CAMBRIDGE SURVEYS OF ECONOMIC LITERATURE

THE METHODOLOGY OF ECONOMICS

This book is an examination of the nature of economic explanation. The opening chapters introduce current thinking in the philosophy of science and review the literature on methodology. Professor Blaug then turns to the troublesome question of the logical status of welfare economics, giving the reader an understanding of the outstanding issues in the methodology of economics. This is followed by a series of case studies of leading economic controversies, whose purpose is not to settle substantive questions on which economists disagree, but rather to show how controversies in economics may be illuminated by paying attention to questions of methodology. A final chapter draws the strands together and gives the author's view of what is wrong with modern economics.

This is a revised and updated edition of a classic work on the methodology of economics, in which Professor Blaug develops his discussion of the latest developments in macroeconomics, general equilibrium theory, and international trade theory. A new section on the rationality postulate is also added.

CAMBRIDGE SURVEYS OF ECONOMIC LITERATURE

The literature of economics is expanding rapidly, and many subjects have changed out of recognition within the space of a few years. Perceiving the state of knowledge in fast-developing subjects is difficult for students and time-consuming for professional economists. This series is intended to help with this problem. Each book will be quite brief, giving a clear structure to and balanced overview of the topic, and written at a level intelligible to the senior undergraduate. The books will therefore be useful for teaching but will also provide a mature yet compact presentation of the subject for economists wishing to update their knowledge outside their own specializations.

Other books in the series
E. Roy Weintraub: Microfoundations: The compatibility of microeconomics and macroeconomics
Dennis C. Mueller: Public choice
Robert Clark and Joseph Spengler: The economics of individual and population aging
Edwin Burmeister: Capital theory and dynamics
Mark Blaug: The methodology of economics or how economists explain
Robert Ferber and Werner Z. Hirsch: Social experimentation and economic policy
Anthony C. Fisher: Resource and environmental economics
Morton I. Kamien and Nancy L. Schwartz: Market structure and innovation
Richard E. Caves: Multinational enterprise and economic analysis
Anne O. Krueger: Exchange-rate determination
James W. Friedman: Oligopoly theory
Mark R. Killingsworth: Labor supply
Helmut Frisch: Theories of inflation
Steven M. Sheffrin: Rational expectations
Sanford V. Berg and John Tschirhart: Natural monopoly regulation
Thrainn Eggertsson: Economic behavior and institutions
Jack Hirshleifer and John G. Riley: The analytics of uncertainty and information
John B. Shoven and John Whalley: Applying general equilibrium

The methodology of economics

OR HOW ECONOMISTS EXPLAIN

Second Edition

MARK BLAUG

Professor Emeritus
University of London

Consultant Professor
University of Buckingham

Visiting Professor
University of Exeter

CAMBRIDGE
UNIVERSITY PRESS

PUBLISHED BY THE PRESS SYNDICATE OF THE UNIVERSITY OF CAMBRIDGE
The Pitt Building, Trumpington Street, Cambridge CB2 1RP, United Kingdom

CAMBRIDGE UNIVERSITY PRESS
The Edinburgh Building, Cambridge CB2 2RU, United Kingdom
40 West 20th Street, New York, NY 10011-4211, USA
10 Stamford Road, Oakleigh, Melbourne 3166, Australia

First edition 1980
Reprinted 1981
Second edition 1992
Reprinted 1993, 1994, 1997

Printed in the United States of America

Typeset in Times

A catalogue record for this book is available from the British Library

Library of Congess Cataloguing-in-Publication Data is available

ISBN 0-521-43061-5 hardback
ISBN 0-521-43678-8 paperback

to my son, Tristan

In the choice of subject to-day [scope and method of economics], I fear that I have exposed myself to two serious charges: that of tedium and that of presumption. Speculations upon methodology are famous for platitude and prolixity. They offer the greatest opportunity for internecine strife; the claims of the contending factions are subject to no agreed check, and a victory, even if it could be established, is thought to yield no manifest benefit to the science itself. The barrenness of methodological conclusions is often a fitting complement to the weariness entailed by the process of reaching them.

Exposed as a bore, the methodologist cannot take refuge behind a cloak of modesty. On the contrary, he stands forward ready by his own claim to give advice to all and sundry, to criticise the works of others, which, whether valuable or not, at least attempts to be constructive; he sets himself up as the final interpreter of the past and dictator of future efforts.

– Roy F. Harrod, *Economic Journal,* 1938

CONTENTS

PREFACE

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.[1]

[1] In chronological order and confining oneself strictly to books: H. Katouzian, *Ideology and Method in Economics* (1980); W. J. Samuels (ed.), *The Methodology of Economics. Critical Papers from the Journal of Economic Issues* (1980); J. Pitt (ed.), *Philosophy in Economics* (1981); B. J. Caldwell, *Beyond Positivism. Economic Methodology in the Twentieth Century* (1982); *W. Stegmüller, W. Balzer, and W. Spohn (eds.), Philosophy of Economics* (1982); L. A. Boland, *The Foundations of Economic Method* (1982); A. S. Eichner (ed.), *Why Economics Is Not Yet A Science* (1983); A. W. Coats (ed.), *Methodological Controversy in Economics: Historical Essays in Honor of T. W. Hutchison* (1983); W. L. Marr and B. Raj (eds.), *How Economists Explain. A Reader in Methodology* (1983); R. B. McKenzie, *The Limits of Economic Science* (1983); B. J. Caldwell (ed.), *Appraisal and Criticism in Economics: A Book of Readings* (1984); D. M. Hausman (ed.), *The Philosophy of Economics: An Anthology* (1984); P. Willes and G. Routh (eds.), *Economics in Disarray* (1984); J. J. Klant, *The Rules of the Game: The Logical Structure of Economic Theories* (1984); E. R. Weintraub, *General Equilibrium Analysis* (1985); A. Mingat, P. Salmon, and A. Wolfelsperger, *Méthodologie économique* (1985); D. McCloskey, *The Rhetoric of Economics* (1985); S. C. Dow, *Macroeconomic Thought. A Methodological Approach* (1985); T. Lawson and H. Pesarian (eds.), *Keynes' Economics: Methodological Issues* (1985); L. A. Boland, *Methodology for a New Microeconomics* (1986); P. J. O'Sullivan, *Economic Methodology and Freedom to Choose* (1987); J. Pheby, *Methodology and Economics: A Critical Introduction* (1988); N. de Marchi (ed.), *The Popperian Legacy in Economics* (1988); L. A. Boland, *The Methodology of Economic Model Building: Methodology After Samuelson* (1989); S. Roy, *Philosophy of Economics* (1989); D. A. Redman (ed.), *Economic Methodology. A Bibliography* (1989); J. C. Glass and W. Johnson, *Economics. Progression, Stagnation or Degeneration?* (1989); D. A. Redman, *Economics and the Philosophy of Science* (1991); S. Gordon, *The History and Philosophy of Social Science* (1991).

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, no doubt because it was badly expressed, tempting me to restate my argument. In addition, some of the case studies in the second half of the book were too flimsy and others needed updating. Finally, new developments in macroeconomics, general equilibrium theory, and international trade theory encouraged me to prepare a new edition.

At first, I had ambitions to double the length of the original book by new chapters on post-Keynesian economics, experimental economics, game theory, and the crisis in econometrics, resolving the clash between Bayesian and classical theories of inference. 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. I have amplified my discussion of general equilibrium theory, the Heckscher–Ohlin theory of international trade, monetarism, and the new classical macroeconomics, and have added a new section on the rationality postulate as the ''hard core'' of mainstream economics. In the main, however, the new edition is substantially the same book as the old. The ambitious additions I had hoped to insert I leave to another book.

Let me now try to restate the central message of the book by way of a comparison between my own account of the methodology of economics and that of Bruce Caldwell's *Beyond Positivism*.[2] Our two books are in striking agreement on many of the substantive issues in economic methodology: *methodology* is not just a fancy name for ''methods of investigation'' but a study of the relationship between theoretical concepts and warranted conclusions about the real world; in particular, methodology is that branch of economics where we examine the ways in which economists justify their theories and the reasons they offer for preferring one theory over another; methodology is both a descriptive discipline – ''this *is* what most economists do'' – and a prescriptive one – ''this is what economists *should* do to advance economics''; finally, methodology does not provide a mechanical algorithm either for constructing or for validating theories and as such is more like an art than a science. We also agree that economic theories must sooner or later be confronted with empirical evidence as the final arbiter of truth but that empirical testing is so difficult and ambiguous that one cannot hope to find many examples of economic theories being decisively knocked down by repeated refutations (but there are nevertheless striking examples of precisely that phenomenon, as we shall see). It is vain to seek an empirical counterpart for

[2] The pages that follow borrow heavily from my ''Comment'' in Wiles and Routh (1984, pp. 30–6).

every theoretical concept employed, which is in any case an impossible objective, but we can achieve indirect testing by considering the network of fundamental concepts embedded in a particular theory and deducing their implications for some real-world phenomena. This is not to say, however, that predictions are everything and that it hardly matters whether assumptions are "realistic" or not. Economic theories are not simply instruments for making accurate predictions about economic events but genuine attempts to uncover causal forces at work in the economic system.

However, this is where the agreement between us stops. I argue in favor of *falsificationism,* defined as a methodological standpoint that regards theories and hypotheses as scientific if and only if their predictions are at least in principle falsifiable, that is, if they forbid certain acts/states/events from occurring. My reasons for holding this view are partly epistemological – the only way we can know that a theory is true or rather not false is to commit ourselves to a prediction about acts/states/events that follow from this theory – and partly historical – scientific knowledge has progressed by refutations of existing theories and by the construction of new theories that resist refutation. In addition, I claim that modern economists do in fact subscribe to the methodology of falsificationism: despite some differences of opinion, particularly about the direct testing of fundamental assumptions, mainstream economists refuse to take any economic theory seriously if it does not venture to make definite predictions about economic events, and they ultimately judge economic theories in terms of their success in making accurate predictions. I also argue, however, that economists fail consistently to practice what they preach: their working philosophy of science is aptly characterized as "innocuous falsificationism." In other words, I am critical of what economists actually do as distinct from what they say they do.

Caldwell, on the other hand, doubts that falsificationism is a recommendable methodology: its structures are so demanding that little of economics would survive if it were rigorously applied. In addition, he can find few signs of economists practicing falsificationism even innocuously. Instead, he advocates "methodological pluralism," or "let a hundred flowers bloom," implying that various schools of thought in economics can be criticized from within, that is, in terms of the criteria they themselves avow. But if all methodological standards are equally legitimate it is difficult to see what sort of theorizing is ever excluded. From the ultrapermissive standpoint of "methodological pluralism," it is not even obvious why we should require theories to be logically consistent, or to assert something definite about the real world, which after all carries the implication that they may be shown to be false.

Caldwell is clearly sympathetic to the methodology of falsificationism but he derives many of his negative conclusions about falsificationism from a subtle distinction between the methodology of *confirmationism* and that of

falsificationism. He notes that most modern economists believe "that theories should be testable; that a useful means of testing is to compare the predictions of a theory with reality; that predictive adequacy is often the most important characteristic a theory can possess; and that the relative ordering of theories should be determined by the strength of confirmation, or corroboration, of those being compared" (Caldwell, 1982, p. 124). These four principles, he contends, define the methodology of confirmationism rather than falsificationism. Falsificationism is a tougher doctrine. In its simplest form, it can be stated in Caldwell's own words: "Scientists should not only empirically test their hypotheses, they should construct hypotheses which make bold predictions, and they should try to refute those hypotheses in their tests. Equally important, scientists should tentatively accept only confirmed hypotheses, and reject those which have been disconfirmed. Testing, then, should make a difference" (1982, p. 125).

Thus, the distinction between confirmationism and falsificationism rests partly on the degree to which theories are squeezed to yield risky implications liable to refutation and partly on whether refutations are taken seriously as possible reflections of fundamental error. Confirmationists make sure that their theories run few risks and, when faced with an empirical refutation, set about repairing the theory or amending its scope; they never abandon it as false. Falsificationists, on the other hand, deliberately run risks and regard repeated failures to predict accurately as a sign that alternative theories must be considered. Obviously, these distinctions are differences of degree, not of kind, and two methodologists may honestly disagree, as Caldwell and I do, as to whether modern economists are more appropriately characterized as "confirmationists" or "innocuous falsificationists."

There are good reasons why falsificationism is hard to practice in economics: any hypothesis is subject to other things being held constant and these other things are numerous and not always well specified; there are no well-attested, universal laws in economics and what general laws there are turn out to be statistical laws or tendencies lacking universal constants; to test a theory we must construct a model of the theory and, unfortunately, the same theory may be represented by a variety of models; and, finally, the data employed in any empirical test corresponds only crudely to the concepts in the theory being tested (Caldwell, 1982, pp. 238–42). However, exactly the same factors operate in physics, chemistry, and biology, albeit to a lesser degree. Indeed the so-called Duhem–Quine thesis states that it is logically impossible decisively to refute any theory, since any test of a theory involves the conjunction of initial conditions and the component elements of the theory, so that a refutation can always be blamed on inappropriate initial conditions. The way out of this dilemma is to lay down restrictions on what Popper calls "immunizing stratagems," adopted solely to protect theories against empirical refutations. These restrictions are important features of the methodology of falsification-

ism, which Caldwell along with so many other commentators on methodological issues simply ignores.

Let us agree that there are no tests in economics (or for that matter in any other science) that are unambiguously interpretable. But that is not to echo Caldwell that disconfirming tests are always ignored in economics or that they always lead to a repair job designed to make sure that there will be no further disconfirmations. The history of economics, and particularly modern economics, is replete with theories and hypotheses that were rejected because of repeated, if not decisive, empirical refutations. ''It is not easy to think of a proposition in economics,'' Frank Hahn once said, ''that all reasonable economists agree to have been decisively falsified by the evidence'' (1987, p. 110). But actually it is perfectly easy. Of course, it all depends on whom we include as ''reasonable economists'' and what is meant by ''decisively falsified.'' But here is an exemplary list: the wholesale rejection in the 1970s of the Phillips curve, interpreted as a stable trade-off between inflation and unemployment; the rejection in the 1980s of a stable velocity of money, scuttling the notion that inflation can be controlled merely by controlling the supply of money, even reducing it to zero in two to three years; the rejection again in the 1980s of the proposition that rational expectations make it impossible to alter real output or employment by monetary or fiscal policy; the rejection somewhere in the 1960s of Bowley's ''law'' proclaiming the constancy of the relative shares of national income going to capital and labor as well as everybody's ''law'' of the constancy of the capital–output ratio in the economy as a whole; the rejection in the 1950s of the Keynesian consumption function making current consumption a function solely of current income; the rejection in the 1930s of the Treasury view on the total crowding out of public expenditure in times of depression; the rejection again in the 1930s of the proposition that real wages fluctuate countercyclically – one could go on almost indefinitely expanding these examples. The notion that economic theories, like old soldiers, never die but only fade away is simply a myth perpetuated by constant repetition.

Thus, Caldwell has recently admitted that at least one of the many arguments he employed over the years as a persistent critic of falsificationism is ''if not wrong . . . seriously incomplete.''

> My error was to claim that falsification is an inappropriate methodology for economics *because* most economic theories cannot be conclusively falsified. To buttress the claim I noted numerous obstacles to getting clean tests of theories in economics. . . . [But] *every* science encounters difficulties in coming up with clean refutations. . . . Thus it is not an effective argument against falsificationism to simply point out . . . that decisive refutations are rare. That problem always exists [1991, p. 7].

The remedy for the problem is quite simple: try harder! Of course, the recommendations to try harder must be capable of being implemented, that is, a

prescriptive methodology like falsificationism must be descriptively adequate or at least not descriptively impractical. At this point in the argument, Caldwell again refers to his former conviction that falsificationism has never been practiced to any significant extent in economics: "neither Hutchison nor Blaug have been able to pinpoint paradigmatic episodes of falsificationist practice. Hutchison's examples (the refutations of Malthusian population theory and of certain unqualified versions of Keynesian and monetarist macroeconomics) involve instants in which usually, after fairly long periods of time, it became evident that a theory's predictions did not come to pass" (1991, p. 9). I hope that a reading of Chapter 12 below will convince any "reasonable economist" that the entire history of postwar macroeconomics furnishes a whole series of paradigmatic episodes of falsificationist practice, that is, instants in which it became evident rather quickly that "a theory's predictions did not come to pass."

Bruce Caldwell set the pattern for others' reactions to the first edition of this book. Daniel Hausman (1985; 1988; 1989) argued that falsificationism is never practiced because it is unpracticeable. Modern economists, he insisted, subscribe to what he calls "deductivism" or what I called "verificationism," whose patron saint is John Stuart Mill and not Karl Popper: "given how poorly supported are various auxiliary statements needed to derive economic theories, it is usually not sensible or responsible to follow Blaug's Popperian advice and to regard predictive failures as falsifying economic theories" (Hausman, 1989, p. 119).[3] After that, it is hardly surprising to be told that Hausman (1989, pp. 122–3) believes in descriptive, not prescriptive, methodology. Again and again, we shall find that falsificationism is an "aggressive methodology" which is critical of much of what passes as modern economics, whereas critics of falsificationism invariably adopt a "defensive methodology," arguing that the business of economic methodologists is to describe the actual practice of economists, which is, in brief, to make the best of a bad job. This is the old distinction between positive and normative economics in the realm of methodology: either we describe, explain, and endorse what economists actually do or we advocate best-practice economics on the supposition that many economists fall short of it.

Lawrence Boland does not go as far as Hausman: falsificationism is implementable but it is not actually implemented. "For Blaug, any practice of what he calls falsificationism amounts not only to devising models which are in principle refutable but also actively attempting to refute such models. With the exception of a brief moment in the LSE seminars, hardly any mainstream economists have advocated such a strict employment of the Popper–Samuel-

[3] Similarly Hargreaves-Heap (1989, chap. 2) uses the Duhem–Quine thesis to attack falsificationism, arguing that as empirical tests are always inconclusive, we have to settle for "understanding" in economics, according to which empirical evidence is relevant but not decisive for theory choice.

son methodological requirement of falsifiability'' (Boland, 1989, p. 10; also 1982, chaps. 10–11). Economists do worry about testability as a requirement of adequate economic models, Boland asserts, but they regard an empirical refutation as a challenge to improve the model so as to raise its ''degree'' of testability and not a reason to reject the theory underlying the model.

Klant (1984, pp. 184–6) and de Marchi (1988, pp. 12–13) likewise have deep misgivings about falsifiability in economics, regarding it as an ideal never attained in practice and at best only attainable to a degree; in short, they leave the door open to falsificationism as a normative methodology. Deborah Redman, on the other hand, has little use for such philosophers of science as Popper, Kuhn, and Lakatos, and regards the Popperian legacy in economics as almost wholly disastrous. Interpreting falsification as ''conclusive disproof,'' she has no difficulty in showing that it does not exist in science, from which it follows that ''defending a theory because it has not yet been 'falsified' . . . is in reality indefensible'' (Redman, 1991, p. viii). The logic of the argument is impeccable. Unfortunately, no one has ever defined falsification as equivalent to conclusive disproof and Popper spent pages and pages in *The Logic of Scientific Discovery* arguing against the thesis that one could ever conclusively disprove anything, pages which Redman (1991, pp. 32–5; also p. 124) actually quotes at length. Having established to her own satisfaction that economic theories cannot be falsified any more than physical theories in her sense of the term, she convicts me of self-contradiction: ''Blaug asserts that economists say their goal is falsification but they do not in fact practise falsification, and so economists are to be reprimanded for doing something that Blaug admits is impossible anyway'' (Redman, 1991, p. 119). Am I alone in thinking that this is a good example of winning an argument by inventing a straw man?

Finally, Bill Gerrard (1990, pp. 201–2), in a useful survey of recent books on economic methodology, winds up the argument by distinguishing between ''radical falsificationism'' and ''dogmatic falsificationism'':

> Radical falsificationism recognises the fallibility of knowledge, stresses the role of empirical testing as a safety valve protecting subject fields from falling prey to dogmatism and acknowledges the difficulties involved in empirical testing as a result of the conglomerate nature of theories. Dogmatic falsificationism, on the other hand, treats empirical testing as an infallible and purely objective means of arriving at certain knowledge.

The first is the methodology of Popper, Gerrard declares; the second is Popperian methodology. He concludes: ''There seems a clear case for more of the methodology of Popper in economics and the elimination of the bastardized version that is Popperian method.''

Among the books Gerrard reviews is Donald McCloskey's *Rhetoric of Economics* (1985), a witty and provocative book expressly designed to purge

economics of all prescriptive methodologies, such as falsificationism, verificationism, or what you will. Economists, argues McCloskey, pay obeisance to an outmoded philosophy of science, which he labels as "modernism," although it is usually labeled as "logical positivism." This matter of labels is not unimportant, for in no time at all he includes within modernism various propositions that have gained currency among economists but that have absolutely nothing to do with the philosophical movement known as logical positivism. Modernism, McCloskey tells us, is characterized by ten commandments, which include such notions as these: only the observable predictions of a theory matter to its truth; facts and values belong to different realms of discourse, so that positive propositions are always to be distinguished from normative ones; any scientific explanation of an event subsumes the event under a covering law; and introspection, metaphysical beliefs, aesthetic considerations, and the like may well figure in the discovery of any hypothesis but are irrelevant to its justification. Such notions, McCloskey points out, are now discarded by many professional philosophers – but economists have paid no attention to these reactions to "modernism" among philosophers and continue to believe that the only "fundamental" proof of an economic assertion is an objective, quantitative test. It is this naive belief in empirical testing as the hallmark of truth that is the real core of "modernism" and hence the Big Bad Wolf of McCloskey's book. "It is hard to disbelieve the dominance of modernism in economics," he remarks, "although an objective, quantitative test would of course make it, or any assertion, more believable and would be worth doing" (McCloskey, 1985, p. 11).

On the one hand, he deplores all hints of a prescriptive methodology; that is, no one is to lay down metatheoretical standards of what is to be considered a good or bad argument. On the other hand, "an objective, quantitative test would make it . . . more believable *and* would be worth doing" (my italics). Yes, it might make a proposition more believable if only because unphilosophical economists tend to take quantitative tests seriously. But why would it be worth doing if it has no bearing on the validity of the assertion? And if it has at least some bearing, why are we not told what bearing it has? McCloskey ridicules the reader who believes that there are some propositions in economics that are either true or false, in which case it is difficult to see why empirical testing should ever be worth doing.

If prescriptive methodology is out, what is left is descriptive methodology or what McCloskey prefers to call the study of "rhetoric" or "conversation." The word *rhetoric* has in recent years required a derogatory meaning, but at one time (roughly up to the nineteenth century) it meant simply the ways of producing an effect on one's audience by the careful use of language; it is the art of speaking or writing persuasively. McCloskey never gives a precise definition of the term *rhetoric* but the general idea of what he is after is, surely, plain enough. Moreover, he provides a number of worked examples of rhe-

torical analysis in the latter half of the book, based on the writings of Paul Samuelson, Robert Solow, John Muth, and Robert Fogel, which are instructive even if one does not share what McCloskey calls the "Anarchistic Theory of Knowledge in Economics."

The mystery is how McCloskey manages to examine the language used by economists without some criteria of "good" and "bad" language, some standards of what to look for in the linguistic devices employed by these economists to persuade their readers to believe them. The fact of the matter is, of course, that he does not manage it: these case studies of rhetorical analysis are filled with implicit metatheoretical judgments. As a case in point, McCloskey's penultimate chapter attacks one of the worst characteristics of modern economics, the confusion between statistical significance tests and tests of substantive effects. This chapter veritably bursts with advice on "good" statistical practice, all of which I personally applaud. But where does such advice come from except from metatheoretical norms, otherwise known as methodology, that is, the logic of the methods employed by the practitioners of a subject. McCloskey is unalterably opposed to Methodology with a capital *M* but much in favor of methodology with a lowercase *m*. What this seems to mean is that you can prescribe many little things – don't shout; be open-minded; face up to the facts; don't fall for your own rhetoric; don't pronounce on large or small effects without giving standards of comparison; don't confuse statistical with substantive significance; replace Neyman–Pearson inference with Bayesian methods; and so on – but you may not prescribe big things, such as eschewing conclusions that are compatible with any and all factual evidence; always comparing one's conclusions with those of competing theories, if such exist; and avoiding changes to one's theories that have no other aim but to account for an empirical anomaly. Unfortunately, I fail to see a rational basis for the implied distinction between deplorable Methodology and salutary methodology, and McCloskey never provides such a basis.[4]

One can imagine a rhetorical analysis of the writings of Milton Friedman on monetarism. Friedman uses some explicit and implicit literary devices that seem to account for his enormous persuasive power and, hence, his influence on modern economics. Having studied these devices, I will probably ask myself at some time whether it is actually true that control of the supply of money is the key to the control of inflation in modern industrial economies. Silly boy, I can hear McCloskey saying, there is no such thing as truth in economics: "Economics, like geology or evolutionary biology or history itself is a historical rather than a predictive science" (McCloskey, 1985, p. 18). But geology, evolutionary biology, and history are retroactive sciences, that is, the validity of these propositions do depend *ex post facto* on empirical data

[4] For other critiques of McCloskey making more or less the same point, see Caldwell and Coats (1984), Rosenberg (1988), Gerrard (1990, pp. 208–12), and Backhouse (1992).

(consider the importance of fossil evidence to the debates on Darwinian theory). Is the same true of economics? Does the validity of monetarism depend, if not on the accuracy of its future productions, on the accuracy of its past retrodictions? Friedman did after all co-author a book on the *Monetary History of the United States, 1867–1960*. Did he verify monetarism by means of historical data on the money supply and the level of prices? Is this an important question to ask? Silly boy, you're doing Methodology again: off with your head!

The idea of studying how economists actually go about persuading one another is a good one, but it is false to assert that all reasons for believing an economic theory are equally valid and that economists in fact regard them as equally valid. However, this is precisely what McCloskey is saying.

> Consider, for example, the sentence in economics, "The demand curve slopes down". The official rhetoric says that economists believe this because of statistical evidence – negative coefficients in demand curves for pig iron or negative diagonal items in matrices of complete systems of demand – accumulating steadily in journal articles. These are the tests 'consistent with the hypothesis'. Yet more beliefs in the hypothesis come from other sources: from introspection (what would I do?); from uncontrolled cases in point (such as the oil crisis); from authority (Alfred Marshall believed it); from symmetry (a law of demand if there is a law of supply); from definition (a higher price leaves less expenditure, including this one); and, above all, from analogy (if the demand curve slopes down for chewing gum, why not for housing and love too?). As may be seen in the classroom and seminar, the range of arguments in economics is wider than the official rhetoric allows [McCloskey, 1987, p. 174].

No doubt, there are many reasons for believing that demand curves are negatively inclined but there is little doubt that if the statistical evidence repeatedly ran the other way, none of these reasons would suffice to make economists believe in the "law of demand." Most "beliefs in the hypothesis" do *not* come from other sources, contrary to what McCloskey asserts, and, of course, they should not. Description and prescriptions agree perfectly with one another in this case as in so many others. And that is the gist of my argument.

I document in this book a striking continuity in the methodological precepts of modern economists, precepts that loosely correspond to Popper's falsificationist strictures. But at the same time, there is no denying that the practice of economists is at best an innocuous brand of falsificationism and at worst a Millian style of verificationism.[5]

[5] Canterberry and Burkhardt (1983), in an examination of 542 empirical articles in four major economics journals over the years 1973–8, found that only three articles attempted to falsify the hypotheses proposed; in all other cases the null hypothesis was accepted, demonstrating that economists confirm rather than falsify. But is this not

My fond belief that economists could be goaded into taking falsificationism more seriously has received some hard knocks over the last ten years. A number of general equilibrium and game theorists have in recent years expressed open hostility to falsificationism (see Chapter 8 below) and a conference on the application of Lakatos's philosophy of science to economics held in 1989 revealed a studious skepticism among the participants about the utility of Popperian and Lakatosian ideas in a subject like economics and, particularly, a disinclination to appraise economic theories in terms of their novel empirical content (de Marchi, 1991, pp. 504–6, 509). It was clear that many economists cannot abandon the notion that mere theoretical progress, a deeper understanding of some economic problems, is of value in itself even if it does not produce any substantive findings about the economy and even if it does not enhance our ability to predict the consequences of economic policies. In so doing, they reflect an increasing tendency in modern economics to pursue theorizing like an intellectual game, making no pretense to refer to this as any other possible world on the slim chance that something might be learned which will one day throw light on an actual economy.

In a letter to *Science*, Wassily Leontief (1982) surveyed articles published in the *American Economic Review* in the last decade and found that more than 50 percent consisted of mathematical models without any empirical data, while some 15 percent consisted of nonmathematical theoretical analysis, likewise without any empirical data, leaving 35 percent of the articles using empirical analysis.

Articles published in the AER

	1972–6 (%)	1977–81 (%)
1. Mathematical models without any data	50.1	54.0
2. Theoretical models without mathematical formulation and without data	21.2	11.6
3. Statistical methodology	0.6	0.5
4. Empirical analysis based on data developed by the author	0.8	1.4
5. Empirical analysis using statistical inference on published data	21.4	22.7
6. Other types of empirical analysis	5.4	7.9
7. Empirical analysis based on artificial simulation and experiment	0.5	1.9

Source: Leontief (1982)

what some of the critics of falsificationism, like Hausman, say? Yes, but they welcome it or else regard it as inevitable, while I deplore it and argue that it is corrigible.

Morgan (1988) has updated Leontief's findings, showing once again that half the articles published in the *American Economic Review* and the *Economic Journal* do not use data of any kind, a ratio that vastly exceeds that found in articles in physics and chemistry journals. Oswald (1991a) has confirmed Leontief's and Morgan's results in the area of microeconomics, concluding quite rightly that a large number of economists treat the subject as it if were "a kind of mathematical philosophy." Perhaps a better expression would be "social mathematics," that is, a brand of mathematics that appears to deal with social problems but does so only in a formal sense. What we have here is a species of formalism: the reveling in technique for technique's sake. Colander and Klamer (1987; 1988) have shown that students in American graduate schools perceive that analytical ability is the chief requirement for professional advancement and not knowledge of the economy or acquaintance with the economic literature. Students are usually shrewd observers of their own chosen profession and they have a sensitive nose for the "hidden agenda" in their curriculum. It is clear that American graduate students have correctly perceived that nothing succeeds in economics like mathematical pyrotechnics, supplemented on occasions by some fancy econometrics.

The fact that graduate education in economics emphasizes technical puzzle-solving abilities at the expense of imparting substantial knowledge of the economic system is simply a reflection of the empty formalism that has come increasingly to characterize the whole of modern economics. And why not? What after all is wrong with elegant economics practiced as an intellectual pastime? There are, I suppose, two answers to this question. One is that some of us suffer from "idle curiosity" about the economy. Much as we enjoy abstract, mathematically formulated economics, we cannot help wondering just how the economy actually works, and most of the lemmas of rigorous pure theory do not really satisfy the desire to understand how things hang together in the economic world. The second answer is that economics throughout its long history has been intimately connected with economic policy, with the desire to improve economic affairs, eradicate poverty, equalize the distribution of income and wealth, combat depressions, and so on, and never more so than in the recent postwar period. But if economists are going to take a stand on questions of economic policy, not to mention advising governments what to do, they must have knowledge of how the economic system functions: we *know* that privatization if accompanied by an increase in the numbers of producers improves the quantity and quality of the goods privatized; we *know* that a deficit on the balance of payments can be cured by devaluation and even how quickly it can be cured; we *know* that inflation can be reduced by a hard fiscal and monetary policy and even what it will take to cut inflation by a given percentage – or do we? All this is to say that economics must be first

and foremost an empirical science or else it must abandon its age-old concern with "piecemeal social engineering."[6]

Granted that economists must ultimately judge their ideas by the test of empirical evidence – that analytical rigor may have to be traded off against practical relevance – it does not follow that they need to endorse the Methodology of falsificationism. The argument for an empirically minded economics might derive from methodological considerations with a lowercase *m*.[7] No doubt, but the fact remains that any metatheoretical recommendation is no better than the Methodology which underpins it. McCloskey notwithstanding, there is no logical or philosophical distinction between methodology and Methodology. And the Methodology which best supports the economist's striving for substantive knowledge of economic relationships is the philosophy of science associated with the names of Karl Popper and Imre Lakatos. To fully attain the ideal of falsifiability is, I still believe, the prime desideratum in economics.

[6] This argument has been most forcefully expressed by Hutchison (1988, pp. 172–3; 1992).

[7] Mayer (1992) argues this at some length in a new book whose title is its message: *Truth vs. Precision in Economics*.

PREFACE TO FIRST EDITION

A fatal ambiguity surrounds the expression "the methodology of . . ." The term *methodology* is sometimes taken to mean the technical procedures of a discipline, being simply a more impressive-sounding synonym for *methods*. More frequently, however, it denotes an investigation of the concepts, theories, and basic principles of reasoning of a subject, and it is with this wider sense of the term that we are concerned in this book. To avoid misunderstanding, I have added the subtitle, *How Economists Explain,* suggesting that "the methodology of economics" is to be understood simply as philosophy of science applied to economics.

To ask how economists explain the phenomena with which they are concerned is in fact to ask in what sense economics is a science. In the words of one prominent modern philosopher of science: "It is the desire for explanations that are at once systematic and controlled by factual evidence that generates science; and it is the organization and classification of knowledge on the basis of explanatory principles that is the distinctive goal of the sciences" (Nagel, 1961, p. 4). There can be no doubt that economics provides plenty of examples of "explanations that are at once systematic and controlled by factual evidence," and hence no time will be wasted defending the assertion that economics is a science. However, economics is also a peculiar science, set apart from, say, physics because it studies human actions and therefore invokes the reasons and motives of human agents as the "causes of things" and from, say, sociology and political science because it manages somehow to provide rigorous, deductive theories of human action that are almost wholly lacking in these other behavioral sciences. In short, the explanations of economists are a particular species of a larger genus of scientific explanations, and as such they present some problematic features.

What then is the nature of economic explanations? Insofar as these explanations consist of definite theories, what is the structure of these theories, and

in particular, what is the relationship between the assumptions and the predictive implications of economic theories? If economists validate their theories by invoking factual evidence, is that evidence pertinent only to the predictive implications of these theories, or to their assumptions, or both? Besides, what is it that counts as factual evidence for economists? How is it that economic theories that purport to explain what *is* are also employed in almost identical form to demonstrate what *ought* to be? In other words, what exactly is the relationship between positive and normative economics or, in more old-fashioned language, the relationship between economics as a science and political economy as an art? These are the sort of questions that will preoccupy us in this book.

Economists have been worrying about these questions ever since the days of Nassau William Senior and John Stuart Mill, and much is to be learned by going back to these nineteenth-century writers to see what economists themselves have rightly or wrongly thought they were doing when they practiced economics. By 1891, John Neville Keynes managed to sum up the methodological thinking of a whole generation of economists in his deservedly famous *Scope and Method of Political Economy,* which may be regarded as a sort of of benchmark in the history of economic methodology. The twentieth century witnessed a similar summing-up in *The Nature and Significance of Economic Science* (1932) by Lionel Robbins, followed a few years later by a widely read book with a diametrically opposite thesis, *The Significance and Basic Postulates of Economic Theory* (1938) by Terence Hutchison. In more recent years, Milton Friedman, Paul Samuelson, Fritz Machlup, and Ludwig von Mises have all contributed important pronouncements on the methodology of economics. In short, economists have long been aware of the need to defend ''correct'' principles of reasoning in their subject, and although actual practice may bear little relationship to what is preached, the preaching is worth considering on its own ground. That is the task of Part II. Part I is a self-contained, brief introduction to current thinking in the philosophy of science, which develops several distinctions that will be used throughout the rest of the book (see Glossary at the back).

After surveying the literature on economic methodology in Part II, Chapters 3 and 4, we turn in Chapter 5 to the troublesome question of the logical status of welfare economics. At the end of that chapter, having gained a more or less complete view of the outstanding issues in the methodology of economics, we are ready to apply the conclusions we have reached to some leading economic controversies. Part III therefore provides a series of case studies, whose purpose is not to settle substantive questions on which economists now disagree among themselves but rather to show how every controversy in economics involves questions of economic methodology. The last chapter in Part IV draws the strands together in an attempt to reach some final conclusions; it is perhaps more personal than the rest of the book.

Too many writers on economic methodology have seen their role as simply rationalizing the traditional modes of argument of economists, and perhaps this is why the average modern economist has little use for methodological inquiries. To be perfectly frank, economic methodology has little place in the training of modern economists. Possibly, all this is now changing. After many years of complacency about the scientific status of their subject, more and more economists are beginning to ask themselves deeper questions about what they are doing. At any rate, there are growing numbers who suspect that all is not well in the house that economics has built. It is not my purpose to coach them to be better economists but, on the other hand, there is little point in merely describing what economists do without drawing some object lessons; at some stage, even the most impartial spectator must be willing to assume the role of umpire. Like many other modern economists, I too have a view of *What's Wrong With Economics?* to cite the title of a book by Benjamin Ward, but my quarrel is less with the actual content of modern economics than with the way economists go about validating their theories. I hold that there is nothing much wrong with standard economic methodology as laid down in the first chapter of almost every textbook in economic theory; what is wrong is that economists do not practice what they preach.

When Laertes tells Ophelia not to yield to Hamlet's advances, she replies: "Do not as some ungracious pastors do, / Show me the steep and thorny way to heaven, / Whiles like a puff'd and feckless libertine / Himself, the primrose path of dalliance treads." Twentieth-century economists, I believe, are much like those "ungracious pastors." I leave it to my readers to decide whether I have made my case in this book, but at any rate, the wish to make that case has been the principal motive for writing it.

The book is essentially addressed to undergraduate students of economics, that is, those who have learned some substantive economics but find it difficult, if not impossible, to choose between alternative economic theories. Such is the growing interest of professional economists in methodological problems that, I dare say, even some of my colleagues will find the book of interest. Other students of social science – sociologists, anthropologists, political scientists, and historians – are inclined either to envy economists for their apparent scientific rigor or else to despise them for being the lackeys of governments. It may be that they will find this book not so much an antidote to envy as a reminder of the benefits that economics derives and always has derived from its policy orientation.

This book has been too long in the making. The first chapter was drafted at the Villa Serbelloni in Bellagio, Italy, where I spent the month of November, 1976, thanks to the generosity of the Rockefeller Foundation. After I left the idyllic atmosphere of the Bellagio Study and Conference Centre, teaching and other research commitments kept me from getting back to the

manuscript during the whole of the academic year 1976–7. Even then, it took me all of the calendar year 1978 to finish it. I received valuable comments, too numerous for comfort, on my first draft from Kurt Klappholz and Thanos Skouras. In addition, Ruth Towse read the entire manuscript, removing most, if not all, of my lapses from correct grammar. For this thankless task, I owe her a debt of gratitude that can only be paid in like coin.