contract and so I do not feel obligated to fulfill its conditions – that is, I do not think I am required to say I do not know anything unless I can prove what I know is true. Forcing us to live up to this contract by requiring that we solve the demarcation problem is an act of intolerance. My alternative is to say: Tell me what problem you are trying to solve and we will see if you have succeeded. Here, anything goes, but this does not mean that I can guarantee that I will be interested in your problem. Nevertheless, you are free to try to convince me that it is an interesting problem.

Against appraisal

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 (much like tenure committees) passing judgement 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? If economists wanted to be physicists they would have studied physics instead. On what basis do they criticize methodologists? Do they say we do not deserve the philosopher's good-housekeeping seal of approval? Who cares? Who says philosophers have a monopoly on clear thinking? Who says we have to solve all the problems that philosophers have been unable to solve for many centuries? So just what problem is solved by passing judgement on someone's theory or methodological principles? Most times, I have difficulty finding interesting problems being solved by methodologists who think their primary job is theory appraisal. But I may be missing something here.

DIVERSITY AS NON-COMPREHENSION

The audience at the 1986 Manitoba workshop were quite unhappy with this critique of pluralism. Some of them called my problem-dependent methodology my 'golf-bag' methodology. I gladly accepted this label but I did find it curious that proponents of pluralism could be so upset by my refusing to take a 'position' or advocate a specific methodology.

All during the 1980s I regularly attended the annual History of Economics Society (HES) conference. While I am not a historian of economic thought, one reason for attending these conferences was that while the sub-discipline of methodology has been shut out of most mainstream conferences, it still plays a prominent role at HES meetings. At the end of each conference day a few of us would meet to compare our various personal perspectives on methodology over a couple of beers. Most often these gatherings would include Wade Hands and Bruce Caldwell, the two most prolific of the new generation of methodologists. The discussion of methodology would often concern Karl Popper and his alleged 'falsificationism'. I repeatedly complained that they did not understand Popper if they thought his views can be fairly characterized as 'falsificationist methodology'. I told them they were confusing Popper with Imre Lakatos and that if they really understood Popper they would see that his view of science is Socratic, based on learning through criticism. A fair characterization would be that Popper advocates what he calls 'critical rationalism'. By the mid-1980s my friends became exasperated with me and simply declared that my view of Popper was 'idiosyncratic'. Apparently I was not toeing the party line and so, even though I had published more about Popper and testability in economics than anyone else, I was not invited to the Amsterdam conference on Popper and testability. Of course, this could also have been due to my so-called 'acerbic' behavior.

At the HES conference in May 1985, Bruce Caldwell chaired a panel discussion on 'Methodological diversity in economics'. His intention was to have me, Donald McCloskey (a proponent of 'rhetoric') and Alexander Rosenberg (a philosopher of economics) present our diverse views and then be quizzed by Uskali Mäki, Neil de Marchi and Wade Hands. McCloskey could not attend, so Arjo Klamer did his best to represent McCloskey's views.[1] What transpired [Caldwell 1987, pp. 207–39] illustrates the reluctance of methodologists such as these to recognize that I was not presenting another superficial position on methodology. I opened with what I thought was a joke.

> [Opening joke:] Bruce, tell me when I have one minute left – I am a trained neoclassical economist and thus unable to sense time.
>
> As a graduate student, after reading the now famous 1959 Klappholz

and Agassi article, I quickly adjusted my view of methodology from the one I had as an undergraduate student. The view I subsequently adopted was that methodology is not necessarily prescriptive. Instead, methodology can be both descriptive and an adequate basis for critical understanding of what economists *actually* do (i.e. as opposed to a set of prescriptions about what economists *should* do). By a critical understanding, I mean one that is based on criticisms of the methodological views often taken for granted by practicing economists.

By saying that my understanding might be based on criticism, it may tempt some of you to conclude that I have thereby smuggled prescriptions in through the backdoor. Such a conclusion would be an error – an error which I have called the 'Santa Claus Syndrome'. For one to be allowed to criticize the existence of Santa Claus, one's criticism need not prescribe some method by which gifts will be provided every Christmas. In other words, *criticism does not imply prescription*.

Of course, by focusing on the explanatory role of criticism, I must stress that I am concerned with criticism that points to errors in the logic of one's arguments – perhaps in terms of either alleged contradictions in, or alleged incompleteness of, one's arguments.

By stressing the importance of logic, some of you may jump to the centuries-old conclusion that I am therefore rejecting what my friend Don McCloskey calls 'rhetoric'. Such an old-fashioned conclusion would be a grievous error. The sole purpose for forming logical arguments is to convince people of the truth of one's argument. Also, it must be recognized that the logicality of any argument may be necessary but it is not usually sufficient to ensure that one's argument will be convincing. It is usually wise to keep in mind the context and purpose of one's arguments and what the audience wants to hear (even though one is not obligated to give them what they want).

Traditionally, methodology (as well as ordinary conversational etiquette) is more concerned with *how* you say something than with *what* you say. The most recent example of this is McCloskey's view that *the* guiding principle should be that all productive discussions must be based on 'polite conversation'.

In this regard I am reminded of two observations which illustrate why I despise any over-emphasis on the 'how' at the expense of the 'what'.

(a) In order to please Don and his many followers who tell us that we should study literature rather than mathematics or logic, the first observation is drawn from well-known literature.

There is a short scene in Henrik Ibsen's famous play *The Doll's House* where Nora and her husband are heatedly discussing Nora's close friend Dr Rank – just as Dr Rank is climbing the steps to their front door. Nora's husband strongly dislikes Dr Rank and he is berating her about her friendly association with Dr Rank. But, when Dr Rank comes through the door, he is greeted by her husband with a most disgustingly saccharine display of politeness – Ibsen despised such *socially required* dishonesty.

(b) In order to please those who worship science (with a capital 'S'), the second observation is from the history of science.

The story is often told about how Albert Einstein's colleagues rejected him and his physics at one crucial point in his career. Their primary reason, so the story goes, was that he was very rude and impolite. Their evidence was that he attended a funeral dressed informally in a sweater.

So much for politeness!!

When I began teaching methodology in the early 1960s, like everyone else who teaches methodology, I had to deal with the tradition that sees methodology as the exclusive domain of philosophers of science. To break with tradition, I stressed that the idea of science is a social concept invented for authoritarian purposes. An illustrative problem that I always used is the following:

If you were a lawyer defending an innocent client charged with some heavenly crime, and you had to select an expert witness, would you choose an astronomer or an astrologer? My economics students would immediately reply that they would, of course, choose the astronomer. Now wait, I would say, I have here an astrologer who in hundreds of cases has never made a false prediction, and the only available astronomer still cannot decide whether or not the universe began with a 'big bang'. The students still said they would choose the astronomer – but they never could tell me why!

Now, isn't 'scientificality' when used this way merely a rhetorical ploy? Is this a function of the philosophical nature of science? Or is this a consequence of how people socially and arbitrarily try to support their arguments with some form of public authoritarianism? – I do not like any of these options! [Caldwell 1987, pp. 210–12]

My first inquisitor was Arjo, who said:

Before I do something like this, I read philosophers, just to get some sense of what the language is, and to sharpen my own language. But I'm not carrying Lakatos in my bag. I don't carry Popper in my bag. I read Richard Rorty; I find him very inspiring. ... I consult literary critics. ...

> Concerning people within economics ... well, I am somewhat bored with a lot of stuff that comes out. I am not surprised that many economists, when they read our material, say ‘What in the hell does *that* have to do with what we are doing?’ We have all these careful, sophisticated, logical analyses of their theories, but they don’t recognize anything in it at all, and it doesn’t do anything for them. And I must say that that is true for a lot of stuff that I read in methodology. [p. 221]

I responded with the following:

> I’m a little concerned about Arjo’s attitude about methodology. I am reminded about the history of mathematical economics. We are doing a survey asking people’s opinions about mathematical economics. ... We all know the problem with mathematical economics as practiced today by people coming out of school. They don’t stop to figure out what the problem is; rather, they say ‘Is there a new *technique* that I can use to solve this problem?’ or ‘I have heard somebody has got this fuzzy-set theory; maybe I should apply that to it’.
>
> If you approach methodology that way, obviously Arjo Klamer is going to have a ball doing what he is doing. Right? But that’s not a problem with *methodology*. That’s a problem with people who don’t want to think for themselves. What concerns me about ... Arjo’s business ... is that [he does] not sit down on [his] own and think about economics. But instead, it’s ‘What did so-and-so say, or what did Popper say, or what did ...’ Well, this is silly. Think first. Then go read them. [pp. 221–2]

Neil de Marchi jumped in with:

> Larry Boland is coming over this morning quite differently from the impression one gets if one reads his [1982] book. He is the most positive of the ... people here. Arjo is coming across as a mere observer ... Larry is at least prepared to grant that we should be critics, and indeed that is the theme of his book, too. What I’d like to know from him is this. If we are all strictly Popperian, ... if we move as he would want us to a Popperian position, and we get rid of this conventionalism, we become critics. Our job as methodologists is reduced to ... drawing out, laying bare, excavating the hidden agenda behind any piece of economic writing. This is criticism.
>
> My question is this: If Boland is not prepared to acknowledge that there is goodness, appropriateness, things interesting, which economists all the time deem certain work to be and other work not to be; if he’s not prepared to allow us any standards, then he is criticizing in a vacuum. I want to know from him why the methodologist should not be in a

> position to take a view of these things. To turn an assertion of his own around, he says at one point in his book, ‘Why should anyone be concerned with the acceptability of empirical knowledge?’ My question is, how can we possibly not be concerned with precisely that? [p. 222]

My response to Neil was this:

> I am, as you can imagine, a methodological terror in my department, and have been for twenty years. I am a terror not because I come in and pontificate about methodology. I never talk to my colleagues about Popper or anything – they couldn’t care less, and I understand that. I understand my audience. All I do is, when they give a seminar on whatever fancy thing they are doing at the time, I will ask them, before they get started, why did they bother to do this paper? Now this is a terrifying question for people. First of all, they spent $50,000 or $100,000 on research, and they know they’ve got garbage, and they don’t want you to let anybody know *that* because they are responsible for the research, and so on. You know, the worst thing you can have is somebody asking the question of why you are doing this. Now Neil de Marchi may view that as a critical question. I just want to know what they’re doing. But somehow it’s viewed as a critical question ...
>
> In fact, I remember that as a graduate student I had a superb capital theory class given by Paul Wells in which the rules of the game were as follows: He would assign a paper for the next class which some student would be called on to present, but no one would know in advance who the student would be. He would do it randomly. So you obviously had to prepare it and be ready to explain it, and you learn a lot that way. And every time these students would come to the front of the class, I’d ask, ‘Why did so-and-so bother to write this paper?’. And this went on for weeks. They hated it. This is a dumb question to ask, and they hated it. One day, about ten weeks into the semester, I showed up late. As soon as I sat down I put my hand up, ready to ask my question, but they said, ‘sorry, we already asked it’.
>
> Now, I realize it is hard to convince people ahead of time that it is worthwhile to do these things. But do it, you’ll like it. [pp. 222–3]

Later Neil decided to try again:

> Larry Boland did not answer my previous question at all. He merely repeated what I had said, which is that our task is to criticize. My question was: criticize to what end? Why should we bother to raise the consciousness of our fellow economists, to what purpose? We must have in mind some sort of criterion of goodness or badness which makes the criticism purposeful. I would like to address that to him and ask if he would mind answering this time. [p. 226]

My response to Neil was as follows:

> I answered your question; maybe I didn't underline it. I said I do methodology all of the time. I don't stop to worry about these heavy philosophical questions about adequacy.... That's a problem that you have to look at in terms of what somebody wants to do. Now the real danger with my answering that way is that it sounds an awful lot like what Arjo Klamer just said. I'm saying that there is no such thing as a hard criterion for the acceptability of evidence – it depends on what you want to *do* with that evidence. In some circumstances it works; in some circumstances it doesn't work. Tell me what you want to do and then we'll talk about it, because I want *you* to think about what *you* want to do, because only that way can we decide what is adequate evidence. So I did answer your question. [pp. 226–7]

FROM IDIOSYNCRATIC TO MAINSTREAM

I tried to understand why Wade and Bruce were able to so easily dismiss my views of methodology and of Popper in particular as being idiosyncratic. The debate at this 1985 panel session seemed to me to indicate that we were talking completely at cross-purposes, particularly when it came to discussing Popper's view of science and methodology. It could simply have been a generational gap since I began studying methodology before any of the other participants heard of Lakatos, but in the 1980s the post-1979 generation for the most part seemed to think Lakatos had the last word on methodology and, in particular, on Popper's views of methodology. Despite our many conversations over post-conference beers, little progress was made towards overcoming our failures to communicate. In an effort to understand my failures to communicate, I re-examined some of the methodology writings of Wade and Bruce. The following subsections report on what I have found.

Wade Hands the reluctant Popperian

Wade Hands' publication career in methodology[2] began with his review of the Lakatos conference volume [Hands 1979]. In this review Wade endorsed to a limited extent the introduction of Lakatos into economic methodology. Wade extended his uneasy endorsement of Lakatos in his review of Blaug's 1980 methodology book which claims to be promoting Popper's 'falsificationism' [Hands 1984a].[3] In the same year Wade engaged in more Popper bashing in his discussion of 'crucial counterexamples in the growth of economic knowledge' [Hands 1984b].

Despite his explicit recognition of the inequality between the views of Lakatos and Popper, Wade seemed intent on pursuing these Lakatosian themes.

In 1985 Wade gave some 'second thoughts on Lakatos' and a 'new look' at Popper and economic methodology [Hands 1985a, 1985b]. He asserted that Lakatos' methodology is 'ill-suited' to economics yet it 'can still provide valuable guidance' (i.e. second thoughts merely mean a more limited endorsement). And three years later, Wade again exercised his membership in the Popper-bashing club by focusing on 'ad hocness in economics' [Hands 1988]. Throughout these various papers about Popper's methodology, Popper was thrashed for crimes which are due more to Lakatos' caricature than to Popper himself. Until recently, it seems that Wade could not entirely give up the Lakatosian viewpoint.

Given my understanding of Popper, namely his emphasis on a critical attitude, I still have difficulty comprehending Wade's criticisms of Popper. In his most recent book [Hands 1993], he now wishes to identify Popper exclusively with what Wade calls critical rationalism. Wade still seems to think there may be something wrong with Popper's view of methodology. In the last chapter of his 1993 book, about 'Saving the Popperian tradition', he says:

> According to *critical rationalism* (hereafter, CR), the (insoluble) problem of justification is replaced by the (far more tractable) problem of criticism; rationality is saved from relativism by *hinging rationality on criticism rather than justification*. ...
>
> I have argued ... that CR represents the Popperian tradition's best bet ... [T]here is a ... general question about the current standing of the Popperian tradition.... *The problem is that it is simply not clear that the terms* Popperian tradition *or* Popperian program *capture anything particularly cohesive anymore*. ... CR is the heir apparent to the Popperian tradition.... The Popperian tradition will continue to have an important role to play in our philosophical discourse about economics. The days where almost everyone preached falsificationism are gone, as are the great novel-fact hunts of the 1980s; but the Popperian tradition will continue to have a role in economic methodology, nonetheless. [pp. 161, 185, 187–8]

I cannot understand why he is so cautious when dealing with Popper's view of science and methodology other than that he is unwilling to give up completely on Lakatos. It would seem that he has still not abandoned Lakatos since the notion of a 'Popperian program' was the invention of Lakatos.

Bruce Caldwell the eager Popperian

I have had less difficulty with Bruce Caldwell's perspective. His is that of a participant observer. That is, Bruce's primary interest has been the history of economic methodology rather than methodology itself. Today, it would seem, if he is to endorse a view of methodology, it would be what he considers to be a combination of Popper's critical rationalism (science is an enterprise devoted to putting forth rational arguments and then criticizing them) and Popper's situational logic (one's decisions depend on one's aims and on the constraints to achieving those aims). Whether this will satisify disciples of Popper remains to be seen (I will discuss this further in Chapter 20). Nevertheless, Bruce has come a long way from his conventionalist beginnings.

As I discussed above, it is clear to me that Bruce, as the young Caldwell, began with a limited endorsement of the conventionalist theory-choice-oriented view of methodology [Tarascio and Caldwell 1979]. Despite his cautiously expressed opinion, I think the middle-aged Bruce has still not progressed far enough beyond the young Caldwell's version of conventionalism (the one which he called 'pluralism'), yet he has made more progress than any other methodologist of his generation.

Bruce is virtually the only methodologist of his generation who has progressed to the point of completely confronting the limitations of the Lakatosian version of Popper. Still, his view of Popper can be most perplexing. Apart from making public his membership in the anti-Popper club, the young Caldwell's 1979 PhD thesis (and its reincarnation as Caldwell 1982, 1994) still is a good general history of economic methodology. This membership is exercised in his comment on Wade's review of Blaug [Caldwell 1984], where he seems to be criticizing Popper by seeing 'falsificationism' as an alternative solution to the conventionalist's theory-choice problem.[4] At a 1989 conference which Mäki held in Helsinki, Bruce presented his overall views of economic methodology [Caldwell 1990]. The Big Question was 'Does methodology matter?' and the focus of the conference (apparently due to Mäki) was 'realism'. The newly middle-aged Bruce seized the opportunity to promote his new version of conventionalism (viz 'critical pluralism'). He also took the opportunity to engage in a little more club-oriented criticism of Popper such as 'Realism is not like Popperian thought: it is not easily accessible, it does not provide simple formulas for demarcation, it does not quickly translate into a set of methodological rules' [1990, p. 68]. While he is obviously attacking the mainstream methodologists' view of Popper, I still want to ask, did 'Popperian thought' ever do this?

As Bruce's view of methodology has been evolving from orthodox

conventionalism, his most perplexing contribution is a 1991 *Journal of Economic Literature* article which purports to be 'Clarifying Popper' [Caldwell 1991b]. Responding to an early draft which he sent me, I asked him a few obvious questions, not the least of which was why he thought *Popper* needed clarifying. This may have been unfair since he may merely be clarifying the mistaken image of Popper that is due to the overly zealous followers of Lakatos. But I am not sure. For example, Bruce explicitly recognizes that Popper did not use the word 'falsification*ism*' [p. 2, fn. 1], yet he insists on using it to characterize Popper. And then he claims that Popper suffers from a contradiction between 'falsificationism' and 'situational analysis', which are seen as two essential aspects of a Popperian 'position'. Bruce appears to be saying that this is Popper's problem. In effect, Bruce seems to see this apparent contradiction as a methodology-choice problem: Popper must choose between 'falsificationism' and 'situational analysis'. Bruce accuses Blaug of giving up situational analysis in order to retain Popperian falsificationism [p. 21]. Surprisingly, Bruce's claimed solution is to have us recognize that both 'falsificationism' and 'situational analysis' are merely two elements in the more general methodology which he calls 'critical rationalism' [pp. 22–7].[5] And about this he says: 'I call this my own solution simply because Popper has never acknowledged that a tension exists between falsificationism and situational logic, and has never portrayed critical rationalism as providing a resolution of the conflict' [p. 22]. Remember, Bruce says this while explicitly recognizing Popper's rejection of the term 'falsificationism'. And thus he seems to be clarifying Popper by claiming to solve a problem that I do not think was ever a problem for Popper. I am confused about Bruce's clarification.

Clarifying clarifications of Popper

As a long-time follower of Popper, to me these attempts to clarify Popper seem very self-serving. Popper never offered a set of formula prescriptions for methodology. The concern for 'falsification' is a consequence of two separate endeavors. First, according to Popper, by advocating the view that scientific theories must be verifiable and thus verifiability is the basic criterion of scientificality, the 1930s logical positivists were wrong for simple reasons of quantificational logic. To explanatory, every theory must include at least one strictly universal statement and thus one could never verify all of a theory's assumptions. Instead, Popper said, *if* one is to have a criterion, then as a matter of logic falsifiability would be more appropriate. Second, his broader concern is to see science as dialectical process in the Socratic sense of dialogue. The center of this process is criticism, and obviously it would be pointless to try to criticize unfalsifiable theories.

Hence, a *minimum* condition for criticizability is falsifiability.

With this in mind, I cannot understand why both Wade and Bruce see a major contradiction between 'falsificationism' and 'situational analysis'. They both appear to be saying that this is Popper's problem. They are wrong. It was never Popper's problem. Again, as readers familiar with Popper's writings can see, this is at best a problem of accepting the Lakatosian view of Popper as true. Wade still seems to see this as an unavoidable fault of Popper [Hands 1993].

I will return to these incomprehensible criticisms of Popper in Part V. It is interesting to note that, with the evolution of Wade's and Bruce's views of Popper, somehow my views are no longer idiosyncratic but apparently now mainstream. To illustrate, Wade now says:

> The critical rationalist view of Popper was introduced into economic methodology by Klappholz and Agassi (1959) and it has been the underlying theme in almost all of Larry Boland's methodological writings; in the last few years critical rationalism has been endorsed by Bruce Caldwell (1991b, 1994) and, in a more guarded way, by me (Hands, 1993). [Hands 1996]

In the next part of this book, I will return to methodology as I think it should be practiced – in a true Popperian manner, rather than the myopic manner typical of mainstream methodologists. That is, methodology should be concerned with problems that economic theorists have to deal with every day and not with what choice criterion we should endorse in economic methodology.

NOTES

1 A transcript of this event was published as Caldwell [1987]. Its partial use here is with the permission of the JAI Press, the publisher of *Research in the History of Economic Thought and Methodology*.

2 I encountered Wade's first work on methodology when I refereed his review article about Stan Wong's 1978 book. Despite my recommendation to the editor, the journal rejected his article on the grounds that they did not publish unsolicited book reviews.

3 Since one of Wade's teachers was the Popperian Noretta Koertge, I am sure he recognizes that one must not confuse Lakatos with Popper.

4 In an e-mail message commenting on an early draft of this chapter, Bruce says he thinks that his references to 'falsificationism' were always directed at the views of Blaug and Hutchison.

5 His definition is not the one given by Wade. On page 25, Bruce says:

> Critical rationalism is a problem-solving approach which ... states that sometimes it is appropriate to evaluate a theory using the strict empirical criteria of falsificationism. But at other times, especially within the social sciences, one is better able to criticize a theory by applying the canons of situational logic ...

However, unlike Wade, Bruce recognizes that the driving force of Popper's view is the emphasis on a 'critical approach' such that the 'goal is to subject all theories to the optimal amount of criticism' [ibid.].

Part IV

Criticizing the methods of economic analysis