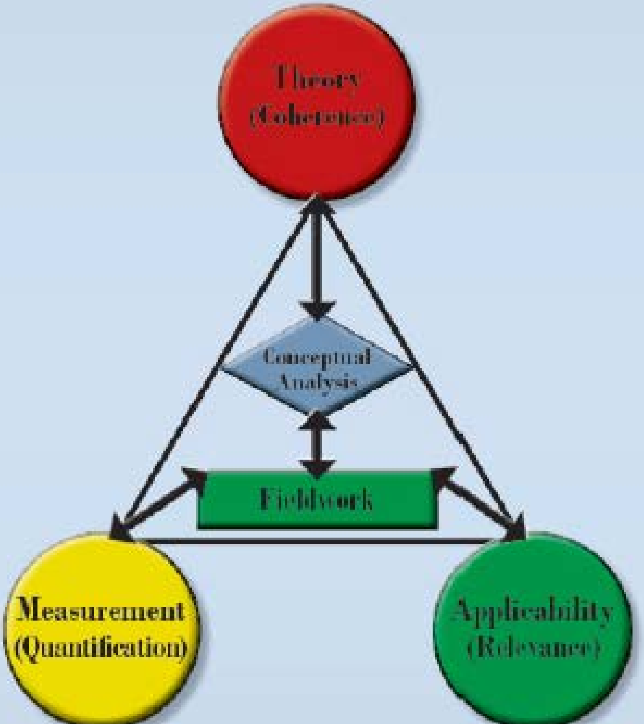
RATIONAL ECONOMETRIC MAN

Transforming Structural Econometrics



EDWARD J. NELL AND KARIM ERROUAKI

With a Foreword by

LAWRENCE R. KLEIN

Rational Econometric Man

Transforming Structural Econometrics

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With a Foreword by Lawrence R. Klein, Nobel Laureate in Economics

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Contents

Foreword

The New School For Social Research has played an important role, contributing to many fields of advanced study in the United States – for example, a recent conference on the work of Franco Modigliani in relation to the prevailing economic situation. This book starts from much earlier work at the New School, the early 1940s Seminar of Marschak and Haavelmo on econometrics, which laid the foundations for the work at Cowles.

Nell and Errouaki have written a very welcome book, coming at a good time. Its message is one of support for the original Cowles approach, agreeing that our work then was on the right track. They correctly understand the intention of the founders, which was to bring mathematics together with economic theory, so as to develop precise hypotheses that could be confronted with data, using methods of statistical inference. The idea was to expand and develop economic theory, making it more realistic, so that it could be put to use to solve some of the world's problems. We felt we had all the answers from a statistical point of view and from the point of view of econometric methodology and economic content; we could make it easy to have a well-organized, well-run economy after the war. It was generally expected that econometric investigations would build up a large body of agreed-upon findings, and that different investigators would normally replicate – or improve on – each other's results.

In fact agreement has been hard to come by; what Jacob Marschak very early on called the 'model selection' problem has stood in the way. Statistical inference alone will not do the job; but it is not necessarily a step forward to try to solve the difficulties by introducing hard-to-justify assumptions – normality in probability distributions, ergodicity in time series. What is needed is greater realism, closer and more systematic attention to what economic agents are actually thinking, planning and doing. I have advocated the use of survey data; the authors here call for fieldwork and drawing on vernacular knowledge.

When the big models, along with every other form of economic investigation, ran into trouble in the 1970s, many investigators turned against the approach. Nell and Errouaki rightly deplore this; structural econometrics got a lot of things right, and presented a reasonable picture of the macroeconomy. People have said that the models failed to predict the effects of supply shocks on the inflation of the 1970s, and that they didn't predict the changes in structure. I believe the economy didn't change in structure; instead exogenous inputs changed a great deal within a largely unchanged structure. And the large-scale models did a good job of predicting recession and inflation.

This book is massive; it covers a great deal of ground, starting from philosophy of science, extending to methodology, and foundations of probability and statistical inference. It then goes on to the basics of structural econometrics and the Cowles approach, especially Keynesian econometric models, and finally covering the critiques of the Cowles approach and Keynesian econometrics, including the critiques of those critiques. The book also presents a number of the authors' own contributions. These include their proposal to overcome the problem of induction and establish the existence of lawlike regularities in economics, justifying the assumption of a 'data generating mechanism'; this leads them to their methodological triangle-circle (MTC) diagram, which summarizes their methodology. In addition to methodology they propose some specific modelling – for example, in regard to wage-price spirals, the analysis of money supply and demand, Keynesian uncertainty, and Minskyian financial instability. These ideas may seem unorthodox in today's context; but they would not have seemed out of place to many of the early econometricians, for example at the Oxford Institute of Statistics. In developing econometric models some people became slaves of the neo-Classical behavioural formulations; in their fear of being 'ad hoc' they chose theoretical lines which were not always well conceived. Many have forgotten, if they ever knew, the lessons of Keynes. Our authors propose to correct this, drawing on their program of fieldwork and conceptual analysis, and suggest some concrete steps along the path to reconceptualizing difficult and controversial areas of macro theory. The authors have succeeded in orchestrating a lively debate over the scientific foundations of structural econometrics. Their book deserves a broad readership.

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Acknowledgments

HOW IT ALL BEGAN IN NELL'S OFFICE AT THE NEW SCHOOL

A little over 25 years ago a graduate student, who had previously studied mathematics, econometrics and philosophy of science in Paris and had worked at INSEE, turned up in my office. He had just finished his studies with us, with perfect grades, and he wanted to work on a doctoral thesis. He had a straightforward project in mind: to rewrite Hollis and Nell's *Rational Economic Man* (1975). It was a great book, he said, but focused on the wrong target. It was not so much *economic theory* that was distorted and undermined by the assumption of rational economic agents as it was *econometrics*. That was where the real problem lay. The arguments should be adapted and redirected before econometrics got lost any further in a morass of misspecifications and unrealistic assumptions. Quite a project! But Karim was persistent and I decided he was right. So we began work, and now *Rational Economic Man* (it was always 'man'; feminist economics has generally been free of 'rational' fundamentalism) has become *Rational Econometric Man*. It took a long time, but we hope it's worth it!

INFLUENCES AND PATHS THAT LED TO THIS BOOK

Professor Edward Nell was the principal PhD thesis advisor and long-standing mentor of Karim Errouaki, along with his two supervisors and mentors, the late Nobel Laureate Wassily Leontief and the late Professor Camilo Dagum at the New School (NY) in the late 1980s. Errouaki owes a great debt to his learned professor and humane friend Professor Nell for his constant help, critical guidance and generous encouragement; and is greatly indebted to all his mentors during the writing of his doctoral thesis and he thanks them for sharing some of their reflections on the scientific standing of econometrics. They were always open to his questions and ideas about econometric methodology, were a constant stimulus for his own thinking in economics, and gave him confidence that he was working along the right lines, pointing out the paths to follow and guiding him along them. Errouaki's doctoral thesis extended and developed the position revealed by Hollis and Nell's *Rational Economic Man* (1975) and, inspired by a novel re-reading of Haavelmo's *Manifesto* (1944), refocused on econometrics. Errouaki's doctoral thesis concluded that what is required is a unified scientific methodology for economics in general, in which econometrics would not be separate, but would play a role coordinated with the rest. Many of the most important recent writings on econometrics do not have the right balance between the three pillars of econometrics to be explained in this book (theory or conceptual coherence, applicability or relevance, and measurement or quantification). Errouaki would like to point out that the task of co-writing this book, *Rational Econometric Man*, was made considerably easier since the publication of Nell's (1998a) magnum opus, *General Theory of Transformational Growth*. In Errouaki's view, Nell provided the blueprints for the rethinking of the foundations of macroeconomic model building and in doing so has paved the way for transforming structural econometrics.

APOLOGIES AND THANKS TO ALL

In a work that has taken as long as this to mature, giving adequate acknowledgment to all who have influenced it is bound to be a problem. Our apologies at the outset to our friends and associates: we have borrowed freely from all of you. We only hope the use we have made of your ideas is constructive.

ESPECIALLY TO OUR FAMILIES

It goes without saying that the work on such a project could not have been sustained without the love and moral support of friends and family during this long and difficult task. First, and most profound, there is our debt to our families. Without their love, support and encouragement, this book simply would never have been finished. For Edward it is to Marilyn Adams that he owes not only a debt for help and support, but gratitude for making life a joy once more. She has been patient beyond belief, and supportive beyond compare. Karim's greatest debt is to his parents, the late Abdeslam Errouaki and Fatima Soussi, his brother Mustapha Errouaki, and Jihane Slaoui Andaloussi, for their love, trust and generosity. We both, but especially Karim, owe thanks to Professor Federico Mayor Zaragoza, former Director General of UNESCO and President of the

Foundation for the Culture of Peace. In the same vein, we owe a special debt to Professor George Matthews, Chairman Emeritus of the Governing Boards of Northeastern University, for support and encouragement, particularly in bringing us together with Nobel Laureate Professor Lawrence Klein (University of Pennsylvania) and other scholars. We cannot thank our friends and family enough.

AND TO OUR COLLEAGUES AND ASSOCIATES

Our book benefited a lot from discussions and exchanges with friends and colleagues who suggested points and challenged our approach, methods and ideas. These discussions over the years helped us to develop our position. Many of them commented on earlier versions of various parts of this work.

To begin with, we would like to express special thanks and gratitude to Nobel Laureate Professor Lawrence Klein, as one of the last remaining originators of the Cowles approach, for accepting our invitation to write the Foreword to this book. His help and guidance gave us confidence that we were working along the right lines, particularly when it came to questions related to methodology. His comments and arguments over the last 50 years have forced us to think more rigorously about the foundations of structural econometrics. We hope that our book can be considered a worthy tribute to his outstanding contribution to the science of econometrics.

AMONG THEM, DECEASED COLLEAGUES AND FRIENDS

As will be clear from the title, this book reflects on and advances the ideas of Martin Hollis (University of East Anglia), as expressed in Hollis and Nell; this should be apparent in the discussions of Haavelmo. But the argument on Induction in Chapters 3, 4 and 5 builds on material originally developed by Nell for use in Hollis and Nell, but set aside then as unfinished; it has now been completed. Hollis's later work turned to other aspects of rationality and reason, including the role of trust. But we hope that we have remained true to his spirit and that he would approve.

Next, we would like to express our gratitude to two important figures who influenced both of us, and commented on the ideas underlying this work, but who are now deceased. These are Nobel Laureate Professor

Wassily Leontief (NYU Institute for Economic Analysis) and Professor Camilo Dagum (University of Ottawa and University of Bologna). Both offered helpful suggestions and criticism on earlier versions of the manuscript, particularly for Chapters 1, 2, 6, 7, 8 and 10. Both were generous in sharing with us their own reflections on the scientific standing of econometrics. Leontief first introduced us to the work of Alain Bonnafous and encouraged us to think critically about empirical methodology right up to his unexpected death in 1999. Dagum patiently went through early versions of parts of the book, and made many very useful comments on it before his death in 2005.

In addition, we gratefully recall Robert Heilbroner (New School for Social Research), who always emphasized the importance of approaching both the institutions of the economy and the theory (supposedly!) describing how the economy works from the perspective of history. Both theory and institutions develop; it's important to see whether the paths of development are congruent or not.

Finally, we regret that we cannot discuss these issues with Hyman Minsky (Washington University, St. Louis). He was a realist and he believed in empirical work but he was suspicious of sophisticated techniques. The data wasn't good enough, and the techniques often discarded information in the process. He knew that models had to be abstract, but he also knew that they had to stick close to the way things really work. He was not only a great guide to the mysteries of banking and finance, but he set an example as a practical and realistic thinker.

OUR COLLEAGUES AT THE NEW SCHOOL

We benefited from many excellent discussions with Professor Duncan Foley. Foley has recently been working on a related book with the theme why and to what extent does Statistics work and we have learned a lot from him.

Professor Willi Semmler is a master of applied work and has always been helpful in calling our attention to applied issues and explaining the pressures and problems leading to the development of new techniques. He read and commented on drafts of several chapters.

Professor Anwar Shaikh has worked extensively with the conceptual problems in translating data gathered under one set of categories, appropriate to a conventional theoretical framework, into the categories appropriate to a different theoretical approach. His careful work has set us an example.

Professor Will Milberg (together with Professor Robert Heilbroner) has written extensively on methodology, especially regarding what they see, following Schumpeter, as a 'Crisis in Vision' in economics. We have tried to bring this perspective to bear on econometrics.

OTHER COLLEAGUES AND FRIENDS

Professor Aris Spanos (Virginia Tech) has been an influential and valuable critic of our work. We have learned much about the philosophy and methodology of econometrics from him, and if we still differ on some issues we nevertheless consider his work beyond compare with most done today.

Professor George Davis (Virginia Tech) has been an astute critic who helped clarify our arguments at many points.

Nobel Laureate Professor Robert Mundell (Columbia University) has a very deep sense of realism about how the economy works and he has won many bets with econometricians! He has always been open to discussion about how best to understand the way the macroeconomy works.

Professor Tony Lawson (Cambridge University and his Critical Realist group) has transformed the terms of discussion in regard to realism and methodology greatly for the better in our view. We have learned from his approach and agree with the importance given to questions of ontology.

Professor Deirdre McCloskey (University of Illinois at Chicago) is always a joy to engage in argument, and normally leaves one better informed but less comfortable than before. We love the challenges she throws out and have tried to meet them.

Professor Alessandro Vercelli (University of Siena) is one of the few people who truly understand Keynesian uncertainty and has attempted to come to terms with it theoretically. We have drawn on his work and have tried to carry it forward.

Professor K. Vela Velupillai (University of Trento) is one of the few scholars who attempt to show that mathematical economics is unreasonably ineffective and has proposed an economics for the future that will be freer to explore experimental methodologies underpinned by alternative mathematical structures. We have benefited from his criticism and tried to meet the challenges.

AND MANY MORE CASUAL BUT IMPORTANT DISCUSSIONS OVER THE LAST 3 DECADES

In addition to those listed above, we would like to mention colleagues and friends with whom we have had illuminating discussions, on econometrics and on economic analysis generally, or who helped advance our project in one way or another: George Argyrous, Michel Armatte, Patrick Artus, Tom Asimakopulos, Ron Baiman, Ronald G. Bodkin, Lawrence Boland, Marcel Boyer, Robert Boyer, Camille Bronsard, Yves Carro, Jean Cartelier, Osiris Cecconi, Ramiro Cercos, James Dean, Antoine d'Autume, Oscar de Juan, Enrique Delamonica, Christian Deblock, Ghislain Deleplace, Meghnad Desai, Jean Marie Dufour, Roy Epstein, Ray Fair, Eladio Febrero, Peter Flaschel, Mathew Forstater, Teresa Ghilarducci, Jean Jacques Ghislain, Christian Gourieroux, Davide Gualerzi, Marc Guillaume, Herve Hamon, Omar Hammouda, Felix Jimenez, Elias Khalil, Peter Kennedy, Stephen Kinsella, Heinz Kurz, Maurice Lagueux, Marc Lavoie, Paul Lewis, Alain Lipietz, Jean Guy Loranger, Cornelis Los, Ray Majewski, Gary Mongiovi, Deepak Nayyar, Jacob Nell, Hasham Pesaran, Pascal Petit, Nobel Laureate Edmond Phelps, Tom Phillips, Christian Pozzo, Christian Proano, Robin Rowley, Bertram Schefold, Mario Seccareccia, Henri Sterdyniak and Ross Thomson. We also regret the passing of Nobel Laureate James Tobin, Nobel Laureate Franco Modigliani, Nobel Laureate Clive Granger, G.S. Maddala, David Gordon, Marc Blaug, Marcel Dagenais, Lise Salvas Bronsard, Maurice Bouchard and Jacques Henry, all of whom helped us with insights. We should add that we found special inspiration in the work of both James Tobin and Franco Modigliani, who based their empirical work on a realistic approach.

THE MARTIN HOLLIS CONFERENCE AT THE NEW SCHOOL

Draft chapters of the book were presented at the Martin Hollis Memorial Conference at the New School in November 2004 (Rationality, Action, and Value in the Philosophy of Social Science: A Conference in Honor of Martin Hollis). We would like to thank Luc de Clapiers, President and CEO of Natixis North America (NY) and the New School for help with funding, and extend our appreciation to all the participants; all helped us to appreciate Martin's work. Many papers and discussions concerned his later work, but a number of papers and discussants addressed wholly or in part the 'Hollis and Nell' issues. We would especially like to thank Margaret Archer, Margaret Gilbert, Russell Hardin, Shaun Hargreaves Heap, Bernard Hodgson, Brendan Hogan, Simon Hollis, A.J. Julius, Tony Lawson, Isaac Levi, Steven Lukes, Richard Miller, Timothy O'Hagan, Alex Rosenberg and Pavlina Tcherneva.

A MEMORIAL TO THE NEW SCHOOL SEMINAR ON ECONOMETRICS

Finally we would like to offer this book as a memorial to all the scholars who participated in the New School International Seminar in Econometrics in the early 1940s. The New School's pioneering role in developing the new and foundational ideas in econometric methodology has not been adequately recognized, and we take this occasion to call attention to this and to honour it. The group of scholars involved was very distinguished, among them J. Marschak, Nobel Laureate T. Haavelmo, A. Wald, and Nobel Laureate F. Modigliani. The Seminar was initiated by Marschak and later joined by Haavelmo. It attracted brilliant economists, statisticians, graduate students and instructors from the New School, Columbia, and the NBER. The ideas presented, especially by Haavelmo, and developed in subsequent discussion, came to the compelling conclusion that least squares had to be replaced by some other approach for econometric work; this led to the probability approach and to the study of simultaneous equations. However, in 1942 Alfred Cowles successfully induced Marschak to accept a joint position as professor at Chicago and as research director of the Cowles Commission for Research in Economics, starting January 1943, and the New School seminar ended.

AND THANKS TO OUR PUBLISHER, EDWARD ELGAR

Special thanks to Edward Elgar who took this project under his wing; we are grateful for his interest in the ideas of this book, and for his encouragement, and patience! It took a long time to complete the work. As usual, the people at Edward Elgar Publishing did a great job. We are grateful to all his staff for their help with turning the manuscript into a book, especially Matthew Pitman, Joanne Betteridge, Rebecca Hastie, Elizabeth Teague and Nicolas Wilson.

We also thank the anonymous referees for Edward Elgar Publishing for their suggestions on how to improve the manuscript.

We would like to thank Beatrice Macguire for her excellent editing job on an early draft of Chapter 6. Michalis Nikiforos transcribed the diagrams of Chapter 11 into a computer program and rationalized the numbering of the equations. John Cogliano reworked the equations in a section of Chapter 11, using Mathematica. Hamza Errouaki prepared the figures and Mehdi Errouaki assisted with the preparation of the Bibliography. Barbara Herbst of the New School provided invaluable help with good cheer and remarkable efficiency. We thank her for her unconditional support and assistance whenever needed.

Finally, Edward Nell would like to thank the US Fulbright Commission for a grant in the Spring of 2009, and Stephen Kinsella for hosting him at the University of Limerick in Ireland while completing this manuscript.

All errors and shortcomings of this work are solely our own fault, and the views expressed in this book do not necessarily reflect those of any institution we were or are affiliated with.

Edward J. Nell
Karim Errouaki

New York, NY, USA

The increasing scale, complexity, and practical success of econometric modelling in recent years require a rethinking of its foundations. Econometricians have made do with a formal description of the nature and objectives of their work which relies too heavily on the example of the experimental sciences, and thereby *gives an incomplete and misleading picture*.

Sims (1982a, p. 317, italics added)

One approach which to my knowledge has been completely ignored is the integration of economic methodology and philosophy with econometrics.

Caldwell (1982, p. 216, italics added)

Philosophy of econometrics is concerned with the systematic (meta-) study of general principles, strategies and philosophical presuppositions that underlie empirical modeling with a view to evaluate their effectiveness in achieving the primary objective of 'learning from data' about economic phenomena of interest. In philosophical jargon it is a core area of the philosophy of economics, which *is concerned primarily with epistemological and metaphysical issues pertaining to the empirical foundations of economics. In particular, it pertains to methodological issues having to do with the effectiveness of methods and procedures used in empirical inquiry, as well as ontological issues concerned with the worldview of the econometrician. Applied econometricians, grappling with the complexity of bridging the gap between theory and data, face numerous philosophical/methodological issues* pertaining to transmuting noisy and incomplete data into reliable evidence for or against a hypothesis or a theory.

Spanos (2007, p. 2, italics added)

Before a thing becomes an object of cognition it must have been a problem, and before it becomes a problem we must have found it strange.

Ortega y Gasset (1946, quoted by Dagum, 1986b, p. 22)

In every scientific venture, the thing that comes first is vision.

Schumpeter (1954, p. 561)

Introduction

THE PURPOSE

This book should really be considered as epistemology, especially as we wish to construe that term broadly.[1] It rests on ontology and takes aim at methodological foundations. The object is to re-examine the scientific standing of structural econometrics as developed by the founders of econometrics (Frisch and Tinbergen) and extended by Haavelmo and the Cowles modellers (particularly Klein) during the period 1930–60.

The early econometricians tended to believe they could test economic theories and discover scientific laws analogous to the laws of physics and natural science. The writers who have examined the history of econometrics have tended to accept this project more or less uncritically. By contrast, we consider this misguided, and based on philosophical error. Certainly, in our view, econometrics can contribute empirical insights that will advance the development of economic theory, and it can specify and identify reliable projectible relationships, but, as we shall explain, these are not the same as the scientific laws of physics, and they are specific to particular periods of history. But they do exist.

The book can be seen as a response to Caldwell's (1982, p. 216) challenge. The quotation from Caldwell suggests integrating economic methodology and philosophy with econometrics. It is still applicable and even more worthy of consideration today.

Spanos (2007, p. 2) elevated the philosophy of econometrics to primacy of place in the philosophy of economics, as the study of 'general principles, strategies and philosophical presuppositions that underlie empirical modeling'; the aim being to understand how to achieve 'learning from data'. This concerns methodology, of course, but also ontology and epistemology. How can we transmute 'noisy and incomplete data into reliable evidence'? We have to know that the data are actually genuine (or at least adequate) instances of the variables of the theory or hypothesis. So we have to understand where the data come from, which is to say, we have to have some sense of the reliability and working of the data generating process. Econometricians still face unresolved problems in bridging the gap between data and theories after all these years! But without such a bridge, the most sophisticated technical methodology will be swept away in a flood of errors.

This worries Spanos (2007), who considers that economic methodology has failed to address the core issue: the theory–data gap in econometrics.[2] Instead, so far, the literature has focused primarily on a variety of less significant issues such as the status of assumptions, the structure of theories, falsification versus verification, Kuhnian paradigms versus Lakatosian research programs, the sociology of scientific knowledge, realism versus instrumentalism, 'post-modernist' philosophy, and so on (see Backhouse, 1994; Blaug, 1980; Dagum, 1986a; 1986b; 1995; Davis et al., 1998; Maki, 2001; 2002; Milberg, 1993; 2007; Redman, 1991). Even in methodological discussions of economic theories in relation to reality, econometrics tends to be neglected (Caldwell, 1982) or misrepresented (Lawson, 1997). Economic methodology itself seems to have problems. When assessing recent work, Hands (2001) contends that philosophy of science is 'currently in disarray on almost every substantive issue' and provides 'no reliable tool for discussing the relationship between economics and scientific knowledge'. But Spanos (2007) thinks this sort of comment is unhelpful and believes that some writing in the current philosophy of science, focusing on 'learning from data' (see Chalmers, 1999; Hacking, 1983a; Mayo, 1996), will contribute toward improving the credibility of economics as an empirical science.

The state of econometric practice bothers McCloskey (1996, pp. 30–33), who has judged that:

> the first tragedy arising from the pride of the 1940s is called 'statistical significance. [It] ruins [econometrics]. The problem comes, not in 'estimation' but in 'testing'. The 'testing' makes no sense at all if it is seen, as it usually is, as answering the scientific questions 'How large is this effect?' or, what is the same thing, 'Does it matter for science?
>
> In rare circumstances the statistical significance of an estimate might be of small scientific interest. In the overwhelming proportion of its uses in economics, it is completely irrelevant. All of modern econometrics has to be done over again.

Methodological debates in econometrics are almost as long-standing as the discipline itself (see Epstein, 1987; Gilbert, 1988; Morgan, 1990a; and Qin, 1993). Boland (1982, pp. 4–5) argued that

> presentations of methodology in typical econometrics articles are really nothing more than reports about the mechanical procedures used, without any hint of the more philosophical questions. The so-called methodological critiques turn out to be critiques of the statistical definitions or statistical tests used in the study in question. Similarly, methodological issues turn out to be questions of

> whether to use 'comparative statics' or whether to use 'a moving average' or 'discrete observations'.

At the time it seemed easier to practice the science than to describe how one was doing it. 'Get on with the job' was the message sent by mainstream econometricians.

The epistemological status of the econometric approach that we propose, however, is different from what might be considered, in fact, to be diametrically opposed to that generally found in the discipline. The polar nature of this difference lends itself exquisitely to the debate on certain fundamental aspects of neoclassical econometrics.

Our point of departure is the research agenda as it was defined by the founders' editorial in the first issue of *Econometrica* in 1933, where Frisch[3] (1933, p. 1) eloquently expounded the hopes and expectations for econometrics, and hailed it as:

> The unification of the theoretical-quantitative and the empirical-qualitative approach to economic problems with a constructive and rigorous 'thinking' similar to that which has come to dominate in the natural sciences.

Pesaran and Smith (1992, p. 1) commented on this:

> We have come a long way since the appearance of the first issue of *Econometrica*, and yet Frisch's call for the unification of theory and measurement is as relevant today as it then was.

Morgan (1990a, p. 264) has concluded from her history of econometrics study that 'by the 1950s the founding ideal of econometrics, the union of mathematical and statistical economics into a truly synthetic economics, had collapsed'.

Structural econometrics, as we understand it, ends in 1960; our study, of course, examines later developments.[4] But the econometrics that we wish to rethink and (in part) revive underwent a major change at about this date. 1960 was the date of the exit of Frisch and Haavelmo from econometrics.[5]

First, Haavelmo (1958) contended that weak theoretical economic foundations rendered suspect the policy value of most econometric models. Then Frisch (1961) chose not to mention econometrics in a survey of types of economic forecasting methods. To paraphrase Frisch, the models had become 'hollow numerical exercises' because they 'failed to represent the effective institutional and political constraints on feasible economic policies'. Our interest is to ask what should and can we learn from the period of history leading up to this point with respect to present and future econometric model building?

Haavelmo (1944) played a crucial role in first demonstrating the need for an explicit 'probabilistic model' for econometric estimation and inference, and then in advocating the 'Fisherian model' as an ideal for this particular purpose, emphasizing the importance of inference and testing in applied economic research. Subsequent work at the Cowles Commission demonstrated that the now standard 'Neyman–Pearson' inferential framework could be applied in econometric regression models.

Malinvaud (1988, p. 197) has claimed that the Cowles Project 'essentially stands untouched and no doubt or questioning can be expressed'. Our objective is to determine first whether or not, and in what sense, the Cowles Project, as conceived by the founders and Haavelmo and developed by the Cowles Commission, is appropriate for the purpose for which it was originally designed. Second, since this is partly an empirical question, we need a methodological framework for empirical study. So we have developed such a framework – the unifying thread of this work – that can overcome most of the methodological problems of structural econometrics. But it requires a new approach to theory.

The Cowles Project, to paraphrase Epstein (1987), raised such high hopes by proposing its structural estimation methodology that the lack of agreed findings is in some ways astonishing. This lack of agreement actually reflects a genuine 'epistemological puzzle'. The problem arises partly from disagreement over how to capture the basic structure that underlies the economic system, including problems of how to select models, both of which undermine the ambition to prescribe how to manipulate the economic system towards stability. But it also arises partly from disagreement over whether the world is, in fact, a stochastic environment that can be captured by superimposing a statistical disturbance on a deterministic model provided by economic theory. In this view, the probabilistic element is admitted only at a second stage, an afterthought following the deterministic first stage, provided in the mainstream approach by the theory of rational choice. Boland (1982, p. 122) argued that 'this conception of the world can be very misleading and thus requires critical examination'.[6]

How do the ideas developed by Haavelmo and the Cowles Group in the 1940s and 1950s stand today? Should their approach be discarded and replaced, as modern critiques have argued? A careful and critical examination of the methodological issues should help us understand how the Cowles Project developed, and how it can be further refined, hopefully providing insight into some of the principal methodological points at issue today.

THE SETTING OF THE PROBLEM

The Complaint

The Complaint here charged that neoclassical economic theory arguably provides the ontological basis (the rational individual) and the corresponding individualistic methodology of the modern econometrics that has come to replace the Cowles Project. The result is that neoclassical based econometrics, which functions at the level of appearances and events, fails to develop any insight into deep structures – it interprets whatever it sees as individuals choosing with some degree of (perhaps bounded) rationality. It simply relates observables to one another, putting choices and actions together into equilibrium patterns.

Neoclassical model builders make no effort to reach through to a deeper level. Hollis and Nell (1975), Lawson (1997) and Nell (1998a) have all argued that neoclassical theory should be rejected as empiricist and deductivist. Of course, this has been argued before, so while the charge is not at all new, it is still as controversial as ever. And it seems clearly true of a great deal of what passes for applied econometric research. However, it is not so clear that mainstream economic theory can be both empiricist and deductive at the same time.

Correlations are precisely 'Humean' constant conjunctions, to use Nell's expression, and the search for them is the practice of relating observables to one another. When neoclassical econometrics seeks to go beyond good correlations and impute causality, the notion it employs – Granger causality – is strictly Humean, depending as it does, mechanically, on temporal priority. Yet, as Nell (1998a) argued, temporal priority is neither necessary nor sufficient for causality, using the concepts in their normal sense.

Boland (1982, p. 122) argued that

> virtually every applied neo-classical model today is a stochastic model. The problem with the concept stochastic, or more generally, with the 'doctrine of stochasticism' – an ontology that asserts that realism means being stochastic – is that it takes too much for granted without reason or evidence. Some economists are fond of claiming that the world is a stochastic environment (e.g., Vernon Smith, 1969); thus technically no model is ever refuted or verified, and hence there could not be any chance of our construing one as a refutation or a verification of a theory.

Consider the role of stochasticism in mainstream economics. Boland (1982, p. 122) argued that 'stochasticism involves model building, as it requires an explicit modeling assumption that might be false, so it should not be taken for granted'. Let's see how modern econometricians deal with this.

Following Nell's (1998a) approach and conceptual analysis, one could argue that there are two worlds (though Nell doesn't so label them): the real world that we observe and the model world of the theory or mathematical model that we construct. The model will always abstract from reality. But sometimes the theory requires that the model consist of idealized actors or circumstances or behavior, so that nothing real could ever closely correspond. This raises special problems that we shall discuss later. When we say the theory (or model) is true, we mean that the real and the model worlds exactly or at least adequately correspond. Many will argue that there are obvious reasons why, even with good theories, the correspondence will not be exact (for example, errors of measurement, irrational human behaviour, etc.). For these reasons, modern economists build stochastic models that explicitly accommodate the stochastic nature of the correspondence (see Boland, 1982, pp. 122–3). For example, we can assume that the measurement errors leave the observations in a normal random distribution about the true values of the model world. This means that the correspondence itself is the stochastic element of the model.

In Haavelmo's perspective, contrary to that of modern econometricians, it is the 'model that is stochastic', rather than the 'world' or the 'environment'. Any test of a stochastic model is as much a test of the assumed correspondence as it is of the theory itself. Modern econometricians do not seem to be willing to go all the way with Haavelmo and thus still to see a possibility of stochastic models being helpful in the assessment of exact theories and models (see Spanos, 1989; 2007; Davis, 2000). It could also be said that stochastic models follow from a methodological decision not to attempt to explain anything completely.

Boland (1982, p. 123) argued that

> one can choose to see the world as being necessarily stochastic only if one assumes beyond question that one's model (the shot at the real world target) is true (and fixed) and that the variability of the correspondence is due entirely to the movements of the target (the real world). Thus, stochasticism can be seen to put the truth of our theories beyond question. There is a serious element of potential intellectual dishonesty in asserting that the environment is stochastic.

Neoclassical econometrics is a major digression from Haavelmo's econometric thinking and the founders' 'unification vision'.

Furthermore, Bonnafous (1972; 1989) argued that in economics as in any field dealing with the real world, the real issue is not 'simplification', but how to simplify without losing the relationship to real-world phenomena. In more general terms, simplification is a necessary part of thought, because simplification results from abstraction. As Krugman

(1997) observed, as soon as one is engaged in thinking – that is, in forming concepts – an abstraction results. But, as Nell (1998a) put it, 'abstraction' is not 'idealization'.[7]

Contemporary currents in scientific thought allow us to abandon the (essentially metaphysical) idea of a necessary and pre-established adequacy between mathematics and reality. To paraphrase Bonnafous (1972, p. 11), the world of inexact science, in particular, does not appear to be organized according to mathematical laws. Furthermore, it is interesting to observe that, in the nineteenth century, at the same time that the idea of a universal truth provided by mathematics was discredited by the emergence of alternative axiomatic systems, because of Walras, a narrow view of rationality rapidly gained dominance in economic science, subscribing to a Platonic and Cartesian approach to science. Leontief (1984a, 1984b) argued that such work in pure economics continues to be widely pursued today.

The Vision

The vision we propose here puts 'methodological institutionalism' in place of 'methodological individualism'. Hollis and Nell (1975) had already both exposed and explained the methodological deficiencies of modern econometrics, before they had become widely realized. Moreover, Hollis and Nell's framework and later Nell (1998a) suggested a way of fixing the problems. The founders of econometrics, Haavelmo and the Cowles econometricians, held a vision of the real world – first expressed in the Cowles Project that provided the epistemic foundation for the econometric field in the 1940s. This vision provides a perspective that is ontologically incompatible with the contemporary view of modern econometricians that developed in the late 1970s and early 1980s.

The history of econometric thought will show that the modern critiques, based as they are on methodological individualism and positivism, have turned into ontological and epistemological failures, proffering inadequate criteria for what exists, and for what we know. We shall argue that Klein's methodological structuralism and Nell's methodological institutionalism offer a new approach, an ontological turn, so to speak, that ensures that socioeconomic reality, understood through fieldwork, will be what defines the terms of the model, and not the other way around.

Nell (1998a) argued that models have to refer to what actually exists; that is what is meant by realism. But models also have to exhibit relationships similar to those in reality, yet in a form that can be manipulated or analysed mathematically. That is how models help advance understanding. But model building cannot be allowed to succumb to the lure of scientism. In particular, a sound epistemology tells us that the social order is necessarily 'open'; that is, it cannot be circumscribed and summed up in a deterministic model. Nor can it be described in terms of stochastic regularities of the sort presupposed by modern econometricians (see also Lawson, 1997, pp. 76–7; Lewis and Runde, 1999, pp. 38–9).

The main argument of the book is that structural econometrics can be redeveloped on the basis of rereading Haavelmo within Hollis and Nell's (1975) framework and Nell's (1998a) methodological institutionalism. We think this may prove to be the most fruitful empirical approach in economics.

OUTLINE OF THE BOOK

It is not our intention to call into question the basic principles of structural econometrics itself. To be critical does not mean to disparage or to destroy in the sense of Lucas's critique or Sims's alternative methodology, but to be lucid and vigilant and ready to call something in question when appropriate. It is 'deconstruction' for better building, as Jacques Derrida would have said!

The book consists of three parts and a general conclusion. Part I focuses on rethinking the scientific foundations of structural econometrics. The main argument of Part I is that there are good reasons for considering Hollis and Nell's (1975) framework as an epistemological foundation for reconstructing structural econometrics, a foundation that complements and extends the original ideas of Haavelmo.

Part I consists of six chapters. Chapter 1 restates and adapts the arguments of Hollis and Nell, shifting the focus from economic theory to econometrics. Chapter 2 connects the Hollis and Nell line of argument with Haavelmo's initial and later papers, revealing a surprising degree of overlap. Chapter 3 examines whether and how claims to have established scientific knowledge can be justified, and this calls for a review of the long-standing arguments over induction, culminating in the recent revival of this literature in the work of Mayo and Spanos (2010), focusing it directly on statistics and econometrics. Chapter 4 presents a justification of scientific laws for the physical sciences (resolving the philosophical problem of induction). The argument is an extension of Strawson's 'descriptive metaphysics' and runs along Kantian lines. Chapter 5 then adapts this approach to economics, first justifying 'economic laws' and then displaying the differences between them and the 'laws' of the physical sciences, finally relating this discussion to somewhat similar ideas in critical realism. Chapter 5's appendix presents a brief discussion of Plato on the economic principles underlying the formation of a socio-economic system. Chapter 6 defines the three conditions for a successful model – theoretical coherence, relevance (how it applies to the real world), and measurement – summing these up in the methodological triangle-circle (MTC) diagram.

Part II is methodological. The current critiques of the methodological foundations of structural econometrics are direct consequences of implicitly accepted but seriously flawed views of the appropriate foundations of econometrics, grounded in neoclassical thinking. Chapter 7 shows that within the neoclassical framework it is possible to improve the performance of a model on any one of these conditions only at the expense of worsening its performance on at least one of the others; the MTC diagram is then used to analyse the major critiques and commentators on the foundations of econometrics (Malinvaud, Lucas, Sims, Leamer, Hendry), contrasting their views with the unification scheme of the founders. Chapter 8 advances the methodological considerations presented so far, re-examining probability and the error term; and then applies the approach to stochastic methods, arguing that it is a mistake to think of the world as stochastic; rather, it is the methods that are stochastic, and understanding this helps us to distinguish between reliable and volatile relationships. Chapter 9 examines two treatments of Haavelmo's probabilistic approach (Davis, 2000; Spanos, 1989), both of whom consider questions of statistical adequacy, then turns to a critical study of Los's (2001) rejection of the probability approach, following this with an exploration of Foley's (2005) Laplacian rethinking of the foundations of probability. We argue that each author has something to offer, but that an important common concern, which was also central to Haavelmo's work – namely the apparently inherent unpredictability of much economic behaviour – may have to be approached in terms of uncertainty, rather than probability. This echoes Keynes. Then all four are subjected to analysis using the MTC diagram, further developing the thesis that models rather than the world are stochastic. Our approach calls for all three to be respected: coherence, measurability and relevance – and their possible relationships must be articulated. This, we think, was part of the vision of the founders of econometrics, especially Haavelmo, and the approach of Klein and Nell can be seen as an elaboration of what might be called a new econometric playing field.

Part III consists of two chapters. Chapter 10 presents our distinctive methodological contribution: a blend of fieldwork and conceptual analysis designed to ensure that our models are well grounded in reality but at the same time are conceptually coherent. Chapter 11 then turns to specification, and outlines a number of elements that will be needed in developing a good macroeconometric model of an advanced economy, covering money, inflation, expectations, together with the basic relations of output and employment, consumption and income, investment, profits and finance, showing how we can distinguish reliable from volatile relationships, and suggesting ways in which this approach can be developed further.

The general conclusion sums up the main arguments developed in the book. It also offers concluding comments on methodology and suggestions for future directions in the study of macroeconometric model building.

Finally, each argument is part of the whole picture, and each is linked with the others. No parts of the book really stand alone; the book as a whole presents a picture of econometrics as a whole. Of course there are many weaknesses and even our best arguments might have been better; but the whole picture suggests that the founders were on the right track: econometrics really can tell us how the economy works and how we can make it work better.

NOTES

1. The term here is defined by Bitsakis (1987, p. 389) as follows: 'epistemology is not a particular science. It is a discourse about science. It investigates the foundations, status, classification and development of the sciences, the function of internal and external factors determining their development, the relationships between theory and experiment, the nature of scientific crisis and revolutions, the status of scientific truth'. For a comprehensive discussion of epistemological issues in economics see Hollis (1987), Cecconi (2000, particularly chs I, IV and V) and Dupuy (2004).
2. Spanos (2007, p. 2) argued that 'discussions of econometric methodology have been primarily "local" affairs (e.g., Granger, 1990; Hendry, 2000; Leamer, 1988 among others) . . . where no concerted effort was made to integrate the discussions into the broader philosophy of science discussions concerning empirical modeling'. He pointed out that there are some notable recent exceptions like Hoover (2002; 2006), Keuzenkamp (2000) and Stigum (2003).
3. Aldrich (1989, p. 33) argued that Frisch's ideas on structure 'were embodied in Haavelmo's 1944 probability approach and his ideas on dynamics clearly influenced Samuelson's 1947 Foundations of Economic Analysis, the works which above all others codified the methodological discoveries of those years'. For a discussion of Frisch's role in econometrics see Tinbergen (1974). For an account of the failure of Frisch's vision, see Lail (1993).
4. For a comprehensive and detailed account of the history of econometrics see Epstein (1987), Gilbert (1988), Morgan (1990a) and Qin (1993). Here we are concerned to bring out the important and still unresolved problems which the founders struggled with and which are highlighted in the Hollis and Nell critique. We will present in Chapter 7 some notes on the history of econometrics. These notes clearly do not aim to be comprehensive, but only to trace some trends in the development of econometrics.
5. The exit of the Oslo professors from econometrics is still an open question in the history of econometric thought. For further details see Epstein (1987, ch. 4).
6. For an account of 'stochasticism' and econometrics see Boland (1977; 1982; 2000) and also Chapter 8 of this book.
7. We will further clarify the difference between abstraction and idealization in Chapter 6.

PART I

From rational economic man to rational econometric man

Economics is probably the most subtle, precise and powerful of the social sciences and its theories have deep philosophical import. *Yet the dominant alliance between economics and philosophy has long been cheerfully simple*.

Hollis and Nell (1975, cover page, italics added)

The accepted views of the appropriate methodologies for empirical investigation of neo-classical economic theories are inadequate to clarify the foundations of econometrics. We presume one cannot understand econometric methodology without first understanding economic theory.

Swamy et al. (1985, p. 4, italics added)

A deep and widespread crisis affects modern economic theory, a crisis that derives from the absence of a vision – a set of widely shared political and social preconceptions on which all economics ultimately depends.

Heilbroner and Milberg (1995, cover page, italics added)

'Rationality' has played a central role in shaping and establishing the hegemony of contemporary mainstream economics. As the specific claims of robust neoclassicism fade into the history of economic thought, an orientation toward situating explanations of economic phenomena in relation to rationality has increasingly become the touchstone by which mainstream economists identify themselves and recognize each other. *This is not so much a question of adherence to any particular conception of rationality, but of taking rationality of individual behavior as the unquestioned starting point of economic analysis*.

[. . .] *the theoretical discourse of economics might just as well be seen as a branch of Kantian philosophy* (which is where some of its ablest practitioners, such as Sen, are clearly disposed to move it).

[. . .] A more fertile economics will be requiring us to live differently. *As Hegel points out, this means essentially to think differently, since those who think differently are already living a different life*.

Foley (2003, pp. 1, 7 and 9, italics added)

Constructive criticism can only be helpful, provided researchers are open to suggestions.

As economic theories and models are human constructs, the question arises whether it is best to discard all that has been constructed in the past and then to start again or to build on the best of the old material and to continue with new ideas.

Granger (2004, p. 99, italics added)

1. Re-reading Hollis and Nell

1.1 RATIONAL ECONOMETRIC MAN

It will be suggested here that it is not too far-fetched to see Hollis and Nell's (1975) *Rational Economic Man* as a scientific foundation for reconstructing structural econometrics, one might say, for 'Rational Econometric Man'. To make this case, we shall have to take a detour, unfortunately, into the fundamentals of positivism, and then visit the secret laboratory containing the epistemological foundations of neoclassical economics. At the door of the laboratory there is a sign that reads: 'No admittance unless you accept a twin allegiance: positivism and individualism'. On this visit, we do not intend simply to defend Hollis and Nell's thesis, but we hope to give some support to the argument that their thesis provides foundations for econometrics.

As mentioned earlier, Haavelmo (1958) has argued that weak theoretical neoclassical economic foundations rendered suspect the policy value of most econometric models.[1] He devoted the end of his career to re-examining the neoclassical theory of investment (see Haavelmo, 1960).

We start here with the following three observations.

First, Spanos (1986) argued that the textbook econometric methodology reveals that econometricians continue to believe that they are adopting a positivist approach while many of them in fact have recourse to a more Popperian form of prediction. Indeed, confronted with Popperian ideas, econometric methodology oscillates first between a projective process seeking to test a pre-established model and an inductive process willing to infer as much information as possible from the facts. If statistical induction stands in an intermediate position, the research of causality takes up the whole spectrum. In its practice as much as in its methods, econometric methodology also hesitates between a viewpoint of refutation and a viewpoint of confirmation, with the need to distinguish the validation against observations of a specified relation or a theory and the validation of a model through comparison with current expectations.

Positivism has always tremendously affected textbook econometric methodology (see Gilbert, 1986b; de Marchi, 1988). We shall argue that the structure of neoclassical theories itself has been subject to the influence of the axiomatic hypothetico-deductive formulation of logical empiricism, and this further complicates the textbook econometric methodology. The sometimes unconscious philosophy is a heady mix of positivism with Popper and pragmatism, seasoned with old-fashioned empiricism. But there is no trace of rationalism, no room for conceptual truths and no way to account for the importance – indeed, the necessity – of fieldwork.[2]

Second, Hollis and Nell (1975) argue that positivism (broadly conceived) has provided neoclassicism with important support, which they then show to be unfounded. But we shall argue that they base their critique of neoclassicism not only on their critique of positivism but also on the alternative they propose, rationalism. Indeed, they argue that rationality is central to neoclassical economics – as rational choice – and that this conception of rationality is misused. Demands are made of it that it cannot fulfil. By contrast, the rationalism they propose provides solid foundations.

Third, as mentioned in the introduction, Caldwell (1982, p. 216, italics added) argued:

> *One approach which to my knowledge has been completely ignored* is *the integration of economic methodology and philosophy with econometrics*. Methodologists have generally skirted the issue of methodological foundations of econometric theory, and the few econometricians who have addressed philosophical issues have seldom gone beyond gratuitous references to such figures as Feigl or Carnap.

It was Spanos's book (1986, especially p. 659), Lawson's article (1989, p. 236), and later Davis's article (2000, p. 205) that drew our attention to Caldwell's comment on the general neglect of philosophical considerations in this area.

Spanos (1986; 1989) can be seen as a response to Caldwell's challenge. Spanos sees 'econometric modelling as a thinking person's activity and not as a sequence of technique recipes'. Spanos (1986, pp. 659–60) observed that textbook econometric methodology 'is deeply rooted in the logical positivist tradition of the late 1920s and early 1930s'. He (ibid.) went on to argue that 'the preoccupation of logical positivism with criteria of cognitive signification and the verifiability criterion is clearly discernable in defining the scope of econometric modelling as the measurement of theoretical relationships'. This is the first weakness of the textbook methodology. Spanos (ibid.) rejected 'this definition as narrow and misleading. Theories are not conceived for the sake of theorising but in order to understand some observable phenomenon of interest'. The second problem 'is related to the treatment of the observed data as not directly related to the specification of the statistical model. This is based on the logical positivists' view that observed data represent objective facts and any theory which does not comply with the facts was rendered meaningless'. The third problem 'is on the emphasis placed on testing theories and choosing between theories on empirical grounds'. Later developments in philosophy of science defy every aspect of logical positivism (particularly the verification principle and objectivity); as Spanos (ibid, p. 660) observed, the 'incorrigibility of observed data' had not yet reached the textbook econometric methodology.[3]

Indeed, as Spanos (2007, p. 6) argued recently:

> In practice, the methodological framework adopted in traditional textbook econometric modelling does not include systematic probing for errors as part of the accepted rules and strategies for learning from data. Unfortunately, this methodological framework is implicit and it's usually adopted without examination as part and parcel of learning econometrics.

Spanos (1986, p. 660) argued that 'the structure of theories in economics has been influenced by the axiomatic hypothetico-deductive formulation of logical empirism'. This has further complicated the implementation of the textbook econometric methodology. Economists found themselves facing 'illegitimate theory conceptualization' (see Nell, 1998a) and 'illegitimate statistical procedures' (see Leamer, 1978). With this mind, Spanos suggested 'a methodological framework where both economic theory and the structure of the observed data chosen have a role to play' (see also Spanos, 1989; 2007).

Lawson (1989, pp. 236–7) provided a different exploratory response to Caldwell. He addressed 'the issues of realism and instrumentalism in the development of econometrics from an explicit philosophy of science vantage point, developing these as two oppositional positions'. Such philosophical opposition 'has often been found to provide leverage to a better understanding of developments in the natural sciences and has suggested that its explanatory potential with regard to econometric analysis may be no less significant'. By focusing upon 'the traditional philosophy of science opposition of realism and instrumentalism in the context of econometric analysis', Lawson has indeed taken steps toward an 'understanding of the subject matter's essential nature and path of development, as well as helped to illuminate the ambiguities that continue to exist'.

Davis's (2000) goal was to take a step towards Caldwell's suggestion by reconsidering Haavelmo's structure of econometrics within Suppe's (1989) semantic approach to the philosophy of science.[4] Davis was the first to use the semantic approach to help interpret the methodological foundations of econometric theory as conceptualized by Haavelmo.

While there has been a growing interest in economic methodology, most economists have turned to the larger methodological issues, such as the scientific status of economics (for example, Mirowski, 1989a) or critical realism (for example, Lawson, 1997; 1999a; 1999b; Dow, 1999), rather than dealing with the methodological foundations of econometrics. Likewise, Spanos (2007) argued that even in methodological discussions concerning the relationship between economic theories and reality, econometrics is invariably neglected (for example, Caldwell, 1982). Even Lawson (1997) and Downward (2002), who have directly addressed econometrics, have considered it only in relation to the issues posed by critical realism (especially Downward, 2003), rather than trying to recast its foundations.

1.2 HOLLIS AND NELL AND ECONOMETRICS

Two crucial questions arise in addressing the problems of econometrics. The first concerns the adequacy of the methodology of neoclassical economics for the job. The second concerns whether we can find a better approach. Similar questions have been dealt with by Swamy et al. (1985, pp. 4 and 47) from a different perspective. They argued that 'the accepted views of the appropriate methodologies for empirical investigation of neoclassical economic theories do not adequately clarify the foundations of econometrics'. They presume that 'econometric methodology cannot be understood without a good understanding of economic theory'. They have shown that so long as 'neoclassical economic theory is built on ordinary logic as represented by Aristotle's axioms, econometric theory must also be based on such axioms so as to ensure a consistent application to current economic theory'. For this reason, to paraphrase the authors, a logical foundation for econometrics that denies any of Aristotle's axioms cannot be used to model neoclassical economic theory.

Furthermore, as Boland (1985, pp. 63–7) put it, 'fuzzy econometric theory would have to be limited to only building econometric models of fuzzy neoclassical theory' – but optimizing models of unique equilibrium cannot be fuzzy. Boland's main conclusion is that 'the econometrics of the founders will forever remain ill-founded pipe-dreams'. Furthermore, Boland (ibid.) is highly sceptical of Swamy et al. (1985) when they claim that 'a system of many-valued logic provides a firmer foundation for econometrics'. Semantics apart, we are also not at all sure what relevance all their philosophical considerations have for econometric practice. The applied econometrician faces many problems in searching for a given model derived from a logically consistent economic theory (which, it will be argued here, should itself be based on a rationalist theory of knowledge), but these problems are not likely to be resolved by recourse to many-valued logic.[5]

So can we determine whether a superior methodology – for example, 'Hollis and Nell's (1975) framework' – could be found for reconstructing the foundations? The first question calls for re-examining the relationship between positivism and the methodology of neoclassical economics, which is what Hollis and Nell did. In this chapter we wish to take the step forward and show how their critiques can become a methodology. To address this properly, we shall have to reconsider Haavelmo's (1944) seminal paper[6] within Hollis and Nell's (1975) methodological framework – and that will be the subject of Chapter 2.

To put these questions in historical perspective, recall the 'debate' over the scientific foundations of structural econometrics in recent years. Arguably this was due in large part to a crisis of vision within neoclassical economics, ultimately deriving from an advocacy of a strong determinist model of explanation copied directly from physics just when physics seemed to be repudiating such a model!

Mirowski (1989a, p. 218) has argued that the development of econometric methodology strongly reflects the foundations of neoclassical economics. These foundations cannot in turn be understood apart from the history of physics. He asserts that:

> Most economists understand intuitively that the neoclassical research program has striven to attain the respected status of modern science, especially physics. Yet few realize the extent to which the progenitors of neoclassicism acted to secure that status. The so-called marginalist revolution in the 1870s consisted largely of engineers directly appropriating the newly developed formalism of nineteenth century physics, changing the names of the variables, and renaming the result mathematical economics.

The neoclassical vision, let us remember, uses equations to describe the optimizing behaviour of consumers and firms with the aim of predicting such behaviour and its consequences. Neoclassicism takes the circumstances in which the behaviour occurs for granted (Hollis and Nell, 1975, p. 17).

Optimizing behaviour is central to managing the identification and specification problems, as Hollis and Nell have explained, drawing on the supply and demand model (Hollis and Nell, 1975, pp. 81–4). They argue that theory provides the econometrician with a way of specifying this relationship properly and identifying relationships that could not, otherwise, have been unravelled from his data (ibid., p. 74). The fact that supply and demand functions are derived from rational optimizing guarantees that they are reliable – they will hold in the future as they have in the past.

Hollis and Nell further argue that theory is a determining factor in the choice of facts to be retained. In these respects, their approach parallels very closely that of Haavelmo (1944). Indeed, Haavelmo (1944) argued that we cannot do without theoretical (economic) tools when devising models to explain real-life events. Some (economic) scheme conceived a priori is a necessary framework for a simple description of real phenomena (ibid., p. 1).

As we shall see, the truth of theory is a normal presupposition of specifications and identifications in econometric work. This is not just true of supply and demand; growth theory would be vacuous without the collection and analysis of growth statistics. Yet the collection of relevant statistics and especially the estimation of parameters have been predicated on the truth of macro theory and of growth theory (Hollis and Nell, 1975, p. 74). We shall discuss these matters further.

It is exactly these circumstances that Hollis and Nell (1975) question as they develop their alternative vision, based on a rationalist theory of knowledge. They are interested in 'structure', which, with its depiction of dependencies between institutions, makes for continuity or disintegration. Its basic constituents are industries, sectors, processes and activities, defined in technological terms. Activity, organized through institutions, uses up products and energy, which must be replaced, and the institutions must be maintained. Replacement and maintenance require exchanges; hence the basic structure – the circumstances that the neoclassical picture takes for granted – actually imply a set or sets of prices! If these are not realized by behaviour, the system will not be able to support itself.

This picture strongly constrains – even undermines – the neoclassical story of decision-making agencies. Nor is the aim to predict what will happen. Instead, they aim to arrive at a blueprint of the economic system that explains how the system responds to institutional changes. The 'blueprint is essentially an analysis of the nature of production and of the social relations surrounding production' (Hollis and Nell, 1975, pp. 17–18). But this takes us into such deep water that we must leave it here for the moment.

1.3 THE METHODOLOGY OF NEOCLASSICAL ECONOMICS AS A BASIS FOR NEOCLASSICAL THEORY

Hollis and Nell (1975) offer both a philosophical critique of neoclassical economics and an innovation in the field of economic methodology. Further, they outline an alternative vision to neoclassicism based on a rationalist theory of knowledge.

First, they dissect the textbook combination of neoclassicism and positivism, so crucial to the defence of orthodox economics against now-familiar objections.

Within neoclassicism, the authors address consumer behaviour (in the form of indifference curves and simple versions of revealed-preference theory) and marginalist producer behaviour in both product and factor markets. Both are based on rational optimizing behaviour. They consider imperfect as well as perfect markets since neoclassical thinking embraces many market varieties and disposes of a whole system for their classification. However, the authors believe that the issues arising from basic maximizing models have extensive implications for econometric methodology (Hollis and Nell, 1975, p. 2). In particular, it is this class of models – rational behaviour as maximizing behaviour – that provides support for specification and identification. And this, they argue, is where the flaw is to be found.

The first four chapters of Hollis and Nell's (1975) book are concerned with the alliance between positivism and neoclassicism. In chapter 1 the authors ask why the failure of a number of predictions does not count as a refutation of neoclassical theories, and provide a philosophical account of the function of 'ceteris paribus' clauses. In chapter 2 they turn to the fundamental role of maximizing notions and show that the appeal to rational choice has made neoclassical theories viciously circular. A way out might be found if deductive theory could justify maximizing conclusions, but this leads straight to a discussion of the analytic–synthetic distinction, and this shows that the approach has reached an epistemological impasse. In chapter 3 a query about the significance of an apparently constant capital–output ratio of 3:1 triggers a debate about theories, hypotheses and induction. Theories (based on this approach), it seems, cannot support inductive conclusions. In chapter 4 they find that, in attaching sense to terms like 'the price of a good', they must deny that facts are independent of theories. In each of these chapters the authors reject the positivist account and unearth weaknesses in the explanatory power of individualism and so, they argue, leave neoclassicism without any coherent methodology or criteria of scientific merit.

1.4 RATIONAL ECONOMIC MAN: METHOD AND APPROACH

Somewhat surprisingly and independently, Hollis and Nell (1975)[7] and Boland (1982) both use a 'cross-sectional approach' to the understanding of neoclassical economic theory and make similar points about the foundations of neoclassicism. We will draw closely on Boland (1982) through the whole of Chapter 1. Considering the importance of his work in economic methodology, especially his well-known critique of the foundations of economic method, we will quote him extensively. But we will also paraphrase – and sometimes reinterpret – his main arguments to avoid too many long quotes.

Taking these points in turn: by 'cross-sectional approach' we mean that they look for the common theoretical themes in widely different areas of economic analysis. Boland (1982, pp. 5–6) argued:

> The traditional approach to the understanding of economic methodology is serial in nature, as is evident in the usual classification of methodology as a branch of the study of the history of economic thought. If we think of the history of thought approach to economic methodology as a 'time-series' explanation of current practice, the obvious alternative would be a 'cross sectional' explanation.

The history of economic thought is a 'time-series' explanation of current practice; we reflect on what we do in the light of the way it is turning out. Earlier methodological studies are re-examined in the light of developments in theory and empirical studies, and it is noted which kinds of studies appear to be the most successful.

Boland argued that, traditionally, methodology has been discussed only in the context of the history of economic thought – that is, in the context of the views of the past methodological debates (for example, Schumpeter, 1954; Heilbroner, 1970; Blaug, 1978). Viewed that way, methodology has often appeared to be of little relevance for everyday concerns of economic theorists (Boland, 1982, p. 5).

Boland (ibid.) argued:

> This popular approach has its shortcomings primarily in that it contributes new life to old relics and skeletons which would better be left to rest in peace. The major shortcoming is that historians tend to focus on high-profile exceptions to the rule rather than on the more mundane, everyday methods that are tacitly employed by practicing economists.

By contrast, Hollis and Nell's (1975) approach is a 'cross-sectional' explanation of the methodology of neoclassical economics.[8] The approach of Hollis and Nell to neoclassical economics was based on Samuelson's (1947) *Foundations* in which it is argued that constrained optimizing provides a unity of method in the neoclassical treatment of many different questions.

Boland (1982, p. 6) argued:

> One of the advantages of cross-sectional approach is that it immediately requires consideration of the reason why a particular methodology is consciously

> perpetuated or why it is taken for granted. This is important as we wish to examine those problems which are 'hidden' largely because they are taken for granted but which constitute the foundation of most methodological strategies pursued by economic theorists and econometric model builders.

He went on to argue (ibid., p. 7) that:

> The cross-sectional approach addresses the philosophical problems that directly or indirectly impinge on the theoretical and practical concerns of present day economists. Within this approach, every essay, research report, article or economic textbook is considered to be written according to a specific agenda.

Heilbroner and Milberg's (1995) approach is another illustration of what we have called a 'cross-sectional approach' (versus a 'time-series approach') to economic methodology. They used the concept of 'vision' at the centre of their critical look at the epistemological status of current economic thinking. The concept of vision constitutes the touchstone against which different economic schools of other periods are to be judged.[9]

Besides sharing the cross-sectional approach, Hollis and Nell and Boland express a common view of the foundations of neoclassical economic methodology (in Boland's words the 'hidden agenda'), holding that it consists of two related but autonomous problems, namely the 'problem of induction' and the 'explanatory problem of individualism'. By examining the hidden agenda of current neoclassical economics, they each offer a fresh approach to the understanding of both economic theory and methodology. It is interesting to note the similarity between Boland's (1982) approach and many aspects of Hollis and Nell (1975), although their conclusions are diametrically opposed.

Some important distinctions do exist between Hollis and Nell (1975) and Boland (1982), but interest here concerns the central theme, which is the foundations of neoclassicism, although we believe that 'ontology' is a fundamental divide. We accept almost everything Boland says concerning the 'hidden agenda' of current neoclassical economics. Boland (1982, p. 188) wrote:

> Despite recent comments by methodologists indicating that Popper's philosophy of science is a guiding light for economists, the fact is that neoclassical economics is still founded on a methodology consisting of 'Conventionalism' mixed with bits of overt 'Instrumentalism' and inadvertent 'Inductivism'. Popper's contribution so far has been limited to improving the methodological jargon. Where Popper sees science as an enterprise built upon systematic criticism, our profession's reliance on 'Conventionalism' to deal with the 'Problem of Induction' has always put a high value on agreement, that is, on having our views accepted by our colleagues. Given that there is no formal inductive logic,

> everyone seems to think that a theory can be considered successful only if it has been included somewhere in the accepted view of economics.
>
> The opinion that there should be one accepted view is immediately open to question. Yet it is an opinion that is at the core of virtually every methodological dispute. The traditional view is that in order to discover the true nature of the economy we must first have the one correct method for analyzing the economy. As the tradition goes, famous physicists such as Newton and Einstein were successful only because they used the correct 'scientific method'. The companion tradition says that anyone who is not successful must be using an 'unscientific method'.

Boland (ibid.) went on to argue:

> The traditional view is misleading on two counts. First, it presumes there is only one correct method for all of science; and second, it reflects a view that would require 'authoritative support' for anyone's explanation of anything of scientific interest.

Furthermore, Hands (2004) considers this account of the role of method part of what he calls the 'received view', based on positivism and Popper, which he considers to be in decline.

Although Boland (1982) goes on to explain what it would take to incorporate Popper's views into neoclassical theory and methodology, it would not make neoclassicism any more sound. Indeed, as noted, Hollis and Nell argued, as Boland did later, that the foundations of neoclassicism, consist of two related but autonomous methodological problems (namely the problem of induction and the explanatory problem of individualism), but they also argued that the neoclassical answers to these problems are unsound, being based on a broadly positivist theory of knowledge that is also unsound. We turn now to Hollis and Nell's framework.

Hollis and Nell's approach to methodology dramatically breaks with the traditional approach by focusing on the problems of the applicability of current neoclassical theories. Coherent theories, describing the behaviour of assumed – thus imaginary – rational agents, are developed. But what are the conditions for applying such theories to actual agents? The neoclassical answer hinges on its view of rational individuals.

By finding the underlying unity in a variety of subjects and models, Hollis and Nell show that neoclassical theories of economics are built upon the same foundation. We shall call this common foundation the 'DNA structure' of neoclassical economics. This also suggests that, using the idea of DNA structure as a conceptual metaphor,[10] we still need to discover the real DNA structure in economics and to arrive at a blueprint of the economic system that explains how the system responds to institutional change. The blueprint is essentially an analysis of the nature of the production and the social relations surrounding production. It should also be based on a sound theory of knowledge. We shall argue that neoclassical economics is far from such an achievement. Our concern here will be the identification of hidden items in the DNA structure of neoclassical economics.

Specifically, this DNA structure consists of the neoclassical answers to two deeply rooted problems: the inductive problem and the explanatory problem of individualism. The central argument of Hollis and Nell's book is straightforward. We shall argue that every neoclassical research programme is designed:

1. to be consistent with acceptable ways of dealing with the inductive problem (the laws of induction) and to adopt a general empiricism in the pursuit of knowledge; and
2. to provide a methodological individualist explanation of economic behaviour of the economy – that is, one that prescribes rational economic man to be posited as the exclusive locus of decision-making.

The common theme, then – the factor providing the solution to the problem of induction and making methodological individualist explanation possible – is 'rational economic man': the individual maximizing agent.

The discovery of the DNA structure in biology solved two major questions of inheritance: (1) how information is encoded in genes; and (2) how genes are copied. The analogy here is that induction concerns how economic information is encoded for use, and the hypothesis of rational individuals tells us how it is used and passed on. The impetus for Watson and Crick was to find one model that would explain both biological behaviour and the chemical processes, whereas many other contenders tried to tackle the problem from either a purely chemical or purely biological perspective.[11]

The rational individual provides one theory that covers economic decision-making and social processes; regardless of social processes or structures, decisions follow from optimizing. Indeed, this approach is widely applied to sociological questions.[12]

As Foley (2003, p. 9) points out:

> The concept of rationality, to use Hegelian language, represents the relations of modern capitalist society one-sidedly. The burden of rational-actor theory is the assertion that 'naturally' constituted individuals facing existential conflicts over scarce resources would rationally impose on themselves the institutional structures of modern capitalist society, or something approximating them. But this way of looking at matters systematically neglects the ways in which modern capitalist society and its social relations in fact constitute the 'rational',

> calculating individual. The well-known limitations of rational-actor theory, its static quality, its logical antinomies, its vulnerability to arguments of infinite regress, its failure to develop a progressive concrete research program, can all be traced to this starting-point.[13]

We shall argue that, by examining the DNA structure of current neoclassical economics, Hollis and Nell have offered a fresh approach to the understanding of both economic theory and methodology, capable of opening new horizons in econometric methodology and theory.

To paraphrase Boland (1982) and Hollis and Nell (1975), we shall argue that every so-called 'applied model' in neoclassical economics is an attempt to model the essential idea of neoclassical theory – independent individual maximization with dependent market equilibrium. Each model is thus essentially a test of the degree to which neoclassical theory is relevant to real-world phenomena. This must be the case if neoclassical theory is to be testable.

Boland (1982, p. 7) pointed out that:

> Those readers familiar with the view of science advocated by Thomas Kuhn or Imre Lakatos will likely consider the common agenda items to be the 'paradigm' or 'research program' [. . .] The most common example of a paradigm is the maximization hypothesis in neoclassical analysis.

Hollis and Nell, however, argued that assumptions are not enough to make the agenda workable: the 'applicability' of the paradigm has to be demonstrated (for more on Lakatos, see Nell (1998a)).

Furthermore, Boland (1982, p. 9) argued that:

> [. . .] every problem-situation consists of a set of one or more objectives and a set of one or more constraints which impede the attainment of these objectives. However, we must be careful here to distinguish between two different problem-situations. One is the situation facing the individual demander or supplier as hypothesized by the theorist; the other is the situation facing the theorist as hypothesized by the methodologist.

To this we turn next.

1.5 PROBLEMS IN THE FOUNDATIONS OF NEOCLASSICISM

Hollis and Nell begin by examining the methodology embodied in every neoclassical theory or analysis – that is, how neoclassical economists explain the behaviour of the decision-makers in the economy. The rational decision-maker's methodology is the primary topic of their book; it is what underlies virtually all specific neoclassical theories. This is rational choice theory, and although modifications and weaker versions are often used, it can nevertheless be argued that this picture dominates the economists' explanation not only of the economy, but also of their own behaviour with respect to methodology. That is, economists apply the rational choice paradigm to their own behaviour, for example, in choosing models to work with. Economists choose to model consumer behaviour by rational choice, instead of, for instance, drawing on models of social norms or psychological urges – and they argue that this is the right choice because it maximizes the return from their theories.

A consequence of this self-referential dominance of rational choice on the economic theorist's methodology is that it is almost impossible for neoclassicists to see methodology as a problem. The question of what method to employ is itself simply a matter of rational choice – choose the method that will yield the greatest return subject to the constraints. (The 'satisficing' approach is fully consistent with this – maximizing is too costly at the margin.) Thus the methodology practised by neoclassical economists will be the same as the methodology assumed to be the basis of the individual decision-making process. The remarkable unity between these two perspectives – Economists choose the method of rational choice by making what they regard as an optimal choice among possible methods – means that economists tend to regard rational choice as a kind of 'natural' given. It is simply the way we think about economic questions.

However, the authors argue, rational choice, far from being a natural given, is a defective construct, one of the major shortcomings of neoclassical economics. The way rational choice is conceived, and the role chosen for it, which dominates neoclassical theory, both in practice and in its conception of rational decision-making, is based on an inadequate theory of knowledge.

The objective, then, as Hollis and Nell would have put it, is to show that death at the roots kills the fruit on the branches. We turn now to the foundations as problems.

1.6 THE INDUCTION AND DEDUCTION PROBLEMS

The foundations of neoclassical economics, the unseen DNA structure of all neoclassical research programs, consist of two items, which hide two related but autonomous methodological problems. To paraphrase Boland (1982, pp. 16–20), one is the acceptance of the need to deal with the so-called 'induction problem'[14] either directly or, more commonly, indirectly by dealing with its variant, the 'problem of conventions'. The other item is the requirement of 'methodological individualism' – that every explanation must assume that only individuals make decisions, and that they make them rationally.

Boland (1982, p. 32) argued that the two problems are not independent, as the latter's existence depends on its support for the former. As we shall see, it would be hard for most neoclassical economists to give up their reliance on individualism – and their reliance on simple maximizing and rational choice – because that would deprive them of the means to deal with the problem of induction by relying on the convention of individual optimizing behaviour. Indeed, most neoclassical economists take individual optimizing behaviour for granted and thus do not see any problems.

Now let us re-examine Hollis and Nell's discussion of the methodological foundations of neoclassical economics with a view to establishing a clear understanding of the DNA structure of neoclassicism. In particular, Hollis and Nell do not share the acceptance of the hidden items as givens. Instead, they argue that, paradoxically, it is this acceptance that has brought about the numerous theoretical problems that avant-garde economists found fascinating as well as the obstacles that hinder the solutions to these problems. Let us therefore examine closely our first hidden item: the induction problem.

1.7 NEOCLASSICAL DNA: THE INDUCTION PROBLEM

All theories of knowledge must address one way or the other two key epistemological problems, namely induction and deduction. Both problems appear as philosophical themes in Hollis and Nell's book. But we limit our discussion here to the induction problem. Let us recall that, before Karl Popper published his *Logic of Scientific Discovery* in 1934, the inductivist view had been predominant in the philosophy of science.

According to the empiricist view, good scientific practice is characterized first by the unprejudiced observation of facts, presented in the form of singular statements. From sets of these singular statements, universal ones (that is, hypotheses, laws or theories) are inferred inductively. And then, from these, singular statements of facts are again inferred. Thus the link runs from facts to theories and back to facts again for verification.

The fundamental problem with this view – Hume's problem of induction – comes in establishing the truth of universal statements. Just because all observed cases, so far, of A are X does not mean the next one will be. Universal statements are never verifiable, and cannot be validly derived from sets of singular observations. However, they can be falsified. So a theory can never be proven, but it can be rejected as being false. It is on the basis of this logical asymmetry that Popper builds his logic of scientific discovery. Let's look more closely.

David Hume (1888) attempted to define human knowledge and concluded that no theory of reality is possible, since knowledge consists only of experience. Objects of awareness are either impressions (sensations) or ideas derived from the former; meaning and reasoning are therefore related to the associations made between impressions and ideas. Étienne Bonnot de Condillac (1754) similarly claimed that all mental faculties evolve out of sensation. To prove his point, Condillac describes a statue that is alive, like us, but completely covered by marble and hence unable to have any form of sensation.

Hollis and Nell begin by tracing first the alliance between positivism and positivist economics. They provide a sketch of empiricism, of which positivism is the 'best worked-out variant', and promise to show that it is an indispensable background to standard introductory chapters on methodology.

Gilbert (1986b, p. 32) argued that:

> The strongest statement of Popperian or positivist economics is the introduction to the first edition of Lipsey's (1963) introductory economics textbook. Positive economics is concerned with uncovering empirical regularities, and models of this activity are the Phillips curve and the theory of the consumption function. A priori theorizing was accorded little role in the introduction despite the fact that most of the rest of the book took a standard rationalist approach to economic theory.

Gilbert (ibid) went on to argue that:

> Lipsey's position[15] in his 1963 book is clearly extreme and evangelical, but the belief that a positive model is appropriate to empirical economics was, by this time, shared by a high proportion of LSE economists and econometricians. In contrast, the senior economics professor at LSE in the mid fifties was Lionel (later lord) Robbins, who saw little in favor of the econometric approach to economics. The vehicle for this movement at LSE was the staff seminar in Methodology, Measurement and Testing (M2T), set up by Lipsey in 1957 almost in opposition to the Robbins seminar, and which continued to function under this name until Lipsey's departure from LSE in 1963.[16]

Since empiricism is fundamental, it needs an accurate definition. According to Hollis and Nell (1975, p. 4):

> Empiricism is, negatively, the denial that anything can be known about the world a priori or without benefit of experience. The history of the world, as an empiricist sees it, is the story of a series of states.

Empiricists reject the rationalist quest for necessity among truths and inevitability among events. In like manner, individualists reject the social definition of man formulated by medievalists and mercantilists and refurbished by Marx.

Hollis and Nell (ibid.) set out three crucial tenets of empiricism:

(i) Claims to knowledge of the world can be justified only by experience;
(ii) Whatever is known by experience could have been otherwise;
(iii) No statement about the objective world depends for its truth on whether it is believed.

Empiricist philosophy of science cannot allow the existence of any necessity about causal connections. Generalizations can be tested by observing whether suitable instances actually occur. There can be no basic difference in kind between causal laws and confirmed empirical generalizations, even if the title of law is reserved for generalizations especially broad, useful, elegant or suggestive. This may prompt the objection that the citing of causal laws is supposed to explain, whereas generalizations merely describe (Hollis and Nell, 1975, p. 5).

Hollis and Nell (ibid, p. 4) argued that Malthus's laws of population, for instance, even if genuine, could not be treated as 'iron laws' in the sense that they reveal what is bound to happen or that statements of them cannot be denied without contradiction. We have to be able to observe instances of causal connections. Accordingly, in neoclassical thinking, the notion of 'cause' must be analysed in a way derived from Hume. At its simplest, to say that A causes B is to say that A is always followed by B in given conditions. Of course, this takes us a step beyond mere observation; but an inductive licence to generalize from observed correlations to universal ones does not offend the empiricist's insistence on the primacy of observation. Indeed, the empiricist needs to be able to take the step, but having tied his philosophical shoelaces together, falls on his face when he tries.

To the above-mentioned objection, the empiricist replies that there is no ultimate basis for such a distinction. To explain an event, it is enough to cite confirmed generalizations from which the occurrence of the event could have been predicted. Prediction is a sufficient weapon since it is an explanation in advance. Prediction and explanation are two sides of one coin, induction, which buys knowledge of what lies beyond direct observation. To infer is to deduce an instance from a generalization; to explain is to cover an instance with a generalization (Hollis and Nell, 1975, p. 5).

We turn now to positivism. The core of nineteenth-century positivism still retains all its importance although it has been integrated in a more forceful and elegant theory about the meaning and truth of statements: logical positivism. The advance of science now becomes the progressive determination of the truth or falsity of statements, since all claims to knowledge are claims to know whether a statement is true. While this may seem an artificial way of putting it, it clears the deck for the introduction of the great engine of logical positivist epistemology, the analytic–synthetic distinction.

For a logical positivist, all cognitively meaningful statements are of two exclusive kinds, analytic or synthetic. Very roughly, the former are statements of language, the latter statements of fact. More formally, a true statement is analytic if it cannot be denied without contradiction or if its truth arises from the meaning of its terms. It is synthetic if there are possible circumstances in which it would be (or would have been) false (Hollis and Nell, 1975, p. 5).

This analytic–synthetic distinction hides an apparently weak side of empiricism, for at first sight, logic seems to be the only way to know which of an infinite number of possible worlds we live in, in the sense that some truths are both necessarily true and informative about our world (Hollis and Nell, 1975, p. 6). We turn now to the problem with induction in neoclassicism.

1.8 INDUCTION IN NEOCLASSICISM

Induction and deduction constitute the philosophical theme of Hollis and Nell's book. In this section we shall focus mainly on the induction problem and conclude with a short note on the deduction problem.

As an epistemological problem, induction calls for a solution from the logic of validation. A theory of knowledge need not explain how we discover causal laws but it must tell us how we know when we have found such a law. To argue that a hypothesis is rendered probable by being obtained from a theory that has previously proved fruitful is to generate a vicious regress (Hollis and Nell, 1975, p. 75).

The induction problem embodies two implicit assumptions. The first is that empirical knowledge requires logical justification since all knowledge claims must be justified. In other words, knowledge is not knowledge unless it is true knowledge. Second, the justification of empirical true knowledge requires inductive, as opposed to deductive, evidence. Furthermore, as Hollis and Nell argue, this evidence, even when acceptable, always points in more than one direction, and generally confirms several conflicting generalizations (Hollis and Nell, 1975, p. 11).

Methodologically speaking, empiricism asserts that any justification of one's knowledge must be logically based only on experiential evidence consisting of singular observation statements. As a result, any solution to the induction problem requires an inductive logic. In other words, there must be some form of logic which permits the formulation of general statements that draw valid conclusions from arguments consisting of only singular statements.

Now we can use Boland's (1982, p. 14) definition, which explicitly states the problem: 'The problem of induction is that of finding a general method of providing an inductive proof for anyone's claim to empirical knowledge'. Boland (ibid., p. 15) argued that 'an argument of this form is said to be moving inductively from the truth of particulars to the truth of generals. If the induction problem is solved, the true laws or general theories of neoclassical economics could then be said to be induced logically from particular observations'.

Economics authors have in fact rarely confronted this fundamental problem in its starkest form. Yet philosophical definitions of the role of theory partly determine the kind of theory that an economist accepts. Indeed, as Hollis and Nell put it, the most hard-headed economist is secretly a philosopher too. Let us see how the induction problem has actually been handled in economics.

The working methodology of modern neoclassical economics is optimizing, and this is held to license general statements. When such optimizing models are then applied, the result will be a general empirical claim which, of course, is a form of inductivism. But, unfortunately, all too often several different optimizing models can be fitted to the same data – not to mention models that do not rest on optimizing. How are we to select the correct model?

The most commonly adopted methodological position, according to Boland, in effect tries to bypass empiricism, and temporarily puts forward a pragmatic solution, conventionalism, hoping that practical justification of the conventions will be enough. Boland (1982, p. 17) reasoned that 'since this problem is not solvable without an inductive logic, most methodological arguments in neoclassical economics today are about the appropriate way to circumvent the problem of induction'. Unfortunately, this shift to a modified form of the induction problem has led to more complications than those raised by the original problem.

Boland (1982, pp. 17–18) argued that 'the aim of the induction problem was a straightforward, objective, evidence-based proof of the absolute truth of any theory. Contrarily, as we shall see, the aim of the problem of conventions is a choice of the best theory according to conventional measures of acceptable truth'. What do those words mean? Boland (ibid., p. 18) went on to argue that 'without an inductive logic, there is no solution to the problem of conventions; moreover, there are many different measures to choose from, and the measure chosen may not necessarily involve inductive evidence'.

Let us now use Boland's (1982, p. 18) definition of the problem of conventions to state the problem that dominates current economic methodology:

> The problem of conventions is the problem of finding generally acceptable criteria upon which to base any contingent, deductive proof of any claim to empirical 'Knowledge'.
>
> Note that although the inductive problem and the 'Problem of Conventions' differ regarding the nature of the proof required for justification, both require Justificationism. The word 'Knowledge' has been specifically enclosed in quotation marks because one of the consequences of the presumed Justificationism is that 'knowledge' is not (true) Knowledge unless it has been absolutely proven true, and deductive proofs always depend on given assumptions.
>
> Where pure Inductivism requires a final (absolute) inductive proof for any theory, 'Conventionalism' requires only a conditional deductive argument for why the chosen theory is the best available. This poses a new problem. Since we assume because we do not know, deductive arguments always have assumptions. Therefore, the choice of any theory is always open to question. That is, one can always question the criteria used to define 'best' or 'better'. Thus, there is always the danger of an infinite regress.

Boland (ibid.,, p. 20) went on to argue:

> it is unfortunate that the term 'Conventionalism' has been promoted as a pejorative one by Popper and his followers. Many can rightfully object to the apparent name-calling that is implied by the use of such terms as Conventionalist, Inductivist, and Instrumentalist, and the like. Few philosophers today would promote themselves as Conventionalists. But more important, in economics it is very difficult to find anyone who exactly fits one of the molds delineated by Popper. Nevertheless, Popper's methodological categorization does serve a heuristic purpose. Despite the possible entertainment value, we do not wish to label individuals with peculiar philosophical tastes. Our only concern here will be the identification of impersonal items on the impersonal hidden agenda of neoclassical economics.

Ultimately, as Boland (ibid., p. 18) observed, 'the problem of conventions calls for providing a justification while at the same time avoiding an infinite regress and a circular justification – all without an inductive logic!'

Let's recall here that 'conventionalism' is designed to deal with the shortcoming that the mainstream profession does not have a direct solution to the 'problem of induction'.[17] Specifically, versions of 'conventionalism' can be used to provide a philosophical perspective for textbooks or when writing about the history of a given science. For example, Boland (1982) argued that Samuelson uses his form of 'conventionalism' to explain the history of demand theory. We can see how the demand theory has changed over time, each change representing an improvement in generality. In his view, the history of demand theory has culminated in the 'generalized law of demand', which is a mathematical relationship between the slope of the demand curve and the nature of consumers' preferences. Another follower of 'conventionalism', Blaug (1978), in his book on the history of economic thought, utilizes a different criterion. For Blaug, progress is seen in terms of improvements in our ability to mathematize economic theories. Thus Samuelson's models are superior to, say, Marshall's because Samuelson's can be represented by mathematical functions, whereas Marshall's view is based on a rejection of mathematical models.[18]

Thus the first item in the underlying DNA structure of any given neoclassical argument is the induction problem. Indeed, Boland (1982, p. 20) argued:

> the first item on the hidden agenda of any neoclassical article is the Problem of Induction. The agenda item usually appears, however, in its weaker, modified form, as the 'Problem of Conventions'.
>
> But the solution of the Problem of Conventions (and, hence, a circumvention of the Inductive Problem) is taken for granted.

It should be apparent that the problem of Conventions is present in most practical work. Two clues to such presence are suggested. The first clue is

> the absence of references to any theory being either true or false. The reason for this lacuna is that, given Conventionalism, if one were to refer to a theory being true, then it would imply that one has solved the Problem of Induction and thus has the ability to prove the theory's truth. But this would be inconsistent since 'Conventionalism' is only adopted because of the failure to solve the Inductive Problem. So, strictly speaking, Conventionalism precludes any reference to truth or falsity.
>
> The conventionalist ban on the use of the terms true and false would present obvious difficulties even for simple discussions. It would also complicate the use of other terms such as knowing and knowledge, as well as explaining and explanation. This seems to be due to a variation of the presumption of Justificationism. That to know is to have obtained true knowledge and, similarly, to explain is to give true explanation.
>
> Although the ban on using the terms true and false in their literal sense is widely, if tacitly, observed, the terms knowledge and explanation do often appear in the literature. In this case, however, an explanation does not literally mean a true explanation because it is considered to be true only with respect to some accepted conventional measures of approximation. (ibid., p. 21)

Boland (ibid., p. 21) argued that the old debates over the theory of imperfect competition (for example, Robinson, 1933; Chamberlin, 1934; Stigler, 1963) are a case in point. The appearance of the books of Joan Robinson and Edward Chamberlin only few months apart was applauded as a case of simultaneous independent discovery of the same or approximately the same great principle. To Chamberlin it was a crowning achievement which he elaborated for the rest of his career. To Joan Robinson it was obviously something far less. In fact, towards the end of her career she referred to monopolistic-imperfect competition as 'a blind alley'. Boland (ibid., p. 21) pointed out too that 'the concept of imperfect competition is argued by some to be empty or arbitrary and unduly complex'. To paraphrase Boland, we achieve simplicity through an appropriate application of perfect competition or monopoly (for example, Friedman, 1953). The choice between two types of approximation thus becomes the subject of controversy: 'a simplifying approximation which gives more positive results, or a generalizing approximation which allows for a better description of what firms actually do? Without accepted criteria of approximation, this dispute cannot be resolved' (Boland, 1982, p. 21).

Boland's second clue to the presence of 'Conventionalism' is the widespread concern over choosing between competing theories or models. He (ibid., pp. 21–22) wrote:

> most methodological articles and debates have been about the criteria to be used in any 'theory choice'. There is virtually no discussion of why one should ever be required to *choose* one theory! The reason for the lack of discussion of the motivation for 'theory choice' is that the 'Problem of Conventions' is simply taken for granted. A direct consequence of accepting the need to solve the 'Problem of Conventions' is the presumption that any article or essay must represent a revealed choice of a theory and that any such choice can be justified. The only question of methodological interest in this case is how to reveal the criteria used to justify the theory choice.

Before moving on, we need to say a few words on the deduction problem. This problem arises as soon as deduction is given some epistemological part in solving the induction problem – that is, whenever theory is to be applied to the world. Deduction concerns the truths of logic, mathematics and other formal systems like kinship algebra and marginalist microeconomics. It deals with the nature of the necessity that marks such truths, and the way such necessary truths are to be distinguished from others. It also deals with the relationship, if there is one, between necessary truths and matters of fact, and whether this relationship serves as a descriptive function or not. In general, theories are supposed to be developed deductively; but for empiricists generally, and positivists particularly, the analytic–synthetic doctrine implies that deductions are empty. The deduction problem as Hollis and Nell develop it is to explain how theory is developed and how theories can generate deductions that apply to the world.

To end this subsection, a brief summary of Hollis and Nell's approach to the induction and deduction problems seems to be appropriate. In chapters 6 and 7, the authors argue that necessary truths are a priori truths, indispensable for the relations that hold in the world market place. In other words, they adopt an analytic perspective, arguing that theories are axiomatic systems on the geometrical model, which must be neither optional nor empty. Hollis and Nell consider the axioms themselves to be putative necessary truths, rather than mere empirical assumptions or favoured hypotheses.

Having thus claimed a truth for theoretical statements independent of the result of testing against experience, and having rejected the logical positivists' account of necessary truths, we shall treat causal laws almost as if they were theorems in applied geometry. Their solution to the induction problem will be, in general, that, without assumptions about continuity in the world, scientific knowledge is impossible and, in particular, that a correlation is an instance of law only if there is a theoretical explanation of its significance.

This approach will, we hope, lead to the rejection of some kinds of economic theory and the acceptance of others. But since we must first overthrow some renowned theories of knowledge and then present Hollis and Nell's rationalist theory of knowledge before arguing the merits of their approach, we shall withdraw, as Hollis and Nell put it, into 'enigmatic silence for the time being'. We now turn to the second hidden item.

1.9 THE EXPLANATORY PROBLEM POSED BY INDIVIDUALISM

For Hollis and Nell, the success of positivism in economics means the success of Utility. Man, illumined by the Enlightenment and anatomized by the utilitarians, was an individual bundle of desires (Hollis and Nell, 1975, p. 480). It is not a mere historical accident if positivism is so attractive.

In Hollis and Nell's (1975, p. 47) words:

> The primrose path is paved with good intentions. The positive economist intended to discover empirical economic laws by testing the implications of his theories against the facts of the world. But he found that this meant rejecting good theories for bad reasons. So he refined his methods by offering instead to test implications against the true values of variables, as measured when ceteris were paribus. Disconcertingly, this left him unable to distinguish the failure of his predictions from the failure of his ceteris paribus conditions or the incorrect adjustment of his observations.

Indeed, the primrose path is inviting. But it leads to the everlasting bonfire. It was necessary to qualify the predictions at first, as economics is only one among several other social sciences, each trying to define its own space.

Even within economics, Hollis and Nell argued, predictions may still fail for reasons that do not normally refute their corresponding hypotheses. For instance, interview and questionnaire data, confirmed by observation, indicate that businesses, both in practice and as a matter of policy, often do not go on investing until the anticipated rate of return has fallen to the current level of the rate of interest. In mainstream economics this has been taken to show merely that not all businesses are fully rational. But Hollis and Nell think the problem is deeper: businesses do know what they are doing; the problem is that the terms of the model don't fit the way business is conducted. Hollis and Nell implicitly emphasize the necessity of fieldwork in economics as an essential way to identify people's real motivations and activities (Hollis and Nell, 1975, p. 53).

A similar proposal had already been made by Haavelmo (1958, p. 355) in his presidential address to the Econometric Society, when he complained that the 'interest of econometricians in the field of pure economic theory had been more in 'repair work' (meaning in the positivist tradition of testing theories), rather than in developing the fundamental economic ideas themselves'. He argued (ibid., p. 357) that 'if we could use explicitly such variables as people's own predictions about prices or incomes or about the effects of their actions, more accurate relations with more explanatory power could be established'.

As stated earlier, understanding people's own ideas is referred to among anthropologists as 'fieldwork'. Nell (1998a) argued that fieldwork allows us to identify the way work is defined and structured. Fieldwork requires 'participation', because it is direct experience of the social practice that allows the observer, through his interaction with other participants, to check the meanings and appreciate the nuances. The observer can thus link language and description to behaviour. That's why 'Verstehen' as a method is widely appreciated by realists among economists. Indeed, realism can only be verified by fieldwork. Nell (ibid.) has argued further that fieldwork should interact with conceptual analysis to produce a method by which to approach economic issues. (By contrast, pragmatism is unable to supply a coherent account of theoretical concepts, especially in relation to empirical work.) We shall argue in Chapter 11 that macroeconometric model building needs just such a method by which to approach empirical work in economics.

Now, in contrast, let's sum up the mainstream methodology. There is no room for fieldwork or conceptual analysis. Theories are composed of definitions, assumptions and hypotheses. Hypotheses assert relations between variables. The validity of hypotheses depends on solving or circumventing the problem of induction. Behavioural economic variables apply to an economic agent, none other than rational economic man offered by positive economics, as in the phrase of the book title. Economic hypotheses were not to be rejected for non-economic reasons. In other words, economics does not study man in general but only economic man. Given rational behaviour and ceteris paribus, the predictions apply to the true values of variables. One of the ceteris paribus clauses requires that the agents whose behaviour is to be predicted be rational. Rational economic man is both the average and the ideal, abstracted from actual marketers with the aid of general assumptions about human desires. The true values of variables are those derived from the actions of a rational agent in given circumstances – and this (conventionally) solves or evades the problem of induction.

The rational individual is central to this approach, and we need to understand just what this agent does and why. This brings us to methodological individualism.

1.10 NEOCLASSICAL DNA: METHODOLOGICAL INDIVIDUALISM

The phrase owes its popularity to Popper; as a research programme, methodological individualism has been identified by Blaug (1980, p. 266, quoted by Boland, 1982, p. 28) as 'the view that social theories must be grounded in the attitudes and behaviour of individuals, as opposed to methodological holism, which asserts that social theories must be grounded in the behaviour of irreducible groups of individuals'. This is the second main item of the DNA structure of neoclassical economics. For further reference we use Boland's (1982, p. 28) definition of methodological individualism: 'Methodological individualism is the view that allows only individuals to be decision-makers in any explanation of social phenomena'. Explanations have to be generalizations, universal statements. So individuals must make decisions that can be generalized: in these circumstances the individual will always do such-and-such. The reason is that such-and-such is the rational thing to do. It is how any rational agent will behave. This then provides a basis for projecting the generalization – a justification for the convention that optimizing behaviour supports universal statements.

But it also raises a number of new issues. Right away we face the problem of deduction. Deductions are analytic statements – they are empty. So, as Hollis and Nell ask, why does 'this is what a rational agent will do' have any greater power than 'this is what a CIA agent will do'? The latter is a deduction from a general statement about what CIA agents do, and such general statements face the induction problem. Why don't statements about what rational agents do face the same problem?

Of course, we do think statements about rationality have greater force than ordinary generalizations, because they can be proven. That is what optimizing models show. But this cannot be claimed in the context of empiricism and positivism, where deductions are simply empty analytic statements. If claims of rationality are to mean anything, they have to be conceptual truths. That means we have to admit the category of conceptual truths, and this opens the door to all sorts of things neoclassical theory wishes to exclude.[19]

Within methodological individualism, explanations do not refer to non-individualist decision-makers such as institutions. Boland (1982, p. 28) argued that 'from the viewpoint of methodology, we need to examine the reasons why methodological individualism is a main item on the neoclassical economics agenda'.[20] Why is it claimed, in effect, that only individuals are real[21] – that institutions are constructs out of the behaviour of individuals – and that only the rational decisions of individuals count as 'true values' of decision variables?

There are more complications here than might at first appear. To paraphrase Boland (1982, pp. 28–9), the case is often presented as if there were a built-in dichotomy, allowing only two exclusive options: 'methodological individualism' versus 'methodological holism'. Given the 'individualism–holism dichotomy', the reasons for promoting methodological individualism could be simply negative – holism promotes a multiplicity of hard-to-authenticate entities.[22]

But other reasons exist for insisting that only individuals are basic, grounded in our perceptions. We can see and hear and touch other individuals; we cannot see, hear or touch institutions or the forces of history. It is perhaps a residue of materialism to insist that what is real is what is directly perceptible to the senses. What individuals do, however, when they are acting responsibly and with full knowledge, is what is in their best interests, rationally speaking. Of course, they often act foolishly or 'without thinking'. But such actions are accidental; their true actions are rationally chosen. (Of course, it is just this sense of 'rational' that is inconsistent with empiricism in general and the analytic–synthetic distinction in particular – the statements ruling out certain actions or classes of actions as 'not rational' will not be analytic.)

1.11 INDIVIDUALISM AND PSYCHOLOGISM

Boland argued that Pareto suggests some related reasons for basing neoclassical economics on methodological individualism:

> All human conduct is psychological and, from that standpoint, not only the study of economics but the study of every other branch of human activity is a psychological study and the facts of such branches are psychological facts. The principles of an economic psychology can be deduced only from facts. A very general view of common well-known facts gave English writers the concept of a 'final degree of utility', and Walras the concept of 'rarity'. From the examination of the facts we were led, by induction, to formulate those notions. (Pareto, quoted by Boland, 1982, p. 27)

This comment connects psychology to induction. We have already argued that the connection between the induction problem and methodological individualism[23] is that the latter can be considered as a research programme aiming to provide a long-term solution to the former.

Furthermore, to paraphrase Boland (1982, pp. 30–31), since non-individualist and non-natural exogenous variables are proscribed we argue that, according to Hollis and Nell, the specification of an appropriate conception of the relationship between 'institutions' and 'individuals' is the main epistemological obstacle that neoclassicism theories of economic behaviour have to face. The existence of institutions poses an explanatory obstacle regardless of the prescriptions of psychologism.

Boland (1982, p. 31) argued that:

> On the one hand, social institutions are consequences of decisions made by one or more individuals. On the other hand, individual decision-makers are constrained by existing institutions – indeed, individuals are educated and socialized by institutions. If any given institution is the result of actions of individuals, can it ever be an exogenous variable? That is, how can institutions really be constraints, if they are shaped by individuals? But if institutions shape and limit the choices facing any individual, and shape the individual as well, are the individual's choices really free? If any institution is a creation of groups of individuals, can it have aims of its own or must it merely be a reflection of the aims of the individuals who created it?

He went on to argue (ibid.) that 'these questions are seldom discussed in the economics literature because the psychologism of Mill or Pareto is widely taken for granted'.

According to Boland (ibid.):

> Methodological individualism alone leads to two primary methodological requirements. First, no institution can be left unexplained and, moreover, every institution must be explained in individualist terms. Second, institutions must always be responsive to the choices of every individual. The first requirement begs a fundamental methodological question about the existence of a set of acceptable givens which would constitute a successful explanation. The second raises the thorny question considered in Arrow's impossibility theorem.[24]

Neoclassical economic man as an individual is a descendant of utilitarian ancestors who is endowed with sovereignty. He is to be first studied in isolation from other individuals and from the institutions surrounding him. According to this view, the combination of social atoms determines the behaviour of social molecules. On this view, economic man may be defined apart from his social setting. Individuals are 'given'; they are not shaped and trained through social practices in any important sense that must be taken into account; they are endowed with knowledge and skills – it is not necessary to consider how these are passed along from generation to generation. The social setting, whatever it is, arises without difficulty from the combined choices of individuals; it is not necessary to consider how it is supported or maintained. Such individuals, pre-social utilitarians, seem to Hollis and Nell to be fictions of the enlightened liberal imagination. Yet these economic agents must be considered the essentially individual bearers of economic variables (Hollis and Nell, 1975, pp. 264–5).

It is commonly accepted that all explanations require some givens (for example, some exogenous variables) whose specification is probably the most informative theoretical assertion in any theoretical model. Boland (1982, p. 32) argued that 'for neoclassical economics, the presumption of psychologism conveniently restricts the list of acceptable givens. Given the psychologistic individualism, the irreducible givens are identified as the psychological states of the individuals in society'. This was commonly assumed in early neoclassical theory.[25]

Such versions of neoclassicism were based on a reductive version of methodological individualism – specifically, one that identified the individuals with their exogenous psychological states (such as their given utility functions). The strict reliance on the reductive version – that is, on psychologistic individualism – always presents a general problem of explanation that we shall call the problem of simple psychologistic individualism: if everyone is governed by the same laws of psychology, then there is no psychological basis for individuality.[26] To avoid psychologism – and to stick to observables, eschewing 'mental states' – later versions looked to behaviourism. 'Revealed preferences' replaced utility. But the revealed preferences had to reflect true choices, and not actual behaviour. This, of course, raises the problem of induction again: how do we know a true choice from an accidental one? If it is because true choices are rational, then how do we explain 'rational'?

It is tempting – and normally done – to endow agents with substantial powers of foresight and clarity, so they do not make mistakes or fail to carefully consult their utility functions. But then how do we relate these paragons to the agents of the real world? If we simply compare the predictions of the model with the data, the best we can get is a match. To call these grounds for supporting the theory is the fallacy of 'affirming the consequent'.

Neoclassical theory restricts the laws of psychology and/or the laws of behaviour to a single law that specifies that everyone faces diminishing marginal utility (or its equivalent). This solution allows people to have different utility functions, or preference maps, and contributes to managing both the general explanatory problem of methodological individualism and the 'problem of conventions'. The only models allowed by the reductive methodological individualism of neoclassical economics are thus those that exclude all variables except psychological or behaviour states and natural givens.

As Hollis and Nell point out, either as psychology or as stylized behaviour, this is appallingly unrealistic. It does not allow for learning what we really think or feel, as we grow older and wiser or experience trial and error. It is as though it is the easiest thing in the world to live up to Socrates' dictum, 'Know yourself'. Nor is there provision for changing one's mind, or for being in 'two minds' about a serious decision – 'my inclinations say one thing, my sense of duty another'. Nor is there any account of how knowledge and skills have been acquired or how they are maintained. Yet all of these features can be seen in the day-to-day conduct of businesses and households.

1.12 PROVISIONAL CONCLUSION

The DNA of neoclassical economics is defective. Neither the induction problem nor the problems of methodological individualism can be solved within the framework of neoclassical assumptions. The neoclassical approach is to call on rational economic man to solve both. Economic relationships that reflect rational choice should be 'projectible'. But that attributes a deductive power to 'rational' that it cannot have consistently with positivist (or even pragmatist) assumptions (which require deductions to be simply analytic). To make rational calculations projectible, the agents may be assumed to have idealized abilities, especially foresight; but then the induction problem is out of reach because the agents of the world do not resemble those of the model. The agents of the model can be abstract, but they cannot be endowed with powers actual agents could not have. This also undermines methodological individualism; if behaviour cannot be reliably predicted on the basis of the 'rational choices of agents', a social order cannot reliably follow from the choices of agents.

We can put our claim in the form of a dilemma:

1. Either economic agents and activities are conceived in such a way that the neoclassical assumptions are sufficient to entail the vision of optimality resting on the two critical theses, in which case the model cannot, in principle, apply to the world in which agents are brought into being and trained in the social context of functioning institutions that have to be supported and maintained by carrying out productive activities that depend on our present laws of physics and engineering; or
2. Economic agents and activities are conceived in a manner consistent with regular reproducibility, in which case the model can apply, but the door is wide open to disequilibrium and sub-optimality – adulteration in the product and exploitation in the factor market are both conceivable, even likely, as the result of optimal decisions; unemployment and fluctuations may be widespread, optimality will be a farce, and according to the authors, there may be a warm welcome to both Veblen and Marx.

Let's consider how to complete the argument: rationality as rational choice is inadequate. It cannot justify induction and it cannot serve to construct social and economic relationships on the basis of methodological individualism. What is needed to rebuild econometrics is realism in theory, which must be based on a rationalist approach that seeks for foundations in conceptual analysis combined with fieldwork. This generates an interactive program in which reliable relationships between realistically defined variables can be specified – and improved – on the basis of conceptual and empirical investigations. These can then be estimated and tested against data, drawing on Haavelmo's (1943a; 1944) and Klein's (1950) original econometric approach and using many standard econometric techniques. That is the programme, but to carry it forward will be the object of Chapter 2.

NOTES

1. Sandmo (1987, p. 580) argued that 'Haavelmo turned away from econometrics to economic theory as his main field of interest. In his 1957 presidential address to the Econometric Society he emphasized the need for a more solid theoretical foundation for empirical work as well as the need for theory to be inspired by empirical research'.
2. Nell (1998a) argued that fieldwork means finding out what people actually do, how they think and behave, and what they mean when they say something. For an account of the fieldwork approach see Chapter 10 of this book.
3. For an account of Spanos's methodology, see Chapter 9.
4. A semantic approach is foremost a tool for conceptualizing and understanding scientific theories. It has been applied to economics in a variety of settings (for example,

Hamminga, 1983; Stigum, 1990). For a recent discussion of the semantic approach see Suppe (2000). For an account of Davis's approach, see Chapter 9.

5. For a critical account of Swamy et al. (1985), see Boland (1985) and Pesaran (1985).
6. Haavelmo (1944) is widely judged to have provided the foundations for present day structural econometrics. Morgan (1990a, ch. 8) offers an in-depth account of Haavelmo's contribution to econometrics. While Haavelmo's place in the history of econometrics has been applauded by many (see Epstein, 1987; Morgan, 1987; Malinvaud, 1988; Hendry et al., 1989; Spanos, 1989; Nerlove, 1990; Anderson, 1991; Heckman, 1992; Qin, 1993; Davis, 2000), except for Spanos (1989) and Davis (2000), few new methodological insights have been drawn from his work.
7. For further details on Hollis and Nell' s (1975) approach, see Errouaki (2004).
8. In the Anglo-Saxon mainstream tradition, Boland (1982) was the first to use the term 'cross-sectional' to distinguish his approach from the traditional approach to economic methodology, which he calls 'time-series' analysis. However, it was actually Hollis and Nell (1975) who first applied the cross-sectional approach to the study of economic methodology. In Europe it was the French economist Carro (1981), who used first the idea of a 'cross-sectional approach' in economic methodology. He applied Bourdieu's (1984) conceptual apparatus and Kuhn's (1970) structure of scientific revolutions to the study of the production of economic knowledge.
9. Traditionally, methodology has been discussed only in the context of the history of economic thought – that is, in the context of the views of past methodological disputes. Viewed that way, methodology has often appeared to be of little relevance for the everyday concerns of economic theorists. Heilbroner and Milberg's (1995) book dramatically breaks with this tradition by focusing on the methodological problems as reflected in the vision of each school of thinking. By examining the economic vision of each school, it offers a fresh approach to the understanding of the crisis of modern economic theory.
10. Metaphors are used in economics all the time. Mainstream economists may think that metaphor has no place in economics. Smith's 'invisible hand' is arguably one of the best-known metaphors. Economic model and the idea of market are both metaphors. The DNA metaphor is used here as an idea generator. Even econometrics and mathematical economics have their metaphors (see Leamer, 1987; McCloskey, 1983; 1985a; 1985b; Mirowski, 1989a; 1994). Harré (1986) argued that the use of metaphor is actually common in all sciences.
11. Watson and Crick proposed a double helical structure for DNA in 1953. For this work, Watson and Crick, together with Wilkins, were awarded the Nobel Prize in Medicine in 1962. After Watson and Crick published their work on the double helix model, it took quite some time before it became widely accepted. The model was so elegant that it deserved to be correct, many of their colleagues agreed, but it took more years before the evidence was assembled. After 7 long years, messenger RNA was discovered and genetic research gathered pace. For more details, see Watson's (2004) book on *DNA: The Secret of Life*. In contrast to the case of biology, in economics, over a century of empirical work has failed to provide convincing, replicable empirical studies supporting the major neoclassical models (Lawson, 1997). Thanks to Professor Federico Mayor Zaragoza for inviting the authors to the Forum BioVision (which celebrates the 50th anniversary of the discovery of DNA and gathered 12 Nobel Laureates), held in Lyon in April 2003. The authors are also grateful to professors Federico Mayor Zaragoza and James Watson for clarifying several issues related to the conceptual foundations of molecular biology.
12. This point was clarified to us in correspondence with Dagum.
13. For an in-depth examination of rationality and economic complexity see Foley (1998). For an account of rationality in macroeconomics see Malinvaud (1995).
14. The problem of induction will be further discussed in Chapter 3. For an account of the Induction and the Empiricist Account of General Laws see Chapters 4 and 5 of this book. For further details see also Nell and Errouaki (2008b).

15. Lipsey's current work is moving in a significantly new direction, following a path very similar to that of transformational growth. He has been examining the way innovations change the character of firms and the working of markets. For an account see Lipsey et al. (2006).
16. Gilbert (1986b, p. 32) observed that 'Kurt Klappholz was one of the staff members who attended M2T, and he introduced Joseph Agassi, one of Popper's graduate students, to the seminar members. Over about half a year, Lipsey notes, they learned, and came to accept, much of Popper's views on methodology'. For further details see de Marchi (1988).
17. Boland (1982) argued that a weak form of conventionalism can be seen in appeals to 'rhetoric', or to the judgement of informed professionals in the field. Well informed professionals in Wall Street accepted the models of risk analysis that set the stage for the crash. In general, as a criterion for justifying claim to knowledge, however provisional, 'persuasion' is not persuasive.
18. For an account of how economics became a mathematical science, see Weintraub (2001). For an account of how mathematical economics is unreasonably ineffective, see Velupillai (2005). For a comprehensive discussion of the formation of economic science as engineering, see Armatte (2010).
19. For further details, see Chapter 10 of this book and also Nell (1998a, part I), where we find the discussion, for example, of 'humans are potentially rational animals'.
20. Boland (1982, p. 28) argued that 'unfortunately, the reasons are difficult to find, as there is little methodological discussion of why economics should involve only explanations that can be reduced to the decision-making of individuals – except, perhaps, for Hayek's arguments for the informational simplicity of methodological individualism'. For further details see Boland (ibid., ch. 2).
21. 'Society does not exist', Margaret Thatcher famously proclaimed, in answer to a question about social theory and policy. For more details see Nell (1996).
22. The social–philosophical basis of neoclassical economics is dominated by the eighteenth-century anti-authoritarian rationalism that puts the individual decision-maker at the centre of the social universe. A rejection of individualism would be tantamount to the advocacy of a denial of intellectual freedom. One can also, of course, point to obvious questions of ideology (Heilbroner, 1966; Foley, 1989), but as an explanation this only begs the question at a different level.
23. Boland argued that Pareto's comments reveal another aspect about the relationship between inductivism and individualism in neoclassical economics. For further details see Boland (1982, p. 30).
24. The issue referred to here is whether choices made by an institution can be rationalized in the same manner as we rationalize an individual's choice. For further details see Arrow (1974).
25. Boland (1982, p. 30) argued that 'psychologistic individualism is the version of psychologism which identifies the individual with his/her psychological state. Whereas "psychologism" is the methodological prescription that psychological states are the only exogenous variables permitted beyond natural givens (e.g., weather, content of the Universe, etc.)'.
26. The methodological view that there is but one permissible set of exogenous variables to which all successful explanations must be reduced is called, by French philosopher Thuillier, 'reductionism'. Blaug (1980) has characterized Popper's methodological individualism as an example of a reductionist research programme. Theorists are bound to explain away any non- individualistic variables, or any macroeconomic propositions that cannot be reduced to microeconomic ones.

I think the most important single development was really the Haavelmo view that we should relate the probability structure to the economic theory structure. The concept of specifying a model with a stochastic expression built directly into it and moving from the probability distribution of the random errors to the probability distribution of the economic quantities *is a very powerful way of thinking about the system.*

Klein (1987, in Mariano, 1987, pp. 409–60, italics added)

What I am here calling *'the probability approach' is simply adhesion to the principle that inference from a sample of observations must proceed within, and be judged with respect to, a prespecified stochastic model that is believed to correctly represent the generation of the data.*

The most influential text at the time seems to have been T. Haavelmo's memorandum, whose manuscript was available in 1941, that was certainly discussed in the New York weekends (at the New School international seminar) and that was published in 1944 under the title 'The Probability Approach in Econometrics'. The memorandum indeed *considers quite carefully the role of the stochastic specification and the problems raised by its choice*.

Malinvaud (1988, pp. 204–205, italics added)

I think it is not unfair to describe the major part of existing economic theory in the following way. We start by studying the behavior of the individual under various conditions of choice. Some of these conditions are due to the fact that the individual has to have contact within his economic affairs with other individuals. We then try to construct a model of the economic society in its totality by the so-called process of aggregation. I now think this is actually beginning at the wrong end.

Speaking very briefly and along very broad lines, *I think that economic theory could make progress by an approach within the following framework.*

Starting with some existing society, we could conceive of it as a structure of rules and regulations which the members of society have to operate. Their response to these rules as individuals obeying them produces economic results that would characterize the society. As the results materialized they will stimulate the political process in society towards changing the rules of the game. In other words, the results of the individuals in a society responding in a certain way to the original rules of the game have a feedback effect upon these rules themselves. From the point of view of economic theory and econometrics it is meaningless to consider these rules of the game, formed by the feedback effect I mentioned, as independent variables.

Haavelmo (Nobel Lecture, 1989, italics added)[1]

The first systematic attempt to introduce the Fisher–Neyman–Pearson (F–N–P) approach into econometrics was made by Haavelmo (1944). In this classic monograph he argued fervently in favor of adopting the new statistical inference, and addressed the concern expressed by Robbins (1935) and Frisch (1934) that such methods were only applicable to cases where the data can be viewed as 'random samples' from static populations.

The Haavelmo (1944) monograph constitutes the best example of viewing the confrontation between theory and data in the context of bridging of the gap between theory and data, where both the theory and the data are accorded 'a life of their own'. As argued in Spanos (1989) it contains a wealth of methodological insights, which, unfortunately, had no impact on the subsequent developments of econometrics.

The part of Haavelmo's monograph that had the greatest impact on the subsequent literature was his proposed technical 'solution' to the simultaneity problem. This was considered a major issue in economics because the dominating theory – general equilibrium – gives rise to multiequation systems, known as the Simultaneous Equations Model (SEM).

Spanos (2010, in Mayo and Spanos, 2010, pp. 229–30, italics added)

2. Haavelmo reconsidered as rational econometric man

INTRODUCTION

We argued in Chapter 1 that there are good reasons for considering Hollis and Nell's (1975) framework as a foundation for reconstructing structural econometrics, a foundation that complements and extends the original ideas of Haavelmo. Haavelmo's (1944) work is probably the most important landmark in the history of econometric modelling. It is a remarkable monograph which, unfortunately for econometrics, became a classic much too early, part of the reason it is misunderstood. But our argument will show in detail that Hollis and Nell's (1975) approach complements Haavelmo's (1944) methodological framework. Then, more speculatively, we shall suggest that their work actually extends the methodology in ways that help to meet some of the widely prevalent objections to structural econometrics. We should point out that Haavelmo (1958) seems to have been the germ of our argument. Let's recall here that Haavelmo has argued that weak theoretical neoclassical economic foundations rendered suspect the policy value of most econometric models.

2.1 HOLLIS AND NELL'S ALTERNATIVE VISION

We argued in Chapter 1 that Hollis and Nell's quarrel with neoclassical economics is based on two points. First, the neoclassical approach concentrates on market interdependence, neglecting the deeper technological interdependence. But this last turns out to limit the possibilities of substitution compatible with the assumed givens, and it calls for explaining the development and maintenance of agents, undermining the approach. Second, they argue that neoclassical economics, with its emphasis on abstract rational choice, ignores institutional and especially class relationships, thus misrepresenting the nature of payments to factors and neglecting the economic significance of power and conflict in societies.

The authors recognized that neither complaint was new. Hobson made the first, Marx the second and many others have since added elaborations.

By contrast, the classical–Marxian view is based on technological interdependence between industries and class relationships between families or persons (Hollis and Nell, 1975, p. 3). The neoclassical approach misrepresents the nature of income payments; they are not exchanges on the same footing with sales of final goods. This is made clear in the flow-of-funds diagram drawn by Hollis and Nell in their introduction (ibid., fig. 2, p. 15). The far-reaching implications of neoclassicism's treatment of income payments has recently been underlined in the work of Godley and Shaikh (2002), who show that this misrepresentation underlies the orthodox approach to money.[2]

Yet this economic quarrel was not the centre of their book and the first bone they picked was philosophical. They disputed not only the positivist doctrines behind orthodox methodology, but also empiricism in general. At first glance empiricism might seem to underlie econometrics; this is seriously wrong. Econometrics depends crucially on 'priors' – for lists of variables and parameters, for specification and identification – and these priors are not obviously 'empirical' in the philosophical sense. Indeed, they cannot be, according to Hollis and Nell, who argue that rationalism provides a better understanding of the role of priors in econometrics.

The authors attacked the influential alliance between positivism and neoclassical economics (Hollis and Nell, 1975, p. 3). In the early part of their book Hollis and Nell advance a largely philosophical critique – to the effect that neoclassical theory cannot be effectively tested because of the peculiar role played by the rational agent. Then in the later chapters they survey the wreckage of the neoclassical ship in search of fresh criteria. They consider revealed-preference theory as a philosophical thesis about the explanation of economic behaviour and suggest an analysis of the concept of action that favours linear programming models against revealed-preference models. But the use of linear programming models needs to be supported with a fresh theory of knowledge, and in further arguments, having rejected pragmatism, they deploy their own solution to the problem of a priori knowledge. Programming models of all kinds are shown to be essentially prescriptive.

They certainly have the power of rationality behind them; but it cannot be deployed on behalf of description or prediction. Production models, on the other hand, are descriptive, but they describe the way things ought to work, given the commitments made by the agents of the system. This paves the way for dealing with the induction and deduction problems together with an axiomatic approach to theorizing that favours programming and production models against predictive models.

Finally, in the last chapters they claim philosophical merit for classical–Marxian economics, coming close to asserting, in the spirit of the axiomatic method, that economics is essentially about relations of production, for production models show how the system supports and maintains itself and its agents. In what follows we shall focus mainly on the arguments that Hollis and Nell have given us in regard to the way economic concepts are related to the real phenomena of the economy.

2.2 TEXTBOOK ECONOMETRIC METHODOLOGY

Spanos (1989, p. 406) argued that 'a retrospective view of the founding period as well as a re-examination of Haavelmo's probabilistic approach in econometrics, can provide insights into the weaknesses of the textbook econometric approach and suggest possible modifications' that might help save what is valuable in the programme. In particular, following Hollis and Nell, we shall suggest that part of the problem is a failure to specify relationships in realistic terms, where 'realism' is based on fieldwork and conceptual truths.

Spanos (1986; 1989) argued that the textbook econometric methodology was formalized during the late 1950s and early 1960s, but as it has failed to live up to the expectations of modern econometricians in the late 1970s and early 1980s, it has been increasingly under attack since then.

The practice of the time is succinctly summarized by Pagan (1984, p. 103) as follows: 'A model postulated, data gathered, a regression run, some t-statistics or simulation performance provided and another empirical regularity was forged'. Considered as a way to quantify theoretical relationships and test theories, this approach to econometric modelling seemed to fail; very few of the specified and estimated empirical relationships survived the test of time – many could not even be replicated. Moreover, theories have typically been revised rather than discarded and replaced when confronted with contrary empirical evidence.

Not surprisingly, under these circumstances, Spanos (1989, p. 405) observed 'a growing discontent with econometrics has been accompanied by a growing interest in developing alternative approaches to empirical modelling'. Several have been proposed. These include the Box–Jenkins techniques (Box and Jenkins, 1970; Naylor et al., 1972), the rational expectations (Lucas, 1976; Lucas and Sargent, 1981), the vector autoregressions approach (Sims, 1980a), Leamer's Bayesianism approach (Leamer, 1978) and the dynamic specification approach (Hendry, 1995a).

Spanos (1989, p. 406) argued that the methodology proposed by Haavelmo includes 'important elements which have either been discarded or have never been fully integrated within the textbook approach'. By reconsidering these elements within Hollis and Nell's framework, we make here a case for an alternative methodology that remains true to Haavelmo's initial vision.

Hollis and Nell's implicit proposal consists of an econometric modelling strategy where important roles are equally assigned to economic theory (based on a rationalist theory of knowledge) and the structure of the observed data chosen combined with the fieldwork approach. Furthermore, a distinguished British econometrician named Johnston (1963 [1984]) argued that it is of great importance in econometric modelling to have as much knowledge as possible of what Klein called the institutional realities of the situation. Indeed, Klein (1982) has emphasized the importance of institutional structure in econometric modelling.[3] He has argued that institutionally-based equations (or in Nell's 1998a terms, models of structure) complement behaviourally-based equations (in Nell's 1998a terms, models of behaviour) in making up the structure of an economy. Both are necessary and models have to refer to what exists. However, most so-called empirical work today is based on number crunching. Fieldwork helps us discover what exists; it does not result in scientific theories, let alone covering-law explanations.

Johnston (1963 [1984], p. 500),[4] in a study of cost–output relationships in coal mining, 'felt it useful to don a safety helmet and work his way through the narrow and twisting seams of a Lancashire coal field in order to see at first hand the nature of the production process before sitting down to pursue the statistics at the regional headquarters of the National Coal Board'. Johnston used fieldwork to understand the mining sector, studying it in its own terms, and then translating those terms into the observer's language.

Klein's methodological structuralism[5] and Nell's transformational growth (TG) framework, which rests on what might be called methodological institutionalism, interacting with fieldwork, provide a method by which to approach macroeconometric model building. This is indeed a kind of structuralism, in that the institutions, which are defined by formal relationship, do in a sense generate the observed phenomena (Nell, 2004). Nell (1998a) argued that knowledge of the functioning of these institutions requires fieldwork or can be gained partly from what Klein (1982, p. 26) calls 'experience in economic life and partly from being made aware of statutes, business practices, or voluntary agreements', all of which make up what Swann (2008) calls the 'vernacular knowledge'.

Furthermore, Klein (1982) points out that it is necessary first to possess the facts about the functioning of the economy in a purely descriptive sense before going on to translate this knowledge into design of econometric structure. It is valuable information for supplementing statistical estimations of coefficients. Epstein (1987) adds:

> This approach is a striking parallel to Marschak's call for interviews with business people in order to clarify specification of the investment function. The difficulty in each case was that satisfactory a priori knowledge was seldom available to make compelling identification of behavioral explanations linking economic aggregates. The early econometricians were agreed that such knowledge was a prerequisite for rational policy formation. (Epstein, 1987, p. 179)

This approach reflects Haavelmo's (1943a; 1944; 1958; 1989) econometric thinking and it is superior to that advocated by pragmatism,[6] which cannot give a coherent account of theoretical concepts, especially in relation to empirical work (and by contrast leads, to use Nell's expression, to 'armchair empirical work'). But fieldwork may be directed towards economic structure or towards behaviour. Indeed, the idea of fieldwork is discussed implicitly and eloquently by Haavelmo (1958, pp. 355–7) in his Presidential address to the Econometric Society. Since it is fundamental to the argument we shall quote it at length:

> It is probably fair to say that the interest of econometricians in the field of pure economic theory has been more in the direction of the 'repair work' that I spoke of, rather than an interest in the fundamental economic ideas themselves.
>
> The idea of bringing econometric thinking into theory at an early stage is not merely that of being able to throw out theoretical schemes which are unrealistic in a narrow sense. We must learn to think of facts to be explained in a broader sense, as things that could be facts even if they are not at present.
>
> The economic relations that we have been trying to establish and confront with facts are mostly of the following nature: Their starting point is some notion of permanent preference schedules. Then there are the various constraints upon choice, as visualised by the economic unit making economic decisions, the actual knowledge or belief regarding technological possibilities, the expectations of prices, and so on. Then there are the links between subjective conditions or constraints and the objective facts on which the decision makers presumably base their information. Finally, there is the question of the relations between decisions taken and actions actually carried out. From this veritable maze of interrelations our customary economic theory extracts some would-be 'net' relations between statistically observable data of prices, quantities, etc., in the economy. The only trace left of the whole 'background structure' will then be the presumably constant parameters of the 'net' relationships derived. At this final stage the thread between the original, hypothetical invariants of the theory and the derived relationships between market variables has indeed become long and thin.
>
> I think most of us feel that if we could use explicitly such variables as, e.g., what people think prices or incomes are going to be, or variables expressing what people think the effects of their actions are going to be, we would be able to establish relations that could be more accurate and have more explanatory value. [. . .] It is my belief that if we can develop more explicit and a priori convincing economic models in terms of these variables, which are realities in the minds of people [. . .], then ways and means can and will eventually be found to obtain actual measurements of such data.

Although Haavelmo doesn't speak explicitly in terms of 'fieldwork', we could interpret his econometric thinking as pioneering the advocacy for the fieldwork approach in econometric modelling. This way, econometrics as a 'unified framework' will go beyond what Haavelmo called 'repair work' upon the logical consistency of theories as submitted to econometricians in verbal or fragmentary mathematical form.

Furthermore, Nell (1998a, p. 105) argued:

> We must confront the fact that neoclassical tradition has little interest in fieldwork, or in structure. Its approach is to identify an agent, termed an individual, and characterized by preferences that possess certain very general qualities. The agent can be virtually any kind of economic actor, for example, a household or a firm, a borrower or a lender, a worker or an employer, because the specifics don't matter. Given the preferences, the method is to construct an optimizing model to predict what that agent will do in various assumed circumstances, responding to market stimuli. As the market stimuli vary, the results of the optimization will vary, giving rise to functional relations. These are then said to hold generally, ceteris paribus. Besides the well known ceteris paribus problems, there are two issues here: agents have not been properly related to the structure in which they are assumed to act, and second, an optimizing model yields prescriptions, not descriptions.

In Nell's terms, this would be to claim that, when these issues are combined with the active portrayal of the mind, this provides good reasons for rejecting the conventional approach and developing an alternative. Such a move leads directly to rejecting the view that markets efficiently allocate resources optimizing in favour of the idea that markets generate and finance innovations competition. This is the basis of transformational growth. We shall argue that Nell's central finding is a reconfirmation and extension of what Haavelmo (1958) and Swamy et al. (1985) have already said when they argued that neoclassical econometrics rests on extraordinarily shaky foundations.

We shall argue in Chapter 10 that in economics, conceptual analysis of fieldwork can then put together the real patterns of behaviour and motivation, in the context of the available and actually operating technology, ways of working, making and doing things. Such conceptual analysis may be concerned with 'deconstruction', a literary analysis taking apart the reported picture, discovering concealed meanings and hidden agendas, both on the part of the observers and the observed. An important part of this will be uncovering the presuppositions of the concepts and activities reported by fieldwork. Or the programme of economics may accept the picture, and set out to construct models that will show how the system works in various ways, including how it may fail to work and break down.

This allows all aspects of statistical inadequacy addressed by Haavelmo (for example, misspecification, respecification and identification) to be coherently and smoothly implemented without any conflict with the theory.[7] These intertheoretical links between Hollis and Nell's alternative vision of economic theory and Haavelmo's structure of econometrics should contribute to solving, or at least easing, the model specification problem. In other words, the problems of econometrics may lie not so much in econometrics itself as in the unrealistic approach to economic theory on which it has drawn to specify its functions.

Within textbook methodology, the distinction usually made between the theoretical and estimable models seems totally unnecessary for three interrelated reasons: (1) the observed data are treated as an afterthought; (2) the actual data generating process (DGP) can be dispensed with; and (3) theoretical variables are assumed to coincide one-to-one with the observed data chosen.

Granger (2004, p. 97) argued that 'the concept of a data generating mechanism "DGM" [also known as data generating process (DGP), see Spanos, 1986, chs 1 and 17] is only an abstract concept used by econometric theorists as the ultimate aim of an asymptotic analysis. It is not considered an achievable structure or objective'.[8] From the statistical perspective, Spanos (1986, p. 349) argued that 'the concept of DGP supposedly supplements the probability and sampling models. It is somehow a crude approximation to the actual data generating process which generates the available data. It represents a summarization of the sample information in a way which enables us to accommodate any a priori information related to the actual DGP as suggested by economic theory'. Haavelmo (1944) does not conceive of the theoretical variables as corresponding directly to a particular observed data series unless the data are generated by 'artificially isolating the economic phenomenon of interest from other influences'.

Think of a set of theoretical variables, for example aggregate demand for money, income, price level and interest rates; dozens of available data series must be considered possible candidates for measuring these variables. Moreover, commonly, none of these data series will adequately measure what the theoretical variables precisely refer to. There are a number of well-known reasons for this: economic data are usually non-experimental in nature and come in one of three forms:

1. Time series, measuring a particular variable at successive points in time (annual, quarterly, monthly or weekly);
2. Cross-section, measuring a particular variable at a given point in time over different units (persons, households, firm, industries, countries, etc.);

3. Panel data, which refer to cross-section data over time.[9] Until recently, panel data have been used comparatively infrequently in econometrics because of the difficulties involved in gathering them. Now such studies are everywhere.

The econometric model builder is rarely involved directly with the data collection and refinement, and often has to use published data knowing very little about their origins. This lack of knowledge can have serious repercussions on the modelling process and lead to misleading conclusions. Ignorance related to how the data were collected can lead to an erroneous choice of an appropriate sampling model. Moreover, if the choice of the data is based only on the name they carry and not on intimate knowledge about what exactly they are measuring, it can lead to an inappropriate choice of the statistical generating mechanism and some misleading conclusions about the relationship between the estimated econometric model and the theoretical model as suggested by economic theory.

It will be misleading, for example, to assume that the theoretical model and the statistical model can be differentiated by a simple white-noise error term regardless of the observed data chosen. The question that naturally arises at this stage is whether we can tackle some of the problems raised by the debate over the foundations of econometrics in the context of an alternative framework.

With this in mind, such an alternative methodological framework should attribute an important role to the actual DGP in order to widen the intended scope of econometric modelling. Indeed, the estimable model should be interpreted as an approximation of the actual DGP. This brings the fieldwork approach to the centre of the scene with the statistical model.

The statistical model will then be defined directly in terms of the random variables giving rise to the data rather than to the error term. It should be specified as a generalized description of the mechanism giving rise to the data, in view of the model to be estimated, because the latter will be analysed in its context.

Here's an example: think of a car and consider two theories of how it works. The first is the internal combustion theory, combined with the theory of gearing and transmission. The second is the theory of spirits with invisible hands. According to this latter theory, the spirits drink petrol, and with their invisible hands move the wheels whenever the accelerator is pressed, turning them when the steering wheel is turned. When the brake is pressed they stop the wheels moving. This theory will fit much of the data quite well, and will enable a model to pass some important tests:

- when there is no petrol for the spirits to drink they will refuse to work;
- when the accelerator is pressed, the car will move, the further it is pressed the faster it will move;
- when the brakes are pressed the car will stop.

With observations of pressing the accelerator and the brake, and observations of the car moving, a function can be specified and estimated. The fit won't be very good because we would need to know whether the car was going uphill or downhill, but presumably adjustments could be made.

However, when the car won't run properly because of a broken piston or a thrown camshaft, this theory will not be helpful. Fieldwork, in this case, would involve getting under the bonnet with a box of tools and a mechanic's instruction booklet, and figuring out how the thing actually works.

The problem of finding a good theory is discussed by Malinvaud and Haavelmo in similar terms to Hollis and Nell (though without the spirits).

For Malinvaud (1980, p. 739), 'the econometrician's job consists as much in defining a good model as in finding an efficient statistical procedure, and the search through empirical statistical data must thus be guided by a good economic theoretical model'. This dependence of the data investigation on a good theoretical model is also pointed out by Haavelmo (1989, p. 15), who insists that 'econometrics has to be founded on theories that describe in a reasonably accurate way the fashion in which the observed world has operated in the past'. This clearly implies that we know from our collective experience how the observed world has worked.

Hollis and Nell (1975) and later Nell (1998a) argued that we cannot expect a theory to jump by itself from the statistics. They question the validity of the empiricist's assumption that the mind is the recipient of sense impressions, organized by definitions and analytic truths. Nell (1998a) argued that to understand and sometimes even to discover these truths of reason, it is necessary to investigate the world and, especially perhaps, to investigate the investigations. The mind is active, not passive, and our theories shape our data. We have to interact with the world in order to understand it, and we have to bring to bear all the methods of investigating we can (for example, Haavelmo, 1958). Besides applying mathematical tools to statistics, this means looking at the economy with open eyes, gathering interview data, carrying out practical activities (shopping, banking, applying for jobs, going to work) and then thinking about what we have found. When we use statistics we have to reflect on exactly what they mean – what do these numbers represent in reality?

If adequate fieldwork has not been done, no one will know what the numbers mean. Fieldwork will give us the concepts, but then the concepts have to be fitted into a realistic structure – a structure that must, however, be more precise, more realistic and, in many respects, more complex than any heretofore available. In formulating its abstract quantitative notions and concepts, theory must be inspired and guided by the techniques of observation in the field. Indeed, Haavelmo (1958; 1989), Hollis and Nell (1975), Johnston (1963 [1984]), Klein (1950; 1979; 1982; 1985), Malinvaud (1981; 1984a; 1988; 1998; 2001), Nell (1998a; 2004) and Nell and Errouaki (2008a) have warned us that 'armchair empiricism' won't do the job.

2.3 HAAVELMO AND HOLLIS & NELL IN DIALOGUE

As stated earlier, Haavelmo's work marks the construction of the probabilistic foundations of econometrics. First of all, it constitutes the product of the development of econometric methods and practices initiated at the start of the 1930s by the members of the Econometric Society and of the Cowles Commission (Morgan, 1990a, ch. 8).

As Morgan (ibid., p. 220) wrote:

> The idea of a full-scale probability approach to econometrics was the work of Haavelmo. It was published in 1944 but had already received wide circulation in mimeo form in 1941. Haavelmo recognized the debt to his teacher, Frisch, for many of the ideas he proposed in this paper (for example for the ideas on identification and structure, put forward in Frisch's critique of Tinbergen's work in 1938), but Frisch's severe doubts about the use of probability theory in econometrics were well known, and in this respect Haavelmo was probably influenced by Wald and Neyman. The genesis of the simultaneous equations model is less clear. Popular versions of that model, and the probability approach, were produced by Haavelmo in 1943a and Koopmans in 1945. The approach also formed the basis of the influential Cowles Commission research programme of the period 1943–7. Their basic programme dealt with a simultaneous equations model with errors-in-equations, although some work on joint 'shock-error' models (errors-in-equations and errors-in-variables) was undertaken by Anderson and Hurwicz (1946) and found its way into L.R. Klein's textbook (1953).

Furthermore, Qin and Gilbert (2001, p. 431) argued that 'Koopmans's technical rigor, Frisch's theoretical vision, and Tinbergen's inventive experimentation combined to lay a solid foundation' for Haavelmo's probability approach.

Haavelmo's achievement was to propose an estimating and testing framework that was probabilistic in nature: by contrast, the econometricians of the 1930s had remained hostile to statistical inference and probability.

The scientific context of the period helps us to understand this new orientation: since the beginning of the twentieth century, several disciplinary fields, starting with physics, were won over by a 'probabilistic revolution'. This very fact promoted the construction of new investigating methods and of new representations of reality.

2.3.1 The Methodological Foundations of Haavelmo's Conception of Econometrics

In Haavelmo's approach,[10] the link between a theoretical model and the estimated equations is considerably more sophisticated than it appears in the modern econometric textbook, where white noise error terms are simply attached to neoclassical theoretical relationships. In ch. I ('Abstract models and reality'), Haavelmo (1944) defines the intended scope of the theory as purporting to provide abstract descriptions of real phenomena of interest. Moreover, Haavelmo (ibid., p. 3) sees theoretical models as human constructs rather than hidden truths: 'It is not to be forgotten that they are all our own artificial inventions in a search for an understanding of real life; they are not hidden truths to be discovered'. In this respect, Haavelmo's approach parallels closely Hollis and Nell's alternative vision of econometrics[11] (see Hollis and Nell, 1975, pp. 65–7). Hollis and Nell and Haavelmo can be compared by drawing on Suppe's (1989) conceptual framework, as a meta-theory.

2.3.1.1 Suppe's conceptual framework

First, we present a summary of Suppe (1989) that draws heavily on Davis (2000). We shall then reconsider and interpret Haavelmo's seminal article within the Hollis and Nell framework.

Davis (2000, p. 206) argued that Suppe's semantic approach is predominantly 'a tool for conceptualizing and understanding scientific theories'. The semantic approach had already been applied in economics (see for example Hands, 1985; and Hausman, 1992), but it was Davis (2000) who first used Suppe's framework in his reinterpretation of Haavelmo's structure of econometrics.[12] However, neither Hands and Hausman nor Davis drew parallels between Hollis and Nell's framework and Haavelmo's structure of econometrics.

In spite of some controversy, we agree with Davis that Suppe's framework is appropriate and helpful: it is presented at a level that does not require any formal model theory and it is accessible to a broad audience.

Furthermore, Davis (2000, p. 207) pointed out that 'Suppe's account is comprehensively worked out and his topic coverage overlaps nicely with Haavelmo's topic coverage'. This will bring to light the similarities between their conceptions of the relationship between the theory and the data and their approaches to theory testing and falsification.

Initially, we follow Davis's (2000, pp. 207–12) presentation. Suppe's approach is broken down into four main components: (A) theories and phenomena as relational systems; (B) experimental methodology; (C) experimental testing and confirmation; and (D) distinguishing between scientific and background domains. Our presentation will be brief and abstract in order to capture the essence of Suppe's approach. Following Davis, we combine (B) and (C) and deal with three topics: (1) theories and phenomena as relational systems; (2) an experimental methodology and confirmation; and (3) distinguishing between scientific domains and background domains.

(1) Theories and phenomena as relational systems An observed phenomenon can be said to be generated by the interaction of specific, but often unknown, variables. Davis (ibid., p. 207) argued that 'in the semantic approach, these variables and their relations form what are called phenomenal systems'. In econometrics, phenomenal systems, following Granger (2004, pp. 96–7), have become known as data generating mechanisms (DGMs):

> To an econometrician the question of reality can be equated with asking about the data generating mechanism (DGM). As we observe new data every month, or over some other period, there must be some mechanism that generates it. To ask what forms this data, it is convenient to start with the belief that economics is basically a decision science. Therefore, the fundamental building blocks are the decisions made by large numbers of decision makers in the economy: consumers, investors, employers, and policy makers.

('A decision science', yes, certainly. But decisions can't be made regularly unless the decision-makers are supported, maintained and renewed regularly. There has to be what we have called reproduction.)

According to Granger (ibid., p. 97), 'the DGM should not be considered a real phenomenon; rather it is only an abstract concept used by econometric theorists, postulated for the purposes of asymptotic analysis'. Granger even suggests 'doubt about its existence', noting that it has to be considered exceptionally complex. Nor is 'finding the truth a plausible objective for an empirical analysis in most cases'. 'The considerable attention paid to the topic of the "true model" in discussions of economic philosophy and methodology has little relationship with practical analysis', in his view.

We shall argue in Chapter 9 that Davis, following Suppe, would have to disagree. Theories are supposed to help explain phenomenal systems, although they do not do so directly; economic theories should help to explain DGPs or DGMs. Davis (2000, pp. 207–208) sets this out as follows:

> By definition, a theory is an abstraction from reality that explains real phenomena in terms of a subset of selected variables. As a result, a theory will have to ignore the influence of many potentially important variables and, either implicitly or explicitly, will have to use a ceteris paribus assumption to exclude the effects of those variables omitted from the theory. Consequently, a theory does not, in fact, actually characterize the phenomenal systems it is designed to explain; instead it describes isolated systems called 'physical systems' (i.e. 'socioeconomic systems', in our terms).
>
> A possible state [of such a system] is the set of simultaneous values the variables in the physical system could achieve at a particular time. The sequence of states represents how the set of simultaneous values evolves across time. If S is a physical system corresponding to a phenomenal system P, then 'the state of S at t does not indicate what [values the variables] in P possess at t; rather, it indicates what [values] they would have at t were the abstract [variables] the only ones influencing the behavior of P and were certain idealized conditions met' (Suppe, 1989, p. 94). Thus the data associated with a physical system is counterfactual and a physical system is an abstract counterfactual replica of a phenomenal system.

Davis (2005a, p. 96) pointed out that 'a physical system does not present the data of a phenomenal system; rather a physical system presents what the data of the phenomenal system would be if the abstracted variables were the only variables influencing the system, and if certain idealized conditions were also met'.

Let's take it a step further. To paraphrase Davis (2000, p. 208), drawing on theory, we reduce a phenomenal system to an isolated system called a physical system, which presents the data as they would be if they were truly isolated. Then we impose the laws of a theory on a physical system; this generates a new entity called the 'theory-induced physical system'. According to Davis (ibid., p. 208), 'in econometric terminology, the physical system is unrestricted and the theory-induced physical system is restricted in accordance with theory'. So a 'theory-induced physical system does not indicate what values the variables in the physical system possess; rather, it indicates what values they would have, under the conditions specified by the laws of the theory' (Davis, 2005a, p. 96). And, to repeat, because 'theories do not describe the actual behaviour of phenomenal systems, but only the possible behaviour of physical systems, a theory- induced physical system must be considered a counterfactual replica of a possible phenomenal system' (Davis, ibid., p. 97).

The semantic view clarifies methodology by differentiating between these three types of systems. Davis (2005a, p. 97) argued that:

> A phenomenal system will have multidimensional attributes (e.g., multiple variables, multiple constants or parameters, multiple states, multiple moments of probability distributions, etc). So, being a counterfactual replica, any specific physical system can at best accurately represent only a subset of those dimensions. If two different physical systems are designed to replicate different dimensions accurately, the two systems are likely to be incompatible in some dimensions. This is to be expected and does not imply that one physical system is correct or preferred to another. Rather, one physical system may outperform another in some dimensions.

Granger (1990, p. 19, quoted by Davis, 2005a, p. 97) has stated the issue this way: 'Should one build different models for different objectives? Given that models are only approximations to the truth, and perhaps inadequate ones, then different models may be appropriate'. These distinctions between phenomenal, physical and theory-induced systems can be found, in different words, in Haavelmo's writings. Haavelmo (1944, p. 70) wrote:

> In constructing schemes we nearly always have some real phenomena in mind, and we try to include in the scheme – in a simplified manner, of course – certain characteristic elements of reality. At the same time we realize that such schemes can never give a complete picture of reality.

These 'schemes' are physical (socioeconomic) systems, isolated and abstracted from the complete phenomenal systems of reality. Davis points out that a physical system cannot be complete, just as a city map cannot give a perfectly detailed representation of a city – not without being as large as the city itself.

Any explanatory item – be it a map, a model or a theory – must in some sense be smaller, less complete and less complex than that which it purports to explain, otherwise it is of little use. Haavelmo (1944, p. 8) also wrote: 'A theoretical model may be said to be simply a restriction upon the joint variations of a system of variable quantities (or more generally, 'objects') which otherwise might have any value or property'. This parallels the distinction between physical systems and theory-induced physical systems, which imply certain restrictions.

(2) An experimental methodology and confirmation Following Suppe, Davis (2000, p. 209) argues that 'a theory of experimental design states the experimental setup and defines procedures for gathering and measuring data'. A theory, however, must explain counterfactual data, 'so a

distinction must be made between the raw data collected and the counterfactual data desired. Raw data are obtained by conducting experiments in accordance with the theory of the experimental design'.

In most of economics, however, at best we have uncontrolled experiments, which means that 'the raw data are obtained from recording "nature's experiments" and thus reflect influences from variables outside the theory' (ibid.). Consequently, Davis (ibid.) wrote:

> the raw data will not represent the counterfactual nature of the theory; the outside influences will have to be removed. Raw data that has been purged of outside influences are referred to as hard data. Converting raw data to hard data usually 'involves employing various correction procedures (such as using friction coefficients, and the like) to alter the observed data into data representing the measurement results which would have been obtained had the defining features of the idealized (variables) of the physical system been met by the phenomena' (Suppe, 1989, p. 68).

Observations and data are generated by the 'phenomenal' systems. Physical or socio-economic systems (and theory-induced systems), since they involve the selection of only certain variables of the phenomenal system, are associated with counterfactual data that would be observed if the selected variables were the only ones affecting the phenomenal system. The testing of a theory therefore requires the use of an experimental methodology for converting the observational or raw data into counterfactual or hard data. The difficulty that arises here for economics is that a 'test' or prediction may fail not because the theory is wrong, but because of faults in the method of conversion of raw data into hard data. This is explored by Hollis and Nell (1975). There are essentially two ways of making the conversion. Davis (2000, p. 209) argued:

> The data of the phenomenal system *P* can be converted into data about the physical system *S* in two ways. Either, (1) the phenomenon is observed in a controlled experimental setting where only the variables specified in the physical system influence the phenomenon; or (2) the phenomenal system is observed in an uncontrolled setting and the other 'outside' factors influencing the phenomenon are known and accounted for in data construction. Both approaches require a theory of experimental design. A theory of experimental design states the experimental setup and procedures for gathering and measuring the data.

Haavelmo (1944) recognized both of these components of experimental methodology; however, as Davis (2000, p. 209) puts it, unlike in the physical sciences, 'the experimental setup in economics is generally fixed by nature, and the data observed is unavoidably very raw. Most of the control available to the econometrician, then, is to be found in the theory of the data'. This theory is used to strip away the effects on the raw data of variables that have been abstracted out in forming the physical (socio-economical) system. Haavelmo's own approach to converting observational data into counterfactual or hard data, as stated by Davis (2000, p. 217), 'is to use the laws of probability theory to marginalize and conditionalize the probability distribution of the endogenous and exogenous variables'. A theory is tested by comparing the set of counterfactual data constructed according to the theory of the experiment (which corresponds to the abstracted physical system) to the set of the theory's principal predictions. Davis (ibid., p. 211) argued that 'when the laws of the theory are statistical in nature, a sample distribution of physical systems is generated by repeatedly observing phenomenal systems'. In this case, according to Davis (ibid.) 'standard statistical techniques (e.g., Neyman–Pearson) can then be applied to determine the probability that the physical systems correspond with the theory-induced physical systems within a predetermined confidence level'. If the probability falls within this confidence level, the experiment is a confirming instance of the theory, while if it falls outside this level, it is a disconfirming instance of the theory. Confirmation and disconfirmation are only relative, due to such considerations as the problem of induction on the one hand and the possibility of inadequate data, or faulty conversion of the data, on the other.

Again, Haavelmo treats the testing of theory in comparable terms. He calls the counterfactual data obtained 'true data', assuming the theory of the experiment has been well designed, and he refers to the predictions of the principal theory for what that data will be as theoretical data. Put another way, true data correspond to physical systems, while theoretical data correspond to theory-induced physical systems. Haavelmo (1944, p. 9) writes: 'It is then natural to adopt the convention that a theory is called true or false according as the hypotheses implied are true or false, when tested against the data chosen as the true variables'. In other words, a theory is confirmed if, and only if, the theoretical data correspond to the true data, based on a probability measure.

(3) Distinguishing between scientific domains and background domains Domains are defined in all sciences in terms of general fields and subfields that form bodies of related items. Davis (2000, p. 211) observed that 'problems and questions within a given domain are called domain questions. In attempting to answer domain questions, other theories, facts, assumptions and information must be taken as unproblematic. This collateral information constitutes the background domain. The scientific domain, then, is the set of theories, assumptions and

facts that are prominent or of direct interest in answering the domain questions'.

To help make the abstract concepts of Suppe's framework more concrete, Galileo's theory of falling bodies will be used as an example where needed to clarify the concepts. Let's follow Davis (2000, p. 211), who wrote:

> Let the theory in the scientific domain be denoted as T_1 and the theories in the background domain be denoted by $T_2, \ldots, T_n$. For example, T_1 could be Galileo's theory of falling bodies and some of the background domain theories $T_2, \ldots, T_n$ would be those supporting the mathematics and measurement systems utilized in testing the falling body theory. These *n* theories may be formulated fully or partially. If all of the characteristic features of the theories are formulated, then there is a full formulation of the theory yielding the fully formulated theory induced physical system, say $\Im \equiv (T_1, T_2, \ldots, T_n)$. Alternatively, if not all of the characteristic features of the theories are formulated, there is only a partial formulation of the theory induced physical system, say $\Im^* \equiv (T_1^*, T_2^*, \ldots, T_n^*)$. Clearly partial formulations are the rule not the exception.

Somewhat surprisingly and independently Haavelmo (1944) and Hollis and Nell (1975) used the example of the law of falling bodies[13] to illustrate a different point. Haavelmo (1944, p. 7) used it as a mechanical illustration of his discussion of how to test a theory against facts. He argued that 'in order to test a theory, either the statistical observations available have to be corrected, or the theory itself has to be adjusted, so as to make the facts we consider the true variables relevant to the theory'. However, Hollis and Nell (1975, appendix to ch.1, pp. 43–6) used the law of falling bodies to demonstrate that there is no helpful analogy between the assumption of frictionless motion in physics and the assumption of perfect competition in economics. The appeal to frictionless motion does not help the positivist economist. This point was further discussed in Chapter 1 of the present volume.

The formulation of these *n* theories could be carried out fully or partially. In the case where all of the characteristic features of the theories are formulated, there is a full formulation of the theory yielding the fully formulated theory-induced physical system. In the event that not all the characteristic features of the theories are formulated, we have a partial formulation of the theory-induced physical system. Partial formulations can be expected to be the rule not the exception. Suppe (1989, p. 140) observed that 'partially formulated theories can potentially admit an infinite number of models'. Furthermore, Davis (2000, p. 212) went on to argue that 'this result is easily documented in applied econometrics and it is at the heart of the model specification problem'. Davis's discussion of model selection will be further discussed in Chapter 9.

2.3.1.2 Hollis & Nell and Haavelmo on the relationship between theory and phenomena

In Suppe's terms, Hollis and Nell's and Haavelmo's conceptions of the relationship between the theory and the phenomenon distinguish between the phenomenal, the physical and the theory-induced systems. We first consider Hollis and Nell's conception.

Hollis and Nell consider the intended scope of a theory to be the specific class of phenomena this theory has been designed to characterize. Within the intended scope, the interaction between variables and the relations between their values and their properties will generate the observed data. These variables and their relations form what Suppe (1989) calls 'phenomenal systems'. In econometrics, 'phenomenal systems' would be called 'data generating processes' (DGPs). Many aspects of a phenomenal system remain unknown.

A given theory does not exactly characterize the 'phenomenal systems' in the intended scope; instead, it describes isolated systems, or what Suppe (1989) calls 'physical systems'.

For Davis, and for Haavelmo the investigator, drawing on theory chooses the way we restrict the phenomenal system to obtain the physical system. But neither discusses this at length, in effect drawing on conventional theory. Hollis and Nell adopt a different strategy. For them, theorizing depends on the ability to select what is conceptually essential and then to define variables expressing these essential features. Conceptual analysis will suggest ways in which these features are related. Such relations are then explored by theory, and applied to the world by abstracting from the inessentials.

The notion of 'real definition' plays an important role in Hollis and Nell's theorizing process. For instance, the concept of 'production' must occur essentially in any analysis of an economic system; human activity uses up material things and must replace what it uses. Human beings must be fed and clothed. So the system must regularly reproduce what it requires. An economic theory cannot be adequate or complete unless it explicitly or implicitly includes an account of production sufficient for the support of the assumed agents. Moreover, a similar line of argument contends that no economic system is viable without exchange efficiency requiring the division of labour among interdependent processes, and this, in turn, not only calls for exchange but, further, relates the concept of 'exchange' explicitly to that of 'production' (Marx, 1967; Nell, 1992c; Sraffa, 1960). If this is correct, it is a necessary truth that any viable economic system includes exchange of the output of production; and this is in fact true of any actual viable economic system.

To determine the kind of a given system, a 'rule of distribution' is then invoked and the taxonomy of possible rules of distribution becomes a part of economic theory as well.

Now we look at the relations between theory and evidence. Davis (2000, pp. 208–209) takes the example of the rate of acceleration of a feather falling to the earth. A similar example has been used in a different context by Hollis and Nell (1975, appendix to ch. 1, p. 43). We can use it here to show how Suppe's conceptual framework brings to light some of the parallels between Hollis and Nell's methodological insights and Haavelmo's conception of econometrics.

Davis (2000, p. 208) wrote:

> The phenomenal system (*P*) would consist of all the variables potentially affecting the rate of acceleration of the feather (e.g., the mass, shape, surface area of the feather, gravity for the given planet, altitude). Now Galileo's theory (*T*) of falling bodies states: in the absence of friction, all bodies fall to the earth at the constant acceleration rate of 32 feet per second. Note the real explanatory variable Galileo retains is the gravity of earth. He abstracts away friction and all variables denoting features of the bodies. The physical system (*S*) characterizing this theory would be that of an environment where objects falling to the earth did not encounter any resistance (e.g., a vacuum). Note that within *S*, the bodies are allowed to accelerate at their natural rate and are not restricted to accelerating at the rate stated by the theory.

In Hollis and Nell's terms, to test a theory such as this one, an experimental methodology must be employed to convert the observational data of the phenomenal system into counterfactual data of the theory-induced physical system.

We now compare Hollis and Nell's approach to Haavelmo's conception of theories and phenomena as relational systems. As pointed out earlier, Haavlemo (1944, p. 8) indeed makes a similar distinction:

> Consider n time functions $x'_1(t), x'_2(t), \ldots, x'_n(t)$. Let *F* be the set of all possible systems of n time functions, and let '*B*' be a system of rules or operations that defines a subclass F_B of *F*. Any system of n time functions will then have the property of either belonging to F_B or not belonging to F_B. The system of rules '*B*' defines a model with respect to n times series. Thus, a theoretical model may be said to be simply a restriction upon the joint variations of a system of variable quantities (or more generally, 'objects') which otherwise might have any value or property. More generally, the restrictions imposed might not absolutely exclude any value of the quantities considered: it might merely give different weights (or probabilities) to the various sets of possible values of the variable quantities.

In our interpretation of Hollis and Nell's conception of theories and phenomena as relational systems, physical systems represent isolated systems of a set of selected variables and *theory*-induced physical systems are generated by imposing the restriction from the theory on the physical systems.

This is exactly what Haavelmo states in the above-mentioned quote. As Davis (2000, p. 213) observed, 'the n time functions are the selected variables; Haavlemo's F denotes a class of physical systems and his F_B the class of theory-induced physical systems'. We point out that because all the variables have been selected a priori, neither F nor F_B refers to reality or the phenomenal system, as Haavlemo (1944, p. 70) recognized:

> we nearly always have some real phenomena in mind, and we try to include in the scheme (physical system) – in a simplified manner, of course – certain characteristic elements of reality. At the same time, we realize that such schemes can never give a complete picture of reality.

Hollis and Nell (1975, ch. 2) require that the data associated with a physical system and a theory-induced physical system be interpreted as counterfactual or hard data. However, the data associated with the phenomenal system, which includes the effects of variables from outside the theory, should be observational.

Haavelmo (1944) also makes these distinctions in section I.3 entitled 'Observational, True, and Theoretical Variables: An Important Distinction'. However, he provides a refined distinction between the hard data that come from the experiment free of other influence (true variables) and the data that would *be* generated by the theory if it were true (theoretical variables):

> True variables (or time functions) represent our ideal as to accurate measurement of reality 'as it is in fact', while the variables defined in a theory are the true measurement that we should make if reality were actually in accordance with our theoretical model (Haavelmo, 1944, p. 5).

With respect to the difference between 'true' and 'observational' data, Hollis and Nell (1975) argued that the categories of observation are only rarely the categories of theory and hence observations must be adjusted to yield the true variables. Observations occur in particular circumstances subject to myriad local or temporary influences; the variables are defined for the general case and can be arrived at only after adjusting the results of observation.

Haavelmo (1944, p. 7) argued that the 'true variables are variables such that, if their behavior should contradict a theory, the theory would be rejected as false. Conversely, when observational variables contradict the theory, they leave the possibility that we might be trying out the theory on facts for which the theory was not meant to hold. The confusion is thus caused by the use of the same names for quantities that are actually different'.

So, as Davis (2000, p. 214) observed, 'observational data correspond to raw data, as they may contain influences from outside the theory, but true data correspond to hard data, because they may be used to test a theory, meaning they are free of influences from outside the theory. True data and theoretical data are the same only if the theory is true.' (See also Hollis and Nell, 1975, ch. 4)

At this point Hollis and Nell take a further step, one that Haavelmo suggests but does not emphasize (until his later work, see Haavelmo, 1960), namely that the need to adjust observations to true values can make theories untestable, and even inapplicable. Hollis and Nell also draw on the example of gravity; they consider a block sliding down an inclined plane, with frictionless motion (appendix, ch. 1, pp. 43–6). The mass of the block times the force of gravity will be a vector mg, that will be separated into two vectors, *mgsin*θ and *mgcos*θ, where θ is the angle of the plane. The first shows the force pushing the block down the plane, the second is the force perpendicular to the plane. But in fact there never is stricly frictionless motion; there is always some friction, although experimentally it can be reduced indefinitely. Using the Newtonian analysis, a variable 'the coefficient of friction' can be defined, and its range determined (it will lie between 0 and tan θ). The model of frictionless motion simply sets this well-defined variable equal to zero. Actual measurements of the velocity at the bottom will deviate from the true velocity (square root of 2*gh*, where *h* is the height from which the block began its slide) due to the effect of friction, which can be precisely measured.

By contrast, many of the assumptions of competition – perfect or imperfect – are quite different. They do not assign zero or constant values to well-defined variables. They are instructions on how to understand the conditions of the model (perfect or 'adequate' mobility of factors) and what qualities should be attributed to the agents (perfect information, instant calculation, perfect foresight). These assumptions are not abstractions; in effect, they 'idealize' the agents and the conditions of the model, attributing qualities that do not and often could not exist in reality. To try to make adjustments of actual values to 'true values' in these conditions is to pretend that the assumptions are of the same kind as those of frictionless motion – but they are not. The effect is to make the theory untestable in principle, since whatever adjustment is made could always be disputed. But worse, the conditions for the applicability of the model are wholly unclear – what counts as an example of an agent to which the model applies? And how much 'imperfection' is allowable before the theory gives different results?

2.4 COMPARISONS AND EXTENSIONS: HOLLIS AND NELL ON SPECIFICATION AND UNCERTAINTY

Drawing on Suppe's framework, we can see that the methodologies of Hollis & Nell and Haavelmo overlap considerably. Both are concerned with identifying strong regularities. Moreover, the textbook approach would benefit from realigning itself with them. Both argue that theory must be taken into account; on this there is agreement. Both argue that theory is needed to define the true variables. An important question is just what kind of theory. Haavelmo suggested that theory must be realistic. Hollis and Nell, and especially Nell (1998a), go further and argue that theory must reflect conceptual truths and must be based on fieldwork. Haavelmo agrees in regard to fieldwork. Both want theories put to the test against the data, and to be modified in the light of the data; both oppose using theory to shape the data to meet pre-existing conceptions.

But in important areas Hollis and Nell go beyond Haavelmo. For example, problems may arise, not from theory as such, but because of an over-reliance on individual maximizing theory. Haavelmo supports realistic theory, but does not criticize specific examples of unrealistic theory. Yet many actual economic relationships simply may not fit the maximizing models. (Arguably, the relation between household consumption and income does not – see Deaton, 1974; and Stone, 1954a). Nor is it plausible in regard to managing current employment. Instances where maximizing seems out of place can also be easily found in business pricing behaviour, inventory management, etc., where rules of thumb are common. By contrast, optimizing is widely used in production scheduling. Fieldwork and clear thinking about the necessary presuppositions of economic activity may suggest better ways of theorizing. This would result in more appropriate definitions of theoretical variables and, importantly, in improved specifications.

Haavelmo does not try to distinguish reliable from unreliable or inherently volatile economic relationships. Hollis and Nell, however, consider programming and production models reliable. The former are reliable because they are prescriptive and depend on rationality. Given a goal and various constraints and conditions, a programming model tells us what the agent ought to do. But it does not tell us anything about what will happen. Production models, on the other hand, are descriptive; they tell us how the system maintains itself. They show us how things work. They are reliable because they are solidly grounded in contracts and commitments, including commitments to use the current technology. These are things that cannot be easily or quickly changed. The point of these models is to show in some detail the interactions by means of which the system works. Predictive models, Hollis and Nell's third category, also purport to be descriptive, but being future-oriented, contain inherently unreliable relationships (as well as reliable ones derived from production models). Unreliable relationships are those that are independent of commitments and contracts, but depend, for example, on expectations of future sales or prices. Such expectations are inherently uncertain, in the sense of Knight (1921) and Keynes (1973), and relationships that depend on them are vulnerable to sudden shifts and changes.

Relationships, then, differ in regard to uncertainty; some are uncertain, other relationships seem quite reliable. These are well understood, and can easily check our knowledge in a number of ways.

We can describe these relationships; we understand why they hold. They rest on social and technological regularities. Of course, there may be data uncertainties, and they may be disrupted by accidental or interfering factors. Here, probabilistic methods will help us deal with such matters, and, using them, we can establish reliable numerical relationships. (Employment and output, consumption and income, the circulation of money, and expenditure and employment multipliers are examples.)

By contrast, other relationships are simply inherently unreliable. We know the variables are connected; we understand why there might be causal pressures. But we cannot measure the magnitudes, and sometimes not even the direction, of these influences. We can list the factors influencing investment, for example, or the stock market; but which factors are more important, and even the nature and direction of the influence, may vary from time to time. Nor can we tell in advance when the nature of the influence will change.

NOTES

1. Reprinted in the *American Economic Review*, Dec. 1997, p. 15.
2. See also Nell (1998a, ch. 12) for similar arguments.
3. According to Klein (1982), some of the more prominent institutions are: the tax system; banking system; system of foreign trade and exchange control; judiciary control of production and consumption; regulation of markets and mechanism of wage bargaining. There is no such thing as an anarchical system of completely free competition anywhere

in the world. Most of the institutions listed by Klein are governed by public authorities but some are motivated from the private side-cartels, quota systems and consumer groups. We shall argue in Chapter 11 that these questions are at the centre of Nell's (1998a) transformational growth approach.

4. Gilbert (1987) has argued that Jack Johnston comes from outside the traditional Oxford–Cambridge–London British academic inner circle. He was appointed to an associate professorship at the University of Wisconsin in 1958. The lectures that he gave at Madison subsequently became his bestselling textbook (Johnston, 1963 [1984]), which systematized the developments of the Cowles work. Spanos (2010, in Mayo and Spanos, 2010, p. 232) argued that the 'textbook approach to econometrics, as formulated by Johnston (1963 [1984]) and Goldberger (1964), can be seen as a continuation/modification of the Cowles Commission agenda. The modifications came primarily in the form of (i) less emphasis on simultaneity, and (ii) being less rigid about statistical modelling by allowing non-IID error terms'.
5. Klein's approach can be described as a kind of methodological structuralism (identifying the underlying structures is the basis of his approach) and it overlaps here with Nell's methodological institutionalism. For more details see Chapter 11.
6. In pragmatism, there is no need to distinguish the essential characteristics of an institution from its accidental properties, because there are no essential characteristics, so no such distinction can be drawn. There is no need to investigate the inner workings of a system, because inner and outer are just a matter of the observer's position. As Nell puts it, an accident of perspective. For further details see Nell (1998a, ch. 3).
7. The issue of specification and uncertainty is further discussed in Chapter 9 of this book. We shall examine three recent interpretations of Haavelmo's work, namely Spanos (1989), Davis (2000) and Los (2001).
8. Gilbert (1987, p. 6) pointed out that the term DGP was coined by Hendry and Richard (1982). He argued that the actual data generating process is unknown and unknowable, but the econometrician must make a sensible guess at approximating this process using sample information. For an account of the concept of data generating mechanism (DGM), see Granger (2004). For further details on the statistical aspect of DGP, see Spanos (1986, ch. 17, pp. 349–52).
9. Economic data such as money stock ($M1$), real consumers' expenditure (Y) and its implicit deflator (P), and interest rate on 7 days' deposit account (I), over time, are examples of time series data. The income data of 23 000 households in the US for 1999–2000 are cross-section data. Using the same 23 000 households of the cross-section observed over time we could generate panel data on income.
10. Although this section draws heavily on Haavelmo's overall work (Haavelmo, 1938, 1939, 1940a, 1940b, 1941a, 1941b, 1943a, 1943b, 1944, 1947, 1957, 1958, 1960, 1989) we shall refer here mainly to Haavelmo's (1943a, 1944, 1958 and 1989). The central focus of the discussion will be on Haavelmo (1944). The presentation of Haavelmo (1944) here avoids the redundancies encountered in a chronological reading of Haavelmo's (1944) work. Davis (2000, p. 212) pointed out that 'Haavelmo reiterates his major themes several times, exploring each theme in more detail at each pass (e.g., compare Haavelmo, 1944, pp. 13–4, 25–7, 50–1, and 69–2). Because of this repetitive approach, Haavelmo's general framework is well established by the middle of Chapter 5 and the remainder of his article concentrates on the mathematical details of estimation and prediction'. Since the interest here is largely philosophical, these mathematical details are not covered. For an account of the mathematical details and an examination of the development of Haavelmo's thinking, see Errouaki (2006).
11. Our presentation strategy in the case of Hollis and Nell also followed a different order from the one adopted in their book. Our presentation of their approach is a cross section analysis of their thesis. For further details, see Errouaki (2004).
12. A fuller and more detailed semantic account of econometrics can be found in Stigum (2003). Also a very interesting mini-symposium on the semantic approach to econometric methodology involving contributions from three authors, namely, Davis, Cook and

Chao, was published in the *Journal of Economic Methodology* (March 2005). For an account of the ideas discussed in symposium, see Davis (2005a; 2005b) and Chapter 9 of this book.

13. Davis (2000, p. 214) argued that Suppe and Haavelmo used surprisingly and independently Galileo's theory of falling bodies to make similar points about different experimental methodologies.

If we compare the historic developments of various branches of quantitative sciences, we notice a striking similarity in the paths they have followed. *Their origin is Man's craving for 'explanations' of 'curious happenings', the observations of such happenings being more or less accidental or, at any rate, of a very passive character*. On the basis of such [. . .] recognition of facts, people build up some primitive explanations, usually of a metaphysical type. *Then, some more 'cold blooded' empiricists come along. They want to 'know the facts'. They observe, measure, and classify, and, while doing so, they cannot fail to recognize the possibility of establishing a certain order, a certain system in the behavior of real phenomena. And so they try to construct systems of relationships to copy reality as they see it from the point of view of a careful, but still passive, observer*. As they go on collecting better and better observations, they see that their 'copy' of reality needs 'repair'. And successfully, their schemes grow into labyrinths of 'extra assumptions' and 'special cases', the whole apparatus becoming more and more difficult to manage. *Some clearing work is needed, and the key to such clearing is found in a priori reasoning, leading to the introduction of some perhaps very vague principles and relationships, from which whole classes of apparently very different things may be deduced*. In the natural sciences this last step has provided much more powerful tools of analysis than the purely empirical listing of cases.

Haavelmo (1944, p. 12, italics added)

A central question of interest to both scientists and philosophers of science is, *How can we obtain reliable knowledge about the world in the face of error, uncertainty, and limited data? The philosopher tackling this question considers a host of general problems*: What makes an inquiry scientific? When are we warranted in generalizing from data? Are there uniform patterns of reasoning for inductive inference or explanation? What is the role of probability in uncertain inference? *Scientific practitioners, by large, just get on with the job, with a handful of favored methods and well-honed rules of proceeding*. They seek general principles, but largely they take for granted that their methods 'work' and have little patience for unresolved questions of 'whether the sun will rise tomorrow' or 'whether the possibility of an evil demon giving us sensations of the real world should make skeptics of us all.' Still, in their own problems of method, and clearly in the cluster of courses under various headings related to 'scientific research methods,' *practitioners are confronted with basic questions of scientific inquiry that are analogous to those of the philosopher*.

Mayo and Spanos (2010, Preface, p. xiii, italics added)

3. Induction and the empiricist account of general laws

INTRODUCTION

Our subject is the foundations of econometrics, but this in turn rests on the philosophy of science. We need to consider whether and how claims to have established scientific knowledge can be justified, and this calls for a review of the long-standing arguments over induction, culminating in the recent revival of this literature in the work of Mayo and Spanos (2010), focusing it directly on statistics and econometrics and offering avenues to solve the recalcitrant philosophical problems of induction, explanation and theory testing.[1]

The traditional problem of induction arises within empiricist philosophy. It is appropriate to start with it, since the early econometricians tended to consider themselves empiricists, even positivists. Spanos (2010, p. 235) argued that 'the initial optimism that was associated with the promise of the new statistical methods of the Cowles Commission to significantly improve empirical modeling in economics became pessimism by the late 1960s'. Morgan (1990a, p. 1, italics added) notes that:

> Econometrics was regarded by its first practitioners as a creative synthesis of theory and evidence, with which almost anything and everything could, it seems, be achieved: *new economic laws might be discovered and new economic theories developed, as well as old laws measured and existing theories put to rest*. This optimism was based on an extraordinary faith in quantitative techniques and the belief that econometrics bore the hallmarks of a genuinely scientific form of applied economics. In the first place, the econometric approach was not primarily an empirical one: econometricians firmly believed that economic theory played an essential part in finding out about the world. *But to see how the world really worked, theory had to be applied; and their statistical evidence boasted all the right scientific credentials*: the data were numerous, numerical and as near as possible objective. Finally, econometricians depended on an analytical method based on the latest advances in statistical techniques.

According to empiricist views there are only two sources of knowledge, reason and experience. The first yields analytic statements, which do not provide new empirical knowledge; the second can yield new empirical knowledge. Scientific laws are said to be empirical universal conditional statements. Reason alone cannot support such statements, since it cannot provide new empirical information. (Lower-level laws may be deduced from higher-level ones, but this does not make reason the source of knowledge on which either sort of law is based.) Since the laws are of universal conditional form, they must hold equally of all instances falling under them, observed and unobserved, past, present and future. Even instances falling under the laws that are unobservable in principle, as in counterfactuals, must be covered. (Had the Roosevelt Administration not sought to balance the budget in 1937, the US economy would not have slipped back into Depression.) But how can empirical observation yield knowledge of, or confirm hypotheses about unobserved, let alone unobservable, phenomena? And if it cannot, what then justifies the implicit passage from the known to the unknown in scientific laws?[2]

Neither of the two sources of knowledge provides the necessary support for scientific laws; yet, considering the immense practical achievements of science, it clearly must embody knowledge in some sense of that term.

Approaches to induction can be roughly grouped according to whether the author proposes to validate it, to justify it analytically, to vindicate it pragmatically or, finally, to treat it as unjustifiable. Within each category, induction can be treated as principally concerned either with statistical or with non-statistical statements, or with both of these. All empiricists distinguish between the certainty of the 'truths of reason' and the confirmation of the best-established truths of experience. Many have argued that induction is the procedure by which we build up the confirmation of a statistical or non-statistical empirical statement, or by which we establish the degree of confirmation of a hypothesis. Hence induction may be said to yield probable knowledge in either of two senses of probable, that of relative frequency or that of degree of confirmation. But it would be a mistake to think either that the problem arises only in connection with probability, or that the introduction of probabilistic concepts of either sort in any way contributes to the solution.[3]

3.1 DIFFERENT APPROACHES

Below we consider the different approaches.[4]

3.1.1 Validation

The most common validation procedure has been to try to assimilate induction to deduction by introducing some general principle, such as

Mill's principle of the uniformity of nature, or Keynes's principle of limited independent variety, which would justify the inference from the observed to the unobserved (confirmation by the observed of hypotheses about the unobserved).[5] But such principles, being clearly non-analytic, could themselves be justified only on empirical grounds. Such a move would either involve petition principii or lead to an infinite regress of higher-order principles.

Obvious as this may seem, the demand for a validating principle is not entirely misguided. Since no general premise could be entirely vacuous (or it would not validate), it could generate a line of demarcation, a general method of distinguishing cases where induction will work from those where it will not. (In this sense Mill's principle can be said to underlie the method of difference.)

The idea is that one can proceed from the known to the unknown only where the principle sanctions it; as a matter of common sense, in econometric work we know that some statistical correlations are merely accidental, and there is no reason to expect them to hold in the future, while others are economically significant. A good validation method would distinguish these cases. Analytic justifications and vindications of induction can provide no comparable demarcation, since they merely support inductive procedures as reasonable behaviour. Of course, the suggested validating principles mentioned above would have been too vague to provide a practically useful demarcation line, even had they validated.

3.1.2 Analytic Justifications

Basically, analytic justifications of induction amount to the argument that induction is justified because, under the circumstances, to act reasonably implies acting on the assumption that what is true of known or observed cases can be projected onto unknown or unobserved cases. Proponents of this view have argued that the demand for a validation of induction is misguided, since

> to ask whether it is reasonable to place reliance on inductive procedures is like asking whether it is reasonable to proportion the degree of one's convictions to the strength of the evidence. Doing this is what 'being reasonable' means in such a context. (Strawson, 1959, p. 257)

The analyticity of this contention is admittedly not obvious, and some arguments for the case must be examined more closely. Pap (1962, part III, ch. 13, esp. pp. 238–44) interprets the rule of induction, *R*, as roughly 'if you have observed that of a large and varied sample of *A*s, a fraction *a* are *B*s, infer that all *A*s are *B*s', and maintains that a request for a justification of it is analogous to a request for a justification of the rule of modus ponens. To provide such a justification would require a proof of *modus ponens*, but any such proof would either itself exemplify or be transformable by logical operations into an exemplification of modus ponens. Formal circularity could be avoided by appeal to type distinctions, each proof relying on a modus ponens rule of a higher type. But this could only come at the price of an infinite regress, since the same demand can be made of each of the higher-type rules. Similarly, since R concerns reasoning about empirical phenomena, the demand for a justification of R can only mean asking for its empirical justification. Just as asking for a proof that $(P \& (P \Rightarrow Q)) \Rightarrow (Q)$ means asking for an exemplification of this form, so asking for a justification of *R* means asking for an exemplification of *R*. The demands are equally, and equally necessarily, vacuous.

There are several difficulties here. The sampling problem remains, despite the remark about 'large and varied samples'. Further, the analogy is weak, in that modus ponens is a far more clearly articulated rule than *R*. In particular we have criteria for the appropriate application of modus ponens, but these are unavailable in the case of *R*, as the literature on counterfactuals shows. Hence Pap's argument does not meet the problem.

In addition, crucially, it is internally incoherent. For, if correct, the argument would actually be self-refuting, since it purports to be a justification of *R*, an answer to the demand, yet it does not exemplify *R*.

This can readily be seen in a fuller version of Pap's argument, paraphrasing and developing what he said, which runs as follows:

1. Any statement is analytic or non-analytic.
2. *R* is non-analytic and universal.
3. Any non-analytic, universal statement must be supported by a justification.
4. *R* must be supported by a justification. (From points 2 and 3.)
5. A justification is a proof that shows a statement either to be analytic or to be non-analytic and true. (Using acceptable principles the statement is shown to follow either from analytic or from non-analytic but true premises.)
6. *R* is the principle by which we show any non-analytic, universal statement to be true. (By definition it is the principle that enables us to get from particular non-analytic true premises to universal conclusions.)
7. Hence if anything is a justification of *R*, it must exemplify *R*. (From points 2 and 6. A justification of *R* may also, of course, contain a number of deductive steps, but at some point it must make use of *R*.)

8. A principle all of whose justifications exemplify itself is justified. (This Pap justifies by the contention that *R* stands to the argument-form 'if . . ., then empirically. . ..' as modus ponens stands to 'if . . ., then . . ..')
9. Hence *R* is supported by a justification. (From points 7 and 8.)

It follows from this that *R* is true, and consequently (points 1 to 9) constitute a justification of *R*. But no step makes specific use of *R* as a principle of inference, nor does *R* appear among the premises. There is nothing empirical here from which a conclusion can be drawn. Hence this justification does not exemplify *R*, which contradicts point 7. Further, had the justification been valid, it would, since it did not use *R*, have proved *R* to be analytic, contradicting point 2.

Finally, the analogy between *R* and modus ponens cannot be sustained. An attempted disproof of modus ponens, for example, could not but exemplify the rule to be rejected in the conclusion, but a case for the rejection of *R* could certainly be made on other than empirical grounds, for example on the grounds that neither of the two sources of knowledge provides sufficient support for it.

In the end, the attempt to provide an analytic justification of induction is an attempt to sidestep the real issue. To say that acting or believing on inductive grounds is, in certain circumstances, what 'being reasonable' means is no answer to the question of whether any grounds for so acting or believing in those circumstances are adequate.

3.1.3 Pragmatic Vindication

Faced with the kinds of difficulties just outlined, some writers, notably Reichenbach (1938),[6] have accepted the conclusion that induction cannot be validated, but have argued that it can be vindicated pragmatically. That is, it can be shown to be an appropriate or optimal means to the end of acquiring empirical knowledge.

Reichenbach, for example, has argued that if a fraction *b* of *A*s are *B*s, that is, if *b* is the limit to which the relative frequencies tend, then repeated use of his rule of induction (roughly 'predict that the limiting frequency is the same as the initial') will enable one to discover it. The reason is that induction is self-correcting: if an error is made, its correction is then embodied in the data on the basis of which the next prediction is made. Eventually every error will be eliminated.

This approach at once does too much and too little. It does too much because it vindicates not one inductive rule but a whole class of inductive rules, which in specific circumstances would give rise to very different predictions; it does too little because it brings us no nearer to knowing whether there are regularities, and, if so, how we can have knowledge of them.

If there are empirical regularities, repeated induction will find them; but we have no way of knowing whether there are any, or even (supposing that there are) whether the current formulation of any given regularity is correct.

Further, Katz (1962, ch. 4, esp. pp. 98–9) has shown that it is impossible to rescue vindicating arguments from indeterminacy. To select one rule from among the vindicated class would require knowing which one led to earliest or easiest success; but this would require empirical knowledge of the actual limit to which the rule tends – which, precisely, cannot be presupposed.

3.1.4 The Sceptical Consensus

Over the years, something like a general consensus has emerged among empiricist philosophers to, in effect, dismiss the problem – an attitude that quickly spread to economic methodologists and econometricians.[7]

The problem strictly posed is admitted to be insoluble, but this is held not to be surprising. It is simply a reflection of the difference between empirical and non-empirical knowledge, and a sign of the significant epistemological fact that all empirical statements, however well entrenched they may appear to be, are ultimately revisable.[8]

This amounts to denying that there is such a thing as natural necessity, whose apparent presence in counterfactuals, scientific laws and so on must therefore be explained away. Finally the traditional problem is dismissed as ultimately trivial or absurd. Obviously we do know what we know quite well enough, and scientists manage very well; the only really interesting and important question is how to distinguish good inductive methods, or good methods of confirmation, from poor ones. This is the real job, and it simply does not matter if the neurotic problem – no evidence is really good enough – cannot be solved. In short, the traditional problem has no significant implications; hence it is not a significant problem.

This view appears to lack philosophical insight. To point to the fact that scientists are continually discovering new empirical truths is no way to defend a philosophical position whose clear implication is that there is no good reason to expect them to be able to do so. The problem of induction is a problem within a philosophical account of science; the fact that there is no corresponding real problem is simply evidence that the account has gone wrong. The proper procedure is not to reject the conclusion, but rather to re-examine the empiricist premises.

Let's take a step forward by considering the fact that a kind of inductive necessity appears to be built into ordinary language. This must either be validated or expurgated, if ordinary language is to be translatable into the canonical notation of the predicate calculus (mathematical logic) – an ambition of many philosophers. Alternatively, if ordinary language is not translatable into a canonical notation, this apparent 'natural necessity' must be explained.

Examples are easy to find: 'aspirin dissolves in water'; 'some rocks are resistant to drilling'. Ordinary economic discussions are full of them: 'this policy would be inflationary'; 'these policies make up a stimulus package'; 'this sales tax is regressive'; 'those policies will lead to a flight from the currency' or 'to a run on the banks'. Quine and others have argued that such inductive, or natural, necessity can be expurgated by appealing to the fact that the predicates in conditionals having the force of 'natural necessity' can be logically reconstructed using relational predicates indicating structural similarities (Quine, 1960, pp. 222ff). Thus '*x* is water-soluble' can be rendered '(*Ey*) (*Mxy* and *y* dissolves in water) where '*Mxy*' stands for '*x* is appropriately like *y* in molecular structure'. But, as Cohen (1962, ch. X) has pointed out, this begs the question, since the suggested paraphrase will work if, and only if, *M* determines the presence or absence of solubility. This in turn depends on the truth-value of a general statement to the effect that for any *a* and *b*, if a dissolves in water, then if *b* has an appropriately similar molecular structure, *b* will dissolve in water – a statement involving precisely the excoriated notion of natural necessity.[9]

A notable implication of some of the ordinary economic expressions above is that the activity reflects some sort of 'contagion' – the run on the banks, the flight from the currency – not easily explained in terms of isolated agents rationally deciding to move their assets. The phrasing suggests a mass movement with elements of a panic. Similarly, other economic phrases in common use – 'confidence', 'expectations' – also suggest a kind of consensus or general opinion that is not well explained by individualistic modelling of rational choices.

But we still haven't said how we should treat these ascriptions of necessity, or, as it has come to be known, 'lawlikeness'.

3.1.5 Lawlikeness

Here, the problem is to mark off those statements of universal conditional form that are 'lawlike' from the rest. If this is not done, the commonly accepted account of scientific explanation is reduced to modus ponens – interesting and explanatory, but hardly peculiar to science. Hempel (1965) and others have tried to define lawlike sentences (or statements: nothing turns on the distinction in this context). One suggested criterion is that a lawlike sentence must be of 'essentially generalized' form; it cannot be equivalent to some finite conjunction of singular sentences.

But this will not rule out sentences like 'All members of the Greenbury School Board are bald', which does not name the members of the School Board, and so cannot be equivalent to a conjunction of singular sentences. Nor will it do to object that the Greenbury School Board has and will have only a small and finite number of members, since to restrict the concept of law to statements with an indefinitely large or infinite number of cases might rule out many useful principles of, for example, astronomy (Hempel, 1965, ch. 12).

In any event, to rely here on the concept 'number of cases' is awkward, as the above sentence is logically equivalent to 'Anyone who is not bald is not a member of the Greenbury School Board', which has an indefinitely large number of instances.[10]

Thus, while being of essentially generalized form may be an interesting feature of many lawlike statements, it is not sufficient to discriminate lawlike sentences, since it does not rule out inappropriate cases, and it cannot be held necessary, since some sentences that should be counted as lawlike are not of this form. (Hempel, however, tends to discount this latter point on the grounds that such laws will normally be derivative.)

A second criterion for distinguishing lawlike sentences from other universal conditionals, put forward by Hempel (1965, ch. 10, esp. pp. 264–70), is that a lawlike sentence should contain only purely qualitative predicates, ones whose 'specification of meaning' makes no reference, direct or indirect, to any particular object, time or location. The most obvious difficulty with this criterion lies in its central notion, the 'purely qualitative predicate', for it may not always be possible, and certainly will not often be easy, to determine unequivocally whether the meaning of a term requires direct or indirect reference to same particular object, time or place.[11] Nor can this problem be evaded by an appeal to a formalized language, since it simply arises again for the primitive terms of that language.

In any case, the criterion is not adequate. For example, 'All model T Fords are black' would count as having purely qualitative predicates, and, if not true now, was probably true at one time. But it would be absurd to elevate one of Henry Ford's idiosyncrasies to the status of a potential law of nature. Moreover, the notion of the meaning of a term in a lawlike sentence may be in as much need of explication as the notion of a general law. Finally, some principles that function in science as laws do actually make reference to particular objects, times and locations, such as Galileo's law of freely falling bodies, and it cannot be assumed without argument that all such laws are derivative.

Perhaps the most elaborate attempt to explicate the concept of the lawlike sentence is Goodman's (1955, ch. 4). He begins directly from the problem of counterfactuals. Lawlike sentences are capable of being supported by observed instances: after a single series of well-planned, properly conducted tests, we can say 'this drug is toxic' (see Mayo, 2010, in Mayo and Spanos, 2010, pp. 113–24). Hence predicates appearing in them can be 'projected' from examined to unexamined cases. The relative extent of such projectability depends on the entrenchment of the predicates involved – that is, the extent to which the predicates have previously been used in successful projections. A well-entrenched predicate is one that has been used in a large number and variety of successful generalizations, like 'green' or 'square'; whereas a predicate like 'member of the Greenbury School Board', lacking entrenchment, could not be expected to support counterfactuals (Goodman, 1955, ch. 4).[12]

But this seems simply to transfer the difficulties from 'lawlike sentences' to 'entrenched predicates'. These are the various different predicates, each one considered together with any predicates coextensive with it that have been successfully projected in the past.

But the fact that one projection or many worked in the past is no reason in itself to suppose even that the same sentence as that projected will be confirmed by its instances now or in the future, let alone to suppose that such success justifies putting the predicate involved in new projections. In short, 'entrenched predicate' is used to mean 'predicate that appears in sentences confirmed by their instances', but the issue is still why some sentences should be so confirmed while others are not.[13]

Indeed, we do not know from this account why any universal conditional sentences should be treated as confirmed by their instances, or even what is to count as 'confirmation'. Nor have we any criterion by which to distinguish 'sentences confirmed by their instances' from other sentences, except the appearance in the former of 'entrenched predicates'. But these are 'successfully projected predicates', and, as just argued, this begs the question, for the whole point of the counterfactual problem is that one swallow does not make a summer; nor indeed do many.

These objections lead to the heart of the matter. As Goodman himself has observed, the difficulties connected with the concept of 'lawlike sentence' are simply the reflection of the difficulties presented by counterfactuals, which are in turn part of the more general problem of induction. The reason is that to give an account of lawlike sentences, it must at least be possible to distinguish them from others of universal conditional form.

But an obvious fact about scientific laws is that they support counterfactuals and are sentences for which induction is justified, at least in some sense in which it plainly is not for other universal conditionals. Thus to characterize lawlike sentences, to mark them off, requires a criterion by which we are entitled to say 'there, counterfactuals can be supported', 'here, induction will be justified, and here it won't'. The point can be put concisely as follows:

Lawlike sentences support counterfactuals; they license appropriate passages from the observed to the unobserved. Call this property of lawlike sentences the inductive property, *I*. Suppose there is an observable property, *P*, which, it is alleged, is always present when *I* is present. Then if *P* and *I* are analytically equivalent, the difficulties posed by *I* will simply be transferred to *P*, as in the case of Goodman's projectibility.

If they are not analytically equivalent, then if *I* implied *P*, *I* could fail to hold when *P* did, in which case *P* might pick out some non-lawlike sentences, and if *P* implied *I*, *P* could fail to hold when I did, in which case *P* might fail to pick out some lawlike sentences.

Suppose finally that *P* and *I* are analytically unrelated, but in all observed cases are perfectly coincident. To know this, we would have to know how to identify the property *I*, which is precisely the problem. We might try to evade this difficulty by compiling a list of sentences generally accepted as lawlike, but then the question arises of whether the accepted is necessarily acceptable. Accepted by whom? In virtue of what?

Second, why should property *P* stand in this relation to lawlikeness, to which, ex hypothesis, it bears no logical relation? How could such a circumstance be explained?

Third, even supposing the relationship between *P* and lawlikeness were to hold for all known sentences of lawlike form, how do we know that it will continue to hold for laws that have not yet been discovered or proposed?

This raises the subsidiary question of the status of '*P* is constantly conjoined with *I*'. Is this lawlike? If it is, this violates the theory of types; if it is not, then there is no reason according to the argument to suppose that *P* and *I* will be constantly conjoined.

These considerations show that the problem of induction is not as harmless as the modern consensus has thought. This will not surprise economists and econometricians, for they know very well that the past is not a reliable guide to the future, and that powers ascribed to agents or things in one situation may not hold in another. What is inflationary at one time may not be at another; the stimulus package that worked for one economy may not work for another. The econometric model that tracked and predicted output for one period may break down in the next. Economists would like to know why. They would also to like to know why their lawlike sentences break down so often. When something is water-soluble or resistant to drilling yesterday and today, it generally will be tomorrow, too. Rocks don't soften up; aspirins still dissolve. But inflationary policies unexpectedly dampen down; stimuli surprise us by not stimulating.

Finally, it is possible to go a step further, and convert the impossibility of justifying induction on empiricist grounds into a more general criticism of empiricist epistemology. Such a move is a natural extension of Lewis's (1929) remarks on memory. Lewis has argued that judging memory to be reliable involves a problem partially distinct from, but precisely analogous to, that of induction, since memory can be judged reliable only on the basis of past performance, which, in turn, must be remembered, and which, in any case, provides no guarantee of future reliability.

He offers a solution to the problem of determining whether memory is reliable, which turns on treating knowledge as a whole, bound together by congruence relations and tied to the world at various points by 'terminating judgements' (judgements arising from direct experience, which can be decisively and completely verified).

But such judgements involve assessing the facts, which requires comparison and measurement, and therefore reliance on constant standards and measures; indeed, even making terminating judgements involves the recognition of resemblances, and for this to be possible we must reliably remember similarities, and we must reliably retain our perceptual skills. Hence, failure to solve the problem of induction undermines Lewis's accounts of memory and perception.

The same sort of difficulty could be raised in other contexts; one could say, in fact, that the problem of induction ramifies through the whole of empiricist epistemology. Not even Hume is free from its damaging implications, for he must explain why people have continued to believe in induction. This he does on grounds of habit; but just because certain habits have been formed in the past does not guarantee that they will be in the future. Indeed, any adequate explanation of belief in induction runs up against the same difficulties that beset justifications of induction.

At this point it might seem as if nothing can be said. But a new approach has emerged recently.

3.1.6 Severe Testing

Deborah Mayo and Aris Spanos (2010) have recently taken up the challenge of providing an account of empirical testing, starting a new line of discussion, and provoking responses, for and against, from Achinstein, Chalmers, Cox, Glymour, Laudan, and Musgrave, among others. Their new approach stems from Popper, whom they clearly revere but seek to improve on. Popper rejects the idea of verification; statements cannot be verified, but they can be falsified. From an account of testing particular statements, they seek to establish general statements, on the basis of well-tested particular evidence, including providing an account of how to pick out those general statements or relationships that are 'lawlike' or reliable – for example, claims that a certain drug is or is not toxic (see Mayo, 2010, in Mayo and Spanos, 2010, p. 120), implying a counterfactual, and that are therefore suitable to incorporate into theories.

This leads to questions about how to account for theory: how can theories be confirmed, and if they cannot be confirmed, why should we accept them? Popper, of course, contended that scientific investigators should seek to falsify hypotheses and theories. Only propositions that could in principle be falsified had scientific status, and only propositions that had in practice survived tests that might have falsified them should be accepted provisionally – 'provisionally', because that is the best we can do. All genuine scientific knowledge is ultimately provisional.

As is well known, there are serious problems with this. The statement of the approach itself is not in any obvious way falsifiable, so on its own account, it does not express knowledge. Why accept it, then?[14] Suppose a theory or hypothesis passes some tests and is falsified by others. Why do the negative tests matter more than the positive? Is one negative test enough? If not, how many tests are needed to decide?

Mayo and Spanos (2010) introduce genuinely new dimensions to this discussion. As noted, just because a hypothesis has shown that it can pass some empirical tests does not 'confirm' it; there is no logical support. But passing tests must mean something. Accordingly Mayo proposes a further step, to require the hypothesis to pass *severe* tests. The notion of 'severe testing' becomes a criterion of support, and also offers a way of picking out reliable or lawlike propositions. The test must be difficult to pass; that is, it must be designed so that it is unlikely to offer support if the hypothesis is false, where 'unlikely' can be expressed formally, in probability terms. The test must be applied carefully, publicly, and must be repeatable by others.[15] If a hypothesis passes severe tests, this will support the adoption of a rule or rules of inference, that will validate claims of the form that evidence, e, supports hypothesis H. Mayo's approach is summarized by Chalmers (2010, in Mayo and Spanos, 2010, p. 58): 'A hypothesis H is severely tested by evidence e produced by test T if and only if H fits e and T has a low probability of yielding e if H is false'.

Mayo provides a formal account of severe tests and error elimination in her discussion of error statistics.[16] Spanos[17] provides further probabilistic foundations for these rules, allowing for the rules to be expressed in terms of specific probabilities; for example, 'rule R will provide a correct inference from evidence e to hypothesis H 95 per cent of the time'. In regard to economics, Spanos develops the idea of a model of statistical adequacy as a form of 'severe testing'; the criteria of statistical adequacy will rule out many widely used formulations that are often thought to be consistent with commonly used data sets.

Spanos finds such inadequacy in well-known treatments of the consumption function; yet we have to ask, suppose a hypothesis is shown to fail to meet the criteria of statistical adequacy; what then? How inadequate, in exactly what way? How much does the inadequacy matter?[18] (In the consumption function example, it was not clear that the error made a serious difference.) And suppose the test is met, the severe test is passed, and the hypothesis is adequate; how good is it? How much do we know as a result? In general, the scientific issue is not whether a hypothesis is wrong, but rather whether or not it is on the right track, moving in the right direction. Very often, we assume the hypothesis is inadequate, and the question is: how to improve it?

So the basic question still comes down to: 'what makes evidence, *e*, count as support for hypothesis *H*, or, even better, as support for improving *H*?' What, indeed, does 'support' mean, since verification is not possible? Passing a severe test, to be sure; but will passing the test today ensure that it will be passed tomorrow? True, the test is designed to be unlikely to be passed if *H* is false – but this just shifts the problem to 'unlikely'. This is based on probability, but how can we be sure the sample was a good or true sample? Confirmation, as in confirming a theory, is equally out of the question; what justifies accepting a theory, then? What justifies working with it and trying to improve it? What does 'improving' a hypothesis or a theory mean, and how do we know when a change is an improvement? Severe testing is a good idea – it is not easy to establish scientific relationships, and statistical adequacy is likely to prove a great step forward in econometrics. But neither really gets us past the problem of induction.

3.2 SKETCHING AN APPROACH TO A SOLUTION

In practice the problem is not 'support'; of course, evidence e supports hypothesis *H* if it is good evidence, where goodness is judged according to normal procedures. This will hold, provided the approach is defined properly. By this, we mean that the problem being investigated has been identified as a scientific problem, one in which scientific variables can be defined, and relationships between them sought, so that the aim of the project is to unearth, or define, uncover, or find and make precise, such scientific relationships between properly defined scientific variables. 'Improving' a hypothesis or a theory means defining the variables and relationships more precisely, so that they fit the evidence better and show more clearly how the real system works.

The point is, when we set out on a scientific investigation, we already know that scientific laws exist in this area – if we did not, it would not be possible to define scientific variables or conduct a scientific investigation. If we know that scientific laws exist in the area of investigation, then evidence *e* will support hypothesis *H* if it is good evidence, and if it is really good it will point in the direction in which the theory can be improved.[19]

So the prior question is whether or not there are scientific laws, in general – is science possible? And in a particular field – is this a possible science? The *existence* of scientific laws must be established in the general case by a philosophical argument; in particular cases, by conceptual analysis and fieldwork. The general case is straightforward, connect Kant by way of Strawson: we know that 'laws' exist, because they *must*; if they did not, then the world would not be the way it is; more especially, the world would not work or behave the way it does. We could not know or describe the world; we could not perceive details or draw distinctions; we could not even raise these questions or have this argument, unless there were reliable relationships between variables that relate to our powers of perception – variables expressing aspects of space, time and matter, for example. This is the argument that has to be made in detail, for each area of science. Chapter 4 will consider, first, the physics of the ordinary world, then will follow more or less the same line of argument, the basis for econometrics – the so-called 'data generating mechanism'.

The role of evidence is to say what the relationships are – whether they are linear, nonlinear, positive, negative, and so on. We need to know how to define and measure the variables – in what units to express them. Evidence tells us *what* the scientific relationships are, not *whether* they are. And here, severe testing indeed finds its place, but it is not in establishing that there are laws, but rather in specifying what those laws are.

Much the same applies to theory and theory confirmation. If a theory is well designed and its variables well defined and measurable – so that the variables apply to the world – then 'confirmation' is neither possible nor necessary. The variables apply and can be measured, so the relationships can be established – and severe testing will make them precise, and will correct errors. Conceptual analysis and observation will interact here in the defining of the variables and establishing of relationships – and then fitting them all together in a model. This, we argue, is the crucial step in modern scientific investigation (see Chapters 4 and 5).

Once a model is set up, it has to be put to work: what does the model tell us, and what can we discover from running it, analysing it? This should give us new insights and new perspectives from which to re-investigate the world, gathering new data and looking at old data with new eyes.

NOTES

1. Mayo and Spanos's (2010) book collects twenty essays from leading figures in philosophy, economics and statistics. The book has injected new ideas into the study of scientific inference and provides a bridge between the current philosophy of science and the scientific practice. It examines important topics such as statistical inference, reliability, theory testing, causal modelling and the relation between theory and experiment. The editors of the book think that venerable philosophical problems surrounding induction, scientific inference and objectivity can be solved.
2. The problem can be described either as one of formulating a general law, applying to unobserved cases, on the basis of observed cases, or as one of confirming through observation a general hypothesis applying to unobserved or unobservable cases. Some philosophers have found here a basic distinction between inductive procedure, the discovery and formulation of scientific hypotheses, and the logic of confirmation. The former description they have held misleading, since scientists do not in fact collect data and then 'induce', partly because most hypotheses contain theoretical terms, like 'electron', themselves constructions out of data. Science is said to proceed by 'the method of hypothesis' or by 'conjecture'. This is surely correct, but not philosophically very important. Whichever terminology we use, the same problems arise. For a discussion of this distinction see Hempel (1966, pp. 10–18).
3. The problem of induction arises in connection with statistical probability in the following way: given a set of *A*s, of which some fraction·, *b* appear to be *B*s, it is argued that if, as the *A*s increase indefinitely, the proportion of *B*s that are *A*s tends to b as the limiting value, then *b* can be regarded as the probability of an *A* being *B*. But, of course, observed *A*s are not infinite in number, nor do we normally know (in the interesting cases) what proportion of all As we have actually observed. When the number of *A*s is infinite, however many the observed *A*s, they will constitute only an indefinitely small proportion of all *A*s. To define *b* as the probability of *B*s among *A*s on the basis of observation, it is therefore necessary to assume that the observed *A*s are a good or fair or typical sample of the whole class. But such a sampling assumption begs the question.
4. The discussion of induction here and elsewhere is selective and idiosyncratic. Important contributions such as L.J. Cohen's (1989) and Hacking's (1983b) – and many others – are left aside, so that we can concentrate on well-known arguments – centering largely on Hempel (1966) and Goodman (1955) – that lead to the conclusion that the problem lies in empiricism itself. We would make a different selection were we tracing the history of thought.
5. For Mill, see Nagel (1950, bk. III, ch. III). For Keynes, see Russell (1948, part V, ch. V), or Pap (1962, ch. 10) and Vellupillai (2000, ch. 5).
6. See Madden (1960) (particularly the paper by Pierce, part 6, pp. 296–9) and Reichenbach (1938, pp. 339–63).
7. See, for example, Clower (1994); Goodman (1955, esp. chs. III, IV); Harre (1960, ch. 5); Hempel (1966, pp. 10–18); Katz (1962); Ramsey (1931, esp. p. 197); and Swamy et al. (1985).
8. Some, like Quine, have extended this doctrine to all statements whatever, claiming that 'no statement is immune to revision'. Hollis and Nell reject this; Nell (1976a) advances a self-referential argument against the position.
9. Cohen in turn offers a theory that tries explicitly to justify, in terms of linguistic conventions, the ascription of natural necessity to empirical statements of universal conditional form. Success in this enterprise would, of course, also validate induction,

although this does not appear to be Cohen's main aim. Statements of natural necessities, he argues, have their truth-values fixed a priori, but the meanings of the terms in such statements are relatively open. The process usually described as experimental confirmation he explicates as one of 'precisifying' – that is, as delimiting the terms more precisely – a position that has some affinities with Toulmin's (1958) views, which we shall discuss shortly. Cohen presents no reasons justifying the procedure of taking a non-analytic statement's truth-value as fixed. Even granted that perhaps we do so, why should we ever do this? What makes it a reasonable course of action? (Its success? In what?) Nor does he provide any criteria for distinguishing cases in which this is appropriate from cases in which it is not. On what grounds are we to decide whether a given non-fulfilment 'precisifies' the terms or falsifies the statement?

10. Nagel's suggested criterion, that the 'scope of predication' must not fall into 'a fixed spatial region or a particular period of time', breaks down because of this ambiguity, as Hempel points out. For example, Nagel's criterion would disqualify 'All apples in this basket are red', while admitting the logical equivalent, 'Anything that is not red is not an apple in this basket' (see Nagel, 1961, p. 58).
11. Even if the problem of meaning could be solved, serious questions would remain. What should be done, for example, about terms equivalent to terms containing particular references? Should they be ruled out only if all such equivalents contain particular references? In that case nothing will be ruled out since some description free of such references can always be constructed. But if terms with some equivalents containing particular references are to be ruled out, then everything will be excluded, since such an equivalent can always be formed.
12. Hempel objects that Goodman's account, because it is based on the entrenchment not merely of a predicate but also of the class co-extensive with the predicate, admits as lawlike sentences that need not be essentially generalized. Hence the set of lawlike sentences admitted by this criterion will be too inclusive to serve in models of scientific explanation (Hempel, 1965, pp. 342–3).
13. Goodman himself appears to think this question can be bypassed. In practice some hypotheses are accepted on the basis of a limited number of positive instances and others are not; since this is so, the accepted ones must be acceptable, and the only question, therefore, is how the set of acceptable hypotheses can be characterized. Such an argument is obviously fallacious. Of course, a careful classification of acceptable hypotheses might go a long way towards explaining what makes them acceptable; but this is quite different from maintaining that no problem exists (Goodman, 1955, pp. 59–66).
14. Falsification, as a strategy, has limitations. For example, 'all haystacks have needles' cannot be verified, but to be falsified we would have to be able to demonstrate 'this haystack has no needle', which cannot be done conclusively, since we might always overlook a needle (Nell, 1998a, pp. 75–80). The principle of falsification cannot itself be falsified, nor can it be verified; and it does not make sense as a 'recommendation'. The significance of this is examined (ibid., pp. 81–7).
15. Observe that the proposition 'Hypothesis *H* cannot (does not?) pass Test *T*' is an inductive generalization supporting counterfactuals, exactly the sort of statement at issue.
16. For more on error and inference see Mayo's (1996) *Error and the Growth of Experimental Knowledge*. Her book offers a launching point for addressing different problems of inference and evidence in the face of uncertainty and errors.
17. Although we are drawing on Spanos's overall work on the philosophy and methodology of econometrics (Spanos, 1986; 1989; 1990a; 1990b; 1995; 2000; 2005; 2006a; 2006b; 2006c; 2007; 2009; 2010) we will refer here mainly to Spanos (2007; 2009; and 2010). For an account of Spanos's discussion of statistical inadequacy, see Nell and Errouaki (2006c).
18. As we shall see, and as McCloskey (1985b, 1996) has repeatedly urged, a similar problem arises with tests of statistical significance. Furthermore, Ziliak and McCloskey (2008) have examined eloquently the chronic abuse of significance testing in economics.

They discussed various philosophical/methodological issues pertaining to the problem and proposed a 'what to do' list of recommendations to address the problem. As Spanos (2008, p. 154) argued: 'The stated objective of this book is to bring out the widespread abuse of significance testing in economics with a view to motivate the proposed solution to the long-standing problem of statistical vs. substantive significance based on reintroducing "costs and benefits" into statistical testing. The authors (Ziliak and McCloskey) strongly recommend returning to the decision-theoretic approach to inference based on a "loss function" with Bayesian underpinnings, intending to ascertain substantive significance in terms of "oomph", a measure of possible or expected loss or gain'. The important question is not whether something passes a statistical significance test, but by how much it is right or wrong, and in what direction.

19. This is not a lightly disguised form of assuming the 'uniformity of nature'. First, it is not an assumption; the necessary existence of reliable relationships between scientific variables must follow from our ability to perceive a stable environment. There must be such relationships as a presupposition of our ability to perceive, draw distinctions and measure that environment. This will be a matter of argument based on evidence and conceptual analysis. Second, we are not talking about nature in general; in each case, in each area of proposed scientific investigation, the case for the existence of scientific laws will have to be established. (The easiest and most convenient way to do this, of course, is to provide the evidence and set it out.)

We can hardly describe such a thing as a law of nature without referring to certain principles of analysis. And the phrase, '*in the natural sciences we have stable laws*', means not much more and not much less than this: The natural sciences have chosen very fruitful ways of looking upon physical reality. So also, a phrase such as *'In economic life there are no constant laws,' is not only too pessimistic, it also seems meaningless*. At any rate, it cannot be tested. But *we may discuss whether the relationships that follow our present scheme of economic theory are such that they apply to facts of real economic life*. We may discuss problems which arise in attempting to make comparison between reality and our present set-up of economic theory. We may try to find a rational explanation for the fact that relatively few attempts to establish economic 'laws' have been successful. *I think considerable effort should first be spent on clarifying these restricted problems*.

Haavelmo (1944, pp. 12–13, italics added)

Whatever is the ultimate nature of reality, it is indisputable that our universe is not chaos. *We perceive beings, objects, things to which we give names. These beings or things are forms or structures endowed with a degree of stability*; they take up some part of space and last for some period of time.

Thom (1975, p. 1, italics added)

It is interesting to note that except for some recent books explicitly about methodology [Hollis and Nell, 1975; Stewart, 1979; Blaug, 1980; etc.], *economics writers have rarely been concerned with this allegedly fundamental problem (the Problem of Induction)*. For most of the nineteenth century, economists simply believed that the Problem of Induction had been solved; thus it did not need any further consideration. Newton seems to claim to have arrived at the laws of physics from scientific observation using inductive methods. *In Adam Smith's time, inductive generalization was the paradigm of rational thinking; Newton's physics was the paradigm of inductive generalization.*

Hume's critical examinations of logical justifications for the acceptance of inductive proofs were largely ignored. Most thinkers continued to believe that there was an inductive logic. Thus there was no apparent reason to doubt the claims made for the scientific basis of Newton's physics. *And there was no reason to doubt the possibility of rational (i.e., inductive) decision-making.*

Boland (1982, p. 15, italics added)

4. Variables, laws and induction I: are there laws of nature?

INTRODUCTION

During the 1950s and 1960s, leading econometricians developed large-scale and detailed models of the economies of the USA and the UK; many smaller models were also developed. These models worked well enough that they became accepted as aids in policy-making, in particular, as guides to understanding the likely results of policy interventions. But in the 1970s they broke down almost everywhere when they were most needed – that is, when crises emerged, hitting the economies of the world with shocks. They failed to predict the onset of domestic and foreign exchange crises, nor did they accurately portray the effects of the oil shocks; in addition, they failed to forecast the consequences of policy responses.[1] But worst of all, they failed to capture the essential feature of the decade: stagflation, the simultaneous emergence of serious recession and strong inflation. Indeed, since these models tended to be built around a well-established Phillips Curve, simultaneous inflation and recession was not a possibility in them. Yet the models rested on strong empirical work; by all reasonable standards, they were well confirmed. Apparently reliable relationships unexpectedly changed or simply gave wrong answers. It appeared that inductive methods had failed.

Were the theories wrong? Were the methods inadequate? What should econometricians have done differently? Surely this would not have happened in physics; well-established relationships wouldn't just disappear. Haavelmo (1944, ch. 2, pp. 12–15) raised similar issues, when he asked 'why economics, so far, has not led to very accurate and universal laws like those obtaining in the natural sciences'. He (1944, p. 16) went on to ask:

> How far do the hypothetical 'laws' of economic theory in its present stage apply to such data as we get by passive observations? By 'passive observations' we mean observable results of what individuals, firms, etc., actually do in the course of events, not what they might do, or what they think they would do under certain other specified circumstances. [. . .] We have to [. . . ask . . .] what we are actually trying to achieve by economic theory. We have to compare its

> [. . .] idealized experiments with those which would be required to reproduce the phenomena of real economic life that we observe passively [. . .]

Haavelmo (1944, ch. 2) raises the question of the degree of permanence of economic laws, asking how to judge the degree of persistence over time of relations between economic variables, holding (1944, p. 13) that these problems are '[. . .] directly connected with the general question of whether or not we might hope to find elements of invariance in economic life, upon which to establish permanent laws'.[2]

To get to the bottom of this, it will help to establish and define the foundations on which reliable econometric relationships rest; in what sense they are reliable or well confirmed, and how do they compare with laws in the natural sciences? Can econometrics predict? The early econometricians wanted to predict what would happen after World War II ended! We can start with the very general question of when and whether any laws are 'justified', in either the natural or the social sciences. Then we shall spell out what a scientific variable is, and how scientific variables enter into functional relationships. We shall then argue that there must be scientific relationships – that is, that such relationships must exist – in both the natural and the social sciences. But we shall see that there are important differences between the two kinds of science.

Part of the programme of structural econometrics was to find and numerically estimate such laws. This project, we argue, is reasonable, justified and important – except for the fact that those carrying it out thought they were looking for laws of the same kind as those in the natural sciences, whereas the laws of economics are significantly different.

4.1 RE-EXAMINATION OF SCIENTIFIC LAWS

To see what is involved in formulating a law, and how this differs from determining its range of application, let us take an example from mainstream economics, bearing in mind that we are looking for features that are common to laws in the social and natural sciences.

4.1.1 Laws of Consumer Behaviour

Consider an ideal consumer: rational, well informed about the properties of the commodities she buys, fully aware of her own likes and dislikes, and possessed at all times of perfect information about the state of the market. From the commodities she consumes, she obtains 'utility', or satisfaction; putting the same thought another way, she consumes quantities of commodities in accordance with her relative preferences. (In practice, such a consumer will normally be a household or an institution rather than a person.)

We describe the consumer's preferences by a utility function, written

$$u = u(x_1, \ldots, x_n) \tag{4.1}$$

where $x_1, \ldots, x_n$ are the n commodities available in the economy.

Utility is assumed to increase with each increase in the amount of a commodity consumed; hence

$$du/dx_i > 0, i = 1, \ldots, n. \tag{4.2}$$

But as the consumption of any good increases, the utility yields of still further increases will fall; hence

$$d^2u/dx_i^2 < 0, i = 1, \ldots, n. \tag{4.3}$$

Utility need not be conceived as a definite amount; there is no need to know how much utility a given bundle of commodities yields, so long as the marginal utilities can be calculated. Hence we can replace (4.1) by the functional U, consisting of all monotonic transformations of (4.1):

$$u = U(x_1, \ldots, x_n) \tag{4.4}$$

where $U' > 0$, $U'' < 0$.

This is sometimes called the law of diminishing marginal utility.

To obtain the consumer's equilibrium pattern of purchases we introduce prices, p_i, and the consumer's income, y:

$$y = p_1x_1 + \ldots + p_nx_n. \tag{4.5}$$

Then to maximize utility subject to income we form the expression

$$v = u + (y - p_1x_1 - \ldots - p_nx_n) \tag{4.6}$$

which we differentiate, setting the derivatives equal to zero.

$$\delta v/\delta x_1 = u - \lambda p_1 = 0$$

$$\delta v/\delta x_n = u - \lambda p_n = 0$$

$$\delta v/\delta\lambda = y - p_1x_1 - \ldots - p_nx_n = 0$$

Here λ is a LaGrangean multiplier. The solution of these equations will give the set of xs that will max u subject to the given prices and income. The conditions imply that the marginal utilities should equal the ratios of the corresponding prices and that marginal utility should be diminishing.

Equations (4.1)–(4.6), together with the definitions of the terms, contain all that is essential to the model.[3] Carrying the argument forward, the familiar income and substitution effects can be derived, and criteria for complementary and substitutable goods can be formulated. Allowing prices to vary will permit the derivation of demand curves and so on.

But it is a striking characteristic of Equations (4.1)–(4.6) that there is nowhere any mention of the consumer; yet these equations make up the model!

The utility, of course, is the consumer's utility; the quantities of commodities are the quantities of commodities the consumer consumes, and, if an actual agent could be found who is rational, well informed, and so on, the model would (perhaps) describe such an agent's (ideal) behaviour.

But however much the agent may be at 'the back of our minds', in any mathematically precise formulation of the law of diminishing marginal utility, or of the conditions for consumer equilibrium, there will be no symbol referring to a consumer, any more than there is a symbol in the gas laws referring to gas.

Far from 'quantifying over consumers' (in the logician's sense), as the positivist account of laws would require, the law is formulated quite independently of any reference to them; they become significant only when the question of application arises.

Quantification in the logician's sense is relevant, however, in specifying functional relationships. For example, in production theory (where the complications of 'ordinal' utility are not present) the production function states, in effect, 'for every level of output there exists a set of input combinations that will produce it, such that successive unit decreases in any one input can be offset by progressively and sufficiently increasing another'. This is known as the law of the diminishing marginal rate of substitution. But is it generally true? Pharaoh ordered the Israelites to make bricks without straw; what were they to do, use more clay?

The law of diminishing marginal productivity makes no mention of the 'representative business firm' that the production function is meant to characterize. Nor is the quantification indicated above trivial or dispensable, for in production theory, unlike utility theory, it is usually thought that 'marginal rates of substitution' will turn negative.[4]

The point can be seen in a made-up example. Suppose we discover, among certain apples, a relationship between their sweetness and colour, such that sweetness, S, increases with redness, R,

$$S = f(R), f'(R) > 0, 0 < R < R'$$

where R' indicates that the apple is over-ripe and turning rotten. It could be argued that this 'really' means

$$(x)\ (Ax \Rightarrow Sx),$$

where 'Ax' is interpreted as 'x is an apple' and 'Sx' is interpreted as 'x is characterized by the functional relation $S = f(R)$'.

But as a matter of fact the relation with which we began does not say that. For it remains to be determined for which kinds of apples Jonathans, Mackintosh, Delicious, and so on the first relationship holds. The relationship might differ significantly for different sorts of apple; for instance, sweetness may increase faster per unit colour change in Jonathans than it does in Delicious, and R' may differ, and so on. It may be that some more general relation between color intensity, e, and sugar content, s, can be found: $s(e)$; this, in turn, may hold in one form or another for many various kinds of fruit, so that another variable, representing kinds of fruit, could be introduced. But in this case, 'kind of fruit' is a variable, F, exactly on a par with 'degree of sweetness', 's' and 'colour intensity', 'e', and instead of the second relationship we would have a third relationship: $S = g(s(e), F)$. Laws relate variables to one another in a precise manner; laws are predicates of things, the bearers of the variables.

To put it another way: utility must be some consumer's utility, a level of output must be some firm's level of output, just as a velocity must be some body's velocity and a pressure must be a pressure on f (say) a gas. But nowhere in the textbooks of economics, any more than those on physics, will a mathematical statement of a law contain any variable referring to the things to which the law applies. The statements of laws are statements, fully quantified and complete in themselves, expressed in mathematical language, and formulated without reference to the things they are about.

4.1.2 Using the Predicate Calculus

This is the practice of mainstream economists. But some philosophers of science have argued that nevertheless the laws so formulated can profitably and revealingly be reformulated in the canonical notation of the predicate calculus.

How could one put the law of diminishing marginal utility into, say, the form, $(x)\ (Px \Rightarrow Qx)$ without distortion? The law is a functional relationship between variables; the suggested form asserts that one predicate of x implies another. Of course it can be done. But doing so conflates relationships that differ in philosophically significant ways. To see this, put diminishing marginal utility into philosophically lawful form:

$$(x)\,(y)\,[(Cx.Gy.Pxy) \Rightarrow (Ez)\,(Uzx.Fzy)]$$

where 'Cx' is interpreted as 'x is a consumer', 'Gy' is interpreted as 'y is a quantity of a good', 'Pxy' is interpreted as 'x possesses y', 'Uzx' is interpreted as 'z is a degree of utility accruing to x' and 'Fzy' is interpreted as 'z increases with y at a diminishing rate'.

Far from spelling things out helpfully, or in any way revealing the logical structure of the law, this compresses everything in Equations (4.1)–(4.6) into a single two-place predicate, 'F'. The form of the positivist analysis would not be changed if z decreased with y, remained constant, or varied in any other way whatever. This simply reflects the fact that the proposed legal form is invariant with respect to widely different functional relationships, provided only that they share a superficial logical structure. So we could redefine the variables and relationships as follows: 'Cx' and 'Gy' are interpreted as before; 'Pxy' is interpreted as 'x paid for y', 'Uzx' is interpreted as 'z is the amount of money x paid' and 'Fzy' is interpreted as 'z is the amount of money y cost'. On this interpretation, the law of diminishing marginal utility has the same form as the analytic statement that if x paid for y there is some sum of money that he paid for it and which it cost.

Nor is it surprising that philosophers attempting to put scientific laws into universal conditional form should have fallen into this confusion. The predicate calculus does not distinguish different kinds of predication, which is unfortunate, as even a glance at the variety of predicates in candidates for lawlike sentences will show.

Many philosophers of science have at times used examples such as 'All ravens are black' or 'All mermaids are green', in which the first term individuates and the second term characterizes. (The first kind of term says what something is, or what the members of some class are; the second kind describes or qualifies.) These examples are perfectly good statements, but if, as we have argued above, scientific laws normally involve mathematically precise functional relationships, both or all variable terms will have to be characterizing terms.

Individuating terms must play a part in the statement of a law's range of application, but they are out of place in statements of functional relationships. The problems posed by using the predicate calculus as the sole instrument of analysis can be brought out more closely by considering the differences between various examples of statements that have been considered lawlike:[5]

(1) All storks are red legged.
(2) All sodium salts burn yellow.
(3) All white phosphorus is soluble in turps.
(4) All gases expand when heated.
(5) All fathers are male.
(6) All unicorns feed on clover.

We have just argued that the form of (1) is inappropriate for a scientific law, and (2) differs from (1) in form only by applying the characterizing term not to an individual or a class, but to a kind of stuff (cf. 'All water freezes below 32°F'). Statement (3), in turn, differs only in that a relation is said to hold between two kinds of stuff so that again there is characterization without variation. (However, (2) and (3) might be argued to be 'lawlike' in a sense that (1) and 6) are not.) Statement (5) is analytic, and even Hempel regards it as a dubious instance of lawlikeness. Statement (6) raises special ecological problems, but is roughly of the same form as (1). Only statement (4) asserts that certain variables are related in a definite way (though perhaps (2) and (3) could be rewritten to show this). But we are told very little about the exact nature of this relationship, or about the limits of variation between which it holds.

Statement (4) is hardly a statement of a scientific law, though it might be used to refer to or describe a law. Many examples like (4) appear in works on the philosophy of science (for example, 'All iron bars increase in length when the temperature rises'). These statements are never precise in the way scientific laws should be; they fail to state the exact relationships with which science must operate. They are, in fact, not laws but statements that express generally what laws state precisely. What the positivists present as an analysis of scientific laws is rather the form of an expression that might be used to refer to laws. Their analysis has a certain initial plausibility because it presents the form of the statement of a scientific law's range of application, and one most easily refers to a law by giving its extension (in the logician's sense of the term).

In general, then, it would be most appropriate to think of the positivist programme as an attempt to provide an analysis of the statement of a law's range of application. But the extension of a law is not a law.

4.2 SCIENTIFIC VARIABLES

A correct and complete statement of the kind of scientific law capable of playing a role in a model or theory makes no reference to what the law is about. It therefore does not contain individuating terms or 'stuff' terms, and it must contain terms capable of variation, of which characterizing terms are perhaps the clearest examples.

Three kinds of terms for ‘universals’ can usefully be distinguished. These distinctions can be presented in two ways, as distinctions between kinds of predicate terms, or as distinctions between kinds of properties or attributes. (We shall use both kinds of language; nothing in our argument turns on a choice between nominalism and realism.) The distinctions are:

Terms: characterizing; mass; count or individuating
Properties: characteristic; stuff; sortal universal or sort of thing

Each kind of term appears in its normal role in such phrases as ‘red stone staircase’, or ‘heavy oak chest’. Characterizing terms are adjectival, the other two substantival; characterizing terms single out and present one quality only, whereas the other two suggest a large number of distinguishing features. Count-words present the idea of something with a definite shape and precise limits; mass words, if material, denote a substance identifiable independently of its form, for example silver, butter, molasses, or, if immaterial, usually denote a state or condition, for example leisure, traffic, safety, success. Some reasons for the importance of these distinctions will be developed below.[6]

To expand the claim above to support an alternative account of laws we shall first have to examine the relationship between variables and characterizing terms more closely. Roughly, but suggestively, a characterizing term is essentially adjectival rather than substantival. A particular thing’s characteristics may vary, but its ‘thing’hood cannot; a lamp can be taller or shorter, red or blue, more or less attractive or expensive, but not more or less a lamp. (Instances of a kind can be better or worse instances, but it is precisely the variation of degree in some of their characteristics that makes them so.) Characterizing terms can be grouped according to the precision with which they determine that to which they apply. (Thus ‘colour’ is general relative to ‘red’, which is general relative to ‘scarlet’; ‘long’ is general relative to ‘34 feet long’, etc.).

To say that something is a metal or an animal is not to characterize it, in the sense intended here; it is to say what it is, and to say this is to imply that it has at least some subset of a (perhaps indefinite) set of identifying characteristics. By contrast, to say that something is red or weighs 10 lb is to characterize it.

A simple criterion, sufficient in normal cases for distinguishing characterizing terms and mass terms can easily be given. Let H be an individuating term; then, given the two terms F and G, F is a characterizing term and G a mass term if and only if: $(FGH \Rightarrow (FH.GH.FG) - (FGH \sim GF)$. For example, 'grey stone wall' implies 'grey wall' and 'stone wall' and 'grey stone', but 'stone grey' is meaningless (or relies on a different sense of 'stone').

On the other hand, F and G are both characterizing terms if and only if: $(FGH \Rightarrow (FH.GH) - (FGH \Rightarrow (FG \sim GF)$. For example, 'large black cat' implies 'large cat' and 'black cat', but 'large black' and 'black large' are both meaningless. A characterizing term picks out and indicates a single (perhaps composite) quality. As a result, a characterizing term cannot, so to speak, stand alone; like functions without arguments, such terms are essentially incomplete. Qualities must be something's qualities; a characterizing term must characterize something.[7] This is not to say that such terms cannot occasionally stand alone, but that it is a condition of their use that they should normally be applied to substantive terms.

This seems evident enough, but a critic might ask what is wrong with such statements as 'red there' or '10 lb here'. Could we not develop an artificial language based on characterizing predicates, cumbersome no doubt, but just as capable of describing the world as a natural language?

The first difficulty in the project arises over 'here' and 'there'. If they indicate definite spatio-temporal locations in some system of coordinates with a specified origin, then the artifical language can get under way; but then it refers to a particular, namely the origin. If this is not specified, then it is no more clear where 'there' is than it is clear which red (patch) is meant.

One might argue that the red (patch) there is in the same place where 10 lb, $10 \times 3 \times 4$ ft, rough, evil-smelling and ugly are. Given enough such predicates, a unique intersection will be determined, and this can be taken as the origin.

Particulars, in short, could be defined as concatenations of characteristics. A further difficulty arises as soon as one takes one's eye off the concatenation. How can it be reidentified as the same again? By being in the same place? But it defines 'the same place'.

How do we know that this combination of qualities can't move? Suppose it were to disappear and another combination the same in every respect except location appeared elsewhere, but 'elsewhere' has no sense. Suppose there were many such concatenations; any one could be taken as the fixed point, and relative position could then be determined.

But to define changes of position or motion, a set of coordinates, known to be fixed relative to one another, is necessary. To define fixed points or coordinates is precisely to define particulars. But in a world composed only of characteristics, even if a complete set of spatio-temporal coordinates were, per impossible, provided, this would still not solve the problem of re-identification. If, at a given point, all characteristics but one are the same as at an earlier time, is the concatenation still the same? If it is not, how can we give a sense to 'changing in some respect'? Suppose, in moving, other characteristics change; has the same thing moved or a new thing come into existence? It might be possible to work out answers to these problems, but, as we have seen, any answer will have to involve introducing references to at least some particulars.

A far easier approach, and the only one consonant with a descriptive metaphysics of science, is to take a vocabulary referring to particulars for granted and assume that characterizing terms are essentially incomplete, requiring application to particulars or to terms that collect or group particulars, or which, like 'stuff' terms, can be 'particularized' for their completion. It might be thought that laws, since they also contain no reference to particulars, are likewise incomplete, and require completion by the references to particulars given in the statement of their ranges of application. But it does not follow that the statement of a law without reference to its range of application is in any way incomplete. Such a statement asserts that a relationship holds between certain variables; it normally takes one of the fully 'saturated' forms; $(x)(Ey)(Rxy)$ or $(x)(y)(Rxy)$, and so requires no further completion to have sense, even though expressions that refer to specific values of x and y are 'incomplete' in the sense that '10 lb weight' must be the weight of something.

We must now consider, first, what a variable is, and then how it is related to the particulars its values characterize. A variable is a collection of characterizing possibilities; it is the set of possible values a characterizing term could take on in characterizing something, where these values stand in a certain definite relationship to one another. The variable must also stand in a certain sort of relationship to what the characterizing term characterizes.

We shall call this last the 'bearer' of the variable, and the relation between it and the variable the 'bearer–variable relation'. For example, the representative consumer is the bearer of the variables 'utility' and 'quantity of commodity *xi* to be consumed and the representative firm of 'level of output of commodity *i*' and the 'quantity of input of factor *j*'. Bearers are particulars; they are economic agents, but they can be aggregates. The bearer of the variable 'market demand for commodity *i*' is the aggregate of individual consumers or households. Note that a household is a particular, but it is an institution, not an individual. Similarly the bearer of the variable 'national income' is the nation, a particular nation, an institution and not simply an aggregate of economic agents.

A rather different account of variables was proposed by Rozeboom (1961, pp. 345 et passim), who defined a variable as 'a function from a set of abstract entities K to a domain D, such that every d in D has one and only one property in K'. He gives as an example, '[. . .] the arguments of the Weight-in-lbs. variable are temporal stages of objects (persons), its possible values are positive real numbers, and if Tom weighs 164 lbs. today, the value of the "Weight-in-lbs." variable for Tom, today, is the number 164'. He writes this as $W(x) = n$, where n ranges over numbers and x over temporal stages of objects.

But this formulation skips over an essential point. What Rozeboom has defined here is not the variable 'Weight-in-lbs' but the function applying that variable to objects in the world. To define the variable 'Weight-in-lbs' is to say what the numbers are numbers of. In the example, they are not numbers of Tom, but of lbs weight; Tom's weight to be sure, but before they can be numbers of Tom's weight they must be numbers of lbs weight.

The variable, in the sense we have defined, is a relation between numbers and weights such that any particular weight is expressible by a corresponding number.

To define this relationship we must have methods for saying 'this weight is the same as, greater or less than, that weight', 'this weight is the sum/product of those weights', 'this is zero weight', and so on. ('Temporal stages of objects', such as 'Tom, today', are references to a basic particular in Strawson's sense – see below.)

The axiom system required will be more or less strong depending on whether the numbers expressing the weights are the natural numbers, the integers, rationals, or reals (see Nagel, in Danto and Morgenbesser (1960, pp. 121–41). We earlier referred to 'bearers of variables'; the 'function' stated by Rozeboom is the bearer variable relation.

It is important to see that the relationship between weights and numbers is of a different kind from that between the variable and its bearer. In the example, weights are expressed in numbers, but Tom is not expressed in weight.

That which is sufficient to distinguish one weight from another is fully accounted for by the assignment of numbers; but the differences between persons are not wholly summarized by their differences in weight.

Two weights cannot have the same number, but two persons can have the same weight. Two things that have weight can have the same weight; two weightings can have the same result, but that result is the same weight, and will always be expressed by the same number. The relation between weights and numbers is one-to-one; that between persons (at given times) and weights is many-to-one.

But the difference runs deeper. Tom has weight, and weight has number, but numbers have no weight, nor does weight have Tom. Weight is a predicate of Tom, and number of weight; and in each case the converse predication does not make sense. But there is a distinction in the way the converse predications fail to make sense.

Rozeboom actually defines a relation between Tom at different dates and numbers, not between Tom at different dates and numbers of lbs, so that his is not the bearer variable relation, but a relation between temporal stages of a bearer and one domain of the variable. Instead of the variable weight-in-lbs, we have a function that would relate age and weight (an increasing function?). Numbers do not have weight, but they do have properties, and 'weight' is a characterizing term; the form of 'numbers have weights' is the same as that of 'weights have numbers' or 'colours have intensities'.

The failure of the converse to make sense lies in some distinction between the categories of predicates applicable to features of the world and those applicable to features of thought. By contrast, the failure of 'weight has Tom' to make sense can, in a rough sense of the term, be considered formal.

'Weight' is a characterizing term, 'Tom' an individuating term; the appropriate role for the former is describing, for the latter referring, and to switch these roles can only engender confusion. A variable clearly cannot be a particular, in the sense in which we are using that term. But equally, it should be clear, neither can its values. The values of a variable are specific relative to the variable, but they are not particulars, in the sense of falling under individuating terms.

Variables imply the possibility of variation; for this a method of distinguishing and ordering the possible values of the variable is needed. The more exact the method of distinguishing and the stronger the ordering, the more precisely the value of the variable can be determined, and, consequently, the more precise the relationships into which the variable can enter. A variable's values, then, can be distinguished from and compared to one another; the variable is the relationship between the values that makes this possible. A variable is therefore a particular kind of universal and the variable-value of variable relation is a relation between universals, and as such is to be distinguished from, for example, the bearer variable relation.

The value variable relation has certain formal features (in a loose sense of the word formal) that will help to make clearer the ways in which it differs from other relations figuring in this analysis.

First, values under the same variable are comparable; those under different variables are non-comparable. We can compare two colours, two lengths, two masses, two speeds, but we cannot compare a color and a length, a speed and a mass.

Second, the assessment of variation involves the repeated application of a standard, which involves recognizing an aspect as the same in different cases and requires a method for determining whether there has been a change in that aspect. It could be argued that this already presupposes some 'uniformity of nature' principle, since it assumes that the same method of assessment will work on different occasions. This presents no difficulty, however, since we do not wish to deny the necessity of such a presupposition.

The value variable relation, as defined here, is a slightly specialized version of what has sometimes been called the determinate/determinable relation. In discussing the latter, Johnson (1921, Part I, p. 175) holds that the familiar phrase 'incomparable' is thus synonymous with 'belonging to different determinables' and 'comparable' with 'belonging to the same determinable'; not that this is the actual meaning of the terms, but that enquiry into the reason for the comparability or incomparability of two qualities will elicit the fact that they belong to the same or different determinables respectively so that they are, in a sense, directly or immediately distinguishable from one another, given the procedure for distinguishing or measuring. What this means can be seen by contrast: redheads are distinguished from the rest of mankind by their hair colour, but there is no property playing an analogous role by which red is distinguished from other colours.[8] Given a method of measurement, we can sort books by size, but there is no analogous property by which we can sort sizes. In a sense they are sorted by being sizes.

This argument is not meant to suggest that the procedures for determining values of variables, especially rather complex variables, do not remove one, several or many stages from the 'immediately given'; the point is, rather, that values of variables cannot be wholly distinguished from one another by characteristics acting as fundamentum divisionis. Only characterizing terms, of which variables themselves are a kind, can properly act in such a role.

Third, predication of a value of a variable implies predication of the variable, but is not implied by it; the variable implies the disjunction of its values. One value of the variable implies the negation of the conjunction of the rest. Nothing can be red or green all over at the same time; nothing can weigh 10 lbs and 12 lbs at the same time. Nothing can weigh 10 lbs and not have weight, be green and not be colored. But 'having weight', while it implies having some definite weight, does not imply any particular weight.

Fourth, the value of a variable is a single item of a specific kind. It is not a conjunction; it cannot be a combination of values of different variables. 'Yellow 10 lbs' is not a value of a variable. Nor can a value of a variable be a combination of a characterizing and an individuating term; 'yellow rose' is not, in the relevant sense, incomplete; it is fully saturated, equivalent to the (or a) 'rose is yellow'.

A value of a variable is a specified degree of a comparative characterizing term; values are neither complete nor are they composite (though a variable, say 'velocity', may be defined as the change in one variable per unit measure of another).

A value of a variable is not recognized by its properties; it is a recognizable property. The relationship we have been examining may be said to be a feature of our normal thought, a feature some variant of which must figure in any descriptive metaphysics.

Scientific variables, however, appear in highly specialized and technical discourse. But there is in fact no philosophically significant separation between ordinary comparison and precise measurement; the difference is one of degree, and the extent of the difference can be quite exactly measured by the strength of the axiom systems governing the comparisons that can be made.

Given an ordinary-language variable, whose values are single non-composite items, are mutually exclusive, and imply the variable, which, in turn, implies their disjunction, then to transform this variable into a scientific one means simply to provide more precise and more reliable techniques of measurement, capable of use in a wide variety of circumstances (precisely the project of econometrics).

Variables, then, as we have described them, have a definite and ascertainable structure. On the one hand, it will enter into laws, functional relationships with other variables; and on the other, it will enter into the bearer variable re1ation with its own bearers. Each of these makes certain logical demands on the variable. For a variable to be part of a functional relation it must discriminate its values in a manner appropriate to the formal operations involved in the relation. A variable whose values can only be natural numbers cannot enter into a functional relation involving differentiation and integration (or even unlimited division); a variable whose values can only be ordered, and so are not additive, cannot enter into functional relations involving arithmetic operations.

Certain constraints are also placed upon variables by the second sort of relationship in which they stand their relationship to their possible bearers. First, not every variable can characterize every sort of bearer.[9] The college has no colour, and the thoughts of even the most forceful thinkers have no accelerating mass.

Second, even though the members of a group of variables may individually be capable of characterizing a certain sort of bearer, they may collectively impose incompatible or mutually restricting demands.

For example, if a consumer is to be the bearer of the variables 'quantity of commodity i to be consumed' and 'competitive demand price for it', where $i = 1, \ldots, n$, then it must be possible for the consumer to maintain both her market and product information and her ability to adjust rapidly to market imperfections, regardless of the actual pattern of consumption she engages in. These assumptions are required, in part, to make it possible to treat prices as uniform, so that every consumer pays the same price wherever and whenever she buys. But information is itself a commodity, marketed by the communications and publishing industries, and the ability to adjust rapidly to market imperfections depends partly on the absence of complementarities in consumer durables, and partly on an effective communications and transportation network.

Hence, an underlying pattern of 'infrastructure consumption' must be assumed; the model cannot determine the supply, demand or prices of this.

Third, even if a group of variables is capable of entering into certain functional relationships, if considered on their own – utility increases at a diminishing rate as food and clothes are consumed – once we look into the bearer variable relation, the putative bearers may lack the characteristics required to make sense of the variables and their relations, particularly in view of the mathematics being used. The calculus requires that the relationships be continuous; the variables are expressed in real numbers, so must be infinitely divisible; the maximizing process requires enormous calculating ability. Yet not only do actual consumers fail to display the necessary powers of discrimination and combinatorial skills, they also apparently refuse to order their preferences transitively, let alone convexly. In this way they fail to be appropriate bearers of the variable 'utility of good *i* to a consumer' (Henderson and Quandt, 1958, p. 86).

This should not be surprising; we can take the point a step further. Consumers appreciate goods for their characteristics – we like apples for their taste, clothes for their warmth and style, sofas for their comfort, etc. But such specific characteristics may not be easily comparable and so may not be rankable either. Do we like the taste of the apple more or less than the warmth of the clothes; do we want more good-tasting food or more warm clothes? Will we – can we – decide on the basis of comparing taste and warmth? Does such a comparison even make sense? Or should we consider, for example, the least-cost combinations of goods – food, clothing, housing, automobiles – that will provide the qualities – nutrition, warmth and style, shelter and transportation – that are needed in order to put together an appropriate life style (Lancaster, 1966; Nell, 1998a)? The claim here is that conceptual analysis – in this case, the analysis of what it means to compare goods and services, and to choose or indicate a preference for some set of goods rather than another – reveals that the conventional utility model is conceptually flawed and needs to be revised.

So, to summarize: lawlike relations are those relations between variables that enter into a model or theory. These are the relations used, for example, in deriving either final states or predictions from given specific initial conditions, or steady state formulae and equilibrium conditions from given general premises. Lawlike relations are keys to conclusions; they can be described as formulae, changes which would significantly alter the conclusions or predictions of a model, given any initial conditions. In the example just given, the equilibrium condition, in which ratios of marginal utilities to prices are equalized for all commodities being consumed, is derived by parametrically varying prices. The so-called law of diminishing marginal utility is the crucial assumption.

We must now examine more closely the way in which variables in laws are related to the bearers of variables and the implications of some of the kinds of inconsistency just listed. For it is in terms of the variable bearer relation that we shall reformulate the problem of induction.

4.3 LAWS, BEARERS AND INDUCTION

Now let's look more closely at the way in which variables in laws are related to the bearers of variables and the implications of some of the kinds of inconsistency just listed. For it is in terms of the variable bearer relation that we shall reformulate the problem of induction.

So let's ask, given our account of the conditions on a scientific law, how can we come to know laws? What does the preceding discussion tell us about induction? First, we can support Toulmin's (1958) contention that a law may be discovered from examination of a single instance. If there actually is a law connecting a set of variables, one good example that can be studied carefully may be the best way to grasp it.

This is not a very difficult point. What makes it important is what it excludes. Since laws must be part of a model, anything not in the model is not a law. 'All consumers are rational', and 'all consumers have perfect information', which would presumably be accounted lawlike by positivists, since they define bearer variable; relations of the model play no role in the model itself. Similarly, 'all consumers are described by a utility function' is not in the model, but $u = U(x_1, \ldots, x_n)$ is; the statement of the utility function is lawlike, but its ascription to consumers is not.

These remarks should not lead one to underestimate the importance of the statements used to set up a model and define its terms. On the contrary, these statements are the ones that will determine the applicability of the model and its laws. The question of applicability is to be distinguished from the question of scope, which is an empirical matter that can only be determined after a favourable ruling on the logically prior issue of applicability; if a model is not applicable in principle, it is not possible for it to have scope.

The problem of applicability arises from the various demands made on the bearers. In the last section we saw that variables make certain demands on bearers as a result of the demands of the operations involved in the functional relations. In addition, we saw that bearers must be specified in a number of other ways, and the question now arises of the mutual compatibility of these various specifications.

To determine its range of application, of course, will require further experimentation, but not continual testing. It is sufficient to determine the range of the relevant variables and the absence of other significant variables within that range. (This latter is an empirical problem, but is not likely to be solved by the compulsive repetition of tests.) Hence we can say, with respect to the problem of induction, that if there is a law connecting a set of variables, then, once its range of application has been established, there is no further problem of the law's failing to hold for unobserved cases falling in its range of application.

But how do we know that some other variable, not previously observed to be relevant, is not hidden in the unobserved cases, upsetting the functional relationship? The answer is that the tests determining the law's range of application were designed precisely to discover and exclude this contingency. However, we can still ask, how do we know that such tests work, or, if they worked in the past, that their results still hold? Such tests work, and their results are reliable, because they rest on established laws other than the ones whose range is under test and upon our abilities to tell when situations contain the same sort of particulars characterized by the same characteristics.

Hence, if there are laws that have been validated, and if such abilities can be justified, then while questions about the adequacy of particular experimental design and the possibility of human error remain, no wholesale scepticism can be maintained.

The problem of validating range of application statements thus reduces to the problem of validating both (at least some) laws and the ability to recognize similarities. As we have already suggested, the ability to recognize situations as the same in the above sense depends upon the reidentification of particulars, especially of basic particulars, and this, we shall argue shortly, depends in the last analysis on the existence of an accessibility to human knowledge of a certain class of scientific law.

The problem of induction can now be reformulated: what reason or reasons can be given for thinking that there are relations between variables? Or, alternatively, could there be a time, or a condition, in which there were no relations between variables, or between the variables of a certain sort? Clearly there can be classes of variables between which there are no, or at least no discernable, mathematical relations – for example, tons of butter produced per annum in New Zealand and the loudness of the bark of a nearby dog.

What reasons then can be given, first, for holding that there can be relations in the required sense between some variables; and, second, for picking out certain classes of variable, say those of physical science, as among the privileged classes of variables? This reformulation of the problem does not involve causality; we are not asking that the relations holding between variables be such that from any given state of a system its past and future can be uniquely determined. The question is whether relations between variables exist; whether they are causal or not is a further question concerning their general form. Moreover, our problem is to show that there *are* relationships between some variables, in some class or classes of variables; not which relations hold between which variables within the class, for that is a problem for science. In short, we are aiming here at the so-called 'neurotic' problem of induction.

Finally, we have defined laws as multi-place predicates of variables, and variables as characteristics of bearers' particulars or classes of particulars, the problem must be understood as arising within a conceptual framework that includes the categories 'particular' and 'universal'. Any presuppositions required to make sense of these categories will therefore also be presuppositions of discourse about induction.

Econometric models, then, are made up of sets of equations, some of which are definitional, sometimes called identities, and equilibrium conditions; while others containing the real content of the model are functional relations between scientific variables. The models describe economies, which are made up of agents, operating in markets, where they carry out transactions following rules. The agents are the bearers of the variables, the variables are universals, the bearers the particulars.

4.4 PARTICULARS, PRESUPPOSITIONS AND INDUCTION IN NATURAL SCIENCE

To deal with the reformulated problem of induction, we shall examine the presuppositions of relying on the distinction between particulars and universals, in this case variables. From this examination an argument will emerge that will show that there must be functional relations among a set of variables involved in the physical sciences.

Values of variables cannot be instantiated unless there are particular things that bear those variables. No mass or weight can exist that is not the mass or weight of something, no colour that is not the colour of something. Even if characteristics could exist apart from what they characterize, variables, since they imply the comparability of their values, could not be defined in a world without particulars. A specific value of a variable can only be assigned as a result of using procedures involving comparison and measurement.

These, in turn, require both an initial identification of a particular and the repeated recognition of it as 'the same' over time. For we must set up samples or standards, for example, of length or mass (or, in the case of time, of regular motion), with which we can compare given instances; these samples and standards must be readily at hand and re-identifiable, which means, in effect, that they must be embodied. Embodiment is embodiment in particulars.

The approach suggested here may seem to run against the grain of much modern thought, which assumes that the predicate calculus, from which singular terms can be eliminated, provides a canonical notation for scientific and philosophic purposes. (Quine, 1960, pp. 171–6, urges this specifically against Strawson, 1959.) But there need be no quarrel here. Such a canonical notation is useful for many purposes, but not for examining the presuppositions of certain ways of thinking. The *x*s and *y*s occurring in canonical sentences of the predicate calculus are place-holders for references to particulars and sets of particulars; thus the notation itself carries the presuppositions we wish to examine, while obscuring the category differences between predicates (Strawson, 1959, pp. 194–8). If we are to understand how it is possible to refer to individual particular things, how to pick them out and recognize them as 'the same' again, it will be necessary to proceed along a different route.

Certain arguments of Strawson's provide a convenient starting point (ibid., ch. 1). To speak of particulars requires that one be able both to identify them and to re-identify them as the same. But in this matter some particulars occupy a more fundamental position than others.

The identification and re-identification of events, processes, states and conditions will normally involve references to material bodies; their consistent re-identification, especially, will not generally be possible without some reference, explicit or implicit, to material bodies.

By contrast, we could regularly carry out the procedures of identifying and re-identifying material bodies without necessarily referring to any other class of particulars. Strawson calls material bodies 'basic particulars', and terms them 'ontologically prior' to the rest.

Speaking very roughly, the reason material bodies occupy such a fundamental position is that they, alone among particulars, at once take up space, are able to move through it and persist through time. A glance at common usage shows that other particulars tend to be identified by their relation to material bodies. Events and processes normally happen to material bodies; states and conditions tend to be thought of as their states and conditions.

A birth or death, for example, is someone's birth or death; a growth is the growth of a plant or a tumour; a making is the making of supper or a hi-fi cabinet. A state is a state of the kitchen, a state of play (which requires implicit reference to, say, the pieces on a board), or the state of European politics (which similarly makes an indirect reference to the facts of geography); a condition may be the condition of the car or of the poor.

But not only are other kinds of particular identified by reference to material bodies, but their extension, location and duration – that is, what is needed to reidentify them – are given by means of such references. Material bodies, by definition, are those things located in space that endure through time. The central role of material bodies can be seen more precisely by supposing that there were none. That is, suppose there were no spatially extended enduring entities. (If nothing extended has duration, then, of course, nothing extended can move, since to move from x to y implies being at x before being at y, and anything of which this is true has duration.) If there were no material bodies, no connections would obtain between spatial systems at different observation times. Lacking a common reference point, two successive observed systems of spatial relations between 'things' in the epistemological present would be like two coordinate systems without any transformation equations relating them. As Strawson (1959, p. 47) says, 'we cannot attach one occasion to another unless, from occasion to occasion, we can reidentify elements common to different occasions'.

It would then be impossible to attach any significance to questions about the spatial relations of a thing from one moment to another, unless it were under continuous observation. It would be impossible to speak of motion, or to define such variables as 'length', 'mass' or 'duration of time'. For 'length' and 'mass' depend on comparisons with particular embodied standards known to be invariant, and 'duration of time' depends on comparison with a standard of regular motion.[10]

Strawson (1959, p. 32) has argued that no connections would obtain between observed systems at different observation times unless such systems were connected by spatially-extended enduring particular objects – material bodies. Without material bodies, two successive observed systems of spatial relations between 'things' in the epistemological present would be like two coordinate systems with no transformation equations relating them. According to Strawson, 'we cannot attach one occasion to another unless [. . .] we can reidentify elements common to different occasions'. (The implication, of course, is that since we do 'attach one occasion to another' – we could not write or read this sentence if we didn't – there must be material bodies. And we will argue that the existence of material bodies, in turn, implies that there must be lawlike relations between variables. In the next section we shall adapt this argument to econometrics.)

To see the nature of the argument more clearly, consider a counter-claim. (In the next section we shall see a similar argument and a similar counter-claim in the case of economic laws.) Dretske (1964) contended that Strawson had failed to show that an ontology without basic particulars – without material bodies – will be unable to give sense to the concept of a temporally continuous spatial framework. Even though successively observed spatial systems have no common reference point, there might be another way to relate them. He (ibid., p. 138) considers a hypothetical spatial scheme in which 'being in the same place' at different times is defined as follows:

> X is in the same place as $Y = df$ for all W, if W is a reference object, then if X is spatially related to W by R, then there exists a Z such that W is exactly similar to Z (in all non-spatial respects) and Y is spatially related to Z by R.

Here 'X' and 'Y' stand for 'expressions used to make identifying references to particulars situated at different times', where these times are not joined by a period of continuous observation.

This definition begs three questions. First, a question Dretske himself raises (1964, pp. 138–9): how do we know that there is always one and only one Z exactly similar to W in all non-spatial respects? Surely if Z and W are to be identified only by their qualitative similarity, there may be many or no Zs corresponding to a given W. He could try to eliminate this possibility by listing exhaustively the non-spatial properties of W. But since continuants are debarred ex hypothesis, there can be no guarantee that there will always be an exactly (or sufficiently) similar Z, and precisely because it is a listing of properties (universals), there can be no guarantee that if there is an object with that set of properties, there will be only one such object. ('Individuating' attributes can, of course, be constructed – 'the whiteness of this wall', 'being in my possession' – but as Strawson points out, such attributes are characteristically constructed around, or dependent upon, reference to a material object or a person.)

Suppose, then, there are two objects, $Z1$ and $Z2$, exactly similar to W in all non-spatial respects, and consider a second question. Does the definition still enable us to determine whether Y is in the same place as X? Clearly this depends on how we understand the spatial relation R, for if Y is related by R to, say, $Z1$ and by $S = AR$ to $Z2$, the difficulty is removed. But this presumes that we can reidentify 'the same relation, R' in the two cases – that is, that we can determine $R1 = R2$ independently of being able to reidentify the objects between which such relations hold. *R* is a spatial relation; to say what it is, one must be able to specify angles and distances. (Dretske uses 'beneath' in his example, but claims that his analysis can be stretched to cover distances.) To say that *X* has the same spatial relation to *W* that *Y* has to *Z* one must already be able to say that the respective angles and distances are the same. But what sense can Dretske give to 'the same angle'? An angle is not determined by one reference point (*Z* or *W*) and another object (*X* or *Y*); two intersecting lines are needed to determine an angle. But this would not be sufficient for Dretske, who implicitly relies on a concept of direction, common to both spaces, for which it is necessary to make reference to a coordinate system.

For example, given that *Z*1 and *Z*2 are exactly similar in all non-spatial respects to *W*, and assuming we know in terms of some coordinate system the angle and distance relating *X* to *W*, suppose *Y* is the same distance from *Z*1, and that when *Z*2 is placed equidistant from *Y*, and the line *Z*1 *Z*2 is taken as a base, the angles *YZ*1 *Z*2 = *YZ*2 *Z* are the same as the angle relating *X* and *W*. Then in the absence of a concept of direction it could be said that the spatial relations between *Y* and *Z*1 and *Y* and *Z*2 were equivalent, and, since *X* has the same converse relation to *W* that *Y* has to both *Z*1 and *Z*2, by taking *X* and *Y* as the respective reference objects, the proposed definition would put both *Z*1 and *Z*2 in the 'same place' as *W*, in spite of their not being in the same place as each other.

Even apart from the question of multiple reference objects, Dretske depends on the ability to compare directions in the two spaces. Consider his use of 'beneath'. Given that two objects are aligned, whether the first is beneath the second, or vice versa, or both are on a level, depends on which way is 'up'; to compare such relations between two pairs of objects in different spaces requires reference to a coordinate system common to both spaces (or translation into one), which is precisely the point at issue.

Finally, Dretske has given us no reason to suppose that the geometry of the spatial system in the two cases will be the same. Space I, for example, might be a normal Euclidean plane, and Space II the surface of a sphere. How, then, would he define 'the same relation *R*'? What guarantee is there that all mappings of such relations from one kind of space onto another will be bi-unique? But if they are not, 'the same place' cannot be defined.

Dretske does consider the case in which no objects in the two spaces are similar. He disposes of the objection that in such a case his definition could not work by pointing out that exact similarity of *W*s and *Z*s is not needed, for Strawson's criteria of 'reidentification' could serve to select (not reidentify) a set of reference objects in the succeeding period(s) of observation (Dretske, 1964, p. 141). The difficulty, in other words, applies to both systems and Strawson's solutions can be applied to Dretske's problem. Besides, there are an indefinitely large number of ways objects can be considered similar.

An analogous move might be thought to provide some comfort in respect to the second and third objections above: Strawson no less than Dretske must be able to reidentify *R* and must suppose the geometries of the two spaces to be the same. But in fact they are not in the same boat, for since Strawson's spatially extended reidentifiable particulars endure (are continuous) through time, he can be certain either that the various observed spaces have the same geometry or that discoverable transformation equations connecting them exist. Suppose the geometry appeared to change between observation times – that is, that geometrical relations within extended bodies changed so that material bodies changed shape. Ex hypothesis these bodies are reidentifiable. Hence the observer is given the shapes before, the shapes after, and the knowledge that the change took place in continuous time. From this information a transformation function can be determined, giving the geometry of the new bodies in terms of the old. Similarly, since material bodies are spatially extended, in reidentifying them it must be possible to reidentify the spatial relations among the parts, and there is no difference in principle between that and reidentifying relations between objects.

Strawson argued that a temporally continuous spatial coordinate system presupposes material bodies spatially extended enduring particulars. Such a spatial framework could not be established unless we could refer to such objects and reidentify them as 'the same' under different conditions. Dretske's objections fall short on two counts; not only does his counter-system fail to pick out 'the same place' uniquely, it presupposes the very spatial framework at issue in speaking of the same relation, *R'*. It is true that if the coordinate system is given, in using it we need not refer to material bodies (since we can refer to the coordinates), but this is not relevant to Strawson's argument.

So, summarizing and simplifying, if we have a space–time framework – which we do – there must be material bodies. What, then, does a vocabulary of material bodies presuppose? Strawson (1959, p. 186) argues that a reference to any particular of any kind presupposes 'that there should exist, and be known, a true empirical proposition of a certain very definite kind, namely one capable of individuating the particular in the context of discourse'. (Hence, a term for a particular is 'complete', or in Frege's terminology 'saturated', by contrast with the terms for universals.) So an agent who can use an expression referring to a particular must be able to expand that expression into a true statement that describes the particular by its individuating features.

If this much can be accepted, we need not go further into the details of

Strawson's argument. Clearly the 'individuating' features cited must be informative. The individuating feature of my dog could not, for example, be that he is a dog. Strawson generalizes this argument to cover kinds of particulars. Just as it must be possible to explicate an expression referring to a single particular by stating a means by which the particular referred to can be identified, so it must be possible to state for a class of particulars the means by which particulars of that class can be identified.

And if such statements are to be informative, they may not depend upon expressions for the particulars or for the classes of particulars (sortal universals) of the kind they are explicating. The case becomes complicated, however, when we consider the presuppositions of referring to the class of particulars as 'material bodies', for these have supplied the basis for the identification of all the rest.

What is left to serve to identify these in a non-circular fashion? Sortal universals are ruled out, since they collect or group particulars into, for example, hierarchies of genera and species, and characterizing terms must also be excluded, since they must be applied to particulars.

Strawson deals with this problem by calling attention to what we have called stuff universals or mass terms, such as 'snow', 'molasses' or 'gold'. These terms are not excluded from appearing in the presuppositions of a statement referring to material bodies, and he maintains that there should be facts stable by means of such sentences as 'There is water here', 'It is snowing', which is a condition of there being propositions into which particulars are introduced by means of such expressions as 'this pool of water', 'This fall of snow'. Further, he argues that sortal terms such as 'cat' or 'apple' can be thought of as composed of the combination of a stuff universal, 'cat-stuff' or 'apple-stuff', and a means of reidentifying instances of 'cat' or 'apple' as the same as some previously observed instance.

Thus all kinds of basic particulars[11] can be thought of as presupposing statements that assert that stuffs are present here and now.

But it is not clear that the argument as stated will meet Strawson's own criterion of non-circularity. For these statements, understood as he proposes, themselves contain references to particular spatio-temporal locations. Yet we are supposed to be able to identify such particulars only in connection with a reference framework provided by basic particulars.

Hence Strawson's suggestion appears to contain just the sort of circularity he wishes to avoid. To this one could perhaps reply that the aim was merely to avoid, in the statements comprising the presupposed class, any direct references to the kind of particulars in question, not to avoid using terms whose use presupposes those particulars. But circles are no less round for being large.

Strawson has advanced a great project, but at this point something is clearly wrong. What it is emerges when we compare the relation between the proposed class of presupposed statements and the particulars they support (material bodies) with the presuppositions of other classes of particulars. A reference to a battlefield supports or adds to a phrase's ability to identify a battle in a way that 'there is water here' does not support or add to the identifying power of 'this pool of water'; similarly for 'it is snowing' and 'this fall of snow'.

As far as the ability to refer or individuate goes, there does not seem to be any difference between the phrases and the corresponding sentences. But it is true that the possibility of our using the referring phrases depends on the possibility of *asserting* the corresponding sentences on suitable occasions.

This point can be brought out by looking at Strawson's discussion of sortal universals, where he argues that the decisive step to particulars is taken when we move from seeing all apples or cats as 'apple' or 'cat' to seeing successive apples or cats as the same or different individual apples or cats.

To take this step we must, for instance, be able to form the idea of a particular cat, say, appearing, departing and reappearing, tracing a continuous path through space and time.

For both stuff and sortal concepts, making the step to particulars involves an act of, so to speak, conceptual perception, of being able to act in terms of the difference between two senses of 'identity' – that of the same stuff and that of the 'numerically' same thing.

To particularize is to take the step from the first to the second; and what is presupposed by basic particulars is the possibility of taking this step, of pointing to particulars.

The analogy with pointing is a good one. Saying, truthfully, 'there is water here', like pointing to it, requires both the target and the act of indicating the target. For such an act to be possible there must exist something capable of being the target, which is all that Strawson discusses, but it must also be possible to perform that kind of act. Yet Strawson does not discuss the conditions for this.

If acts of pointing can be performed, then it would seem to follow that we can identify basic particulars and establish the spatio-temporal reference system discussed above (Strawson, 1959, p. 207 et passim), unless, perhaps, it could be possible to perform the required acts of conceptual perception – of pointing – without, in doing so, referring to basic particulars.

The answer to this question involves two distinct steps. The first is relatively easy. By treating Strawson's 'stuff-placing' statements (for example, 'here is water') as acts of perception, we have automatically introduced a reference point, namely the perceiving agent.

But the fact that the agent is there, somewhere (and has appropriate abilities), is not sufficient to guarantee that he can know where he is, identify and reidentify other things or establish and determine his relations with them. It merely means that if he can do these things, then he has a reference point prior to other basic particulars, namely himself. (But we can always imagine another observer at a different point; we have to show how their observations can be coordinated.)

The second move in the argument is the fundamental one. To make the step to particulars of any given kind, the perceiving agent must be able to relate what the concept of that kind indicates is its sort of stuff, e.g. 'water' or 'cat', to time and space. This is possible only if what the concept indicates is related to space and time in a way the perceiving agent can comprehend. To see this point, we must consider first what is meant by saying that an amount of stuff is related to space and time.

Suppose 'stuffs' – gold and snow, cats and apples – could, like redness and roughness, occupy the same space at the same time. How, then, could one body act on another? How could one thing be attached to or support another? It is the impenetrability of stuffs, the fact that they saturate space, that makes it possible to determine the boundaries of things. If things interpenetrated and did not act on one another, how could boundaries be determined?

An essential notion underlying 'stuff' is the saturation of space over time, filling it to the exclusion of other stuff during that time. But there is another crucial feature: it has the ability to move through space. This calls for some caution: we need to explain or derive the ability of stuff and material bodies to move through space. The first step is to argue that there must be a distinction between motion and rest, although it must be motion relative to an observer. If an observer cannot distinguish motion and rest, then we could not tell if matter and things moved or were moved through space, nor of course could we say how fast they moved, or whether they accelerated or not, so then it would have to be as if everything remained in place for all time. Nothing could happen. Hence it must be possible for things – stuff, matter – to move through space. But such motion will take time,[12] giving rise to the concepts of velocity and acceleration.

Note that 'rest' is relative to an observer; the driver of a car is at rest relative to the car, but in motion relative to the road. When nothing is acting on a material body, let us say, it will be at 'rest' relative to an observer; this is simply the *slowest* motion relative to that observer, the lower limit. If it continues to be the case that nothing is acting on some amount of matter, it will stay at rest – that is, at the lower limit of motion relative to that observer. Now suppose it is initially moving relative to that observer, for whatever reason; then, if nothing acts upon it, it will continue to move at that same rate (Newton's First Law).

If matter and things can move and change the rate of motion, this makes it possible for one thing to act on another. Since two amounts of matter – 'stuff' in the most abstract sense – cannot fill the same space, an attempt to move them into one space will simply produce an action of each on the other. But which is acting on which? No answer is possible; both are acting in equal and opposite ways. Generalizing this, we could say that to every action there is an equal and opposite reaction, which is the core of Newton's Third Law.

In the absence of a force, mass moves at a constant speed, which may be anything, including zero? Two amounts of stuff that are the 'same' in terms of this essential notion have the same mass or are the same amount of matter if, colliding in equal and opposite motion, they produce equal and opposite actions on one another. The idea of force is that it is acting on matter to change its velocity; force causes mass moving at inertial velocity to change, to accelerate or decelerate (Newton's Second Law). This analysis is not a derivation, but further argument along these lines could develop the conceptual framework of Newton's Laws of Motion.

The earlier statements that stuff/matter must be related to time and space can be explicated. The claim now becomes that there must be relations between time, space, matter and motion or changes in motion. Further, just as 'amount of matter' can be defined in terms of equal, stronger or weaker action (change of motion), so 'duration of time' can be defined in terms of equal, greater or smaller distance traversed by given body in uniform motion; and 'equal distance' or length can be defined in terms of the spatial congruence of adjacent bodies at rest. In other words, these (relational) properties are variables; they are variables whose definitions are closely interwoven and dependent, in ways to be discussed shortly, on the existence of functional relations between them.

Echoing a long philosophical tradition, we shall call these the 'primary properties', but no sense other than that just given should attach to this terminology.

In summary form, the argument so far runs: scientific laws are relations between variables; variables are ordered sets of characterizing terms; values of variables cannot exist apart from bearers; bearers are particulars or classes of particulars; non-basic particulars are identified and/or reidentified by reference to basic particulars; material bodies are the basic particulars; particulars presuppose facts and classes of particulars presuppose classes of facts; basic particulars presuppose the possibility of performing an act, the act of 'particularizing'; for this act to be possible, two conditions must be met: first, there must be 'stuffs'; second, stuff must be related to space and time, or more precisely, it must be possible to perceive the relations between the properties.

But if we can show that there must be relations between primary properties, that this is a necessary condition of any system of discourse involving a distinction between terms for particulars and universals, then we will have shown that the existence of functional relations between at least one set of variables is a condition for any empirical statement of the kind we know to make sense.

Suppose there were no functional relations between the primary qualities, or, more precisely, between the variables representing these qualities. Given how fundamental these concepts are, it is very difficult to state just what this would imply. Suppose, for example, that a putative body is moving at a certain rate (in the absence of relations, we cannot give a sense to these concepts) over a certain period of time, but that it is arbitrary how far it traverses. No sense would then be attached to 'distance', nor, consequently, to any concepts, like 'velocity', into whose definition it enters.

Again, suppose that events in different locations could stand in an arbitrary temporal order, so that, say, if at X, A precedes B precedes C, and at Y, D precedes E precedes F, then B and E could be simultaneous, while F preceded A, and C preceded D. No sense would then be attached to 'moving from X to Y in a given time'. Alternatively, suppose that when bodies acted on one another they produced irregular or unpredictable results. It would then be impossible to define a measure for mass or force.

Nor could the concept of 'simultaneity' be defined, since the determination of the 'same time' in different locations depends on the transmission of force – that is, upon bodies acting on one another.

Nor is it easy to see how a unit measure of time could be established. To show that something is a 'particular' means, as we have seen, to have a method for reidentifying it, for tracing its continuous path through space and time. But if 'distance', 'the same moment of time', 'duration' or 'motion' cannot be given a sense, then no such path could conceivably be traced. No particulars would be identifiable.[13]

Suppose, next, that there were functional relations between the primary variables, but that they are different for different (spatial and temporal) 'parts of the universe'. There are two cases here. First, if we know the scope of the different relationships and transformation rules by which they can be converted into one another, then we can derive a single set of higher-order laws governing the entire universe. Second, if there are no such transformation rules, or if they are not discoverable, then, as in the previous case, we will be unable to trace the spatio-temporal path of a particular from one space–time to another.

Suppose there were functional relations but that they were different for different kinds of stuff, or for stuff and sortal universals. Again, if there are transformation functions, we have simply a higher-order set of relations. If there are not or if they cannot, in principle, be known, then instead of matter we must consider different kinds of matter. What happens if they are combined? If the combination leads to a definite relationship, then the combination principle has, in effect, established that a higher-order relationship exists. Hence, if they are to be unrelated, combination of the different kinds of matter must be disallowed. But not only must combination be prohibited; things composed of the different sorts of matter cannot be spatially or temporally related to one another since, if they were, they would have a common relationship to space and time. Thus no interaction would be permissible; the only kind of matter that agents could interact with is the kind they are themselves made of.

The preceding argument validates induction for physical laws connecting variables for space, time and matter, on the grounds that such laws are necessary presuppositions of identifying and reidentifying basic particulars. (The validation is based on necessary conditions for the way we think; not for the way we just happen to think, but for the way we could not conceivably not think. Building on Strawson (1959, ch. 7), we have already argued above that a language without particulars is not coherent.) Basic particulars material bodies in turn are necessary for identifying and re-identifying other kinds of particulars; and making reference to particulars is a necessary feature of the conceptual scheme we actually employ in science. This was shown in our investigation of scientific laws, which we found to be re1ations between variables, which in turn were groupings of comparable characterizing terms – terms capable of being ordered.

Such terms, we argued, must be applied to particulars; the statement of the particulars to which a variable or law applies gives its scope or range of application, and the determination of this is a separate matter from the formulation of a law.

These two aspects of scientific laws' formulation and application thus essentially involve the universal particular distinction, with the consequence that the preconditions for that distinction are also preconditions for scientific discourse. If there are re-identifiable particulars bearing universal characteristics, then there is no neurotic problem of induction.

Wittgenstein (1921) argued in the *Tractatus*, 6.36: 'If there were a law of causality, it might be put in the following way: there are laws of nature. But of course that cannot be said: it makes itself manifest'.

It cannot be said in physics; it is only manifest there. But it can both be said and be demonstrated in philosophy.

NOTES

1. A further problem was that simple autoregressive statistical models outperformed the big econometric models as regards forecasting, a point we discuss later. However, VAR (vector autoregressions) and related models may forecast, but they don't and can't explain. They don't model the economy at all. They succeed in predicting only because there are stable relationships in the economy that give rise to regular outcomes. The ability of those models to predict rests on such relationships, but they don't account for them in any way. All they do is express the statistical reliability of the underlying relationships. In the end, this is not economics; it is simply statistics: interesting and useful, but neither economics nor econometrics. For an account of the VAR methodology, see Pagan (1987; 1995); Gilbert (1987); and Epstein (1987, ch. 7); and also Chapter 7 of this book.
2. Haavelmo (1944, ch. 2, p. 13) reminds us that 'when we use the terms "constant relationships", or "unstable, changing relationships", we refer to the behavior of some real phenomena, as compared with some behavior that we expect from theoretical considerations'. The 'notion of constancy or permanence of a relationship is, therefore, not one of pure theory. It is a property of real phenomena as we look upon them from the point of view of a particular theory'.
3. Standard expositions of the theory of consumer behavior can be found in virtually any textbook, following the basic formulations of Slutsky, Hicks, Allen and Samuelson, and Hicks. For standard presentations a generation ago, see Allen (1956); Henderson and Quandt (1958); and Malinvaud (1972). Contemporary authoritative treatments hardly differ at all, see Varian (2007).
4. A point we shall return to: mainstream economics has elegant laws, but frequently fails to explain or offer insight, because few consumers or firms can be found in reality that meet the standards imposed by the theory. Often there is nothing wrong with the theory as such – it isn't false, it just fails to apply to anything, or fails to apply to the relevant cases. Utility theory, however, may be more problematical in that it seems to be misconceived; consumers obtain 'utility' – whatever that is – not from the things they buy, but from the *characteristics* of the things they buy. See Lancaster (1966) and Nell (1998a).
5. These examples are all taken from Hempel (1965). Jeffreys (1961, p. 128) considers the hypothesis that 'All animals that have feathers have beaks'; this is taken up later by Swamy et al. (1985). Penguins have beaks but no feathers; that some dinosaurs with teeth had feathers may not have been known when Jeffreys first suggested the proposition. In any case, this is not science; it is general description of a classificatory relationship that could be explored scientifically. To do so would require getting down to details in physiology and biology: how do the physiological systems work, and why have they developed the way they have?
6. For further discussion cf. Jespersen (1924, pp. 72–108 and 198–201; 1933, pp. 78–91 and 206–10); and Strawson (1959, pp. 167–73).
7. Various troublesome cases arise, of course. 'Brass' is surely a mass term, but the suggested criterion would make it a characterizing term in 'tall brass lamp', but not in 'polished brass lamp'. The distinction used to be marked by a comma, 'tall, brass lamp', but this practice has fallen into disuse. The trouble is rooted in the distinction between mass-words and countables; neither 'tall' nor any adjective of shape can modify 'brass' or any other mass-word, since anything with a definite shape has precise limits and so could be individuated.
8. Scarlet is also a colour, and clearly something can be both red and scarlet. But clearly something that is scarlet is also red; 'scarlet' is a sub-value under the value 'red' of the variable 'colour'. See Johnson (1921, ch. XI).
9. Some of the difficulties this problem raises for at least one branch of economic theory are discussed by Little (1957, ch. X, 'Indivisibilities and consumer surplus').
10. To avoid the collapse of the perceptual present into the infinitesimal, it is common to

define the epistemological present as of sufficient duration for the act of recognition. See Strawson (1959, p. 32).

11. (Ibid., p. 203). Strawson distinguishes two kinds of statements introducing particulars: those that 'introduce1' particulars into discourse provide the means of identifying that particular – and statements that 'introduce2' kinds of particulars into discourse. These latter provide the means by which that *kind* of particular can be identified. Introduction1 involves 'presuppositions1' of facts by which particulars can be identified; introduction2 involves 'presuppositions2' about *classes* of facts. We shall avoid this terminology.
12. If it were truly instantaneous, there would not be a 'before' and an 'after'; so the thing would be in two places at the same time. But if there is a 'before' and an 'after', there must be a point in time between them, since time is infinitely divisible. Hence there could have been a quicker movement, starting from that in-between point, and ending at the after point. So we can compare speeds and so on. At the outset, it was at rest, then it moved up to speed; so we have acceleration.
13. Suppose one or more basic functional relation changed, say, from linear to non-linear, from positive to negative, or perhaps from Euclidean to non-Euclidean. Imagine sitting comfortably in the living room, and then the geometry changes as all right angles widen, as the floor slowly sags, as the clock on the counter melts into a puddle. The tile squares become round, squares still, but round ones now. The sofa, like some political parties, becomes red and green all over. The table and chairs are floating, but melting in mid-air, too, melding into one another. No objects are identifiable; things and aspects of things blink into existence and out.

No assumptions about economic behavior are absolutely true and *no theoretical conclusions are valid for all times and places*, but would anyone seriously deny that in the matter of techniques and analytical constructs there has been progress in economics?

Blaug (1978, italics added)[1]

I have been gradually led to a twofold conviction: human psychology remains fundamentally the same at all times and in all places; and the present is determined by the past according to invariant laws. *It seems to me that, to a very large extent, the social sciences must, like the physical sciences, be based on the search for relationships and quantities invariant in time and in space*.

Allais (1997, Nobel Lecture, italics added)

Sensible analysis of any economic system, capitalist or not, has to *pay attention to the characteristic motivations and institutions of that society's way of organizing production and consumption*.

Capitalist societies are not all alike, as Heilbroner would have been the first to recognize. Even within the collection of historical and easily imaginable versions of the capitalist economy, there is enough variation to require nontrivial adaptation of basic economic principles to alternative institutional settings. From Irish peasants to post-Deng China, from the postwar of the United States to corporatist Austria, from the French indicative planning to Erhard's Germany, and from Thatcher's England to Sweden's Middle Way, there are enough similarities and enough differences to require substantial tailoring to fit. *I do not think any purpose is served – not even Heilbroner's own – by the suggestion that there is clearly defined capitalism and therefore a unique vision for economic theory*.

Solow (2004, pp. 204–205, italics added)

Historically, theory has generally held the pre-eminent role in economics with data being given the subordinate role of 'quantifying theories' presumed to be true. In this conception, whether in the classical (19th century) or neoclassical (20th century) historical period or even *in contemporary 'textbook' econometrics, data does not so much test as allow instantiating theories:* sophisticated econometric methods enabled elaborate ways 'to beat data into line' (as Kuhn would say) to accord with an assumed theory. Since the theory has little chance to be falsified, such instantiations are highly *in severe* tests of the theory in question. Were the theories known to be (approximately) true at the outset, this might not be problematic, but in fact *mainstream economic theories have been invariably unreliable predictors of economic phenomena, and rival theories could easily be made to fit the same data equally well if not better*.

Spanos (2009, p. 2, italics added)

5. Variables, laws and induction II: scientific variables and scientific laws in economics

The pattern of argument set out in Chapter 4 will now be adapted to the case of economics, to justify the existence of 'economic laws'. With important caveats, economic laws are indeed lawlike and economics has some similarity to natural science; but there are significant differences, too, differences that the early econometricians may have failed to see clearly.

5.1 ADAPTING THE ARGUMENT

By 'scientific lawlike relationships' we mean precise or exact and measurable functional relations between scientific variables, variables that have been defined to be measurable, and to apply to appropriate bearers, and such that these variables and their relationships are or can be related in coherent theoretical systems. These are the relationships that a successful econometric programme might uncover.

The preceding argument took the world of physical objects for granted, and asked what are the presuppositions on which our ability to re-identify such objects rested, showing that without such an ability we could not make sense of space, time and motion. That is to say, orderly relationships regarding motion, place and the duration of things – the behavior of the material objects of ordinary life – rest on our ability to recognize individual material bodies as 'the same again'. Then we showed that if we can carry out such re-identifying, we can also define functional relationships between scientific variables – that is, laws or lawlike relationships.

In the same way, we shall now take the social world for granted; we presume there is a social order, though we need not spell it out in any detail. At a minimum there are persons, and they can communicate; they have language.[2] The agents can do arithmetic. They can produce goods and services, supporting and reproducing themselves, passing on the skills needed to carry on the social order.[3]

The reproduction of a social system can be thought of in relation to Aristotle's four causes. The purpose or end of the system is reproduction with a surplus, where the latter is realized in a certain way (rents under feudalism; profits and net wages under capitalism) and assigned to various uses. That corresponds to the final cause. The form of the system and its pattern of working is presented in the equations that describe the interdependence of the parts – and this corresponds to the formal cause. The material cause is given by the material requirements – the using up and replacement of the various items of equipment and support required by the different activities of the system. The proximate cause would then be the incentives to normal behavior – feudal obligations and religious duties (along with rudimentary markets) in feudalism and the familiar market incentives in capitalism.

The project, then, will be to explore the extended implications, the presuppositions that underlie the assertion that not only does this social order exist; it rests on an economy. Working out these implications is an important part of conceptual analysis. Looking for the 'presuppositions' means trying to unearth things that we often take for granted, but without which the social world and the economy could not exist. This means asking, for example, how could a social order exist, if there were no trust? If the contacts between people were a 'war of all against all'? Suppose there were no language; how could there be trading? (Perhaps there could, if there were trust, and if people could understand each other's gestures; but it would surely be limited.) How could markets function if there were no ownership rights, no enforceable contracts?

5.2 WHAT IS IMPLIED BY STATING THAT A SOCIAL ORDER EXISTS

We are concerned not with the process by which institutional structures are formed, but with what they are, and what their continued existence presupposes – the necessary conditions for their (prolonged or continued) existence. Laws, we shall find, describe the connections between variables that characterize agents (actors in roles), but these roles must be connected into or embedded in a structure.

A social order must be reproduced; everything produced is used up or worn out, and must eventually be replaced; people grow old, lose their faculties and must be replaced, too. A social order is a system of reproduction. But reproduction is not necessarily driven by *economic* forces; the motivation and the organization could be provided by religion, for example, or by a powerful and comprehensive monarchy (or both combined, as with the early priest-kings). Complex and sophisticated civilizations, capable of great architecture and great art, have come into being and lasted for centuries or even millennia, without using money as we know it, or relying on economic incentives as we know them. Think of Ancient Egypt, Mesopotamia, the Maya and the Inca, the Indus Valley. Arguably, markets in our sense did not exist in much of the ancient world, nor are they to be found in many primitive societies. Many scholars, for example, have concluded on good grounds, following Polanyi, that Ancient Mesopotamia had no marketplaces and, moreover, that the patterns of distributing goods and allocating labour did not exhibit the characteristic interplay of supply, demand and price (Renger, 2005). Yet Mesopotamia reproduced itself, with changes to be sure, but in essentially the same form, for over two millennia.[4] And it kept records in value terms. Yet, is this enough to permit us to speak of the 'economy' of Ancient Mesopotamia?

5.2.1 An 'Economy Exists'

A market must have a definite place (whether in geographical, social or virtual space). It must be held at a definite time or times. Market agents must be authorized or empowered to engage in transactions and close deals; their actions are driven by economic incentives – they are seeking to accumulate wealth.[5] (But this was not generally the case in the ancient world or in feudalism.) Moreover, to say that an economy exists is to say that it persists through time; an economy has a past, a present and a future which are all interlinked where the market interrelates past present and future, so the economy can persist, that is, exist over the long run. And it has a material basis.

A market means that agents have access to alternatives, which in turn implies that they must be able to make comparisons. 'Economy' implies that the transactions are carried out efficiently, so that no more than is necessary in some sense will be given up on either side; regular transactions of this kind seem to imply a value, but for such values to become widespread and general it must be possible to compare, so there must be a system of valuation; it must be possible to keep accounts; market agents must know whether or not they are within their budgets.

They must keep accounts, because they must know that their revenues cover their costs – if this is not the case, they cannot replace their stock; they will not be able to reproduce.

5.2.2 Baseball

What is it to say that 'baseball exists' (in a country)? Surely it means that the game is played there on a regular basis. But what does that require? Many particular aspects may reasonably be ignored, such as whether it is professional or amateur, whether it is played in stadiums or on public fields, whether the players wear uniforms or not, and so on. But for baseball of any kind to exist in a continuing sense, to persist, new players have to be recruited and trained, and all players, old and new, have to be supported. The players must know and follow the rules; so there has to be a system for correcting and/or penalizing mistakes. There have to be places and times for games, so that all players can show up for the game. Diamonds must be set up and maintained, and someone has to make and provide bats, balls and gloves. Games are played, scores are kept, and the teams are maintained and reproduced. (The rules of the game can change, but not too fast or too much.)

5.3 THE CLAIM THAT SCIENTIFIC LAWS MUST EXIST

Our argument that scientific laws must exist will be based on drawing out the conditions for the existence – the continued existence of economic agents, capable of transacting, identified by their roles in the system of reproduction.[6]

There is an analogy here with the earlier argument for physical laws, based on the role of material bodies, but there is also a sharp contrast: material bodies just exist. That is to say, matter is, and is conserved. It doesn't wear out and have to be replaced, whereas the elements of the social order on which we are basing our argument do wear out and must be replaced – or the social order in question may go the way of the Maya or the people of Mohenjo-Daro and Harappa.

The natural world is what it is. But the social world is what we make it. We create and sustain the social world, indeed we design it – though for the most part not consciously. We are not even (usually, quite) conscious that we make and sustain the social world, but we do. And if we change our patterns of sustaining and maintaining or supporting the social world, it will change, or break down, or even vanish, as so many previous civilizations have vanished, leaving temples and cities buried in the jungle.

Strawson's approach builds on the presuppositions that underlie the ability to re-identify material bodies as the same again; it is a matter of the presuppositions of perception and identification. But economic agents are not just re-identified as the same, they must be *made* the same again. Or they must be replaced by another or others who are (produced to be) the same in all relevant respects.

Variables must be scientific variables, as defined above. That is, to say they must be measurable, they must figure in coherent theoretical analyses, and they must be appropriately related to the appropriate bearers. We can distinguish three kinds of such variables in the economy: value variables, quantity variables (modified by quality), and pure numbers. And of course there can be combinations of these, ratios of value to quantity, and revenue variables, products of quantity times value, and so on.

So 'laws' follow from the basic structure of reproduction, which has to be orderly and unchanging or changing only slowly. The structure need not be economic; the incentives could be provided by religion or habit and custom, or be imposed by a state administration. The further step to economic relationships comes with the development of property relations.

5.4 ELEMENTARY REPRODUCTION AND INSTITUTIONS: THE SOCIO-ECONOMIC SYSTEM

At the most basic level, two kinds of institutions are essential for social reproduction: those that produce the goods that support the system, and those that socialize the people who run it. Clearly these are interdependent – and they operate simultaneously. The first produces basic goods and services (food, clothing, shelter, tools, and so on), and the second, socialization and training, as carried out in families, churches and through various kinds of schools (until recently in human history, for the higher orders). Production institutions need appropriately-trained personnel to replace those who retire or die; socialization institutions need goods and services in order to function.

Production institutions use inputs to produce outputs in a routine and predictable manner. The object is to turn out certain goods or provide certain services as defined by norms and expectations. Goods and services are produced, consumed, and replaced. That is, goods and services are used up in producing goods and services, some of which replace those used up, just as trained and socialized people engage in the training and socializing of new generations, who will eventually replace them. A network or system of production roles will make up a producing institution, one that turns out results that then figure in the activities of other institutions. A firm makes steel, which is then used in making machinery; the machinery is used by another firm which makes coke, which is used in steel-making; and so on. A blacksmith makes tools, some of which he also uses.

5.5 A DIGRESSION ON PROBABILITY

An unexpected and surprising connection to probability theory arises here: in order for firms to plan their activities, judgements have to be made about how many of these tools will wear out in a period, and how many workers will die or retire; these judgements will affect the planning of both inputs and outputs, and they are necessary. Planning cannot be done without them. The striking thing is, they are of the form '*x*% of *a*s are *b*s', a statement of relative frequency. They are based on the current 'draw' from an indefinitely large pool. These judgements will be repeated again and again, period after period, making up an indefinitely large set of samples, thus defining an asymptotic distribution. Note that to describe or operate the socio-economic system (the DGM) will also require the other concept of probability, judgements of 'the weight of the evidence', since operating the system will require making calls about the relative likelihood of outcomes when facing unique circumstances. These points will come up again in Chapters 7 and 8.

Socializing institutions also involve routines and regularities. The position of 'schoolteacher' carries certain duties and privileges related to the purpose of imparting education; when someone takes on such a position, they implicitly and sometimes explicitly undertake to carry out those duties – regardless, for example, of the fact that they may be uninterested in the activities in question, and have taken the position for their own private reasons (for example, to be near their girlfriend, to have a job while waiting for a better position in advertising, etc.). The exact duties, of course, depend on the particular situation, but the general concept of the role 'teacher' implies a generalized natural intent to educate. Similarly, a farmer has a natural intent to grow crops and raise animals, but particular farmers may have very different immediate intentions, for example to sell off portions of their land, or to collect subsidies. But these are no part of the role 'farmer'. (These are modern examples, but the same applies to roles in feudal society, or in the ancient world.)

Families produce children and begin the process of fitting them for their likely, or sometimes pre-destined, roles in society. Families are only the beginning: churches, schools, and training programmes provide formal instruction and shaping; peer groups, fraternities, brotherhoods and the like impart the informal code. In short, socializing institutions shape people and provide them with the skills society needs. There is a darker side, too; sometimes these institutions also prevent their charges from becoming 'overqualified' for the types of jobs and positions available. The overqualified are likely to be the undersatisfied.[7]

So reproduction takes place on two levels: that of the population, and that of the set of commodities (goods and services). Goods and services, of course, are used and used up in the production of goods and services: input–output models exhibit the way this works, and show how the exchanges of goods and services reflect the technological interdependence (Leontief, 1951; 1987; Nell, 1998a; Sraffa, 1960). Although it has not been very widely discussed, there is a closely related process of reproduction involving the population of a social system. People work in producing goods and services, which are used in producing each other and in supporting people. Both people and goods and services are 'worn out' or 'used up' and replaced, in a regular manner. We usually think of people as ageing, rather than as wearing out – but some occupations are so stressful, taxing or hazardous that those in them really do wear out. In other cases, people lose their energy or skills in the normal process of ageing. But whatever the reasons for loss of ability by working persons, trained personnel must eventually be replaced.

Let us spell this out further. People are born and brought up – trained and socialized, 'prepared for life'. This preparation is institutionalized: it takes place in families, in schools, in churches, and it is also carried out in informal settings (Nell, 1996, ch. 3). People so prepared (and often graded and classified) then take on roles in the system, roles for which they have been prepared, and to which they are appointed through established procedures. 'Taking on' these roles is a matter of making a commitment, of assuming obligations, sometimes expressed in contracts, other times understood by social convention. Everyone is familiar with elections, appointments, promotions, the granting of tenure, job contracts, and so on. These appointments will often be based on grades and achievements during the period of preparation. In carrying out the duties and activities appropriate to the roles to which they have been appointed, they perform productively, interacting and exchanging with others. Eventually, of course, they grow old and lose their abilities, so they have to retire, and will finally die; but they will be replaced in a regular manner by a new generation, appropriately trained and prepared, as they were.

Socializing institutions are likewise composed of roles. Consider a university: it is an organization of teachers and students, where these roles are interlocked in a particular way. A high school is a different organization of teachers and students, defining the content of the roles differently. Both produce graduates, trained at different levels, but ready to take on various roles, chiefly in firms, but some graduates go into teaching. In turn, both universities and high schools fit into the overall system of social reproduction, along with elementary schools, corporations, small businesses, farms, government institutions and every other system that provides jobs and draws on other enterprises for materials and inputs or for consumption goods for its job-holders. (Not all producing roles are jobs, of course: 'father' and 'mother' bring up and socialize children, but are not usually paid for this. Yet turning out a new generation is certainly part of the reproduction of the social system.) The particular producing system specifies what kind of teacher, farmer, worker, and so on, we are considering, and the overall system determines what the particular producing systems will be able to do.

5.6 PRODUCING AND REPRODUCING A SURPLUS

In both the population reproduction and the commodity reproduction systems a 'surplus' may be produced. Allais (1997, p. 8) argues that:

> whatever the economics considered, whether in the past or in the present, the whole human economic activity comes down to the search for, and the realization and distribution of surpluses according to fundamentally invariant processes.

Consider the production of a surplus of goods; this simply means that more of at least some good or goods is produced than is used up in the aggregate of production, while no good is produced in an amount less than is needed in overall production. First we need to explain how a surplus might arise – yet this is not so difficult: everyone is familiar with producing faster, more efficiently, or forcing workers to work harder and longer. The next question, however, is: who is to get the surplus? The nobility, or the military? The Church? The King? Any or all of these are possible under feudalism. Or will it go to those who actually produce it, as the socialist tradition has always demanded? Or will it go to those who own, organize and control production, as under capitalism?[8]

Consider next the level of population. Malthus held that a long-term rise in real wages would lead directly to an increase in population; higher real wages would produce a surplus population, more people than jobs. So the excess labour supply would eventually drive wages down again, settling at a (socially defined) subsistence level. Clark (2007)[9] takes this up and describes what he calls the 'Malthusian Trap', in which he believes human societies were caught before 1800: an increase in per capita output would lead to population growth relative to land, leading through diminishing returns to a decline in per capita output. By contrast, Marx argued that faster growth would drive up real wages, leading to mechanization – especially in agriculture – throwing workers out of their jobs. The surplus labour so created would then drive down wages, raising profits, and eventually growth, so that the cycle repeated itself. This, he contended, was the correct account of the 'reserve army of labour'; the surplus of workers arose not because the population grew excessively, but because mechanization and technology reduced the number of jobs.[10]

Population is supported by consumption, and a rise in output, increasing the surplus, can lead to a rise in population or to larger, healthier and smarter people, which is to say, a population of higher quality. An increase in net consumption per capita becomes an investment of the surplus of consumer goods. This can either bring about a growth of population, or – crucial to development – it could be an investment in 'human capital', where the new generation is 'more valuable' per capita – more educated, more skilled, healthier, and so on, than the old. Civilization depends on this.

Given the system of reproduction, together with arrangements for the disposition of surpluses,[11] we can speak of a 'stable social system' or a 'stable social order'.[12] This will have to be unique, or, at least, different practices will have to be compatible. There will normally be one and only one set of social arrangements governing the reproduction of the system. If there are competing religions, one will become dominant; if there are competing monarchies or states, one will conquer the rest. Customs and habits will merge and meld, until they are compatible. As for markets, competition will achieve a balance, driving out the weaker players, until an equilibrium of some sort is reached.

5.7 INSTITUTIONS AND WHAT EXISTS

There are stable, established ways of supporting a population and reproducing it; these are carried out by people trained for that purpose. The well-defined activities and the training that prepares people to do them can be said to be institutionalized. Social structures depend on rules and on agents following the rules, and knowing how to apply the rules correctly in new circumstances, innovating if necessary to make the rules fit. The rules and the incentives to carry out the duties and responsibilities of the various roles in society may be defined and enforced by religion, or by the administrators of the state. Or they may be a matter of custom and habit, enforced by social pressures. Or, as we shall see in a moment, the market system may develop and take over the process of reproduction.

An important first step – which can only be sketched here – is to distinguish the social system and its characteristics and relationships from its current citizens and their characteristics and relationships. This applies to institutions at all levels. The club and the interests of the club should not be identified with its present membership and their interests – though of course there may be a large overlap; but, just the same, the country is distinct from its current population.

How can the club exist separately from its members, the office from its holder, the job from the worker? Institutions must be identified in relation to persons,[13] but they are defined in terms of functions to be fulfilled, which imply duties to be carried out. These are allocated to various offices or positions, which are granted powers and responsibilities. The whole is made up these parts, fitted together to perform complementary functions. The relation of part to whole here is that of component to composite, not item to list or unit to aggregate. The component is defined by its role in the composite, as piston to engine, captain to army, sales manager to company. (Some writers would describe these as 'internal relations', which is to say that the connection is, in part, necessary or conceptual.)

To hold such an office or position is to step into a role to which one must be appointed or somehow confirmed, and which carries duties, rights and powers. To be appointed, one must be qualified, one must have the appropriate knowledge, skills and abilities; that is, one must be a certain type, the product of socialization.[14]

In short, institutions are what exists in society; they are constitutive of the social order. To say that an institution exists is to say that there is a set of practices that enable it to be maintained and replaced, in regard to both equipment and personnel. These practices are themselves organized and grouped in such a way that they are the responsibilities of appropriate roles. The roles, in turn, are well defined in terms of powers and responsibilities, calling for appropriate skills. Training and socialization practices prepare agents; selection procedures assign trained or socialized agents to the roles for which they are (best) suited, as older agents retire. This is a process of reproduction: agents are trained and prepared, then selected to perform the activities of society where these activities interlock, so that what each uses up in its normal procedures is replaced by the output of other activities in their normal operations. (Again we see that these interlocking activities are 'internally related'.) Goods and services are used to produce goods and services, which in turn support agents acting in roles that manage and carry out the work of producing and using the goods and services, in the process creating and bringing into actuality a certain set of 'lifestyles'. If this is a reasonable account of the 'underlying structures' of a social order, then it would not seem unreasonable to accept, at least provisionally, the neo-Ricardian account above as a first step in an analysis of economic phenomena.

5.8 SUPPLY AND DEMAND IN MAINTAINING AND REPRODUCING INSTITUTIONS

To follow rules, to perform a job, requires acting, which means using up energy and resources. It also means becoming exhausted, tired, worn out, and ageing. (This is a case of the 'material causes' – in Aristotle's sense – of social activity.) Resources – energy and materials – have to be replaced. The replacements will have to be made on a regular basis, and the act of making them will wear out tools and equipment. With developed technologies, more is likely to be produced than will be used up in the aggregate of the production processes. This sets the stage for an 'input–output' approach, as in the classical models of production and distribution.

If the jobs in production and management are to be performed on a regular basis, then personnel replacement and renewal must also be regular. That is, if the institutions are permanent, then training of new staff will have to take place regularly as well. Suitably prepared new agents will have to replace old ones, as the latter retire or die.

The existence of economic agents who own and transact value presupposes that they must have been trained and educated as well as having been supported, born and brought up. Training of reading, writing and arithmetic implies sustained study, for which there must be social relations – learning a language, learning arithmetic, learning a subject, implies being taught or at least using materials that have been prepared, such as books and study materials. Economic agents have been socialized; studying and learning are time-consuming activities that have to be supported. A teacher–pupil relationship implies regular repeated activity.[15]

Institutions are staffed by appointment; an agent must be chosen or selected in some way, by public criteria, and then, in some public ceremony, appointed, sworn in, given the office. Potential agents, having been prepared by the institutions of socialization, are fitted into the structure by selection and appointment. We can understand this by distinguishing the ceremonies that mark entry into office, and assuming the corresponding powers and responsibilities, from those that mark graduation from an institution of training or socialization, and receiving the appropriate qualification. Appointments are normally made from among those who have earned the appropriate qualifications.

The obligations of agents in certain positions in given circumstances may be debatable, but they are nevertheless facts and there are socially defined procedures to determine them. They are presented in statements of the form: 'in this society it is the duty of agents in position x to perform actions y in circumstances z', or 'it is a norm of this society that in circumstances z, agents in position x will do actions y'. Although they are general, these statements are not 'lawlike' empirical generalizations, nor are they 'analytic'. Nor, finally, are they normative statements, although they report normative rules. They report facts, namely that in certain circumstances, in certain (or perhaps all) human societies, certain norms hold sway. Determining their truth requires 'fieldwork', in the tradition of anthropology, guided by conceptual analysis (Nell, 1998a, chs 3 and 4).

So agents are produced and reproduced, and, in the process, learn specialized skills that prepare them for roles that in turn are defined in terms of the division of labour. This implies that regular transactions must take place. The pattern of these transactions and the relations between the different kinds will emerge as lawlike. That will be our argument: there are lawlike relationships in the economy because there have to be, for economic agents to exist and be reproduced through market processes.

5.9 TRANSFORMATIONAL GROWTH AND INSTITUTIONAL DEVELOPMENT

The approach here, of transformational growth (TG), rests on what might be called 'methodological institutionalism'. This is, indeed, itself a kind of structuralism, in that the institutions that are defined by formal relationships, do in a sense generate the observed phenomena, forming the basis of what econometricians call the 'data generating mechanism'.[16] Institutions may be related to one another in formal and legal ways, and these relationships – internal relationships – may also be considered structures, as in the account sketched above.

The important point is that institutions rest on obligations and promises that agents, appointed to various positions, will do their duties.[17] The institutions, as a whole, function because the agents in the various roles do as they are supposed to, carrying out their duties on time and to a reasonable standard. Expectations – including market expectations – typically rest on obligations: bondholders expect to be paid – the bond is a promise to pay. We know that the police are sworn to uphold the law, and we expect them to do so. We know that a certain firm has promised to license one of it patents to another firm. We expect it to honour its promise.

The powers attributed to or conferred on certain offices also rest ultimately on obligations. The traffic police have the power to pull drivers over – because it is the duty of citizens to obey the law. The general can command his troops to march or to open fire, because it is the duty of soldiers to obey; indeed, they have taken an oath to do so. The manager of the company can hire and fire employees. It is a power conferred upon him by the Board; it is part of their job for employees to obey their superiors.

To perform a duty, do a job properly or fulfil a function entails following rules. But rules cannot be followed mechanically, because circumstances change.[18] Each rule has to be adapted to changed circumstances, which means understanding its intent, and then seeing how that intent can best be carried out in the new or changed conditions. This requires analysis and creativity. It requires an active mind (Nell, 1998a, chs 3 and 4), which is the most fundamental reason for the 'openness' of socioeconomic reality.

5.10 THE ACTIVE MIND

The active mind is implied by the fact that agents, acting under obligations, must be able to apply the rules to new situations. They must be able to redefine their obligations, and what it means to meet them to adapt to new circumstances. This implies that agents can always reconceptualize their obligations, that they can always invent or create new powers, develop new forms of control over nature, and define new relationships with each other.[19] This is an important foundation for the transformational growth (TG) approach; it is always possible to change the way we relate to the world. But agents cannot do this just as they please; the ability of agents to bring about changes and innovations will depend on the framework of institutions within which the activity of innovation is carried out. The central institution here is the market, for it provides both incentives and support to the active mind, in rethinking the way we relate to the material world. In particular, it supports developing control over the world.

5.11 POINTING: THE CARTESIAN MOVE

As in our examination of Strawson, there is an analogy with 'pointing'. The perceiving economist, the outside analyst, must be able to read the scene, and must be able to point to the crucial aspects – to see the intangible and search out the hidden – so as to read the inner meanings. This is necessary, for it is how we know that the observing economist will be able to understand the economy, and what the agents are doing. In short, the outside economist must be able to communicate as well as the agents can with each other.

How do we know this can be done? In principle, it can be done because the outside economist is able to think through and analyse the transactions; being able to identify and re-identify the variables, an outsider observer with the same language and other skills must be able to understand the relationships. This is essentially the cartesian move. Because the observing economist understands well enough to ask the questions in detail, she or he can also find out the answers. Put another way, we understand the flow of money and goods, because our own reproduction is part of that same kind of flow.

There must be an ability to communicate, to understand intentions, and to be able to understand the proper intention, meaning the intentions an actor in that role ought to have – just as a policeman ought to be out to catch robbers, not to collect bribes – and to compare those with the intentions that seem to motivate the actual behaviour we see. Getting at intentions is notoriously difficult, though, since those with bad intentions do their best to hide them. But we have plenty of ways of dealing with this; it is one of the things the courts do, for example.

5.12 CONDITIONS OF PERFORMANCE: TRUTH-TELLING

Social and economic variables are defined by intentions and meaning, expressed in language; and language requires communication: performative utterances, speaking to someone, thus doing something to, for, or with someone else. This is social; it is communication, and this presupposes conditions for understanding another person. We recognize the basic emotions, we recognize similarities, but for communication there must be agreement on meaning, on denotation and on connotation. This requires more than seeing things the same way; it requires that both parties have a way to make sure the other party is not deceiving them. They must not only agree; they must commit themselves to the project of communicating, not just for now but for the indefinite future. They have to adjust their thinking to each other.

5.13 THE ECONOMY AND THE MARKET

Strawson's argument turns on the presuppositions necessary for identifying and re-identifying basic particulars, which occupy space and move through time. For economics, we need basic particulars that hold and transact value over time, namely economic agents who enable re-identification. To show that something is an economic agent, we must be able to identify and re-identify its position in the circuit and trace the pattern of its transactions through time, showing that at the end of the circuit the agent will be restored and ready to function again – that is, reproduction takes place. We pick out a transaction by reference to the owners of the goods, money, services or whatever it is that has moved in the transaction. Material bodies can be identified and re-identified without reference to any other particulars, but economic agents must be identified, and their reproduction accounted for by reference to their position in the economic system. (Actual agents, of course, are real persons located in space and time; but they are persons acting in roles which assign them duties, powers and privileges. It is these roles that identify them as agents in the economic system.)

Economic agents, institutions or persons acting in specific roles, with specific powers and responsibilities, will be the bearers of variables which characterize them or their actions and transactions. But the variables are not just descriptive, as in the natural sciences; in addition, the bearers and variables are linked in complex ways by ownership. A material may have a colour, but it does not own that colour. The bearer of the variable 'supply of commodity *x*', however, must actually own commodity *x*, or otherwise have the right to sell it, granted by its owner. A demander who pays for it must own the funds, or have the right to use them (for example, if borrowed). The utility or satisfaction a household gets from consuming goods is the household's utility; it belongs to it, though not quite in the same sense that the goods do. Households must acquire, come to own, whatever they get utility from. Firms own their technology. So variables do not simply characterize bearers; they are connected by the ownership relation.

The 'economy', we argue, implies a market system of some sort. By saying that in a particular society 'an economy exists', we mean that the regular reproduction of the structure of that society is carried out by a market system; the society continues to exist because of activities carried out under market incentives. By a market system, we mean that there are relations of Ownership, *O* relations, and these are expressed in terms of Value, *V* relations. The economy, however, does not necessarily imply capital, or capitalistic relationships.

5.14 CAPITAL

Capital is a way of organizing production and reproduction, ensuring strong competitive motivation, and stimulating the restless energy that is needed to drive innovation. It rests on the wage–labour relationship: the business firm hires labour, but what labour produces belongs to the firm. Capital 'turns over', flows in a circuit, and always seeks the highest rate of return. This competition tends to establish a general rate of profit. These profits, invested, lead to growth; but failure to invest brings on sluggishness and hard times. The result is restless fluctuation between boom and slump. Capital has a historical cost, and can be valued by its current earnings, but its long-run or true value, known as its 'present value', is the discounted value of its stream of expected future earnings. So the present depends on the future!

Capitalism implies a circuit of value that organizes production and distribution, consumption and exchange, carrying out reproduction and making orderly expansion possible. The turnover of capital comprises a circuit in which all transactions required for reproduction are monetized. Every transaction is an exchange expressed in value but carried out in money. The nodes of the circuit are the points of transactions and the agents are those taking part in the transactions. To be part of the system is to be an agent, therefore included in the circuit of value, a transactor.

An advanced economy is a system of reproduction organized capitalistically, thus governed by relationships expressed in circulating value (money). But more primitive economies are possible in which ownership relations are widespread and transactions are made in value terms. (Think of Marx's descriptions of petty commodity production, or the Western frontier; going back further, we find feudal agriculture and medieval markets; in the Third World, we find plantation economies (commercial but seldom fully capitalist), indentured labour, slave societies, etc.)

5.15 RELATIONS IN THE SYSTEM OF REPRODUCTION: OWNERSHIP AND VALUE

In an economy, reproduction is carried out by means of market transactions. Transactions involve the exchange of ownership. Ownership is ultimately ownership of value, and value is realized in exchanges.

5.15.1 Transactions

A delivery of a good changes the possession; a transaction changes the ownership. Transactions require that both or all parties must have ownership of the items being transacted, and they must transfer this ownership. The items must be what the owners declare them to be; the transaction will not be valid if there is fraud. The transaction must be followed or completed by delivery of the physical goods or items. (None of this arises in the simple 'representative agent' case, which therefore cannot be a model for the economy.) For transactions to take place regularly, with the agents making exchanges, there must be not only ownership but also the ability to judge and enforce contracts. (Without ownership, transactions would be too uncertain for regular exchanges. In the absence of ownership and rights, 'might makes right'; it would be a war of all against all.)

Transactions precede, are simultaneous with, or succeed each other. Transactions are smaller, the same size, or larger than each other. We specify the agents, the date or time position, the order, the amount, the direction of flow, and the juncture or position in the circuit where it takes place. This enables us to locate and identify a transaction, but says nothing about the exchanges or payments themselves.

Transactions must take place at arm's length; there must be diverse activities that need each other's products, reflecting division of labour and specialization and based on diversity of interests; and these interests must clash: one party's gain will be another's loss, although the loss may be notional, since both parties may actually gain from the exchange; but one party's gain could be larger at the expense of the other party.

Agents are households, firms, banks, funds (portfolios), and government. To understand the system, we – the observers – must know that the agent can do the job, has the qualifications to carry out the responsibilities of the position, and that the agent's intentions are those that she or he ought to have (that is, that are consonant with the position). So, of course, we must know the duties and powers of the position.

Thus we must be able to ascertain intentions, and compare them with responsibilities. We must be able to clarify the office and its requirements; we must be able to recognize and validate ownership and transference of ownership.

Commodities and services are described by sortal universals or stuffs – but there is no question of identity comparable to the issue Strawson faced, for 'this butter' is particularized, first, by being a pound of butter, and second, by being sold yesterday at the corner shop to Mrs Filbert. The identity problem is not re-identifying the commodity itself – this pound of butter or that, this car versus that – but of identifying the owner. Which of a group of identical cars it is, is not important; what matters is whose car it is – yours or mine? This cannot be gleaned from any inspection of the car itself, as a physical object. Nor can we tell from physical data alone who made the payment to whom.

5.15.2 Value and Ownership

Economic variables are at bottom intangible. They may be embodied in material things, as monetary value may be embodied in silver coins, but value and ownership are inherently intangible. Money, prices, incomes and capital values cannot be seen or touched; they are not material, we can't smell, hear, lift or weigh them. We can verify the weight of a chair, and ascertain that it is made of wood; but we cannot similarly verify its value. We can ascertain that it is an antique by fixing the date of its production and discovering that it was made by Thomas Chippendale in London – but why those facts of time and place and person confirm or verify its value cannot be seen or explained by any facts of nature or laws of nature.

Value arises from the transactions made to ensure the regular reproduction of the system. Agents need value and value relations to be able to make comparisons of different 'bundles' of goods and services, and to be able to calculate their budgets and keep accounts.

5.15.3 Value

So, what is value? How can we verify that the value in a transaction is the correct value? More generally, how can we identify value and re-identify it, how do we know that this thing or this transaction has a certain specific value, and that this value we see here is its true value? (If a transaction or a commodity has a certain value, it can only have that value; it cannot simultaneously have another value.) Value, of course, is a fundamental idea in all systems of economics and political economy, and the view we present here makes no claim to originality; we adopt the classical idea that 'value' refers to the set of relationships that are essential to reproduction, distribution and growth. The relationships in question are proportions; the outputs of the system must stand in certain proportions for reproduction and/or expansion to be possible; exchanges between sectors must take place at certain ratios for reproduction to take place; flows of saving and investment must balance, and so on.

Two kinds of value can be distinguished (Sraffa, 1960): basic or reproductive value exists when something has value because it enters essentially into the exchanges that enable reproduction to take place. This means that when variables and parameters change, the commodity or service or asset will always have a definite unique positive price. *Non-basic values* hold for commodities or services that do not enter essentially into the system of reproduction, but instead represent the spending of income on household conveniences or luxuries that are supplied by firms drawing on basics, and again for all the parameter changes the goods or service will always have prices. In general, value exists for all reasonable parameter ranges.

If we say that this thing has the same value as that, we mean that they exchange one-for-one with each other. But suppose they never exchange – because they are things that are not likely ever to be swapped. Three classic women's handbags by Judith Lieber may have the same value as a new Toyota Camry, or a small industrial lathe, but no one would expect to trade them. (Perhaps in a family the mother and two daughters would buy the handbags rather than the car.) To say that these two – the group of handbags and the car – have the same value is to say that if any agents wanted to purchase either, they would pay the same price. (These two values are ultimately grounded in the exchange ratios established in reproduction and enforced by competition; values of goods that don't trade with one another are established by comparisons and opportunity costs – see Kurz and Salvadori, 1995; Nell, 1998a; Pasinetti, 1977; Schefold, 1997; Sraffa, 1960.)

5.15.4 Value and Space

Do commodities have value in the same way as material bodies take up space? Commodities take up value space in budgets. Just as stuff or matter fills space and saturates it, costs or expenditures take up space in budgets. Just as there cannot be two different stuffs in the same place at the same time, there cannot be two different values in a transaction at a given time or place. If x amount of goods trades for \$$y$, it does not trade for \$$z$, where z does not $= y$.

To say that x has the same value as y, or as y had yesterday, we express value in a measure; x equals the same number of units of value as y. We are all familiar with the normal behaviour of value: it expands in production, since production normally generates a surplus, and it is reduced in consumption. In ordinary practice, value is expressed in money; value is conserved in transactions, so money is conserved in circulation. Whatever is chosen as the standard has to be demonstrably stable, as Ricardo and Malthus knew. Money can fluctuate independently of the reproductive system, so for analytical purposes the Classics chose to express value in labour time (Nell, 1998a; 2004).

Agents acquire value, possess or hold it over time, and transact with it. But different values cannot occupy the same social space at the same time; an agent has a sum of value to transact – it is one and only one sum at a time. The conservation of value, and the uniqueness of transactional value at any time, together provide the basis for the 'law of one price'. Why does this matter? Because it allows for scientific economic variables to be defined. That is why budget constraints constrain. The amount of value in a particular time or place can be defined in terms of exchange of equals. The variables are typically expressed in value. Those that are not tend to be functions of those that are. Apart from some engineering relationships, all economic variables are either expressed in value, or are functionally related to variables that are expressed in value.

This brings us to a point that may seem surprising at first, but is already implicit in what we have said: many expressions apparently involving only quantity variables must actually be classified as value relationships, in our sense. To take a famous example, Sraffa's standard commodity is a value relationship – it is a set of proportions of quantities of particular significance in reproduction and growth. Quantity relationships that are dual to price relationships will also all be value relationships in our sense. The productivity of labour is a value relationship in our sense, as is the consumption function, the wage–profit tradeoff, and other macro functions. Expressions involving pure quantities, a mix of price and quantity variables, or pure numbers like interest, are value relationships in our sense if they are part of (or derived from) a model or models based on the reproduction, expansion or working of the basic system. A formula for cracking petroleum is not a value relationship; it is a statement of technology. A list of inputs and their costs is not a value relationship, but a balance sheet is.

5.15.5 Ownership and Endowment

'Owning' is fundamentally a value relationship; owning is owning value, whereas possession is having control of a thing, a commodity or an artifact, in which 'control' means the ability to decide on its use. But the rights over, or claims to, something – an artefact or a commodity or an asset – are expressed in value; the ownership is ultimately not to the actual physical item, but to its value. The value is what you can insure, or what you will receive in compensation if it is destroyed. The value is what is protected by insurance, and is what enters into transactions. (If a particular item has a special personal importance to an agent, that importance becomes a special value that can be insured.)

All economic variables stand in ownership relations and are expressed directly or indirectly in value. Prices, revenues, costs, loans, bonds, stocks, credit and capital are all expressed in value and stand in ownership relations. Quantities in equilibrium have to stand in certain proportions, and this is a value relationship.

All and only owners are economic agents. Agents are endowed with the ability to earn and support themselves and their families. Everyone in the system is endowed with some kind of education, preparing them to work. All agents thus own their own labour power, at least; some inherit property as well: land, houses, businesses, financial instruments. People may also own personal property that can serve as collateral for a loan. But those who have nothing – no marketable skills, no things or plot of land against which to borrow, etc. – cannot be part of the system.

Agents who have been prepared by the 'socialization' processes will be appointed to roles; in these roles they will 'own' various assets, or, better, they will represent or act for the ownership interest. As CEO they will manage the assets the company owns; as Board Members they represent the shareholder interest. The shareholders may be pension funds, trusts, universities and other companies – all institutions. There may be no or few *individual* owners. Ownership relations thus may hold between assets and positions (this land belongs to the Crown), or between assets and institutions (those trucks belong to the company), or, finally, between assets and actual people (he inherited that house.)

In short, *O* relations connect agents – bearers of variables – to the variables; they define applicability – that is, they give us the right to say that this agent's behaviour can be described by this or that variable. *O* relations are matters of fact; they ascribe skills to workers and managers, for example, which can be checked by seeing if the agents have passed tests or taken training. The same goes for firms: Does a firm have the right to use a certain technology? Has it actually mastered the technology? Households and firms are endowed with assets; how did they obtain them? By inheritance, by transfer, or through a successful deal? These are factual questions, not matters to be settled by making assumptions. Whether an agent has something marketable or relevant to market activity – a skill, a set of commodities, an asset, a plan or a goal – is a matter that can be determined by objective criteria.

In the neoclassical framework, however, there is one major exception: preferences or utility functions. We say the household or the consumer 'has' a preference set, or a utility function, with certain characteristics; and this function is the foundation of the theory of household demand. But there is no comparably objective and straightforward way of checking on the function, so long as we treat preferences subjectively. There are no tests, or contracts that tell us someone has acquired a set of preferences on a certain date; and it is hard, just looking at behaviour, to determine the shape of an agent's preferences. If they do something inconsistent with the supposed utility function, have they made a mistake, or changed their mind, or is the actual function different? There is no way of telling. However, we could go beyond passive observation and ask what a household or firm is *committed* to do. The Board may have voted to adopt a goal and a plan on a certain date, so that this will govern the behaviour of the firm going forward. A household may have consciously adopted a certain lifestyle, based on social norms, as determined by fieldwork. In these circumstances preferences could be given empirical content.

V relations, of course, are fully empirical; they connect the variables and express the lawlike functional relationships between them. But, as we shall see, some of them are reliable, and are in many ways comparable to the laws of science, while others are volatile and only temporarily appear to be lawlike. But both are needed in our models.

5.15.6 The Argument for Necessity

Suppose there were no values, no value transactions, no holding value over time; there could not be competitive comparisons, and costs and revenues could not be calculated, or balanced. Accounts could not be kept (unless Soviet style 'material balances' were introduced, but such balances cannot provide equivalences). Without values, the economic agents could not carry out their roles in the reproduction circuit; they could not make the necessary comparisons.

Suppose there were no functional relations and no reliable relations, but only random interactions, if any, between scientific economic variables. The exchanges needed to enable replacement would not happen, and this would preclude regular reproduction. Agents need to engage in regular, timely consumption – for example, of food and drink, heat, energy, clothing. Irregular availability of goods and services could bring weakness and death. Without regularity the *system* could not exist.

Suppose there were no owners, or that 'owners' had none of the powers needed to carry out transactions – think of this as an analogy with the supposition that there were no material bodies. Without *O* relations, there could be no exchange, since exchange is a transfer of rights; without exchange 'price' cannot be defined; and without price, 'income' is left unhinged. Just as we could not define length, mass, duration and so on if there were no material bodies, we could not define value, exchange, price, income, wealth and so on unless there were ownership and transactional abilities, together with a number of well-defined agents possessing *O* rights, thus making up markets.

Why must there be *O* and *V* relations? Why not carry out the transactions necessary for reproduction as religious ceremonies? (This was partly the case in early feudalism.) Or perhaps they could be carried out in celebrations of reciprocal gift-giving, as with the 'potlatch' rituals in the

Pacific Northwest? Religion would tend to eliminate or control the war of all against all; so, albeit less reliably, would the gift-giving rituals. The standard answer would be that transactions (and so reproduction) would be more efficient and more certain when variables are related to bearers by *O* and expressed in *V*. From our point of view, this may not be the best way to put it, but it is certainly not wholly wrong. Without *O* and *V*, reciprocity would be based on moral conventions; there would be no measures of productivity. If the transactors are not owners, they may not be responsible, and so may not carry out the transactions fully. Nor will the transactions be based on comparison shopping; *O* and *V* allow for comparisons of proposed transactions to be made, and alternatives to be considered and measured. This allows for competition to develop and drive the process. *O* and *V* allow for the calculation of costs and gains, for keeping accounts. In short, *O* and *V* relations are necessary; for an economic system to exist and persist:

- the economic variables will stand in *O* relations to the bearers; and
- the variables will be expressed in *V* and will stand in *V* relations to each other.

This, then, is the foundation of the 'data generating mechanism' (DGM). The basic DGM is the set of transactions that enable the regular reproduction of the socio-economic system.

5.16 ROBINSON CRUSOE

There is an interesting analogy here with Dretske's attempt to provide an account of spatial relations without material bodies: Robinson Crusoe is presented in the textbooks as a representative agent, making economic decisions – but there are no markets! Instead we have the case of an isolated representative agent, like Robinson Crusoe alone on his island. When there is a single agent, there are no genuine transactions, so there is no value. Value arises in a network; value is the reflection of the transactions that hold the network together. Competition is necessary to correct mistakes; a single agent faces no correcting power. The single agent may push an activity to the point where the return from that activity has fallen to the level of the return at the margin of other activities, thereby achieving some kind of local maximization (textbook Robinson Crusoe). But this is not exchange; it is one-shot maximizing, a temporary equilibrium at best. An exchange takes place when one party transfers ownership of a commodity or asset in return for a payment in value, the payment, in turn, being a reciprocal transference of the ownership of the value.[20]

5.17 RESOLUTION OF THE INDUCTION PROBLEM FOR ECONOMICS

So there must be laws; that is, there must be some stable functional relationships that hold unchanged between economic variables over long stretches of time. These will reflect the key characteristics of economic agents during the process of reproduction and expansion. These relationships will hold simultaneously, and will describe simultaneous – or sometimes sequential – activities that will be interdependent in various ways. The variables characterize the behaviour of agents, who will be identified by role, place or position, where these define their duties, powers and privileges. Note that they are not best thought of as aggregates of variables characterizing individuals; they characterize components of the system. Components – for example, sectors or classes – are not aggregates; they are defined by function and position in the system, so that we have the consumer sector, producers of capital goods, services, workers, farmers, banks, and so on. Each has certain duties or normal activities, and these relate in definite ways with the others.

But unlike laws in physics, economic laws may be reliable or volatile; reliable laws may be so generally, in a wide range of cases, or specifically, here and now in this case; they may be reliable in both content and form. Reliable in form means that the form of the relationship – number of variables, increasing, decreasing, sigmoid, and so on – will not change; reliable in content means that the numerical values will stay constant or remain within narrow ranges.

Just as there must be laws, reliable laws, so there must be volatile relationships. These are functional relationships between well-defined scientific variables – in that respect, they are no different from reliable relationships – but they are capable of changing suddenly, without warning. Parameters may shift; even the form of the function may change. These relationships must be unpredictably changeable partly because of innovative ideas springing from the 'active mind' – invention and innovation – but there is another related and very important reason: a capitalist economy will normally be a growing economy, and a growing economy needs both to form expectations about the future and to base decisions on these expectations. Since the future cannot be known, this process must be uncertain; so new information may lead to sharp changes in expectations, and hence in decisions.

5.18 SCIENTIFIC LAWS MUST EXIST, RESOLVING THE REFORMULATED PROBLEM OF INDUCTION

The problem of induction for the case of economics, then, is settled by the argument that there must be scientific laws. However, there are major differences from the case of physics. In macroscopic physical science, laws cannot change form or cease to exist. But in economics they can: the nature of the economy could change; feudalism could and did develop into capitalism. Underdeveloped countries develop, in the process changing the institutions by which reproduction is carried out. With different institutions, many of the lawlike relationships could be different – not wholly different, to be sure, but still different. (Think of the change from the moderately stabilizing price mechanism of the nineteenth century to the volatile multiplier-accelerator of the twentieth.)

5.19 THE SOCIO-ECONOMIC SYSTEM: 'LAWS' OF AGGREGATE SUPPLY AND DEMAND

When the system is operating, reproducing itself, it does so at a definite level of aggregate activity; this is the level of aggregate supply. The system's normal level of capacity is determined by previous investment; there are farms, factories, shops, malls and offices. Each has been constructed as a result of investment, each requires staffing at a certain level in order to operate as it was designed to, and each employee must have certain qualifications in order to handle the duties and responsibilities of the job. To be a member of the system one must be an owner – determined by inheritance – or be employed by an owner or manager of capital (the capital could be borrowed from an owner). (In advanced economies the level of capital and the way it has been invested in the various sectors will set the scale of the system.)

When all positions are filled and all sectors are operating as they should, the result will be the normal level of output. But this output must be exchanged for value; for efficiency in transactions, that value will be circulated in the medium of money, metal or paper during most of history, now immaterial accounting balances or computer entries. Money incomes must be paid, and the incomes appropriately spent for the normal level of supply to be circulated and consumed, and otherwise used or invested.[21] But there will normally be definite relations between variables that determine the level of aggregate demand and how it interacts with aggregate supply.

5.20 LAWS OR LAWLIKE RELATIONSHIPS AND SCIENTIFIC VARIABLES: EXAMPLES

We argue that laws must exist because they are conditions for the continued existence of agents-acting-in-roles. And there are many examples of propositions that have been put forward in various economic fields as laws or as being 'lawlike', starting with the 'law of one price', together with the related idea of the uniform rate of profits, and uniform wage rates, all enforced by competition. Many of these have strong empirical support, but none of them meets the standards for lawlike propositions that routinely prevail in the natural sciences. All are riddled with exceptions and special cases; moreover, natural laws hold here as they do everywhere, now and in every era, but economic laws tend to differ in many details from one society to another, and may be seriously different from one age to another.

For example, we might start with the microeconomic law or laws of supply and demand, namely that prices tend to vary in the same direction (sometimes in the same proportion) as changes in the ratio of demand to supply. An important early claim that was widely accepted for a time held that price variations revolved around the labour value. This has proved hard to justify, even though there is some evidence. For demand and supply, however, there is good evidence, provided it is properly interpreted; not so, though, for the 'laws' advanced to explain demand and supply respectively, namely the law of diminishing marginal utility, and that of diminishing marginal productivity. Marginal utility is not an operational concept (nor is 'revealed preference', although that is another argument). Marginal productivity can be made operational, but a great deal of evidence shows that in today's economy, in industry and much of services, unit costs are constant. And there are many instances of increasing returns to scale, though perhaps not enough to make a case for a 'law'.

Adam Smith, on the other hand, argued that productivity depended on the division of labour, which he held to be limited by the extent of the market; he presented a conceptual analysis to support his claim, and he also backed it up with facts gleaned in part from fieldwork.

The relations in macroeconomics are based on obligations, contracts and social norms. For example, for those without substantial wealth, consumption depends on current income, and will be governed by the social norms relevant to the life of the family or household. So $C = C(Y, \ldots)$ will be reliable, which does not mean that it will always be rigidly the same. There will be random fluctuations, but they will lie around a central target. The fluctuations in turn will usually have understandable causes, which may sometimes explain the pattern of the distribution. There may from time to time be significant changes, as new household products and technologies emerge, but fieldwork should reveal these. The relation between output and employment reflects technology and the contracts and norms that govern employment. So $Y = Y(N)$ will also be reliable, and likewise may be subject to shifts and changes; in the short run this may include 'labour hoarding'. Then there will be a further relationship in most modern economies: consumption demand rises when the real wage rises, so that the real wage and employment tend to move together. By contrast, in Craft economies, the real wage and employment will tend to move inversely. In modern economies, fluctuation in investment will lead to movements in the same direction in consumption – the multiplier relationship – but in Craft economies investment and consumption will tend to move inversely, providing a limited stabilizing effect. In both cases the process of economic adjustment will be a movement towards (or around) a 'demand equilibrium' (Nell, 1992a), a balance of injections and leakages (in the simplest case: $I = P$).

In growth economics, laws have been proposed, some of which turn out not to fit the evidence very well. For example, the Marxian law of the falling rate of profit has been studied intensively but inconclusively. Likewise, there have been many studies of the neoclassical inverse relation between capital intensity and the rate of profit, and the corresponding relation between employment and the wage rate (marginal productivity of labour). Most of these have been flawed because of problems in moving from linear cost data to conclusions about the form of production relationships (Shaikh, 1980; also see Medio, 1980). Moreover, relationships that hold in the Craft economy may not hold in conditions of mass production. Better confirmed, perhaps, are Kaldor's Laws, and Verdoorn's Law, which show that demand pressure tends to lead to productivity growth. This is related to the debate over 'Okun's Law', which shows that a rise in demand at a high level of employment will lead to a less-than-proportional increase in employment. (A 1 per cent increase in demand will tend to bring a 0.3 per cent fall in unemployment.)

A number of important transformational growth regularities have been proposed, some of which constitute what might be called the 'Victorian equilibrium':[22]

- the (somewhat imperfect) constancy of the 'great ratios' (Klein and Kosobud, 1962);
- government in relation to GNP, G/Y, rises as per capita income, Y/N, rises, over the long term;

- employment in agriculture falls as *Y*/*N* rises, again over the long term;
- 'Gibson's paradox': prices and interest rates are positively correlated in the Craft economy;
- primary prices show more volatility than manufacturing prices, and both are more volatile than money wages.

What does it mean to say that a consumption function, or an output–employment function, 'exists' for an economy? It means that there are identifiable agents standing in *O* relations with well-defined variables, standing to each other in *V* relations. This picture will be grounded in obligations and contracts, reflecting social norms and expectations, where these are all part of the organization of regular maintenance and reproduction. These norms will define a target or central tendency, while the deviations in turn will be due to various 'exogenous' forces, which may be understandable but too distant or irregular to encompass within the model.

Now contrast this with a liquidity function, or an investment function, these being much less reliable. These are also based on norms and the organization of the basic system of reproduction, but they do not centre on a reliable target or focus, because at crucial points such a target – the desired holding of money, for example, or the amount to be spent to construct new plant – must depend on our assessments of the future.[23] But we do not and cannot know the pattern of future bond prices, or the way future markets will develop; we can only make educated guesses, and these will shift, perhaps radically, with new information and developments.

This helps explain why we can expect that today's multiplier will be the same as yesterday's. The multiplier is based on reliable relationships, which rest on obligations, contracts and social norms. If the multiplier changes, it must reflect changes in the level and pattern of maintenance and reproduction. Of course, it may be asked, how can we be sure? Or rather, how sure, if at all, can we be? This is a different question; the argument so far tells us that reliable relationships must exist; it is a question of ontology that there *are* laws or lawlike propositions in economics – but a successful answer does not tell us how to find them, or how to know that we have found them. To do that we will have to investigate *O* and *V* relationships, we will have to identify our variables and show that they really do characterize our agents. That we are *able* to do this is an epistemological question, which we address in the next chapter on the methodology of model-building; actually doing it, of course, is doing econometric science, which we address in the later chapters.

5.21 ECONOMIC EXPLANATION AND THE 'NORMAL WORKING' OF THE SYSTEM

Services or producing institutions, then, carry out the activities that enable the social system to maintain and reproduce itself, while the socializing and training institutions prepare the next generation. Such a system can be modelled in greater or less detail, showing how it works. That is, the internal relations between the roles and the institutions can be simplified and presented all together, abstracting from inessentials, and organized to allow us to focus on whatever questions are uppermost. Our approach, as we have seen, distinguishes between 'bearers' and the variables they bear (Nell, 1966). Bearers are agents and institutions (as defined by conceptual analysis and fieldwork), variables are (measurable) characteristics of the bearers and their activities. For example, households are the bearers of the variables consumption and disposable income. Firms are the bearers of output and investment. Models are made up of relationships between variables, but these relationships are constrained by the fact that they must be consistent, collectively, with the conditions for the continued existence of the set of bearers.

For example, we might divide output into capital goods and consumer goods (and services); then show how capital goods and labour are used in producing capital goods and consumer goods; how output is sold for investment and consumption; and sales revenue divided into wages and profits. Then we might fit all this together, first into social accounts; then, by making some simple hypotheses about what the various kinds of expenditure depend on, into a model that presents a picture of how the level of output and employment is set and how and why it varies.

In other words, a 'picture' that is exactly right.[24] (And once we have a picture, we can tell a story, bringing out what lies behind the picture . . .) This is not about the choices, rational or otherwise, of abstract agents with implausible or impossible abilities (for example, Fisher's (1930) utility-maximizer choosing between present and future consumption, updated recently by Cochrane, 2001). It is about a system of interlocking activities that generate products and trained agents that make it possible for the system to support itself and replace used-up products and worn-out agents, so that it can continue indefinitely. A picture is a very good idea; it will show the different parts of the system and how they interact to determine the outcomes.

But wait a minute! 'Determine'? Earlier we agreed that the economic system, and the social order generally, must be considered essentially open. How can we have a determinate model? Nothing could be easier – and it is done all the time. The model shows how the system works, how it is supposed to work, and it may well also be used to show how it might break down, and what sorts of things might cause it to do so. No one can predict exactly how a given car is going to run, but the engineer's design tells us how it is supposed to run. No one expected the World Trade Center to implode, but the architect's plans can be used to reconstruct how it happened. Determinate models show us how the system, whether designed or emergent, is supposed to work, how it all fits together and how it produces the outcomes that keep it going. But that doesn't mean that such models tell us what *will* happen. Remember, some relationships are inherently volatile; they can always change unexpectedly. So other outcomes are possible, parameters may change, relationships may shift, innovations may take place. But the model, if it is a good one, shows us how the structure hangs together.

Let's develop this. If our conceptual analysis shows that the underlying mechanisms have quantitative aspects – and our fieldwork gives us some idea of what the relationships look like – then we need to set up quantitative models. We want to understand how the mechanisms work, so we need to study them 'in isolation'. The system is open, but to see how a given mechanism works, it is necessary to set it apart, to examine it on its own, by artificially holding other factors constant. This is Marshall's method of 'ceteris paribus'. We know the other factors are not constant, and we know that our mathematical assumptions are only approximations. But we want to see how the system works.

So we apply the model, using it to interpret the world. We compare what the model tells us with what actually happens, as far as we can measure it statistically. In this way, the model can be assessed, and its explanatory power determined. Its shortcomings will become apparent, and ways of improving the model may be suggested. But it is also likely that the model itself will call for new ways of looking at the world, pointing to some new dimensions to measure. This will bring more information, with which to develop the model. The improved model may then suggest even more new aspects to measure, bringing still further improvements in the model. In other words, we enter on an interactive interrogation of the world, using the model to understand what is happening, and using that understanding to improve the model (see Nell, 1998b). We approach the truth; we can get a picture of it, but we never finally capture it.

5.22 EVOLUTION

Next, given that reproduction will never be perfect, there will always be variations, 'mutations' if you like; and these lead to a process by which institutions evolve. That is, changes will take place, and more successful forms will tend to be preserved and passed on. In a limited sense, this has to be the case. The system has to be reproduced; the environment of the society/economy will be changing, and there will always be slight variations, 'mutation', in the design and production of goods, services, skills, training, and so on. Some of these will be better suited to the changing environment, some worse. They will compete, in short; and over time the system will change. In addition, however, the active mind will be at work. There may be large, deliberate changes. For institutions can be and often will be modified by intentional, organized action – although the results will very often be quite different from what was intended. But in these cases also, selection procedures will pick the variations that survive and are passed along. So institutions develop, evolve.

Selection may be based on biological reproduction – those institutions survive that enable the group to reproduce more effectively than groups organized by other institutional forms. Or selection may be based on something else. The market is a selection process; it evaluates how successful technologies and business practices are. Those that are profitable are supported and flourish; those that are not, however aesthetically pleasing or morally sound, will perish. Of course, a society that is profitable will also tend to be successful in biological reproduction.

Evolution can be studied and modelled; but, being a process of change, it will not generally be describable by lawlike propositions, other than those already alluded to. But these are sufficient to make it possible to develop a serious and precise science, although we must not expect it to look the same as physics.

NOTES

1. Quoted by Boland (1982, p. 155).
2. Later we shall consider the implications of this world having an economic aspect – that is, where persons are acting as economic agents conducting economic transactions. A 'transaction' means that a 'good' moves in economic space, from one 'owner' to another. Usually such transactions are carried out in a specially designated area of social space called a market. But there can be 'reproduction' without an economy.
3. Plato sketches a minimal socio-economic system in chapter VI of *The Republic*. For an account of Plato see the Appendix to this chapter.
4. Why not call it a 'planned economy', since the distribution of goods and labour was directed by the state in conjunction with priestly authorities? Moreover, famously, we know detailed accounts were kept on clay tablets, and these included records of credit and debt. But did they *plan*, that is, calculate an optimal course of action, and was it an *economy*, that is, were the activities driven by economic incentives? The answer to both is, surely, no. Also see Polanyi (1944).

5. The unrealistic assumptions of orthodox economic theory have been criticized by many economists, but Bourdieu (2005) argues that we must go further. He contends that supply, demand, the market and even the buyer and seller are products of a process of social construction, and so-called 'economic' processes can be adequately described only by calling on sociological methods. One has only to examine an economic transaction closely (as he does in his book for the buying and selling of houses) to see that an approach based on these abstract assumptions cannot explain what happens in reality. Bourdieu shows that the market is constructed by the state, which can decide, for example, whether to promote private housing or collective provision. And the individuals involved in the transaction are immersed in symbolic constructions which constitute, in a strong sense, the value of houses, neighbourhoods and towns. Instead of seeing the two disciplines in antagonistic terms, he suggests that it is time to recognize that sociology and economics are in fact parts of a single discipline, the general object of which is the analysis of social facts, of which economic transactions are in the end merely one aspect. Bourdieu fiercely opposed rational choice theory as grounded in a misunderstanding of how social agents operate. Social agents do not, according to Bourdieu, continuously calculate according to explicit rational and economic criteria. Rather, social agents operate according to an implicit practical logic – a practical sense – and bodily dispositions. Social agents act according to their 'feel for the game'. See also Chapter 10 of this book.
6. We are claiming the possibility of a kind of knowledge that post modernists, for example, seem to deny: precise but general knowledge of how systems work over a considerable stretch of time. Many mainstream economists and econometricians have apparently given up on this and lowered their sights. But they have given up too easily – they were misled because their theoretical approach was defective, and they had (from positivism) an incorrect idea of what scientific knowledge in economics would look like, so they often didn't recognize it when they found it.
7. In a retrospective article on apartheid, in *The New York Times*, 20 September 2009, Hendrik Verwoerd was quoted saying that education and literacy among the 'bantu' population must be kept limited so that they would never know the life they were not allowed to take part in.
8. A simple case is presented in Nell (1996, ch. 1), showing both how a surplus might originate, and how it could be distributed according the static principle of the labour theory of value, or the dynamic principle of profits and investment. See also Nell (1998a, ch. 7).
9. McCloskey (2007) argued that Clark's theses on genetic influence, that 'the main failure of his hypothesis is, oddly, that a book filled with ingenious calculations [. . .] does not calculate enough. It doesn't ask or answer the crucial historical questions'. She went on to conclude that 'Clark's socio-neoDarwinianism, which he appears to have acquired from a recent article by some economic theorists, has as little to recommend it as history'.
10. Marx must surely be given some credit; mechanization and technological unemployment are found virtually everywhere in capitalism. But Malthus is more questionable. It is true that before 1800 there were few sustained increases in per capita income, but the appeal to widespread diminishing returns in agriculture is surely misguided. On the contrary, when new lands were opened up, they tended to be as fertile as existing lands; indeed, land in current use was often somewhat exhausted, so that the new lands were sometimes markedly superior (Nell, 1998a). What is true, however, is that it was very difficult to extend agriculture to new lands; they had to be cleared, roads of some kind had to be built, and the defence perimeter had to be extended. The new settlements would need churches and midwives, and, later, schools. In short, to make any significant extension of cultivation would require a 'big push', in the sense of the term used by development economists. (Or perhaps a 'broad push', rather than a big one – 'broad' because it encompasses social and political as well as economic development.) This, rather than diminishing returns, was the true obstacle.

11. The conditions for successful reproduction also define the limits or boundaries of the society and its economy. The centre is the realm of full reproduction; the periphery is defined by partial or occasional participation in the system.
12. The physiocrats, for example, understood the significance of reproduction very well. They based their approach to economic questions on developing a proper understanding of the processes of production, consumption and social regeneration. The social order they describe lies somewhere between feudalism and capitalism. It certainly has feudal elements, land-owning nobles and serfs, for example, but it also has capital earning profits. And, curiously, it has artisans in a manufacturing sector, which is held to be 'sterile' – that is, unable to earn profits. Manufactured goods sell for a price that covers the cost of inputs and artisan labour but earns nothing more. Food is grown that supports agricultural producers, and also supports the artisans in the towns. The surplus supports the landowners and the state. Food is grown using ploughs, harnesses, wagons, yokes and all the other necessary equipment which has to be made, repaired and replaced; artisans use tools to make tools; they also make furniture, clothing and cloth, and many kinds of luxury goods for the landowners. Peasants and artisans must be supported; tools, equipment and food will be used up regularly and must be re-produced and replaced. Rents must be paid and spent. All these goods must be circulated by money, and the way this works is shown in Quesnay's *Tableau Économique*. By understanding this correctly, they argue, the state could encourage improvements in these processes, levy taxes that would support the state without damaging them, and support the forces of competition that can be expected to lead to self-regulation in this system. For a recent account of Quesnay's *Tableau Économique*, see Cartelier (2009).
13. Identified and re-identified, in Strawson's sense (Strawson, 1959). Institutions are particulars in relation to social and economic variables, of which they are the bearers (for example, households, firms, banks in relation to consumption, investment and lending), but they cannot be identified and re-identified 'on their own', so to speak. They must be identified in relation to persons, and their acts.
14. Those appointed to or taking on roles will have to perform; that is, they will perform speech-acts – 'You are under arrest' is an act, not just a statement of a fact. Even more so is 'I pronounce you man and wife'. Speech-acts, especially what are called 'performative utterances', rest on strong presuppositions, among them that:

    - there must be generalized truth-telling, and specific correct intentions;
    - the actor must be appointed or confirmed in appropriate roles with the necessary powers, having had the required training or preparation;
    - the actor must be able or qualified to carry out the duties of the role correctly;
    - the situation, or setting, must be appropriate;
    - there must be an appropriate audience, with the required joint partners.

15. Why couldn't agents just be born knowing all they need to know; why couldn't the information be injected into their brains? This would require a high order of technology, something the world has not seen yet. To bring such a technology into being would imply a complex system of R&D, plus the medical manufacturing of the required devices, the training of doctors, etc. In other words, it would also imply a regular set of relationships, making up a complex social order.
16. We can distinguish at least a large and a small sense of 'institution'. Banks, corporations, labour unions, government agencies, schools, universities and hospitals are 'institutions' in the larger sense. Not walking on the grass, talking in a whisper in libraries, shaking hands, and saluting the flag are institutions in the small sense, which will not be considered here. For an account of transformational growth and institutional development see Errouaki (2003).
17. 'Making a promise' is an act that changes social relationships. This adds a dimension to the earlier discussion of the necessity of 'openness'. Can making promises, and

keeping them, be fully determined? Here is a potential counter-example: consider the act of making the only promise you will ever make, namely, 'never to keep a promise'. Could this be the determinate outcome of a model? If so, it must of course be clear just what you do: do you keep your only promise, or not? (Do you, the Liar, for once tell the truth after all, as you acknowledge your lies?) And you can't wriggle free by claiming you do both, since then you would do nothing. They cancel. But you acted, you made a promise, or an assertion. You had an intention to act. Promises are the basis of contract, of credit, of obligations and duties. If a deductivist model cannot predict promises or the keeping of promises, it cannot successfully model social behaviour.

18. Wittgenstein paid special attention to this set of problems.
19. TG finds this a point at which social enquiry differs importantly from the examination of natural systems. In social analysis it must always be possible to distinguish what an agent does from what happens to the agent. No such distinction has to exist in physics, nor in most chemistry. In biology, however, it begins to be important, and it is fundamental in the social sciences.
20. This is the textbook Robinson Crusoe. Defoe's Robinson Crusoe, as Steven Hymer (1980, ch. 12) points out, was quite a different story, a slave trader armed with guns, with which he took charge of Friday, and set up a colony.
21. 'Say's law' tells us that the level of money demand will always be just right, the Goldilocks level, not too much and not too little. But this is unbelievable. There is no mechanism that will ensure that the level of aggregate demand will always be correct, nor is it always clear just what the normal level of aggregate supply should be, since under the pressure of heavy demand the system might readily adapt to producing at a higher level – as has happened in wartime.
22. There appear to be some general features or regularities in capitalist development:

    - Rises in K/N and Y/N lead to RUM (rural-to-urban migration);
    - Rises in K/N and Y/N lead to rising G/Y;
    - A rise in the real wage leads to mechanization, so to a rise in K/N.

    (Note that all three of these 'uniformities' tend to hold in both boom times and in slumps. In a contraction, prices will fall, and marginal farms will fail, so RUM will increase. In a slump, Y will fall, but G will possibly even have to rise to cope with the social unrest, so G/Y will rise. In a slump, the factories and equipment will be idle, so it would be a good time to introduce new equipment and greater mechanization.)
23. What does it mean, and how is it different, to say that a demand curve 'exists' for a certain commodity? A textbook demand curve shows the response by households or consumers to the stimulus of a perceived price in a market, where this response is assumed to be governed by a set of established preferences. The curve might be said to 'exist' if the preferences do – although no one has yet been able to empirically estimate utility functions. But as Robert Heilbroner (1970, pp. 203–204) has argued, following Adolph Lowe (1965, pp. 34–9), the response may be based on special expectations rather than normal conditions. '[A] price rise, interpreted as a precursor to further price rises, will induce additional rather than decreased buying. A penalty for, say, hoarding, read as the sign of worse to come, may bring about a rush to hoard [. . .]' However, very reasonable estimates have been made for household expenditure functions, an obviously related but different concept (Stone, 1954a), and from these some kinds of demand functions can be inferred.
24. Many writers on methodology have spoken of the importance of 'vision' – Schumpeter (1954), for example, and more recently Heilbroner and Milberg (1995). 'Vision' is a broader, more metaphysical idea – a commitment to a general approach – but the picture brings the vision down to earth. For example, two contrasting visions/approaches, the neoclassical and the post-Keynesian, are boiled down to two contrasting pictures in

'The Revival of Political Economy' (Nell, 1972). Later we will consider the implications of this world having an economic aspect – that is, where persons are acting as economic agents conducting economic transactions. A 'transaction' means that a 'good' moves in economic space, from one 'owner' to another. Usually such transactions are carried out in a specially designated area of social space called a market. But there can be 'reproduction' without an economy.

APPENDIX: PLATO ON THE FORMATION OF A SOCIO-ECONOMIC SYSTEM

It is not a new idea that whatever exists in society does so because it is produced and maintained. Nor is it new to claim that this keeping or maintaining of the social order is the basis of society, and is fundamentally based on economic principles – specialization and the division of labour, comparative advantage, interdependence of production and exchange, for instance. And that these lead to economic institutions, such as marketplaces, shops and shopkeepers and currency. All of this can be found, for example, in Part II of Plato's *Republic*, especially in chs VI and VII. We also find the idea that agents are specially trained for their roles and that the educational system must regularly replace agents as they age and die. Agents specialize and the system is not only built upon the division of labour, so it seems clear – relying on Cornford's (1941) translation – that Plato understood the idea of comparative advantage.

Plato's *Republic* is the story of Socrates's arguments with the young elite of Athens, and how he develops in these arguments the idea of justice as it should be embodied in a republic (city-state). But the Socrates of the *Republic* is Plato's creation, many years after the death of the real-life Socrates. No doubt many of the arguments and the general position can be attributed to the real Socrates, but most commentators think the *Republic* is fundamentally the work of Plato, not just a recounting of what Socrates actually did (as opposed to the *Apology*, where Plato very carefully sticks to what actually was said and done by Socrates.)

Socrates, Cornford (1941, p. 53) says, 'starts to build up a social structure from its necessary rudiments'. He went on to argue that 'society is considered merely as an economic structure providing for the lowest of need' (ibid., p. 54). 'The purpose is to establish the principle of specialization or division of labor as dictated by Nature' – that is, 'according to natural aptitudes' (ibid., p. 59).

Quoting from the text of ch. VI (Cornford, 1941, p. 55), entitled 'The rudiments of social organization' [our interpretation in brackets]:

> [A] state [a socioeconomic system] comes into existence because no individual is self-sufficing; we all have many needs. [. . .] Having all these needs, we call in one another's help to satisfy our various requirements [. . .] when we have collected together a number of helpers and associates to live together in one place we call that settlement a state [. . .]
>
> [I]f one man gives another what he has to give in exchange for what he can get, it is because each finds that to do so is for his own advantage [. . .] [free exchange benefits both parties]

[This system] will owe its existence to our needs, the first and greatest need being the provision of food to keep us alive. Next we shall want a house; and thirdly such things as clothing.

We shall need at least one man to be a farmer, another a builder, and a third a weaver [. . .] shall we add a shoemaker and one or two more to provide for our personal wants?

Is each one of them to bring the product of his work into a common stock? Should our one farmer [. . .] provide food enough for four people and spend the whole of his working time in producing corn, so as to share with the rest; or should he take no notice of them and spend only a quarter of his time growing just enough corn for himself, divide the other three quarters of his time between building his house, weaving his clothes and making his shoes? [Specialization and interdependence give rise to exchange.]

[N]o two people are born exactly alike. There are innate differences which fit them for different occupations [. . .]

[A] man [will] do better [than] working at many trades [. . .] [by] keeping to one only [. . .]

[W]ork may be ruined, if you let the right time go by [. . .] more things will be produced and the work be more easily and better done, when every man is set free from all other occupations to do, at the right time, the one thing for which he is naturally fitted. [The worker specializes in doing what he is best at; this is the basic idea of comparative advantage.]

Socrates goes on to note that many more craftsmen will be needed

if the farmer is to have a good plough and spade and other tools, he will not make them himself. No more will the builder and the weaver and shoemaker make all the many implements they need.

He is alive to the needs for basics and raw materials,

[adding] [. . .] cowherds and shepherds to provide the farmers with oxen for the plough, and the builders as well as the farmers with draught-animals, and the weavers and shoemakers with wool and leather. [The interdependence of specialized production.]

But the rudimentary community will very likely need things it cannot make or grow; it will need imports, so

there will have to be still another set of people, to fetch what it needs from other countries [. . .] [and the state] must produce enough goods of the right kind for the foreigners [. . .] and if [. . .] [the merchants] are to do business overseas, we shall need quite a number of ship-owners and others who know about [. . .] trading [. . .]

And of course, this will all

mean having a market-place, and a currency to serve as a token for purposes of exchange. [. . .] [and the] city must include a class of shopkeepers

> who sit still in the market-place to buy and sell, in contrast to merchants who travel [. . .]

Finally, almost an afterthought,

> there are also the services of yet another class, who have the physical strength for heavy work, though on intellectual grounds they are hardly worth including [. . .] hired laborers [who] sell the use of their strength for wages.

The rudiments so presented are enough to provide for good health, so that

> they will live pleasantly together; and a prudent fear of poverty or war will keep them from begetting children beyond their means.

Socrates then goes on to consider '[. . .] the growth, not just of a state, but of a luxurious one'. (Cornford, 1941, ch. VII: 'The luxurious state')

> some people [. . .] will not be satisfied to live in [a] simple way; they must have couches and tables and furniture of all sorts, and delicacies, too, perfumes, unguents, courtesans, sweetmeats, all in plentiful variety. [. . .] we shall have to set going the arts of embroidery and painting, and collect rich materials, like gold and ivory. [. . .] [The state will now] be swollen up with a whole multitude of callings not ministering to any bare necessity: hunters and fishermen [. . .] artists in sculpture, painting, music; poets [. . .] actors, dancers, producers; makers of all sorts of household gear [. . .] everything for women's adornment; [. . .] more servants, children's nurses, [. . .] lady's maids, barbers, cooks, confectioners [. . .] a great quantity of sheep and cattle, too, if people are going to live on meat.
>
> . . .And with this manner of life, physicians will be in much greater request.

Clearly the society in question is gone and there are limits to what historians can reconstruct, but it would have been possible in principle to have established input–output tables, showing the methods of production, distinguishing basics and luxuries, as in Sraffa (1960). The distribution of income could have been analysed, along with the demand for unskilled labour. The relationship between categories of luxury consumption and distribution could have been studied, and the determinants of the balance of payments could have been examined; Plato provided the materials for a macroeconomic analysis.

There is more. Socrates seems to suggest, perhaps ironically, that with the development of luxury – cooks and confectioners, perfumes and unguents, living on meat – there will be a greater need for physicians. Is this true? What is cause and what is effect? This looks like a good multiple variable question. Define a set of variables that will proxy the development of luxury, and gather observations over an appropriate period. Then gather information about the demand for physicians' services, allowing for an appropriate lag. Finally, run regressions and try to answer whether or not more luxurious living leads to a greater need for physicians.

In short, Plato has not only provided us with a sketch of the basic reproduction system of a society, a foundation for macroeconomics; he has also suggested a plausible econometric problem.

Experience has shown that each of these three view-points, that of *statistics, economic theory, and mathematics, is a necessary, but not by itself a sufficient condition for a real understanding of the quantitative relations in modern economic life. It is the unification* of all three that is powerful. And it is this unification *that constitutes econometrics.*

Frisch (1933, p. 2, italics added)

Economists are not physical scientists. Despite the way we sometimes talk and write, we do not estimate parameters which define the truth. If we think carefully about what we are doing, we will emerge, I think, both more confident that much of applied econometrics is useful, despite its differences from physical science, and more ready to adopt our language and methods to reflect what we are actually doing. The result will be econometrics which is more scientific, if less superficially similar to statistical methods used in experimental sciences.

Sims (1982a, p. 335, italics added)

Ontology is prior to epistemology, and both ontology and epistemology are prior to methodology. That is, ontological statements have epistemological consequences. Holding a realistic ontology, we must – in order to avoid inconsistencies – transfer our realism to epistemology and methodology. The converse is not true. *From a successful methodology, we may not cogently infer that our underlying ontology and epistemology are correct.*

Volmer (1984, quoted by Dagum, 1986b, p.10; italics added)

No model is an economic model unless it includes criteria which make it applicable in principle.

Hollis and Nell (1975, p. 27)

When it comes to physical science, few people have problems with the idea that to study complex systems it is necessary to build simplified models. When we turn to social science, however, the whole issue of modelling begins to raise people's hackles.

The problem is that there is no alternative to models. We all think in simplified models, all the time. The sophisticated thing to do is not to pretend to stop, but to be self-conscious – to be aware that your models are maps rather than reality.

Krugman (1997, pp. 73–9; italics added)

6. The concept of the ‘model’ and the methodology of model building

INTRODUCTION

Recently, Spanos (2007, p. 2) argued:

> The philosophy of econometrics, as an integral part of economic modeling, is currently at its infancy, with most econometricians being highly skeptical about the value of philosophical/methodological discussions in empirical modeling.

Our argument so far claims to have established that there are lawlike relationships to be found, and that there have to be such relations. They have to exist because ‘socio-economic systems’ exist (we live in them; economics and econometrics study them), and the existence of such systems is brought about and maintained by the regular patterns of activity that reproduce the positions and relationships of the system. The socio-economic system is the ultimate source of regular economic data, the basic ‘data generating mechanism’; it provides and supports the social framework in which all economic activity takes place. The fact that such a framework exists implies that there must be regularities in economic activity.

But the fact that regularities exist does not necessarily mean that it will be easy to discover them. For one thing, the actual or, to use a term we shall meet later, the ‘phenomenal’, socio-economic system – the real world – presents a buzz of confusion, irrelevancy and extraneous material intermixed with the essential and important data. This has to be sorted through, so that the key relationships and variables will stand out.

How are we to do this? How will we know we have found the correct items? We have provided an ontological argument; now we have to show how we can come to identify these regularities, and learn how the system works. We face an epistemological problem: how can we know? How can we be sure? And this in turn will require a methodology. So here we propose a methodological framework, which will be defined as the methodological triangle-circle (MTC) diagram. It is designed to illuminate some of the less emphasized methodological insights of the econometrics debate, and, most significantly, to serve as a practical instrument to judge the degree of operationality of specific models. The framework should help us to answer the following question: to what degree may we expect our model to fulfil its objective; that is, to work.

The MTC diagram (see p. 171) is a dialectical representation of the relationship between theory (or coherence), applicability (or relevance) and measurement (or quantification). The diagram relates these three, and it is 'dialectical' because the representation allows us to see the reciprocal interactions between these. We also show that our picture of these three concepts is closely analogous to Volmer's triangle of ontology, epistemology and methodology (see p. 170), and further that we can find a very similar vision among the founders of econometrics.

The idea of a triangle diagram (TD) was first introduced by Bonnafous (1972) (see p. 181).[1] This may well have been the first book on econometric methodology. Although Bonnafous's examination of the Cowles work does not offer a fundamental reinterpretation of the history of structural econometrics, his investigation of the statistical foundations of textbook econometrics does uncover some important features of econometric methodology. Bonnafous argued that textbook econometrics is founded entirely on the logico-mathematical structure of 'the model' in relation to the general theory of estimation.

Bonnafous (1972) applied the TD mainly to analyse specific problems, namely the methodological difficulties of the statistical foundations of textbook econometrics.[2] He concluded that textbook econometrics responds to the problems with the operationality of 'the model' by favouring the requirement of what he calls 'consistency'. In so doing, it authorizes the measurement of the parameters of the model, but without, unfortunately, providing any other guarantee of the 'relevance' of the hypotheses in the absence of results that would invalidate those hypotheses in a distinctive manner. He used the Klein (1950) model throughout the book in order to illustrate how the various concepts and procedures related to specification and estimation are utilized in practice. Klein (1950) models I–III exemplified the new Cowles contributions and a programmatic modelling approach, which departed from Tinbergen's modelling style.[3] Klein's models could therefore be said to constitute an illustration of those results. The models were expected to demonstrate that the structural approach would revolutionize economic science, as anticipated by some of the Cowles people.[4]

The proposed methodological triangle-circle (MTC) diagram framework is an extension of Bonnafous's (1972) triangle diagram (TD). But instead of representing the concepts (theory–coherence, applicability–relevance and measurement–quantification) as the points of the triangle (as Bonnafous does), we exhibit each as a circle, the three circles standing in a broadly triangular relationship. The MTC is also an attempt to further develop his argument so as to illuminate the importance of certain less emphasized methodological insights of structural econometrics, as stated above.

A philosophical version of a triangle diagram (TD) to chart, evaluate and analyse the scientific foundations of a discipline was suggested by Volmer (1984).[5] His triangle shows the interactions between ontology, epistemology and methodology, and was later elaborated by Dagum (1986b). Volmer advanced an illuminating statement that is relevant to the rethinking of the scientific foundations of a structural econometrics. He argued that from a successful methodology we may not cogently infer that our underlying ontology and epistemology are correct.[6] According to Volmer, 'ontology is prior to epistemology, and both ontology and epistemology are prior to methodology. That is, ontological statements have epistemological consequences. Holding a realistic ontology, we must – in order to avoid inconsistencies –transfer our realism to epistemology and methodology. The converse is not true' (Volmer, 1984, quoted by Dagum, 1986b, pp. 10–11). Dagum (ibid.) argued that Volmer cogently objected to the coexistence of ontological realism and epistemological idealism.[7] This thesis is an important point to be developed later in this book; conceptual analysis is essential to developing coherent theory, and fieldwork both tells us what there is and provides clues to how to measure things. We shall explore both conceptual analysis and fieldwork in Chapter 10; measurement and statistics will occupy us in Chapter 8.

In Chapter 7, the MTC diagram will be applied to the debate over the methodological foundations of structural econometrics to illustrate the different weights and roles accorded to theory–coherence, applicability–relevance (or realism) and measurement–quantification. We shall represent the weights by circles of various sizes, and show how they relate by positioning them in relation to each other. This will provide a clear picture of the different approaches, helping to clarify relationships that many readers have often found puzzling. Our taxonomy is easily represented by the MTC diagram. Other modelling issues, such as the role of statistical models, will also be explored using the MTC diagram; different approaches generate different-looking MTC diagrams. We hope the general MTC framework will provide useful insights for readers struggling with the general language and structure of models.

The argument here draws heavily on Bonnafous (1972; 1989). But it integrates and extends the work of several authors (Bonnafous, 1972, 1989; Dagum, 1986a; 1986b; Errouaki, 1989; 1990; Hollis and Nell, 1975; Nell, 1998a; 2004; Nell and Errouaki, 2006a; Volmer, 1984) into a framework that is easily represented by the MTC diagram. Before presenting the

MTC diagram, however, and using it to explore the problematics of the model, it will first be useful situate 'the model' on the economic scene by locating its introduction into the broader historical trajectory of scientific advancement.[8] Indeed, the elaboration of 'the model' may be considered a clear theoretical shift in the history of science itself.

6.1 THE MODEL IN HISTORICAL CONTEXT

The notion of models is familiar to scientists. The language of economics is the language of models. Economists think and communicate in this language. A clear understanding of this language could enhance our comprehension of the benefits and limits of formalization. Indeed, Davis (2003, p. 2) argued:

> Graduate students are bombarded with many types of models: theoretical models, econometric models, programming models, calibration models, statistical models, empirical models, simulation models, et cetera. Students are immersed in the technical details of manipulation and implementation of specific models. The student must first be able to apply a model successfully within the classroom before he can successfully apply it outside. However, this immersion can leave the student seeing the trees, but not the forest. That is, the student may understand the intricacies of specific models in isolation but may not understand the commonalities that exist across different models.

He (ibid., p. 2) continues:

> Often economists disagree simply because they are using the term 'model' in different contexts. More importantly, with a general knowledge of the structure of models, the student is less likely to either overstate or understate his contribution to a research programme. It improves the ability to easily recognize important contributions. By having a better understanding of the general language and structure of models, the student can more easily identify which ideas are most relevant for importation (exportation) from (to) the economics literature. Simply stated, a better understanding of the general language and structure of models enhances the likelihood of professional success.

Bonnafous (1989) pointed out that the word 'model' itself expresses a kind of renunciation of reproducing the reality. He considered it at once the vaccine against the naivety of absolute truth, and the antidote to essentialism.[9] Furthermore, Ullmo (1969) pointed out that it is the favoured theoretical instrument of a conception of economic knowledge that admits the idea of varying stages of knowledge.[10]

The appearance of the model on the economic scene, replacing 'theory as the workhorse of the discipline', was a direct response to a methodological necessity. Quesnay (1759) appears to have been the first economist to set down in some details the rudiments of an economic model.[11] Indeed, Bodkin et al. (1991, p. 3) argued:

> Francois Quesnay's *Tableau Économique* was a major accomplishment for his time and it is reasonable to regard this construct as the first stylized macro-economic model. Moreover, the tableau was both quantitative and dynamic, designed as it was to indicate cyclical (or perhaps secular) improvements or decay.

Quesnay's contribution was not the outcome of the 'Aristotelian theoretico-empirical' approach to model building, but rather an 'analogic mode of inquiry', whereby his famous 'tableau économique' evolved from a biological analogy – that is, the blood circulation in human beings.

Before the concept of the model emerged, advances in the field of economics were generally brought about by developing theory. This, however, was a highly problematical undertaking. Traditionally, in terms of methodology, science had accorded theory the status of a working truth throughout experimental phases until the contrary had been demonstrated, notwithstanding the fact that theory is by its very definition an abstraction, a reduction and, necessarily, to some extent, a distortion of reality. Now, of course, this working assumption that a theory is true until proven otherwise created a procedural situation characterized by an inherent ambivalence that on a certain level might be said to blur the lines of the playing field on which the experiment is conducted.

However, this ambivalent situation, in which a potential truth is treated as a truth, posed no great problems in the case of those scientific disciplines in which the successful repetition of experiments can be carried out. However, in the case of economics, a field whose data appear to be based on events each of which is unique in terms of time and space, and therefore unrepeatable, theories, unlike those in chemistry for example, cannot rely on a process of repetition for validation. In view of this, theory's status as a potential truth might reasonably be said to carry far less weight. But remember, Chapter 5 pointed out that the economy rests on a socio-economic system that reproduces itself; which is repetition, of course. Unfortunately that system, in turn, is embedded in a socio-political and cultural matrix that is continually changing, posing the question of how to separate the two. From a scientific point of view, then, using models as an approach to understanding the economy might be said to face a major problem. We will return to this.

The most important purposes of scientific model building can be synthesized by the phrases 'model for a theory' and 'model as a theory'. The former specifies a model within the framework of an established theory, whereas the latter embodies a theory, and thus model and theory comprise the scientific knowledge (see Dagum, 1986b). Lucas (1980, p. 697) argued:

> A 'theory' is not a collection of assertions about the behaviour of the actual economy but rather an explicit set of instructions for building a parallel or analogue system – a mechanical, imitation economy. A 'good' model, from this point of view, will not be exactly more 'real' than a poor one, but will provide better imitations. Of course, what one means by 'better imitation' will depend on the particular questions to which one wishes answers.

Boumans (2001, p. 439) pointed out that Lucas's approach was not to aim for more realism in the models, but on the contrary, to advocate 'superficiality'.

Strongly related to the phrase 'model as a theory' is the theoretical empiricist approach to scientific model building. It purports to find a new theory, a new scientific explanation, as the outcome of a process of interaction among observations, ideas and reason, which distinguishes theoretical empiricism from both empiricism and idealism. For example, Leontief's input–output model, Haavelmo's structural econometrics, Klein's methodological structuralism and Nell's methodological institutionalism belong to this philosophy of science approach.

The development of econometrics was catalysed by a desire to reinforce the scientific component of economics (Morgan, 1990a). For, without empirical tools, even very simple economic problems, for instance how to establish that some economic magnitude is influenced by a certain causal factor, might be difficult; and the question of how strong that influence is would be unanswerable. Little wonder, then, that the approach using the model empirically rapidly became the preferred instrument of economists with an interest in applied problems, displacing theorizing.

Morgan (1988), like Davis (2000), whom we discuss later, has contended that empirical models are the bridge between theory and data, but notes that this requires a two-way matching between theory and data. Boumans's (1999) account further examines the complexity of theory–model–data relationships. He uses examples from business cycle theories to show that models are built by integrating many ingredients apart from theory and data. As a result, even though it might be true that all econometric methodologies might invoke some components to deal with the gap between theory and data, not all fall into Davis's theory-first account.

Recently, the study of models has placed them on centre stage in the philosophy of science. As Suppe (2000, p. 109) puts it: 'Today, models are the main vehicle of scientific knowledge'. Suppe's view is similar to Morgan and Morrison's (1999), in which they argue that models are 'autonomous agents' in the sense that they have the merit of being not entirely dependent on theory or data. For a similar reason, Giere (2004) proposes the term 'model-based approach'. He suggests using this term to cover all those philosophical theories that focus on models.

Scientists at that juncture were also becoming increasingly conscious of the necessity to elaborate methodologies for dealing with and attempting to comprehend and quantify uncertainty.

We argued in Chapter 2 that the theory of probabilities had provided an approach to randomness and stochasticism as it resulted from approximations, and acceptable methodologies had already stemmed from that significant step forward. The elaboration of the econometric model, then, might be seen as the product of these advances, of this quite neat and scientifically acceptable relativization of truth and reality, and these fresh and brilliant approaches to dealing with stochasticism.

Quite naturally, then, the econometric model, formulated along guidelines suited to the exigencies of this 'new science', gradually replaced its competitor, ad hoc and a priori theory, as the favoured instrument of economists, and might be said to be a partner in the revised approach to uncertainty in the field. Engendered by demands for empirical clarity and a new scientific approach, then, the model has provided a prime vehicle for further advancement, resulting in a most interesting symbiotic relationship. It has offered economists a medium in which a given and relativized 'reality' can be broken down into cross-sectional images and reconstituted as a simplified picture, while adhering to science's relativized definition of 'truth'. And it has also offered the discipline an impressively versatile tool with a four-fold capacity. It may serve as an instrument of analysis, of prediction, and of simulation; and, additionally, as a framework for the organization of knowledge. It might be labelled, in sum, the 'multipurpose tool' of economics – one that, moreover, has enabled the discipline to advance in practical ways that arguably would have been unachievable without it.

6.2 THE MODEL'S DEFINITION

The definition of the model proposed by Marschak (1953, p. 1) is 'a simplified construction destined to explain reality or to act on it'. Haavelmo (1944) argued that a simplified construction was necessary at certain moments of economic investigation. It is surely necessary to proceed to form reductionist and abstract cross-sectional views of what we believe we know of multidimensional reality (not only economic dimensions, but also social, political, cultural, and so on), but then these almost fictitious abstractions must be pulled together into a model that recreates an image of the state of things in the relevant dimensions. The model concretizes this retaking of a part of the multiple dimensions, where controlled experimentation, if it were possible, would freeze the remainder, so nothing in those dimensions could affect the analysis. Nell (1998a) argued that the model is therefore not only the multi-purpose tool of economics – an instrument of analysis, of prediction, of simulation, and of organization – but it is also an indispensable tool of economic investigation.

We begin with a brief look at some other traditional definitions of the model. Frisch (1933, p. 2) terms the model:

> A synthetic construction in which statistics, the assembly of observable facts, theory, the research of explanations of reality, and mathematics, the rigorous tool for the integration of facts and theory, are each constantly in service of the other.

A rigorous definition of the model in economics is proposed by Dagum (1986b, p. 31):

> An economic model is an idealized and simplified formal representation by means of a theoretico-empirical set of singular scientific statements concerning the observed characteristics of regularity and stability of a given field of research.

Morgan and Morrison (1999)[12] have shown that models in economics function as if they were physical instruments. According to these authors, models function as such because they involve some form of representation. This representative power enables us to learn something about what the model represents. In other words, Morgan and Morrison (1999, p. 12; quoted by Boumans, 2001, p. 431) treat models as quasi-empirical objects:

> we do not learn much from looking at a model – we learn more from building the model and manipulating it. Just as one needs to use or observe the use of a hammer in order to really understand its function; similarly, models have to be used before they will give up their secrets. In this sense, they have the quality of a technology – the power of the model only becomes apparent in the context of its use.

Offering a more detailed perspective, sharply distinguishing formal relationships and conceptual interpretation, the 'model' has been defined by Nell (1998a, p. 113) in the following way:

> A model can be said to have two aspects, or to be composed of two kinds of elements. On the one hand, there is the purely formal part, and on the other, there is the interpretation which clothes the formal skeleton with meaning. The

> formal part of a model consists of an algorithm in some formal calculus. Two algorithms commonly used in economics are, first, maximizing a function of many variables in the differential calculus (usually subject to some constraints), and secondly, determining the existence of a solution to a set of linear equations, namely that the rank of the augmented matrix equal the rank of the coefficient matrix. Each of these formal models is purely abstract and must be given an interpretation; it must be applied to a subject matter. This requires making its variables and relations represent certain concepts. Thus one variable will stand for 'price', another for 'quantity demanded', and so on. By this route the maximization algorithm, for example, can be made the basis of the model of demand theory, in one interpretation, and supply theory in another. The formal side of the model thus provides the method for the determination of the unknowns in terms of the given conditions, while the substantive interpretation applies this method at hand.

This idea of model needs to be related to 'theory'; a theory may be exemplified by several somewhat different models. Indeed we might say that a theory may be considered the general principle(s) plus the main related models, together with the conditions for the application of each.

Two important ideas are present here, which we will develop further. One part of the model is its formal machinery, maximizing, solving equations, and so on; another is applying that machinery to the world, defining the variables and relationships as representing aspects of reality. This can be done in several ways, and in other work Nell distinguishes two kinds of models, structural and behavioural. Nell (1998a) argued that structural models show how institutions and systems work. They don't refer to agents or to what the agents know or want, or how or why they calculate. They outline a set of rules, and show the outcomes of those rules and relationships interacting when some variable is set in motion or some parameter changes. The model does not refer to agents; the variables are not ascribed to agents. (By contrast, the variable 'the quantity demanded' is the household's demand – it is ascribed to the household.) But a structural model – an input–output model, or a Sraffa model, or some kinds of growth models – describes a system, or a set of institutions. In Hollis and Nell (1975), as indicated in Chapter 1, prescriptive or programming models are also discussed. These are models designed to analyse a situation and suggest the best course of action in the circumstances. This is the basis of management consulting.

The idea of a model also offers economists the possibility of a probabilistic quantification of the uncertainty arising from an absence of information (concerning data and variables) or from a deliberate simplification of same through the use of random residual terms and confidence intervals.

For now, we work with the short definition (similar to Marschak's) suggested by Guitton (1964, p. 484, quoted by Bonnafous, 1972, p. 2, our translation) who wrote that a model 'is a simplified construction intended to explain reality or act upon it'. By 'construction' here we will mean, following Nell (1998a), that the model consists of two parts – formal and interpretative – to which we shall add the purpose or objective. Further, we shall add the article 'a' before 'reality', for just as it is necessary to relativize the concept of truth in order to work with it according to acceptable scientific procedures, so it is equally requisite to relativize the concept of reality. Indeed, Bonnafous (1972, ch. 1) argued that 'a' relativizes the reality being studied by designating a particular segment of it, which will be further reduced by a process of statistical selection and expression.

The two functions of the model are 'to explain' and 'to act'. We recall here Marschak (1953, p. 1) when he stated that 'knowledge is useful if it helps to make the best decisions'. A Spanish philosopher, Juan Luis Vives (1492–1540) observed a similar thing in the sixteenth century when he pointed out that 'knowledge is of value only when it is put to use'.

These two functions were not seen in the 1930s as belonging to distinct realms by the founders, but were, rather, treated together. Econometricians since then, however, have perceived the necessity of clearly distinguishing the approaches, perspectives and procedures of these functions. These functions are not only fundamentally different, but belong to two distinct realms in econometrics: the exploratory and the applied.[13]

6.3 THE COMPONENTS OF THE MODEL

These include: subject, object, and objective; and are discussed below.

The object of a model – in fact, of any investigation in general – is obtained from a perceived reality that cannot be viewed as completely independent of its own subject/author, who has defined the object and selected the information by which he or she wished to understand it, with a definite objective in mind.

6.3.1 Subject

Initially, the model is of course conceived within the mind of its subject or author, who has a rational purpose in view in conceiving it. The subject, moreover, is not only the originator of the model, but is also the selector and landscaper of its reality, the architect of its methodological framework in the real world, and its essential driving force, or motor. There is clearly, then, an ongoing dynamic between the author and the model as its development progresses. For the author is not only the creator and the landscaper of the model, but also represents, in very practical terms, one of the essential physical tools of its operationality – that is, its one initial moving part; it might even be said that the author is its physical link with the real world. Moreover, if the author might be fairly said to drive the model's progress (which would clearly come to a sudden halt if he were hit by a truck), the converse might also be purported to be true, for having designed its objective, framework, variables and game plan, the author is then obliged to obey the model's own dictates as he works through the process, just as it might be said that the structure of the model is obliged by its definition to hold the author to the design originally imposed on it. What we have here, then, might be viewed as an interesting synergetic situation in which author and model play a dual role as master and servant to each other.

Once the model's methodology is set in motion, what, if any, are the causal implications of such a dynamic? Pure empiricists would be obliged by the fundamentalist 'hard core' of their school to maintain that there are no implications whatsoever. We would suggest that empiricists will be hard pushed to prove their 'no implications' position when the human is the subject/author, and, on the other hand, as in the case of the econometric model, also the object. On the contrary, it is precisely this circumstance – that economists building models are also in their daily lives economic agents – that makes fieldwork both possible and indispensable in getting to the true definitions of relevant variables.

6.3.2 Object

The purpose of a model, as expressed in our original definition, is 'to explain a reality or act upon it'. It is that reality, as circumscribed by an initial analysis, that becomes the model's object.

When one attempts to build and use a model, one has, of course, a specific reality in mind. But even a very specific reality can be viewed as composed of innumerable variables whose interactions may be too complex to quantify fully. So the elements selected by the author from these innumerable variables that make up the segment of reality that is the model's object must now be defined as 'a collection of fabricated and simplified objects'. Bonnafous (1972, p. 10) argued that this interpretation was clearly demonstrated by Bachelard (1968) in the case of physics and by Canguilhem (1965) for the natural sciences.[14]

The reality to which the model refers, then, is made up of what we believe we know about the object of the model – that is, a certain accumulation of bits of knowledge, among which is included a set of organized pieces of information, namely the data. Because it is a selection of pieces of information, however – not wholly arbitrary, but not determinate either – it not only represents a reduction of reality, but to some extent a distortion. Hence it is a far trickier reality to deal with, to forecast, or to prove things about, than, say, the cat which represents the reality of the veterinarian, or the tub of water that Archimedes leapt out of.

An important distinction may be drawn between the object of models in the applied realm, as above, and the object of models in the explanatory realm, which, as we shall see as we examine the objective of the model, might be said to be theory itself.

6.3.3 Objective

Davis (2005b, p. 139) argued that 'a model's objective is the purpose for building the model (e.g. forecasting, hypothesis testing, and control)'. The features or dimensions of a model 'are components that can be generated by the model (e.g. the one step ahead forecast, the conditional mean function, the conditional variance function)' (ibid.). A model attribute is 'a measurable representation of a dimension such as parameter estimates' (ibid.). Obviously, as Davis pointed out, 'multiple attributes of the model may provide information on a single dimension (e.g. parameter estimates, t-statistics, F-statistics on the conditional mean) or a single attribute may provide information on multiple dimensions (e.g. Durbin-Watson statistics on the conditional mean and variance)' (ibid.).

For the purposes of this book, the term 'objective' might be defined as the purpose, function or end for which an econometric model is conceived. A model might be designed, for example, to formulate an explanation for, or to define, or, again, to undertake an action within the framework of the reality that has been designated as its object. The objective of the model may be classified as belonging to one of two categories: the 'exploratory realm', comprising models that are designed to explain; or the 'applied realm', comprising models that are designed to support action or policy.[15] These domains may be seen as distinct, both in methodological terms and in terms of the philosophy of science. Now we discuss the objectives of models in each of these categories.

The objective of models of the 'exploratory' realm are abstract in nature, and concern the investigation and analysis of economic theory itself (the latter might indeed be called its object). The approach and methodological framework of this exploratory/explanatory objective is mathematical in nature, comprising a rigorous body of postulates, theorems and symbols. The general objective of models belonging to this category, as we have mentioned, is to 'explain' statistical objects and their relations. Within this category, the particular goal of a model may be as general as the elaboration of the basic mechanisms that create the phenomenon manifested in the object, or as precise and specific as a study of the effects of a new tax system.

The second or 'applied' realm is the domain of practical studies on the nature of economic behaviour in the 'real world'. The framework and approach here are those of empirical science. Econometricians who specialize in this field are concerned with the specification and estimation of econometric models. These models of the applied realm, which are sometimes called 'decision models', are designed to act on a reality and, more specifically, may be said to have a forecasting objective based on a variety of possible action hypotheses selected according to forecasts of what may transpire.

The purpose of a model belonging to the explanatory domain, whose objective is analytical, is to concretely reassemble pertinent elements of the multidimensional reality (which is its object), each element of which the fictive controlled experiment must freeze-frame in turn, so as to see how it all works.

Some models in both realms may aim to establish or analyse causality. Causality is a concept that cannot be banished or ignored – the agents of a socio-economic system must know (precisely or probabilistically) what their actions will bring about, so observers are entitled to make the same judgements. But causality cannot be inferred from the statistics alone, nor does it show directly in the equations. It has to be inferred from an understanding of the behaviour, and from the way the system transmits effects. In many cases, the model will show probabilistically defined causal decisions, which may be used to interpret a statistical dependence or to draw from a set of stimulus-response type functions some specific consequences that can be compared with observations. This it accomplishes in two ways: first, by taking the relevant components of the reality defined above, and breaking them down into a series of cross-sectional images, abstract and reductive. Second, by providing a framework in which the components thus formed may be arranged in such a way that the reality in its multiple dimensions is reconstituted in a simplified picture.

Models of the exploratory or abstract realm may in fact be imported into the applied realm and accorded a function there. But, once transplanted, these models may not carry the same weight in terms of validity in this domain, for the chief concern here is not whether abstract concepts and calculations are coherent with one another and proceed logically from a given set of premises to a correct mathematical conclusion, but, rather, whether a particular mathematical or statistical statement corresponds to observations about the real world. In this sense, these transported models might be likened to products that have been used in a manner which runs contrary to the manufacturer's instructions so that the guarantee no longer holds.

6.4 THE CONCEPTUAL FRAMEWORK

The MTC framework is presented, subject by subject, in the same order that Bonnafous uses to present his arguments. Our presentation draws heavily on Bonnafous. We have chosen to paraphrase and sometimes reinterpret his main arguments instead of translating long quotes and adding comments. We will synthesize his ideas and highlight his most significant contributions, while integrating them with our own.

A 'simplified construction' is unlikely to be a precise representation of reality. But this need not present a problem. Science has long been freed from the myth that it could ever reveal absolute truth. Ullmo (1969, p. 312, quoted by Bonnafous, 1972, p. 4, our translation) expressed it best: 'A truth from our representations of a phenomenon corresponds to the real variable of successive stages in scientific advancement; these are the essence of the scientific process'. We move towards the truth by successive approximations; our scientific statements become clearer, more refined, more complete, and they fit together into better models. The fact that they are incomplete, unrefined and so on does not contradict our solution to the problem of induction. That argument demonstrated that we were justified in formulating scientific relationships between scientific variables; these relationships can be expected to hold over time and in various places. They can be supported and amplified, and improved, with further evidence, but they will never be complete or the final word.

Bonnafous (1972, p. 4) argued that the quantitative economic models are developed from these approximations. In more general terms, simplification is a necessary part of thought, because it results from abstraction. We argued in the Introduction that in economics, as in any field dealing with the real world, the real issue is not whether simplification takes place, but how it takes place without losing its relationship to real-world phenomena. But, as Nell (1998a) pointed out, 'abstraction is not idealization'.

It is important to clarify and emphasize the difference between abstraction and idealization. Nell (1998a) argued that abstraction is a process of focusing on particular aspects of some (concrete) phenomenon, with the aim of individuating or picking out particular features while ignoring others. Notably, it is not the case that the existence of the neglected features is denied; rather, they are (momentarily) left out of focus and relegated to the periphery of our attention. Abstraction, then, is a matter of bracketing features of the phenomenon under investigation rather than of denying their existence. Abstract reasoning makes claims that it is hoped do not hinge on the neglected features of objects to which the reasoning is applied. By contrast, idealization involves the ascription of features to an object that it does not in fact possess – that is, features that are false when predicated of it. Thus theorists who use idealization invoke fictions, objects that exist only in the realm of ideas. Furthermore, regarding the distinction between abstraction and idealization, it is important to highlight that critical realism (CR) overlaps here with transformational growth (TG). Indeed, CR points out that, just because it is impossible to comprehend the entirety of complex aspects of the socio-economic world in one go, it does not follow that theories and models are justified in employing descriptively false assumptions (Lawson, 1997; O'Neill, 1994; Runde, 1996).

From this point of view, models are comparable to theory. Bonnafous (1972, pp. 4–5) argued that:

> what appears to set the two apart is the fact that employing a model is a conscious resignation to the fact that one is making an approximation. It is an approximation in relation to the 'real variable' of the state of knowledge, while new theory is forwarded in the hopes that it becomes 'one of the successive stages in the advancement of science'.

This is why we sometimes speak of models in the inexact sciences, but of theories in the exact sciences (the Bohr model of the atom would not be a counter-example). In addition, as Bonnafous (ibid.) observed:

> the theory of probabilities has given science the means to understand uncertainty as it results from approximations. Forecasts provided by statistical analysis have given a considerable boost to a wide variety of disciplines, such as atomic physics, genetics and the social sciences. Econometrics therefore offers a probabilistic quantification of the uncertainty arising from our ignorance or our deliberate simplification through the use of random, residual terms and confidence intervals.

In the scientific method, therefore, the presence of approximations is not a reason to rule out the use of a quantitative model. Moreover, within the recent history of economic thought the quantitative model appears to be the preferred research instrument for seeking to put some order into the mystery and complexity of outward appearances. It is without a doubt, as Bonnafous (ibid.) observed, 'the essence of the scientific method'.

The model's purpose is defined in our initial definition: this instrument is built 'to explain reality or act upon it'. It is clear that when one attempts to build and use a model, one has a specific reality in mind, as well as identifiable explanations or actions.

According to Bonnafous (1972, p. 5) 'reality, as circumscribed in an initial analysis, becomes the model's object. Its objective is to formulate an explanation or define an action'. This can be very general: it can, for example, consist of explaining the basic mechanisms that create the phenomenon found in the object. It can also be more precise, such as a study of the effects of a new tax system.

Bonnafous (ibid., p. 8) argued that:

> When the objective is simply one of description, we do not really have a 'model' but rather a current statistical technique: a statistically observed linear relationship between two magnitudes is only a model when a role is attributed to the magnitudes. If one is found to be determinant of the other, the relationship is an explanatory model; if a projection of one can be calculated from a projection of the other, it can be used as a forecasting model. Description is clearly a different level of utility as compared to explaining or forecasting, and a statistical finding is fundamentally different from a model.

Defining the object and the objective of a model amounts to specifying how we expect it to operate. Here, we shall follow Bonnafous (1972, ch. 1) and call the model's ability to operate as expected its 'operationality'. A model becomes an instrument when it is operational. Bonnafous (ibid., p. 7) observed that 'since this instrument is considered scientific, operationality requires a discipline; it must satisfy certain requirements'. We consider the study of these requirements as being fundamental to an examination of the debate on the methodological foundations of structural econometrics.

6.4.1 The Requirements for Operationality of the Model

Bonnafous (1972, pp. 7–8) defined a model's operationality quite simply as its ability to operate as expected. Following Bonnafous, our first step, then, is to identify these essential requirements for operationality and, this done, to engage in a study of their interrelationships. Scientific practice has established that these requirements are three in number and there is also general agreement concerning their essential attributes.

Bonnafous (1972, p. 7) argued that the concepts to be used in a study vary to some extent both in their content and designation, leaving a certain freedom for authors in selecting our terminology.[16] The terms we have selected are: theory–coherence, applicability–relevance and measurement–quantification.[17]

6.4.1.1 Theory–coherence

In the most general sense, a model's coherence may be defined as its compliance with the principle of non-contradiction. This coherence requirement is further subdivided into two categories: first, the absence of internal conflicts, that is, conflicts inherent in the model's design; and second, the absence of internal/external contradictions with regard to the model's objective and the technical capacity of its contents, methodology and structure to meet it. More practically, however, the coherence requirement means that the model presents a working structure.

6.4.1.2 Applicability–relevance

The model's applicability–relevance, according to our definition, represents the link or nexus between its logical and mathematical structure (particularly in its digital form) and the true nature of its object (in so far as it can be logically perceived and understood). The mathematical terms and relations must be given conceptual meanings, meanings that can be found in the world. It cannot be assumed that plausible sounding mathematical relationships actually refer to appropriate elements of the world, or even if they do, that they do so accurately. The mathematical precision may be wholly spurious. Even if the variables refer to real-world elements that can be measured appropriately, the mathematical structure of the model may be distorted. The model's mathematical structure refers to the arrangement of all its equations and, when they exist, its probability distributions. Thus, even if the variables are well-defined, the real world may exhibit relationships that are non-linear and sometimes unreliable, while the model presents straightforward linear equations. Finally, even if all these are correct, the model's logical structure may be defective; this refers to the distribution of respective roles (cause and effect) among its variables and in all the other relationships. It is assumed that excessive issue of money causes inflation; the model is set up so that it is easy to solve for prices as a function of money. In the actual circumstances, price increases generate demand for additional loans, and loans create money. Causality runs the other way, but it is difficult to solve the model for money as a function of prices.

6.4.1.3 Measurement–quantification

This requirement may be defined as the potential of all the magnitudes present in a model to be estimated. This requirement refers as much to the model's variables, for which a sample must be available or attainable, as it does to the parameters that must be estimated based on the sample of variables. The variables must be defined in such a way as to be 'countable', and there must exist procedures for measuring and collecting statistics.

So, following Bonnafous (1972, p. 8), the three conditions:

1. are essential to establishing a model's operationality;
2. are sufficient when taken together; but
3. contain latent mutual contradictions, creating problems with the operationality of the model.

6.4.2 Three Conditions Required for Operationality of the Model

The first requirement of theory–coherence is that of not carrying internal contradictions. If there are any such, the mathematical formalism, and by extension the 'simplified construction' of the model, will have no meaning. But non-contradiction is too weak a condition; the theory and its conceptual framework must be adequate for the task. Consistency must extend to the model's objective, also clearly a necessary condition for operationality. How can a model be operational if its mathematical formalism does not allow for the desired calculations? Bonnafous (ibid., p. 9) observed:

> How could a model explain joint variations in four variables as a function of other variables (assuming this is its objective) if it only has three equations? This means that the condition of consistency is tied to considerations outside the model, since, if it concerns the compatibility required between the model's objective and its mathematical formalism, the objective must lie within the field of mathematical knowledge.

The model cannot rest on calculations or demonstrations that cannot be done. We argued earlier that, in as much as a model has been developed to 'explain the real world or act on it', it must be an approximation of this reality; it must be relevant. This is clearly a condition that requires explanation in conceptual terms; it concerns the compatibility of the formalism determined by the model's consistency and the concepts that properly describe its objective.

Bonnafous (1972, p. 10) argued that it must be noted that:

> The object of a model, or of an investigation in general, is obtained from a perceived reality that is not independent of either the subject (who has defined the object and selected the information by which he or she wants to understand it) or the state of knowledge (which largely determines the nature and quality of the information). It is then clear that the experimental event is an artefact, and the object is in fact a collection of fabricated objects.

Perhaps are the most striking example is the relationship between magnitudes of national accounting and Keynesian analysis.

Relevance appears to be a necessary condition – the model has to reflect the reality it is trying to explain – but it is also a condition that has to be verifiable. Without the possibility of some form of verification, the model should not be considered a scientific instrument, and we cannot evaluate its operational possibilities.

The econometric model comprises an essential component of theory. However, just as the concepts of truth and reality have of late been relativized for the purposes of operationality, so has the concept of theory. Science tends now to define theories not as 'truths' but, in a paradoxical about-face, as almost the reverse – that is, as a series of 'revisions' according to which each successive theory constitutes the replacement of an inferior theory with a 'less inferior theory', moving along the trajectory of scientific progress. Each succeeding theory is advanced, then, in order to overcome the faults or inabilities of previous provisional theories or explanations; and each, in turn, presents new problems and obstacles, and so new questions – which are held to be as valuable to the progress of science as the solutions that they provide. These 'flaws' are now seen to be, and quite accurately, the very engine of the scientific method. For, if these difficulties and obstacles ceased to arise, or if our means of attaining knowledge should cease to be able to discern them, scientific progress would of course come to a complete standstill. This redefining of theory represents one of those paradoxical paradigm shifts, which make science such an incomparably interesting game: each step, as theory advances, moves along the continuum of scientific progress, where moving forward rests on the paradoxical notion that each step is inherently flawed and will eventually be rejected or corrected. Who would have thought a few decades ago that one day 'error' would be logically engineered to replace 'truth' as the Hope diamond of science!

The model therefore 'uses data as well as quantifiable and quantified variables, because an objective appreciation of its relevance requires verification and therefore the measurement of the magnitudes that it brings into play' (Bonnafous, 1972, p. 12).

This means that only ideas resulting from an operative definition should appear in the model. Ullmo (1969, quoted by Bonnafous, 1972, p. 12, our translation) has expressed this idea in this way: 'an operative definition is a definition that includes the description of a regular process for discovering, measuring and more generally attaining and identifying the idea thus defined'. In a follow-up statement, Bonnafous (1972) argued that Ullmo advances an idea that would make a neoclassical economist shudder: 'the first methodological requirement of science is to only work with ideas defined in this way'.

The condition of measurement therefore, as observed by Bonnafous (ibid., p. 13):

> appears necessary for the model's operationality, because measurement is required in establishing the model's relevance. But it can also be directly involved through the model's objective. For the forecasting objective, this goes without saying. In most cases an explanatory objective also requires the estimation of parameters that play a determinant role (propensity to consume, capital coefficient etc.)

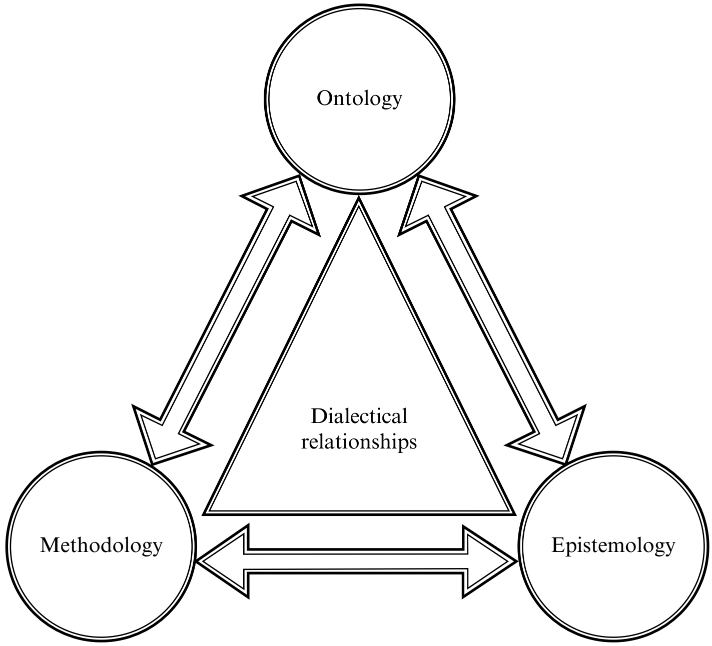


Figure 6.1 Volmer triangle diagram

Bonnafous (ibid., p. 14) concluded that the model's capacity to fully attain its objective depends on each of the conditions that we have singled out. It remains to be shown that the presence of each is necessary as a condition of the model's operationality, and the presence of all is sufficient.

Then we would be ready to present both the Volmer triangle diagram (see Figure 6.1) and the MTC diagram (see Figure 6.2), providing the visualization of the relationships between the three terms – namely, theory–coherence, applicability–relevance and measurement–quantification.

6.4.2.1 Interpretation of the Volmer triangle diagram

In Figure 6.1, we can visually appreciate the interdependence of ontology, epistemology and methodology. What we claim in our ontology sets the stage for epistemology by establishing the fundamentals of existence, the basis for what we know. What exists, the kinds of things that exist, constitutes the reality that we come to know. Epistemology is concerned with establishing the grounds for knowledge – that is, on what basis we validate our claims. These grounds, in turn, give us the framework for setting up the ways we can learn and test our knowledge, our methodology.

Obviously, these three fields of philosophy interact. Poor understanding

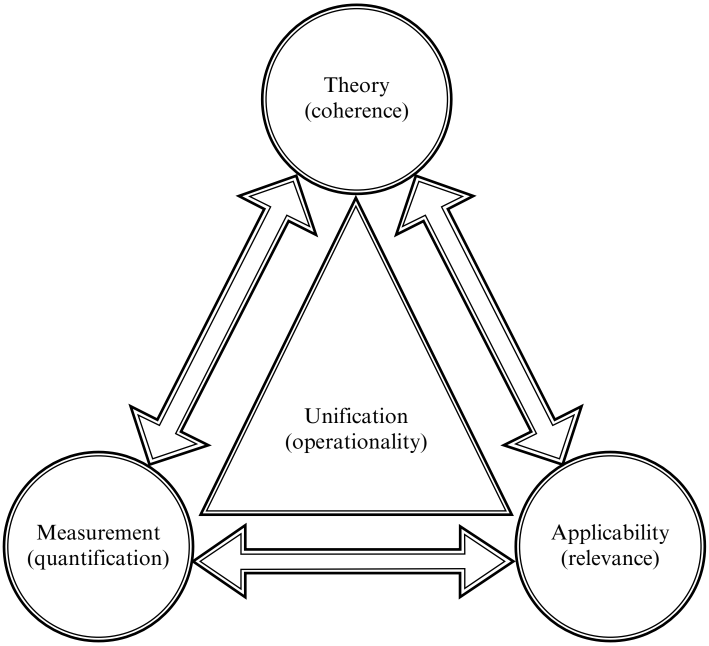


Figure 6.2 *The MTC diagram*

of what exists will undermine our analysis of the grounds for knowledge, which in turn will influence our methods. But advances in method will deepen our understanding of grounds, and will lead to a better appreciation of what exists.

6.4.2.2 Interpretation of the MTC diagram

As stated earlier, the MTC diagram is an extension of Bonnafous's (1972) triangle diagram. But instead of representing the concepts as the points of the triangle (as Bonnafous does), we propose to exhibit each as a circle; the three circles standing in a broadly triangular relationship.

All three aspects of the model are needed to ensure operationality. If the theory is not coherent, the model can't work; if it doesn't apply – that is, doesn't refer to real-world counterparts – it is useless, another fairy tale. And if it is coherent and applicable, but its variables and relations cannot be measured, then, while perhaps not wholly useless, it is of limited value. The socio-economic system runs on value and quantity; it *has* to be measured.

Note the analogy with Volmer. Theory–coherence corresponds to ontology; the theory tells us what there is (for the purposes of the model); it provides the list of variables and relationships to be studied. Applicability–relevance concerns how these theoretical variables relate to real-world counterparts; it is by taking this step, relating the theory side of the model to the world, that we turn our theory into knowledge. So this corresponds to epistemology. Finally, we gather data, measure and test; we estimate and work with the model, to set up the interactions between model and applications. This is methodology.

6.4.3 Back to Operationality: A Methodological Examination

It will become apparent that while these three terms are both necessary and sufficient to constitute the operationality of a model, they have the bad habit of coming into conflict with one another. The fact is that these conflicts are widespread, and seem to be in some sense written right into the script of traditional neoclassical model building. Now such inherent conflicts, of course, render the operationality of the model difficult to achieve, to say the very least, and render an establishment of its absolute scientificity, according to current methodological practices, impossible. The model represents the most powerful and useful tool currently available to economists in their bid to respond to the challenges of multidimensional reality and of complex causality, yet, at the same time – and perhaps, in direct proportion – it presents the greatest methodological challenge that econometricians face today, one that amounts, for the neoclassical tradition, to a Catch 22 situation: the more precisely and fully we present the theory, the less applicable (relevant) and more difficult to measure it becomes.

Now let us re-examine these conditions of operationality one by one.

6.4.3.1 Theory–coherence once again

A model's coherence, as we have stated, refers to the state of its compliance with the principles of logic as manifested by an absence of both internal conflicts – that is, those inherent in the model's design – and of internal/external contradictions with regard to the model's objective, together with the capacity of its contents, methodology and structure to meet that objective. In this, of course, coherence differs from relevance, which deals with the model's ability to represent its object. We shall now examine internal and external coherence in turn.

Internal/external coherence of objectives Internal coherence may be defined as the capacity of the group of values for the magnitudes under study to simultaneously satisfy all the equations of relationships. Now the reasons for designating internal coherence – that is, within the model's structure, between its terms – are self-evident. For, unless this requirement of an absence of internal contradictions is met, any mathematical formalism imposed by the methodology of the model (and by extension, by the 'simplified construction' of the model itself) will have no meaning. This said, we should add that these internal contradictions are in fact rare and may arise in both the causal component of a model, and in the quantitative component. In the case of the first category, the domain of theory, this requirement demands that the same causes produce effects that are not contrary within the model – that is, that the theories it presents comply with the rules of Aristotelian logic. Indeed, it might be said that the importance of a rigorously correct usage of terms such as 'and', 'or', 'if then' and 'not' might be analogous to the importance of correct usage of mathematical terms, for example the signs plus and minus, and the numbers in the domain of mathematics. It should be remarked that cases of interior conflict in the exploratory realm of logic and theory, however, can generally be corrected rapidly. (This was the case, as many will recall, with the first publication of the model representing the fundaments of Arrow's impossibility theorem.)

Coherence in the model's mathematical component is also a self-evident requirement. For how can a model be qualified as operational if its interior mathematical formalism precludes it from being so?

The quantitative model, then, is required to adhere to the structures of proper mathematical formalization. Although obvious, one of the technical advantages of mathematics is that formalization carries with it certain structural guarantees of its own validity. The rules of algebra are well established and can be accorded absolute confidence in their application within the model. Moreover, when mathematical incoherencies do in fact occur, they can usually be corrected; it is only infrequently that they bring about an irreducible incoherence within the formalization in question.

Unlike the quantitative model, however, the purely discursive model is subject to more serious difficulties regarding its capacity to meet the requirement of internal coherence. The discursive model, after all, deals with, and calls into question, causal relations, which have no mathematical formalization to back them up; thus, coherence is obviously more problematical both to establish and to demonstrate. The construction of a discursive model, then, cannot be undertaken lightly, and requires profound critical examination during its phase of conceptualization. Indeed, even when this examination has been conducted extensively, and the model is grounded as solidly as possible on the structures of logic, these discursive models can very frequently, if not always, give rise to some debate.

The second, more common, and perhaps thornier, group of problems of coherence are those posed by conflicts arising between the objectives of the models in relation to their terms, structure, and mathematical and theoretical components. For the successful outcome of a model-building project must obviously rest not only on a rigorously logical infrastructure or, if it is quantitative, on a clearly delineated logico-mathematical structure (coherent within itself); but it must also provide a methodology that is consistent with its pre-established objective.

To illustrate internal/external coherence, consider an example: the case of a model designed to provide an account of prices and quantities in a system of 'general interdependence'. First, we have to be clear whether this means under long-run normal conditions, or whether we want to model dynamics, say over the business cycle. Or do we want to examine changes in business practices over the 'product cycle'? Are we considering a craft system, where small-scale artisans produce a single product or a small specialized list, or are we examining mass production by giant multidivisional firms operating advanced technologies? In each case the model must specify the objective, and spell out its implications; then, in order to achieve consistency, the model must express the equations for quantities and prices in a form that reflects the conditions implied by the objective – the technology, the degree and nature of competition, etc. Finally, of course, these equations will have to be the right number, they will have to be independent, they will have to have appropriate formal properties, linear or non-linear, etc., for if this were not the case they could not be solved.

In order for a model to achieve coherence it is not sufficient merely to elaborate a logically acceptable theoretical response, which conforms to the objective of the model. It is also necessary that the determinations thus formalized reflect in an acceptable way the concepts that reasonably describe the actual state of things. This conformity of the model to that which we believe we know about reality is the second condition of its operationality. As we have said, we call this second fundamental requirement 'relevance'.

6.4.3.2 Applicability–relevance once again

Relevance deals with the relationship of the model to its object; we could speak of 'applicability'. We have defined it as the conformance of an econometric model to the state of things in the sector of ('real-world') reality, which it has demarcated as its object. In more specific terms, as we have said, it represents the link or nexus between the model's logical and mathematical structure (particularly in its digital form) and the true nature of its object (in so far as it can be logically perceived and understood). It also refers to the relevance of the model as it is determined in particular by the model's objective and a specific state of mathematical knowledge. On a conceptual level, in terms of the relativized concepts of the new science (and in a manner that best reveals its essential problematics), it might be defined as: 'the quality of the approximation of a very provisional reality by its simplified (indeed, sometimes distorted, and one might even say on occasion fictitious!) representation'.

Clearly we have on our hands, then, in our desire to meet the exigencies of science in terms of this requirement, a rather daunting task!

Here, however, the concept of relevance, in the light of modern science, is not incompatible with the working concept of an approximation. Accordingly, economists now have at hand such tools as the theory of probabilities and the statistical methods and techniques of approximation. (This kind of simplification, however, while both methodologically acceptable and necessary, is not only reductive but to some extent distorting.)

There are tools in place, then, to help establish relevance according to these revised definitions, but still the task of demonstrating that a viable relationship exists between the (in truth, unquantifiable) segment of real-world reality a model purports to represent, and the formalized and reductive structure of that representation, is far from easy. (When this is taken into account, in the discourse of economists, it is highly recommended to avoid the term certainty!)

6.4.3.3 Measurement–quantification once again

The possibility, indeed the requirement, of establishing and quantifying *V* relations, is, of course, a fundamental criterion of scientific inquiry with regard to socio-economic systems. Therefore the model, which from the outset was designed and designated as a scientific instrument, must have within its methodology an adequate system for such verification.

The ability to measure,[18] as we have stated, indicates the degree to which the validity of the structure, relevance and responses of a model may be verified. For an econometric model, measurement rests on three requisites: establishing the validity of the statistical sample; the ability to estimate the model's parameters; and the possibility of estimating instrumental variables required for testing the probabilistic hypotheses.

Measurement, then, is required on several levels. In the section on coherence, we referred to the necessity to meet both internal and internal/external requirements, and the same, we see, is true of measurement. For while internal verification is of course required, science also demands a mode of verification that arises from the necessity of comparing the model to the actual state of things. This requirement may be implied by the very objective of the model. For example, a forecasting model cannot meet the requirement of measurement with algebraic expressions alone: it is constructed in order to produce coded results describing the probable evolution of measurable magnitudes, and in this case the condition of measurement is even more pressing.

The condition of measurement is three-fold. It comprises: the measurement of causal relations, the measurement of quantities put into play in the model; and the measurement of the parameters of the equations.

We should add that while the first two requirements apply in the same general way to both causal and quantitative-type models, in the case of the third, as we shall see later, a distinction must be made between the two categories. First, however, we examine the three levels.

6.4.3.4 The measurement of causal relations

In dealing with causal relationships, what we are basically trying to demonstrate or prove is: 'when *A* occurs, *B* occurs as a result'. It's not just that *A* is always followed by *B*, or that they occur together. *B* occurs because *A* made it happen. It should go without saying that, in terms of scientific method, only real controlled experimentation would permit such a watertight conclusion. A chemist might establish, for instance, that a strip of litmus paper does indeed turn pink when dipped in the same liquid chemical compounds five times in a row! But why does the liquid cause the colour to change? For that we need to know molecular chemistry – we need a theory that explains how things work. By contrast, econometrics is in a very different situation, for it starts from relationships based on arbitrary reductions abstracted from a reality in which every causal relationship or event is in fact unrepeatable, unique in time and place. Moreover, these events tend to be results of a confluence of an infinitely large number of variables. In short, it is hard to determine what should be abstracted from what, and where to draw the line between the economically relevant and the rest. In some sense, we face a relationship between an unquantifiable reality and a fictitious and to some extent distorted picture. Fortunately, a certain level of verification may be achieved via indirect testing for these causal relationships, first gathering information through fieldwork, and then developing conceptual analyses, specifically developing a causal model which experiments with the relationships, and permits us to infer new statements or comparisons that may be suitable for statistical testing. But it may not be easy to show that *A* causes *B*, that the deficit *causes* expansion. Or does it drive up interest rates? Does an increase in the money supply cause inflation? Our answer is that the model must be placed in the framework of the *O* and *V* relationships, which define the powers and opportunities of the agents, and then we need to examine their motivations and expectations, to see what they are actually trying to do. They are acting with causal intentions, trying to bring about certain results. We can fit this together with what we know of the structure of the system, to see how these intentions are likely to work out. On this basis we can develop or draw on theory.

Evidently, if we stick to the model alone, it may be difficult to meet the requirements of scientific method in the pure sense here. Indeed, the apparent impossibility of this task represents one of the fundamental dilemmas of the model as a scientific instrument. To get to causality, we have to take a further step and endow the model with the status of being an (accurate) expression of an established conceptual framework! And this has to be based at least in part on fieldwork.

6.4.3.5 The measurement of quantities concerned

Needless to say, scientists of the empirical school are far more comfortable with straightforward measurement of quantities in the model. Because the model uses data as well as quantifiable and quantified variables in order to establish, for example, under which circumstances, as a result of factor A, the entity B must increase or decrease, proper scientific practice requires that the accuracy be established by a rigorous measurement of well-defined magnitudes. There must be good standards in data collection and compilation, and the numbers must be expressed in terms of reliable units. This calls to mind the old debates about the possibility of defining 'an invariable measure of value'. The difficulties elaborated in those debates must make us uneasy about comparisons of 'value' in widely different economic contexts. Nevertheless, with suitable qualifications, measurement is achievable.

6.4.3.6 The measurement of the parameters of the equations

The causal model, as we have mentioned, is a structure that consists of equations designed to formalize a causal system by elaborating functional relationships, where these go beyond the model and express relations supported by an established theory. Usually, when we consider this type of model, we tend to focus only on those examples constructed of mathematical components, for it is these that may be most precisely and properly validated. But a model which is defined as strictly causal and which proceeds via formulations of logic alone, may be considered non-quantitative (though the term is sometimes used loosely). For, like the quantitative model, this kind of causal model may also be composed of a variety of symbols – linguistic, however, in this case – representing quantities, proportions and levels, and it may put into play measurable quantities for which solid statistical data is found to be available. The only difference between these two, then, would appear to be the extent to which the dependencies are the subject of a mathematical formalization.

This is crucial for measurability, however, for while the mathematical terms of the causal model may be scientifically verified, the same of course does not hold true of its logical terms. But the measurement by itself cannot confirm the claim of causality; the mathematics may make the claim more precise, but it does not make it more plausible.

As regards the criteria of measurable parameters, these may be considered measurable only if they are operational concepts whose definitions encompass a pre-established system for their own measurement. The parameters of the model have to be derived from the reduced form, and this requires knowing how to go from the latter to the former. (There could be a verification process, which, while not standardized, has been tailor-made to the case of a particular model. But its validity must be pre-established.) Measurement is most useful at the level of quantitative models where it permits a decisive evaluation of the pertinence of the model: ideally, the equations having been formalized and statistical data being available, established theory permits us to proceed from an estimation of the reduced form to the parameters of the model, and to establish with the observed values of the variables that each one of the equations is really verified, to its nearest residual term. Of course, the crucial issue is: where do we find, and how do we establish, the theory that makes this possible?

6.5 THE PROBLEMS WITH OPERATIONALITY OF THE MODEL

6.5.1 Definition

The difficulty of getting useful results from a theoretical construction may seem to stem from an opposition between (theory) coherence and relevance (applicability), for example, where the idealization of an agent required by theory makes it impossible to find a real-world counterpart; or it might simply be a problem of the 'fit' between the formalism of the theory (continuity, smooth functions) and the (lumpy, discrete) reality that this formalism is intended to represent. But there is more to it than this, including the fundamental role played by the measuring process.

Theory takes the shape it does partly because of the available mathematical instruments. A good illustration is the omnipresence of linearity in econometric models. Yet there is absolutely no reason that relationships of statistical reality should be linear; furthermore, one notes that economic reality is often incompatible, even as an approximation, with the mathematical tool (be it linear or non-linear). (Later we will see that assuming linearity can be done in a way that is in fact less restrictive than it appears;

nonetheless, the absence of a pre-established harmony between mathematics and reality, inasmuch as we can be aware of it, is one of the sources of the coherence–relevance opposition.)

A further source of the difficulty in getting useful results is related to the condition of measurement. The measurement of model parameters from statistical samples requires the use of estimating techniques that were developed within the theory of probability and are based on specific probabilistic hypotheses (normal distributions of residual terms, independence of random variables, etc.). Here again there is no particular reason for these hypotheses to match a statistical reality.[19]

Furthermore, since the theory of probability is part of mathematics, this possible cause of opposition is an instance of the previous condition: the condition of measurability, required for the verification of the condition of relevance, can come into conflict with essential aspects of theory. Consequently measurability could come to preclude consistency, because the theoretical foundation of measurability does not meet the condition of relevance (Bonnafous, 1972).

The model's theoretical approach is determined in part by its objective; but at the same time the model has to apply to, and has to properly fit, its object. The object and the objective are clearly related, but it does not follow that they are compatible. We can imagine a trivial example of a problem: suppose the model's objective is to forecast two well-defined variables – that is, to estimate two equations that link two explained variables to appropriate explanatory variables. This can be done on the basis of a well-regarded theory. But suppose the object – the socio-economic system – in its statistical reality only exhibits variation in one of these! In such a case, with the given data, operationality cannot be achieved. More and different data may help; or it may be that we learn that the supposed variable is actually constant in many circumstances.

Difficulties regarding operationality arising from the conditions for relevance are frequently met. Biologists are very familiar with this, splitting into two opposing camps; those who support models that work (that can be solved, provide insights, and so on) and those who want models to represent reality. Bonnafous (1972, p. 19) argues that 'structuralism, as presented by Levi-Strauss (1958), has in some ways confused the debate by studying the relationship between the active production of a model and a presumably neutral observation of events: the model is supposed to be the counterpart of a real object,' namely some aspect of a socio-economic system, but it is also the product of a social process which is itself another part of that system. The social process will affect the way the object is seen, and so will bias the product. Because it is produced or patched together, it cannot accurately reflect neutral observation, and so must be some kind of 'unreality' even though the 'patching together' should make it fit its object. We agree that modelling is a social process and that knowledge is a social product, and is not independent of the paths of discovery. But science is precisely working out how to free knowledge from these bonds so that we can test and develop our hypotheses accurately and impartially.

Indeed, Bonnafous (1972, p. 19) pointed out that raising this problem seems to run counter to another statement by Levi-Strauss (1958), in the same text, in which he says, 'the model must be constructed in such a manner that its functioning can take account of all the observed events'. All the events? The point of theory–coherence is to define a boundary between what the model can explain, and what it takes for given.

The operationality of the model is illustrated by Figure 6.3 which expresses the fact that these three conditions for a satisfactory attainment of the model's objective – coherence, measurability and relevance – are both necessary and mutually dependent. Figure 6.3 is a modified version of Bonnafous's TD (see Bonnafous, 1972, p. 21).

Bonnafous (1972, pp. 21–2) argued that the main causes of model failure, listed at the bottom of Figure 6.3, are connected by implications: incompatibility (3) is a special case of incompatibility (2), which is itself one of the possible causes of incompatibility (1).

6.5.2 Interdependence and Relationships between the Three Terms

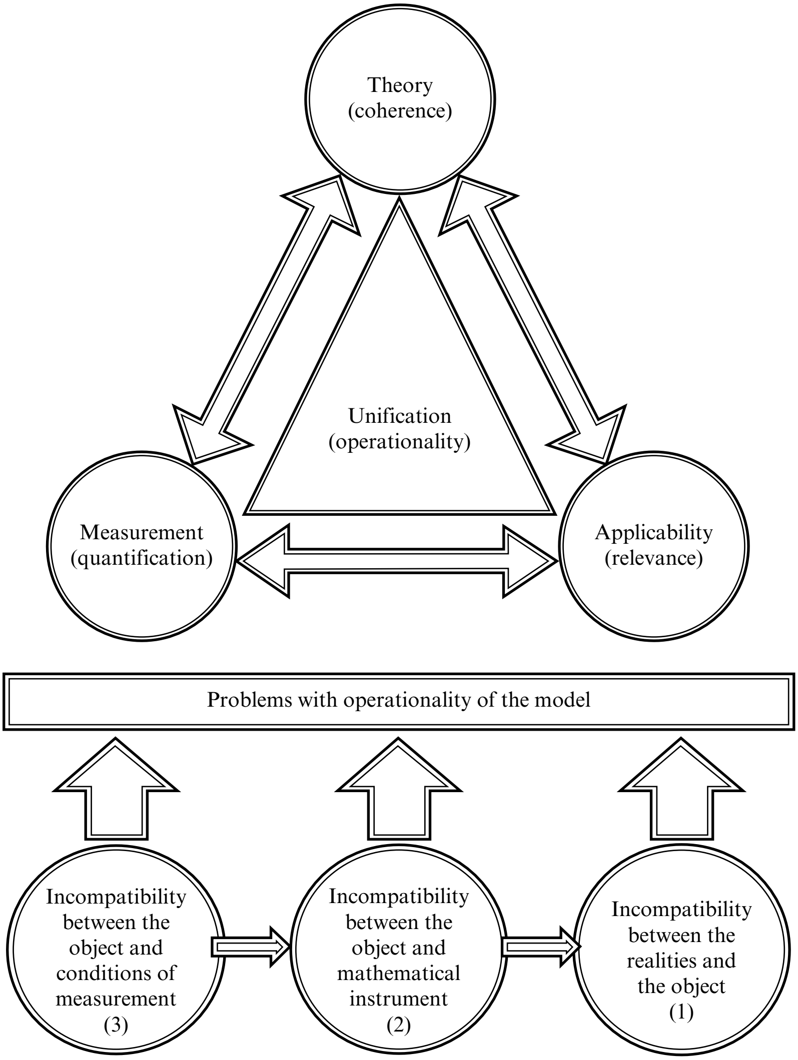
Having defined and established the nature of our three requirements, we shall now turn to the subject of their interdependencies: measurement–relevance, relevance–coherence and measurement–coherence.

Subsequently, we shall raise a point on the philosophy of science. It seems that the econometric model's hard-core requirements of coherence and relevance correspond quite neatly to two of the traditional positions in the philosophy of science. We refer, of course, to the age-old induction versus deduction debate. It will be seen, as well, that both in the times of the Greeks and in the field of contemporary economics, a third philosophical school, like the third requirement – measurement – represents a link between the two poles.

And now let's examine the relationship between the requirements pair by pair.

6.5.2.1 Measurement–relevance

The interdependent relationship between these terms is self-evident. Relevance, as we have established, is of course a necessary condition for operationality (for one can't get very far with a model that comprises features or components that are at odds with the statistical reality which its



Source: Based on Bonnafous (1972, ch. 1, p. 21).

Figure 6.3 Principal causes of conflict

purpose is to explain or act upon). Relevance is also, however, a condition that must necessarily be verifiable, for without the possibility of verification in a measurable way, the model cannot be established as a valid scientific instrument, and cannot therefore be termed operational at all. It is interesting to note that the dependence between these two terms might be said to be reciprocal. For, on the one hand, in order to establish that the requirement of relevance has been met, there must be means of measurable verification. On the other, in order for the requirement of measurement in this area to be met, the acceptable correspondence of the model's reality as designated in statistical terms and the structure and game plan of the model itself – in other words, that which must be measured and proven to be relevant – must first be elaborated in a relevant way by its author.

6.5.2.2 Relevance–coherence

Clearly a model's ability to accurately correspond to its objective – that is, its coherence – is contingent on its ability to accurately represent the statistical reality it is designed to explain and act upon – that is, its relevance. The dependence between these two requirements, in the case of econometrics, however, is not just a one way-process. For if the scope and limitations of the object, or statistical reality of the model, also, of necessity, determine the limitations and possibilities of the objective, then conversely the definition and scope of the objective may also determine the choice and nature of the statistical reality selected. We should add, moreover, that the model might be said to represent a common denominator in which the causal language of the objective and the statistical language of the object may have the technical possibility of conversing in a symbolic Esperanto in order to arrive at a synthesis. And since this synthesis is the 'raison d'être' of the model, it might be fairly said that the model's structural and methodological relevance to its object and its structural and methodological relevance to its objective are essentially interlinked, interdependent and equally required.

6.5.2.3 Measurement–coherence

Finally, let us note that the requirements of measurement and coherence find themselves, of course, inextricably linked if a model is to be termed operational in a scientifically acceptable way. It must, in other words, be measurably demonstrable according to the criteria of scientificity that there are no fundamental contradictions or gaps between the structure or game plan of the model and its ability to meet its predetermined objective. In other words, the formalism of the objective must lend itself to the resulting statistical methods. Internal coherence – the calculation of a model's unknown parameters – must also, of course, be measurable if a model is to be accorded operationality status with respect to the terms of scientificity. Conversely, the fulfilment of the conditions of measurement is contingent on the initial arrangement of the elements of the objective and the elements of the interior structure in a consistent and successfully measurable manner.

6.6 CONCLUDING REMARKS

This examination of the relationships between the terms, pair by pair, by means of what we might call a 'triangle of logic', has now reasonably established that the related conditions of theory–coherence, applicability–relevance and measurement–quantification imply the operationality of the model. So, first, our three terms comprise the basic requirements of operationality; second, the presence of all three is sufficient as a condition of that operationality, for together they establish that the model's essential components – its object, its structure and statistical/mathematical/logical game plan, and its objective – are measurably consistent and relevant to one another.

The *O* and *V* relations defined in Chapter 3 provide important connecting links between the components of the model. *V* relations establish measurability. Any variable expressed in *V* can be measured in numbers. It may be either determinate or stochastic, but either way it will be measurable. *O* relations, on the other hand, establish applicability or pertinence, since they connect agents (and institutions) with variables. Thus, consider an agent who owns a valuable asset, say a machine that he uses himself at times and rents out at other times. Suppose we propose to develop a model describing that agent's behaviour with respect to that asset, showing how he divides the time between his own use and rental as a function of the rental price and other variables. If the model is cast in the terms actually employed in the *O* relation, for example legal or property language, it will necessarily be applicable. (And since the asset is valuable, variables describing it will be measurable.) The agent and the machine will both be described, for example, in contracts; the agent's rights and liabilities will be spelled out, the characteristics of the machine will be given, and so on. Hence defining the *O* relations precisely will tell us what exists in the economy, as John R. Commons understood long ago (see Commons, 1924). So the *O* relations ensure applicability – and, moreover, provide terms and concepts for theoretical coherence.

Thus the *O* and *V* relations are directly related to the MTC diagram: *V* relations ensure measurement, and *O* relations applicability. Take this one step further: we have already established our interest in fieldwork and conceptual analysis, and it is easy to see that these correspond, respectively, to relevance and coherence. Fieldwork done properly will reveal *O* and *V* relationships, as well as other ones, and will tend to ensure relevance; and conceptual analysis of some sort is virtually required to ensure coherence. Fieldwork will enable us to take in and understand the concepts that guide social and economic practices; conceptual analysis will develop them into theories. As for measurement – that is what the Cowles project was all about!

NOTES

1. The authors extend appreciation to the memory of Professors Wassily Leontief and Camilo Dagum who drew their attention to Bonnafous (1972) and to the complementarity between Bonnafous's approach and our work. Bonnafous's book was drawn upon by two leading French econometricians in the 1970s, Guitton and Malinvaud. Both authors have produced important econometric textbooks (Guitton, 1964; and Malinvaud, 1964). Bonnafous applied the TD framework mainly to examine the logic of econometric investigation. Throughout his book, he used the Klein (1950) models I–III to illustrate his main thesis and as a test case for his approach. He also devoted a long section to Leontief's input–output model to illustrate his point. He argued that Leontief succeeded in swapping a good part of the substance of the general equilibrium model for the satisfaction of the contradictory requirements of operationality. Bonnafous argued that Leontief's model is a remarkable example of methodological innovation. For further details, see Bonnafous (1972, pp. 22–32). For an account, see Errouaki (1989; 1990) and Nell and Errouaki (2006a).
2. Bonnafous (1972) outlined the principal difficulties inherent in the construction of an econometric model, suggesting that they are recognized, at least implicitly, by all authors. The list compiled by E. Kane (1968) in accordance with the mnemonic proposed earlier by Courchene – the name of the British economist MALTHUS – included multicolinearity, autocorrelation, heteroscedasticity, under-identification, and also the difficulties inherent in the insufficiency of data, the cost of the estimation procedure and, more simply, errors of specification. For further details see Bonnafous (1972).
3. Since their publication, Klein (1950) models I–III have been systematically selected by econometricians wishing to test novel methodological refinements. Lawrence R. Klein's 1950 work, *Economic Fluctuations in the United States 1921–1941*, is a monograph of 175 pages. Given the importance of Klein's work here, see the monograph for further details on the economic context of economic theory, the microfoundations of his equations, the aggregation problem, data collection and statistical measurement and other technical details. Klein's model was published under the auspices of the Cowles Commission in 1950, but it post-dates that institution's principal theoretical results concerning the problems of measurement. For an account of the Klein model, see Bonnafous (1972), Errouaki (1990) and Cercos et al. (2008).
4. In Chapter 7 we will show that this important empirical work played a crucial role in the history of the methodology of structural econometrics and was at the centre of the debate in the 1970s over the scientific foundations of structural econometrics.
5. Nell (1998a) is closely related to Volmer's point. See the discussion in Chapter 10.
6. Dagum (in correspondence with the authors) argued that few econometricians realize the extent to which apparently reasonable methodology is not enough to guarantee that the underlying epistemology and ontology are correct. Camilo Dagum's scientific work distinguishes itself for the originality and completeness of methodological solutions, its wide and composite view of phenomena and problems, its logical and mathematical rigour, and for the continuous and difficult search for a syntactical and semantic connection between theory and reality, between abstraction and observation.
7. Dagum (1986b) argued that in the history of philosophy of science we can identify three principal streams of thought in the quest to provide foundations for scientific knowledge. We could classify them as: empiricism, idealism and theoretical empiricism. Idealism is ontologically realist but epistemologically idealist, since it does not deny the existence of an external world but asserts that the model representation of this external world is a scientist's mental construction carried out with the purpose of providing himself with a convenient instrument to be used to accomplish objectives such as description and prediction of events. However, theoretical empiricism has the property of being both ontologically and epistemologically 'realist'. Asserting that there exists an external world – whose objects of knowledge are matter, life and society, even though we might not be able to make observations – is ontologically realist. It is

epistemologically realist because it maintains that the function of scientific methodology is to find out properties of this external world. It purports to find a new theory, a new scientific explanation, as the outcome of a process of interaction among observations, ideas and reason, which distinguishes theoretical empiricism from both empiricism and idealism. We shall argue in Chapter 10 that Nell's (1998a) methodological institutionalism is an illustration of theoretical empiricism. Nell's approach integrates observation–sensation and reason, within a philosophical frame of reference, leading to model specifications possessing coherence, relevance and capability of measurement. For an enlightened discussion of philosophy of science approaches in economics, see Dagum (1986b).

8. The MTC might be considered as a model in its own right, and minimalistic in its simplicity – indeed, as spare and simple in its basic design as the process of its elaboration must be lengthy and complex.
9. Bonnafous's (1989) book provides a good account of the concept of the model and a lucid examination of epistemological issues in economics.
10. See Bonnafous (1972, p. 4).
11. Quesnay appears to have been the first economist scholar to set down in some detail the rudiments of an economic model. The model is today a crucial notion in the social and physical sciences. Armatte (2005) re-examined several philosophical and historical works on models, and offers elements for a genealogy of this category, ranging from its use by Maxwell and Boltzmann in physics to the debates of the Vienna circle on model theory in mathematical logic, and later, the emergence of the notion in the field of social sciences around World War II. For an account see Armatte (2005).
12. Boumans (2001, p. 431) argued that 'Morrison and Morgan's account of understanding that is gained by building and using models fits into a longer tradition that started with what Galileo took to be intelligible and the model of intelligibility that he developed'.
13. See Kane (1968) for a brief discussion of both realms in econometrics.
14. This point was further clarified to the authors by Dagum in correspondence.
15. The distinction between the exploratory realm vs the applied realm in econometrics is briefly discussed in Chapter 8.
16. Bonnafous (1972) selected the terms coherence, pertinence and measurability. For further details on selecting terminology, see for example Badiou (1969) and Guillaume (1971).
17. 'Theory–coherence' and 'applicability–relevance' correspond roughly to the philosopher's distinction between 'sense' and 'reference'. Hollis and Nell discuss applicability at length. See Hollis and Nell (1975, ch. 4).
18. For further details on the problem of measurement in econometrics, see Chapter 8.
19. For further details, see Chapter 8 and Bonnafous (1972, ch. 2)

PART II

The critiques and the foundations

The critics of the neoclassical Keynesian synthesis of macroeconomics usually cite a failure of such models to anticipate the great surge of inflation during the 1970s. That appears to be a major factor motivating the angry young men into developing new models: empirical models without theory; rational expectations models; new classical models; monetarist models. *I have not seen their numbers, which would be indicative of whether or not they have something better, but I do know more about the actual anticipation of the inflation surge and would like to present a different side of the story*.

There were at least two very different approaches to follow, at that time, in macroeconometric model building. We could have reacted as the angry young men did and tried to construct an entirely new model specification, with a new method of estimation too. Or we could have reacted by building in more detail [. . .] in order to be able to interpret these events better [. . .] *It seems better to try to be constructive by improving existing models rather than to declare an immediate need for something radically different and new*.

Klein (1985, pp. 289 and 292–5, italics added)

When we look back and try to give a broad evaluation of the achievement of the simultaneous equation work of the 1940s, we, of course, know that the theory was not complete by the end of this period. Alternative estimators had to be discovered, small sample properties to be investigated, nonlinear simultaneous equation models to be considered, efficient computational soft-wares to be built, even pedagogical presentations of the theory and of its algebra to be found. *Nevertheless, after thirty more years of theoretical research in the field, the Cowles Commission construction essentially stands untouched; new wings and pinions have been added, good maps have been drawn, but the central building needs no repair. This was a perfectly sound and impressive piece of methodological work. No doubt or questioning can be expressed.*

Malinvaud (1988, pp. 196–7; italics added)

The initial optimism associated by the promise of the new statistical methods of the Cowles Commission to significantly improve empirical modeling in economics turned into pessimism by the late 1960s. After two decades of laborious efforts to build large theory-based macroeconometric models, and millions of dollars spent by Central Banks and other government institutions, the results were more than a little disappointing. *The combination of the Cowles Commission and the newly established textbook approach to econometrics did very little to allay the doubts created in the 1950s that empirical modeling in economics was not an effective tool in learning from data about economic phenomena of interest, nor was it useful for appraising different theories or forecasting and policy decision purposes*.

Spanos (2010, p. 235, italics added)

It is remarkable how rapidly and completely academic interest shifted away from serious probability-based policy modeling after the rational expectations 'revolution'. This reflected aspects of the sociology of our profession that remain with us. Despite the recent pickup in academic interest in these issues there remains substantial resistance to giving them academic respect. We need to preserve the momentum of this research.

Sims (2011, Nobel Lecture, italics added)[1]

Even if economic philosophers have usually been indefinite about the economics that is being criticized, they seem to be in general agreement that econometrics is often at fault.

If one wants to be critical of an area of research, an obvious strategy is to find respected members of the field who are being constructively critical themselves, such as Sims, Leamer, and Hendry (in econometrics), and emphasize the critical comments from them without mentioning the corrective or improved techniques they propose. I find this unbalanced presentation of an active debate is sometimes reported by economic philosophers.

Granger (2004, pp. 103–104, italics added)

7. Debating the foundations: a new perspective?

INTRODUCTION

Klein takes a strong stand in the debate on the foundations of econometrics, and offers a constructive response to the problems of the Cowles model, placing himself at the opposite end of the spectrum from the rejection advocated by the angry new generation of econometricians. In addition, Klein (1957; 1979; 1982) argued that building institutional reality into a priori formulations of economic relationships (through fieldwork) and refining basic data collection have contributed much more to the improvement of empirical econometric results than have more elaborate methods of statistical inference.

Furthermore, Ray Fair (1994, preface) argued that his research is 'a rallying cry for the Cowles Commission approach'.[2] Indeed, Fernandez-Villaverde (2008, p. 691) observed that 'Fair is to be applauded for his position: first, and foremost, because there is much of value in the Cowles Commission framework that is at risk of being forgotten. Fair's (1984, 1994, 2004) books may play a decisive role in the transmission of that heritage to newer generations of researchers'. He (ibid., p. 686) went on to argue that reading Fair's books as 'a trilogy is a rewarding endeavor that yields a comprehensive view of what the Cowles Commission approach is all about, how to implement it, what it can deliver, and how it compares with the alternative methodologies existing in the profession, in particular with the increasingly popular estimation of dynamic equilibrium economies. But perhaps to understand this comparison better, it is necessary to glance at the history of macroeconometric models'.[3]

Malinvaud (1981, p. 1374) also presented a constructive response to the problems of the Cowles model, contending that:

> Successfully carrying out the research program of Jan Tinbergen today remains the same challenge that it was at the beginning. But how shrewd were the audacious men who launched it! More than forty years after, it is one of the subjects, which most interest economists. None of the reorientations, which it has undergone, or which it still must undergo, fundamentally affects the original view from which it has developed.

As Morgan (1990b, p. 158) observed, Malinvaud referred to the Cowles simultaneous equations methodology as a 'castle':

> It is a must to go on top of the keep and to look from there at the surrounding landscape. If one does not do so, one will be definitely handicapped and clumsy when making assessments about exogeneity, about identifiability or about estimation bias resulting from interdependence between phenomena.

Malinvaud (1988) exhibited a degree of unease about the economic relevance of the Cowles model, yet insisted on the highest standards of mathematical rigour in its application. As Epstein (1987, p. 7) observed:

> like the Cowles people, Malinvaud retains the greatest interest in devising operational policies and stresses the need for a priori assumptions to allow construction of multi-equation models with large numbers of variables but small data sets.

The trouble is, things aren't working. Epstein (1987, p. 225):

> Unquestionably, it has been the empirical results that have aggravated a methodological split in the ranks of econometricians. One tendency is basically atheoretical and makes little use of economics or statistics to interpret the output of estimation procedures. Christopher Sims, with the VAR, and Herman Wold, with soft modelling, are two principal figures in a movement away from discovering underlying structure. They have nearly abandoned structural estimation. The advantages of [Cowles] methods would seem to lie in their simplicity, particularly for forecasting, but [there have been] few successful and compelling economic applications.

Modern critiques have argued that reality has shattered the illusion of the Cowles econometricians that they had captured the basic structure of the economic system, and could therefore successfully prescribe how to manipulate it.[4] At the Cowles Commission, the primary task of econometrics was seen to be the development of statistically efficient methods for the estimation of structural parameters of an a priori specified system of simultaneous stochastic equations. This latter was drawn not only from economic theory, but also from realistic and careful observation and conceptual analysis. Their explanation of economic events is based on an ontology, which is opposed to what we have called in Chapter 1 'methodological individualism'. As Epstein (1987, p. 64) observed:

> The Cowles workers shared Haavelmo's view that empirical work in economics would best proceed scientifically by the specification of a model as a set of identified structural equations together with an assumed stochastic distribution of the error term. What soon came to be called 'Cowles Commission Method'

did not, however, enjoy automatic acceptance by the economics or even the statistics profession.

In the early 1940s an entirely different approach was proposed. The idea then was to make the statistical analysis part of economic theory itself (see Haavelmo, 1943b, 1944; Koopmans, 1941; Mann and Wald, 1943). While there is some danger in seeing this as an endorsement of stochasticism, Haavelmo was quite aware of the limitations of such an approach and was careful to stress that the approach necessitated separating our stochastic models from our exact theories. Moreover, he stressed that his approach required a thorough commitment to stochastic modelling with no hope of returning to the world of exact models (see Haavelmo, 1944, pp. 55–9).

The perspective here is that modern critiques of the methodological foundations of structural econometrics have followed the wrong road. To be more accurate, these critiques raised important points but the critics then falsely came to believe that they had built solid theoretical econometrics on sound ontological and methodological foundations. In particular, they uncritically assumed that neoclassical economic theory was adequate and that therefore they possessed a good understanding of the economic system and its processes of structural change.[5]

The main argument of Chapter 7 is that the founders of modern econometrics, Haavelmo and the Cowles econometricians (particularly Klein), held a vision of the real world first expressed in the Cowles model which provided the epistemic foundation for the econometric field in the 1940s. This vision provides a perspective which is ontologically incompatible with the 'contemporary view' of modern econometricians developed in the late 1970s and early 1980s.

7.1 THE BACKGROUND OF THE DEBATE

The early debate between Keynes and Tinbergen over the role of econometrics in testing business cycle economic theories[6] focused on the limitations of econometrics as a tool of testing economic theories. Keynes's (1939) critique compared Tinbergen's econometric work to alchemy. Friedman's later (1940) critique raised the issue of model selection when the estimation procedure repeatedly used the same data to discriminate between plausible competing theories.

Keuzenkamp (1995, p. 2) commented on the the Keynes–Tinbergen controversy:

> Keynes disliked econometrics. Moreover, he did not understand much of it. This, at least, is the view of many economists and econometricians who recall

> their vague, and usually indirect, knowledge of the Keynes–Tinbergen controversy (Keynes, 1939, 1940; Tinbergen, 1940) [. . .]
>
> The controversy with Tinbergen is frequently regarded as a deplorable clash between an old and a new era in economics (e.g. Stone, 1978; Morgan, 1990a; Malinvaud, 1991a). Occasionally, Keynes is credited with raising the problem of misspecification, without having an established vocabulary in which to formulate the problem.

Keuzenkamp (ibid.) went on to argue that: 'Keynes' critique is not primarily one of mis-specification. It is neither based on an objection to econometrics and probabilistic inference in general, nor does it follow from an outdated misunderstanding of the crucial issues at stake'. He went on to note that: 'Keynes' arguments can be traced back to his 1921 book *Treatise on Probability*, where the "principle of limited independent variety" is introduced as the basic requirement for probabilistic inference. This requirement is not satisfied in case of investment, where expectations are complex determinants. Multiple correlation, sometimes thought to take care of required ceteris paribus clauses, does not help to counter Keynes' critique'.[7]

Malinvaud (1991a, p. 636), however, noted that:

> in order to implement his ideas in England, Keynes encouraged the macroeconomic and econometric work of Richard Stone whose use of it for policy would ultimately depend on macroeconometric models of the Tinbergen type.
>
> The common view at Cowles was that Tinbergen's work was not quite right; indeed Tinbergen was very seldom quoted when it now appears that he should have been. Again Tinbergen was an outsider.
>
> I also want to emphasize strongly the point about economics being a moral science. [. . .] It deals with introspection and with values, it deals with motives, expectations, psychological uncertainties. One has to be constantly on guard against treating the material as constant and homogeneous. It is as though the fall of the apple to the ground depended on the apple's motives, on whether it is worthwhile falling to the ground, and whether the ground wanted the apple to fall, and on mistaken calculations on the part of the apple as to how far it was from the centre of the earth.

Keynes (1973, in Moggridge, vol. 14, pp. 319–20) raised a serious point in a light-hearted way:

> It will be remembered that the seventy translators of the Septuagint were shut up in seventy separate rooms with the Hebrew text and brought out with them, when they emerged, seventy identical translations. Would the same miracle be vouchsafed if seventy (econometricians) were shut up with the same statistical material? [especially] if each had a different economist perched on his *a priori*.

Keynes's objections to Tinbergen have been widely misunderstood,[8] and have even been cited as showing a lack of technical sophistication and an obstinate Luddite opposition to quantitative work! This is quite wrong; Keynes (ibid., vol. 14, Letter to Harrod, 16 July 1938, p. 299) welcomed empirical work:

> I think it most important [. . .], to investigate statistically the order of magnitude of the multiplier, and to discover the relative importance of the various facts which are theoretically possible.

Keynes (ibid., p. 300) added a little later:

> The specialist in the manufacture of models will not be successful unless he is constantly correcting his judgment by intimate and messy acquaintance with the facts to which his model has to be applied.

In fact, the technical objections raised by Keynes (ibid.) tended to be on the mark, presaging much later discussion. More important, however, were his philosophical concerns. Keynes raised questions of uncertainty, of the difficulty of digging deep enough to get to the true mechanisms, and of different researchers reaching agreement, all of which suggests there are limitations to econometrics as a tool for testing economic theories (for example, Klein, 1982).

Returning to Keuzenkamp (1995, p.4) again:

> Unlike Tinbergen, who was very pragmatic [. . .], Keynes was preoccupied with the logical conditions for probabilistic inference [. . .] Keynes argued that the application of statistical methods to the analysis of investment behaviour (the example presented by Tinbergen, 1939, to clarify his method) was the least promising starting point as this is a case where those logical conditions were not even remotely met [. . .]
>
> This logical point may have been phrased obscurely, but it is worth further investigation for a better understanding of the foundations of econometric inference – even today.
>
> Keynes' argument [. . .] runs as follows. Statistical testing of economic theories is a form of induction. Induction needs a justification, an 'inductive hypothesis'. A 'principle of limited independent variety' may be invoked for this purpose, but has to be justified as well. The justification depends on the issue whether the number of causes or generators of phenomena of interest is limited or, to the contrary, unlimited or complex, and whether they can be known a priori. In cases where interdependent expectations are involved, this is not the case. Investment is an instance where such expectations matter more than anywhere else. Disregarding the issue by invoking a ceteris paribus clause (in a statistical model represented by conditioning and adding a stochastic error with known properties) is not warranted. In short, Tinbergen does not (and would be unable to) justify the inductive hypothesis. In fact, his inductive claims are very modest but, therefore, one may wonder what use his effort is, and this indeed Keynes does [. . .]

> Econometric modelling of investment has turned out to be a notoriously difficult issue in applied econometrics; even today [. . .] Keynes' objections were not altogether misguided.

It is ironic, in view of Keynes's criticisms of Tinbergen, that Keynes's macroeconomic theory came to play such a central role in the advancement of econometrics. Most of the new mathematical theorizing in economics was not easily applicable to real data in the 1950s, but Keynesian-style macroeconomic theories were, and they offered real opportunities for model building in econometrics. Indeed, the Keynesian theory was simply asking to be cast in an empirical mold (for example Klein, 1950).

These discussions Klein (1950, p. 1) credited with making it possible to 'formulate more sharply the structure of the economic system and thereby to gain added simplicity and accuracy not available to Tinbergen at the time of his work'. Model simplicity was obtained because Keynesian economics could be characterized in a very few equations involving macroeconomic aggregate variables, related in a realistic way, in contrast to Tinbergen's complicated business cycle theories and use of disaggregated business cycle variables. (Of course, later efforts to provide microfoundations would strip away the realism.)

As stated earlier, the major factor motivating modern critiques of the methodological foundations of structural econometrics is clearly exposed in Klein's (1985) passage quoted on p. 188. Causes are always harder to isolate than effects, but it is difficult to escape the impression that the proximate cause was the failure of large macroeconometric models to anticipate the surge of inflation during the 1970s. They failed just when they were most needed. Indeed, Pesaran (1987, p. 14) has argued:

> Mainstream macroeconometric models built during the 1950s and 1960s, in an era of relative economic stability with stable energy prices and fixed exchange rates, were no longer capable of adequately capturing the economic realities of the 1970s. As a result, not surprisingly, Macroeconometric models and the Keynesian theory that underlay them came under severe attack from theoretical as well as practical viewpoints.

With the significant changes taking place in the world economic environment in the 1970s, arising largely from the breakdown of the Bretton Woods system and the quadrupling of oil prices, econometrics entered a new phase of its development.

The arguments around the problems of the methodological foundations of structural econometrics over the 1970s and 1980s have changed in form but little in substance. Indeed, as Gilbert (1987, p. 44) observed:

> McCloskey is recognizing that the new econometric methodologies have both expanded econometric vocabulary and changed the syntax. It is arguable that certain innovations are more useful than others, but in this the economics profession, who, a generation back, discarded confluence analysis and seized upon regression, will be the judge.

Certainly, so far as the USA is concerned, the critical situation in the subject seems especially related to the persistent crisis in the economy and in economic policy in the 1970s. It seems that the problems of policy may be intractable or insoluble – given the present set of tools. This, in itself, denotes a crisis in the subject. But do we blame only the econometricians? Or also the economists who formulate their theories in an inappropriate way? Pasinetti (1982, p. 40) thought that:

> The fault is perhaps a bit on both sides. If the theorists are sufficiently induced in the direction of specifying their theories in such a way as to make them empirically testable, the econometrician should be able to tell them how to proceed, or at least contribute to tell them how to proceed.

Nothing approaching a coherent consensus may exist regarding the explanation of the changing behaviour of the economic system, and this may be said to constitute the source of the debate over the scientific foundations of structural econometrics.

But structural econometrics cannot, or should not, appropriately be required to undergo a crisis simply because of the inability of econometric models to suggest policies that could have taken Western countries in the 1970s out of the morass of stagflation, particularly in the medium term. It is because they have proved unable to explain what was happening. And this is partly, perhaps largely, a matter of theory, but it is also a problem of econometrics.

However, we shall argue, it is not because the founders' vision was wrong. Rather, we think that the version of Keynesian theory that underlay most work was seriously inadequate, and alternatives did not seem acceptable (perhaps partly for Cold War reasons). Mainstream Keynesian theory did not provide sufficient grounding in production and distribution, and marginal productivity theory was unsuitable; but Leontief's approach did not mix easily with Keynes's – and did not provide a theory of wages. The Phillips Curve lacked a theoretical foundation, and stood on weak empirical grounds, but seemed the only game in town. The standard Keynesian model provided too simple an account of money and finance, and it attempted to account for investment in a mechanistic way, in spite of Keynes's explicit reference to 'animal spirits'.[9] Money was exogenous in *The General Theory* (Keynes, 1936), though already endogenous in the *Treatise on Money* (Keynes, 1930). The influence of the interest rate on investment turned out to be hard to find; Klein's work in the 1940s and 1950s failed to find much evidence of it (Epstein, 1987, ch. 4). Many recognized these weaknesses, but as in the post-Keynesian literature today, alternative approaches seemed to be fragmented and did not come together to make up any generally acceptable viable alternative. (Not that most econometricians looked very hard for alternatives; only the neoclassical portion of the spectrum was visible.) Of course, later on, when mainstream model builders tried to provide rational choice microfoundations, it just made everything worse by undermining the genuine touches of realism in the Keynesian approach.

7.2 DEBATING THE FOUNDATIONS

We shall examine here briefly some of the typical arguments of the modern critiques. Although presumably these critiques are quite widely known, it is important to examine how far they sank in and whether they were taken seriously. On the other hand, has their effect been that of a shower of rain on ducks' backs? Inevitably, there has been some convergence in the different approaches, but it will be most useful to present them in polar fashion, so as to isolate their distinct features.

Here, we shall briefly summarize the achievements of structural econometrics as a background to guide the reader through the critical arguments of modern econometricians.[10] What follows will draw closely on Epstein (1987), starting from his claims that structural econometrics originated in the work by Tinbergen and Frisch in the 1930s on business cycles, later elaborated by the Cowles group.

The Cowles econometricians had all been extremely optimistic about the chances of success of Tinbergen's empirical approach to economics in the mid 1930s. Epstein (ibid., p. 223) argued that they believed that his single equation business cycle work 'could be adopted to yield decisive tests of different economic theories and to design effective policies for changing an economic system'. Structural estimation 'was an ingenious extension of standard statistical methods for the analysis of laboratory experiments'. The Cowles econometricians had remarkable successes in 'discovering the formal statistical properties of simultaneous equations models'. Early econometricians were confident that their models would 'indicate how to change the underlying structural relations of the economy to achieve economic and social goals'. Structural econometrics 'was expected to revolutionize the determination of economic policy'.

Haavelmo formulated Frisch's objections in terms of what is known as the simultaneous equations model. Epstein (1987, p. 56) pointed out that:

> Haavelmo was the first econometrician to rediscover the Working bias as a general phenomenon in any complete system view as a set of stochastic equations. Haavelmo's discovery added a shocking new level of complexity to Tinbergen's work since no one had previously seen any real kinship between market models and macro dynamics. Haavelmo took his example of the simultaneous equations bias directly from Frisch (1933). Clearly alluding to the uncritical OLS estimation of familiar demand and supply curves, Haavelmo demonstrated its inconsistency in the two variable model. The Haavelmo bias appeared to be of enormous importance for statistical inference.

Haavelmo gave the first modern treatment of the identification problem and simultaneity bias. Koopmans then generalized the approach.

Morgan (1990b, p. 153) observed that

> both structural equations and systems of equations predated Haavelmo's classic paper on simultaneity and probability of 1941 (published in 1943a and 1944), and correctly cited by Epstein in Chapter 2 for the discovery and solution of the problem of statistical simultaneity. These terms seems to have been generally accepted as equivalent only in the 1950s, and not in the earlier period.

Another important fact pointed out by Morgan (1990b, p. 153) is that, 'contrary to the impression given by Epstein, structural estimation is not necessarily, and was not historically, synonymous with simultaneous equations estimation'. Furthermore, his judgement is that 'the Cowles collection of ideas marks the end, not the beginning, of an intense period of methodological discussion and concept formation [. . .] [l]eaving until later the emphasis which should or should not be placed on the Cowles contributions compared to those of Haavelmo'.

The Cowles econometricians assumed 'the endogenous variables under study to be governed by an equal number of co-acting laws that operated in the aggregate'. The first problem 'was to determine whether these separate underlying laws were unambiguously recoverable from the observed data'. If so, one could consistently estimate the parameters by a variety of methods. 'The statistical methods developed by the Cowles econometricians were a major intellectual achievement in the advancement of the econometrics field'. They solved 'the basic identification problem and derived an asymptotic theory for statistical tests' (Epstein, 1987, p. 223).

Of course, many theoretical problems remained, as Koopmans especially well understood.[11] Epstein (1985, p. 3) argued that simultaneity actually introduced a whole new source of difficulty for empirical work. Exogeneity assumptions were crucial and not subject to test. Identification restrictions often had weak justification in economic theory. The FIML solution was extremely tedious. Moreover, it was obvious that their models had very few degrees of freedom with annual data and the small sample distributions of their estimators were known to be biased. No one knew this bias was preferable to an OLS estimate of structure.

Furthermore, as Epstein (1985, p. 3) noted,

other well known econometric difficulties were of long standing and not unique to structural estimation. Errors in variables were acknowledged but not explicitly introduced into the analysis. Annual observations were not enough and quarterly data were much desired. At the same time, serial correlation in the error term was known to affect identification and estimation, but no good test for it existed at that point, particularly for the difference equation model.

All these statistical problems were eventually solved, at least in principle. However, the problem for which there was no real solution was what Marschak[12] called the 'multiple hypotheses', now known as model selection. Indeed, Epstein (1987, p. 106) noted:

Multiple hypotheses was the problem that has since been renamed model selection. In 1946 Marschak called it the still remaining core of current criticisms against the application of statistics to economics. He properly distinguished between the algebraic problem of computing parameter estimates and the statistical problem of determining the true size of significance tests when many different models were tried with the same data.

Haavelmo's probability approach had led to the simultaneous equations estimator. But as a science of inference, it could only be complete if confidence intervals could be assigned to the estimates. The reality of empirical work often made the true size of such tests quite unknown. The difficulty of model specification at Cowles is summarized by Patinkin's (1948) study of US manufacturing:

I have no idea of the magnitudes of the confidence intervals for the parameters estimated. The basic estimating procedure leads me to believe that they are very large, for the basic procedure consisted of adding and subtracting variables until reasonable results were obtained. To handle this type of problem we must have a further developed theory of multiple hypotheses (quoted by Epstein, 1987, p. 106).

The Cowles econometricians could not see a good way out of this dilemma. To their lasting discomfort, it also became the statistical basis for Milton Friedman's critique.

> Friedman attended many of the Cowles Commission seminars during 1946–8 and continued to make the same criticism he had earlier leveled at Tinbergen. His question was: How does one choose a model, given that numerous possible models exist for the same period? Marschak once answered him simply that more data would reveal the true hypothesis. This argument assumes that the true model would be revealed by the accumulation of more data because the coefficients of false models must asymptotically approach zero. Friedman was not content with this and, not unlike Koopmans, made a plea for more published information on methods and models that proved had to be unsatisfactory. He understood the problem of multiple hypotheses but never suggested any theoretical approach to its solution (quoted by Epstein, 1987, p. 107).

Friedman's point is that the problem of multiple hypotheses made structural estimation, to use Epstein's (1987, p. 108) expression, a 'blind alley for empirical research'. Furthermore, like many other economists associated with the NBER in those years, Friedman's vision of the economy was radically different from Haavelmo and the early Keynesian econometricians. To illustrate Friedman's vision, it is worth quoting Friedman's description of the working of the multiplier from an essay that outlined a proposal for wartime tax policy:

> The increase in income that will accompany expanded outlays on armaments depends on a complex of interrelated factors, many of which cannot be observed before the event: who receives the increased outlays, how much of it they decide to save, [. . .] the reactions of consumers to price changes, the anticipations of consumers about future price movements and availability of supplies, the extent to which entrepreneurs try to expand their capital equipment, the costs that entrepreneurs must incur to expand output, their anticipations about future price movements and hence their inventory policy, the flexibility of wage rates and prices of other factors of production, the demand for credit, the policies adopted by the banking community. The expansion in output depends on the quantity and kind of unused resources, the mobility and transferability of these resources, the rapidity with which output can be increased, (and) the degree of competition (quoted by Epstein, 1987, p. 109).

In Chapter 11 it will be argued that these alleged difficulties are all manageable, drawing on fieldwork and conceptual analysis, and bearing in mind the distinction between reliable and volatile relationships. By contrast, as Marschak acknowledged, the problem of multiple hypotheses creates serious difficulties in econometrics. Epstein (1987, p. 69) explains that:

> Marschak was anxious to claim a special epistemological status for simultaneous equations estimation. He described it as the rational empirical approach: the only possible way of using past experience for current rational action (policy as distinct from passive prediction). This is certainly accurate provided

> the correct model is known. It does presuppose, however, that the available aggregate data actually represent homogenous underlying behaviour. The individual events in a rational economy [. . .] take place within a host of determining factors that are admittedly left out of the analysis. The aggregate error terms [. . .] do contain past experience that is relevant for rational action. But to elucidate these factors [. . .] would require an analysis [. . .] more the province of the historian.

The Cowles econometricians' empirical experience has been less satisfactory. Indeed, Epstein (1987, p. 3) has argued that their 'empirical work between 1946 and 1952 was no better than Tinbergen's in accurately forecasting beyond the sample period'. There is 'little evidence that large macroeconometric models estimated to date are consistently able to forecast out of sample better than very naïve alternative methods'. The Cowles econometricians 'often repeated goal of providing useful analysis of structural change seemed out of reach'. They came 'to believe that many of the basic problems with their models were the ones that the other critics – including Keynes – had emphasized in Tinbergen's results. They also felt the force of their own criticisms on poor identification and dubious exogeneity assumptions'. Furthermore, contending schools of macroeconomic theory have not yet been resolved by econometric studies.[13]

7.3 THE MODERN CRITIQUES

Five major contenders for the best methodology title may be distinguished. We shall refer to the 'Lucas', 'Sims', 'Leamer', 'Hendry' and 'Malinvaud' methodologies, named after those individuals most closely identified with the approach. Generally, each procedure can find its origins further back in time, and each is in fact the outcome of a long-term research programme that has many contributors apart from the above-named authors. We shall ignore some convergence in their views, and present them as sharp contrasts, highlighting their distinctive features. They will be discussed here in increasing order of their conformity to the original structural econometrics.[14]

7.3.1 Lucas

Let's start by presenting Lucas's economic vision. Vercelli (1991, pp. 128–9 and 136) wrote:

> Lucas sees Keynesianism as a temporary deviation from the mainstream scientific progress in economics – a pathological phenomenon for which he offers both an explanation and a remedy.

Lucas's economic vision goes back to the neoclassical picture in its monetarist form. The perfectly competitive market is considered the most desirable institutional arrangement for the economic system because of its presumed capacity for very rapid self-regulation and the supposedly related maximization of welfare. Any malfunctions of the market are ascribed to erratic Keynesian monetary policy [. . .]

Lucas never pretends to have anything new to say at the level of first principles. He only claims to have pointed out the path whereby Friedman's theses could be given an analytical basis firmly grounded on the principles of general economic equilibrium [. . .] Thus Lucas's original contribution is essentially at the methodological and analytical level. [. . ..]

Lucas defends himself by asking to be judged not on the realism of the hypothesis but on the usefulness of his assumptions.

Regarding the Lucas Critique, Vercelli (1991, p. 136) addressed

his [Lucas's] criticism of those large-scale econometric models, rightly or wrongly called Keynesian, which are used to predict the behavior of industrialized countries.

Let us begin with the pars destruens. As the structure of an econometric model consists of rules for optimal decisions on the part of economic agents, and since these rules vary systematically along with the structures of the main series which characterize the 'environment', it follows that any change in the rules adopted by economic policy, which in fact is interpreted as one of the main series mentioned above, will systematically alter the nature of the econometric models. For the purpose of short-term predictions, according to Lucas, in many cases this problem may prove to be of small importance, since significant changes in the structure of the series analyzed, including those directly controlled by economic policy, are not seen as very frequent events. On the other hand these considerations are held to be fundamental for problems involving evaluations of economic policy. In fact in this case comparisons between the effects of alternative economic policy rules based on existing econometric models cannot be considered reliable, whatever validity may be shown by these models in the period for which they were worked out, or in short-term predictions (Lucas, 1981, p. 126).

Hence if we wish to use an econometric model to evaluate the relative validity of alternative economic policies, we need a model capable of elaborating conditional predictions. That is to say, it must be able to answer questions like: how would behavior change if certain economic policies were altered in specific ways? This is possible only if the structure of the model remains invariant when the economic policy rules change (ibid., p. 220). This is the crucial point where the new classical economists' equilibrium method intervenes in an essential way. According to Lucas, only an equilibrium model in the sense first defined can show this type of invariance in the structure of the coefficients. By contrast, any disequilibrium model involving elements such as excess demand, involuntary unemployment, etc. – like the Keynesian models – is said to be inherently incapable of passing this type of test.

[. . .] Unfortunately his constructive proposal does not prove equally convincing: the new 'classical' equilibrium models, which are supposed to replace the 'Keynesian' ones, do not seem able to escape that criticism.

Vercelli (1991, p. 137–8) went on to argue that:

> Despite the importance of the matter at stake, Lucas's argument is surprisingly weak. He states the problem correctly, recognizing that the invariability of parameters in an economic model cannot be guaranteed a priori. Yet he considers it 'reasonable to hope that neither tastes nor technologies vary systematically with variations in countercyclical policies' (Lucas, 1981, p. 220). Economic reality, it would seem, can be divided into two levels: that of phenomena, characterized by erratic movements (disequilibria, in this peculiar sense) and by structural instability of parameters; and on a deeper and more basic level – one is tempted to say 'essential' level – characterized by the parameters of general economic equilibrium, which are considered structurally stable.[15]

Lucas's (1976) persuasive paper has convinced many economists that treating reduced forms as 'structural' in policy evaluation is a worthless procedure. The reason is that, once policy changes, rational agents will change their behaviour to conform to their new expectations, based on their appraisal of the policy in the light of their understanding of the economy. He suggests that, instead, we should estimate the parameters determining private sector behaviour as a function of the Policy Rule. He argues that, when we have done so, we shall find that the reduced form of our model will have changed in response to a change in policy rule. The stochastic properties of the new and the old reduced forms should be compared to find the effects of policy.

As Boumans (2001, p. 439) observed:

> The underlying idea, known as the Lucas Critique, is that estimated parameters that were previously regarded as structural in econometric analysis of economic policy actually depend on the economic policy pursued during the estimation period. Hence, the parameters may change with shifts in the policy regime. Lucas's 1976 paper is perhaps the most influential and cited paper in macroeconomics in the last 25 years and contributed to the decline in popularity of the Cowles approach. The Lucas Critique was an implicit call for a new research program. This alternative to the Cowles program involved formulating and estimating macroeconometric models with parameters that are invariant under policy variations and can thus be used to evaluate alternative policies. And the only parameters Lucas hopes to be invariant under policy changes are those describing 'tastes and technology'.

Malinvaud (in Holly and Phillips, 1987) commented on the Lucas Critique:

> I think the critique [Lucas Critique] is worth considering. Misspecification may indeed distort the result of any statistical procedure. And there are many reasons for misspecification in econometrics, unfortunately. Since the formulation of Lucas' critique, however, little proof has been provided that the

> misspecification that he pointed out played an important role. In principle, this critique is quite valuable and one should look at its importance in applied work. But I have the feeling that the critique has been overemphasized. There are many other reasons for misspecification that may be more serious than Lucas' critique. But that is an open question. It must be further studied. (Ibid., p. 283)
>
> [. . .] What I object to in some of the rational expectations literature is not the general equilibrium aspect of it, but simply the fact that it is applied to such simple-minded models that they bear little relationship to the facts – for instance, models in which it is assumed that the general price level is directly dependent, quarter after quarter, on the condition of equilibrium between the demand and the supply of money. I am sure that the phenomenon of determination of the general price level is much more complex and, therefore, we should have a good model of reality with its complexities before we fully apply the general equilibrium discipline. (Ibid., p. 285)

On the issue of the usefulness of large scale macroeconometric models, Malinvaud wrote: 'if we can get away without a large scale macroeconometric model, good! But if in order to take into account the important complexities of the phenomenon that you are analyzing you need a large scale macroeconometric model, then you should use it' (ibid., p. 285). Klein (in Mariano, 1987, pp. 441) does not agree that the big macromodels failed that badly:

> there is a perception that large-scale macro models have, in some sense, failed [. . .] [for example] to predict the effects of the supply shocks on the inflation of the 1970s, or the change in the structure of economy. I think [. . .] the economy didn't change in structure, but that exogenous inputs changed a great deal within a similar structure. Procedures that follow such a line can produce quite good results for the period. The different inputs account for the change in the industrial composition. [. . .] [A] scientific analysis of what happened in the 1970s [. . .] will find that the large-scale models were out in front in predicting recession and inflation. [. . .] [I]t will be very difficult to find an alternative approach that does consistently better.

Commenting in 1987 on the rational expectation approach, Klein (in Mariano, 1987) wrote:

> [. . .] some young macroeconomists [feel] that expectations have been the major element in causing the big swings in the economy. In my view, expectations are important. In the original inspiration of national macromodel building, expectations had always played a big part. [. . .] [A] new generation wants to treat expectations differently [. . .] [but] the only way to handle expectations satisfactorily is to explain people's expectation behavior by means of the best information we can get as to what expectations are and why they are as we measure them. I have great confidence in sample survey techniques [fieldwork], and we use them in our models. They have been investigated in Pennsylvania dissertations [. . .] the best way of dealing with expectations is to model stated expectations as they are ascertained in sample surveys. (Ibid., pp. 441–2).

In short, Klein supports fieldwork and he wants it integrated with other sources of information – that is, he wants it subjected to conceptual analysis.

> I am a proponent of combining different sources of information, and the information source in this case is cross-section data from survey investigations. They should be integrated within macro models, just as I think input-output systems should be integrated. I think that basically, we are information-short, since we can neither generate as much information as we want nor use the kind of information that we would like to have. We should milk whatever sources of information we can get, rather than transform or manipulate conventional time-series samples. The best way to deal with the problem is to enlarge the sample by getting new information. That's precisely what we are doing by using cross-section surveys taken from the people who create the expectations (Ibid., p. 442).

Klein went on to argue that

> the approach of rational expectations (or, better expressed, own-model-generated expectations) is asking too much of the data. It asks the data both to generate the expectations and provide the model estimates with simulation. That is overworking the data.
>
> Now, I think that for expectations – unless we get fresh information – we have an identification problem. From an econometric point of view, we used to characterize the problem of using the same data to estimate first the variance–covariance matrix of observations error and then coefficients based on these, as 'eating one's tail' – to make the sample try to do both things. I think that the people who want to use the sample to generate expectations and then estimate the model are also eating their own tails. They are not getting new insights as to how expectations are formed – they are assuming that their methodology is correct without validating that assumption.
>
> [. . .] little attention [is] paid to whether they are right or not, only to the fact that it is a procedure that makes expectations endogenous. I deplore the willingness to make very strong assumptions about the way expectations are formed, simply for the sake of getting very definite analytical results [. . .] people want manageable problems, problems that can be worked on with their own signature on authorship. It is very important for [. . .] academic [. . .] rewards. (Ibid., pp. 442–3)

Lucas's (1972) article and the one from 1976 go together. This is recognized by Lucas himself, particularly insofar as expectations plays an essential role in his 1976 article, in which the Phillips curve was used as an example. He argued that econometric models, once their reduced forms had been estimated, were conventionally used either for purposes of forecasting or for purposes of exploring the variants of economic policy. In his 1976 article, Lucas radically criticizes this methodology.

Dagum (1986c) claimed that Lucas's argument is as follows: the parameters used were obtained through estimations pulled from time-series data during a period in which certain choices prevailed in economic policy, and hence led to certain very specific decisions. The expectations of agents and, as the case may be, their behaviours, reflected those choices. Therefore, in the future, and even more so in the case of the model's use for purposes of simulating economic policy measures (since expectations and behaviours will change due to supposed new economic policy choices), the 'real' values of the structural and reduced parameters will change as well, and the values estimated on the basis of prior data will be found to be erroneous. As a consequence, the lessons learned from these exercises will be methodologically unfounded. Lucas's critique is beyond reproach, if not its effective impact, and explains that, at least in academic circles, modelling and forecasting techniques have evolved considerably, entailing the discrediting of macroeconometric models elaborated through traditional methods.

Although the rational expectations hypothesis (REH)[16] – agents expect what the true model predicts – was advanced by Muth in 1961,[17] it was not until the early 1970s that it started to have a significant impact on time-series econometrics and on dynamic economic theory in general.

What brought the REH into prominence was the work of Lucas (1976), Sargent and Wallace (1975), and others on the new classical explanation of the apparent breakdown of the Phillips curve. The validity of the Phillips curve was at the centre of the macroeconomic controversies of the late 1960s. It was simultaneously called into question by Friedman (1968)[18] and Phelps (1967; 1968; 1970).[19] The idea that a real magnitude (unemployment) could depend upon a nominal magnitude (the inflation rate) was in effect in contradiction with the conception of the neutrality of money. In order to reconcile the empirical result (the decreasing relation between these two variables) with this stylized fact, both of them advanced the concept of a natural unemployment rate and challenged the idea that there could be a long-term arbitration between inflation and unemployment.

On the ontological level, one could argue that in the rational expectations (RE) vision there are neither behavioural nor technological nor institutional 'rigidities' – one person's rigidity may be another's stability! – nor is there uncertainty about the future. This suggests that the theory must be reconceptualized to understand capitalist development.

As we noted, Klein (in Mariano, 1987) criticized REH for being mechanical and observed that the best way to model expectations was to base them on fieldwork:

> The present generation of economists is not leading us in any fruitful direction for studying expectations. Expectations are endogenized and introduced in a

> very mechanical way. This method has very little behavioral content and very little informational content [. . .] the best way is to go to the source of expectations and find out [through fieldwork] what people actually expect or anticipate and to endogenize that within the framework [. . .] That means that we should integrate sampling investigations on subjective expectations together with market and accounting data for the economy and treat that as one big system with the subjective expression of expectations as endogenous variables. [. . .] This approach will have true informational content because we will be trying to model people's stated expectations in a realistic way.
>
> We must take account of the life of these expectations. In fact, it is rather short, and that means we have to have repeated subjective observations. I find the European business test surveys, the surveys of consumers, the various surveys of inflation, the statistics on orders, the statistics on housing starts, and all the things we call anticipations variables to be very important. They need to be integrated directly into the models. (Ibid., p. 420)

Klein (ibid., pp. 420–21) went on to argue the need for a behavioural basis for distinguishing anticipated and unanticipated components, while agreeing that

> the modern discussion of expectations has one useful piece of scientific content, namely, that expectations are based on the latest information that is available to agents. We have people's stated expectations, and we simultaneously know the state of the stock market, the state of the bond market, the movement of inflation rates, and the movement of monetary instruments. We should relate expectations to such pieces of information as are available to everyone at the same point of time [. . .]
>
> [. . .] in many present treatments [. . .] people try to separate out what is anticipated and unanticipated just on the basis of indirect observations of data and the imposition of assumptions on those data without having any direct behavioral basis for saying we have observed something that is either anticipated or not anticipated [. . .]

The discussion can be illuminated by comparison with the Cowles treatment of errors in variables:

> some of the issues are similar to our treatment of the errors of measurement in econometric modeling. We had long discussions about this problem at the Cowles Commission. We decided that we would not base the probability structure of our models primarily on the distinction between the true value and the observed value of economic variables. That distinction involved the generation of errors of measurement. Unless we have special information about how accurately something is measured in a relative sense, we will not be able to implement the theory of inference based on measurement error. But we lack such information. The fact that we lack that crucial piece of information has meant that there has been a lack of identification in systems, and many estimation methods break down directly.
>
> Now we find the present generation of econometricians trying to do the

impossible, trying to separate something they don't observe into anticipated and unanticipated components. These are subjective components, and we have no confidence that they are getting sensible answers. There is a complete unwillingness to confront expectations variables with what people say their expectations are. That is our only shred of observational material that can be brought to bear on the solution of the problem. (Ibid., pp. 420–21)

Furthermore, Klein argued that the principal idea that was impressed on everyone at the Cowles Commission was that

structural models must have a theoretical base in economics. We worked on the neoclassical specification of models. We worked on the aggregation problem, we worked on the market-clearing problem, and we recognized that all modeling should have a theoretical base [. . .]

In later years, I think some people became slaves of the neoclassical behavioral formulation without taking account of the aggregation problem. In their fear of being 'ad hoc' they chose theoretical lines which were not always well conceived. Many of the things that people thought were theoretical were not good if you take into account the aggregation problems that were involved. In my own approach, I have insisted that there must also be a theoretical basis for equation specification, and there must also be a close correspondence with reality. There must be forecasting tests. I think that many of the present generation of researchers are not careful with forecasting tests and are not careful with reality, but are over-impressed with pure theory-spinning that isn't going to lead to significant improvements in the system.

[M]acroeconomics [. . .] has taken what seems to me [. . .] a fruitless turn [. . .] imposing [. . .] predictive testing and economic theory, under the constraint of aggregation, is a better way to proceed. [. . .] [A] system that is not well conceived will not stand up under severe forecasting tests. It might stand once, it might stand up twice, but if we replicate the forecasting exercise often enough, frailties will show through. The only real thing we have to go by is predictive testing, and it takes a long time to build up a satisfactory record. (Ibid., pp. 416–17)

As Klein pointed out above, we certainly think that the specification of a model should give more attention to aggregation issues. On the aggregation problem, Malinvaud (in Holly and Phillips, 1987, p. 285) wrote:

I think that these aggregations issues are of many different types and the important issues of aggregation vary from one case to another. I do think that people are not spending enough time in dealing with these aggregation issues. There is indeed a risk that the use of representative agent behaviour in the construction of models is a source of some misspecification. But it can be and, therefore, it should be more seriously looked at. Now, as to the estimation of the 'deep structural parameters,' of course, one should try to estimate the deep structural parameters and not the surface structural parameters. It is always better to go deeper. But my own experience as an economist is that very often we cannot pretend to go very deeply into such matters. Again, what I object to

> in the expression 'deep structural parameters' in certain rational-expectations models is that these rational-expectations models do not contain deep structural parameters. They contain parameters that the author of the model has classified as deep structural, but really they are not. They are not even good parameters of a model, whether at the surface or deep into it.

Malinvaud went on to argue that:

> I should not give the impression that I am against the study of rational expectations models. In particular, a number of theoretical issues are worth looking at with rational expectations hypotheses, or more simply with perfect foresight hypotheses. Moreover, the rational expectation hypothesis is important in the study of a number of problems, such as problems of information in economics and many other issues. (Ibid., p. 286)

Here Malinvaud is in full agreement with Klein regarding the importance of expectations and the inadequacy of much recent work. The issue, to paraphrase Klein, is that the rational expectations approach is 'asking too much of the data. It asks the data both to generate the expectations and provide the model estimates with simulation' (Klein, in Mariano, 1987, p. 442). As suggested by Klein (ibid., p. 419), 'the best way is to go to the source of expectations and find out [through fieldwork] what people actually expect or anticipate and to endogenize that within the framework of models'. But the results of fieldwork have to be brought into the analytical framework: 'That means that we should integrate sampling investigations on subjective expectations together with market and accounting data for the economy and treat that as one big system with the subjective expression of expectations as endogenous variables' (ibid., p.419). The fieldwork has to be subjected to conceptual analysis.

Furthermore, the well-established socioeconomic facts of the inertia to change, the R&D lag structure, the partial adjustment of institutions and imperfect information are ignored, placing RE models outside history. Nell (1998a) portrays development as a cumulative process in which economic institutions and technologies evolve in a single, mutually engendering dynamic. Simply put, theory must have the task of understanding this cumulative process and should assign the market the role, not of allocating resources, but of generating forces that bring about innovations. The latter, in turn, change the way markets work.

According to the REH, policy will have no (equilibrium) real effects, not even in the short run; the Phillips curve is always vertical. Of course, as Phelps and others observed, this depends on everyone having the same model of the economy.

Moreover, it can be argued that the claim that the reduced form varies with the policy rule is not as dramatic a critique as often thought. Like the standard simultaneous equation methodology, the rational expectations methodology, which attempts to meet the Lucas Critique, is not contradictory to loosely restricted empirical modelling of the traditional kind. It does not imply that such models give a false description of historical data, only that the interpretation of them that yields policy prescriptions should be different from that implicit in standard econometric estimation. In principle, structural parameters are functions of reduced form parameters, both in standard simultaneous equation methodology and in rational expectations methodology. On either approach, loosely restricted time series models may provide a standard of fit and a descriptive guide to the formulation of good simple models whose parameters are structural.

This conciliatory note would be a comfortable one to end on. However, it can be argued that the RE critique of econometric policy evaluation has sent the profession down a false trail. Indeed, Pesaran and Smith (1992, p. 9), noting that an 'important role of economic theory is to produce general, unifying insights' that promote our understanding by making well-chosen abstractions from the complex mass of details which constitute reality (and thus opens rather than closes the door), go on to contend that

> [c]ontinued adherence to the Rational Expectations Hypothesis is now closing rather than opening doors, inhibiting for instance the study of how agents' learning processes may form part of a history-dependent process which allows a determinate equilibrium to be singled out from the multiplicity of the equilibria which obtain in general equilibrium models.

From the methodological point of view, Dagum (1986c) has argued that by recognizing the essential role of expectations in the dynamics of real economic processes, RE models start to recognize the historical dimension of economics. However, the specification of a set of a priori assumptions to account for the formation of expectations, places RE models entirely outside history. Moreover, the REH does not stand statistical tests of validation and, in fact, it is refuted by the performance of observed economic processes. Furthermore, Dagum (1986c) argued that those entertaining some doubts about this statement are invited to perform a statistical test on the difference between the forecasts of the most prestigious and best informed (having the best information set) econometric and forecasting research firms in the USA and the observed outcomes, in particular for the quarterly results of 1981 and 1982.

The RE critique provided a good example to illustrate how nearly any claim to have found a probability model, which might objectively be claimed to be structural, is likely to prove false if faced with a drastic policy intervention – or, indeed, any other kind of major change. Policy changes will certainly affect some kinds of economic behaviour. But the supporters of the REH have provided little analysis of which agents, and which of their decisions, will be affected by what kinds of policy changes. In practice, changes in corporate taxes are unlikely to have much impact on the composition of household budgets, for example.

But the positive programme of RE econometrics, namely to estimate identified, structural models to be used in predicting the effects of change in policy rules while taking account of induced changes in expectational mechanisms, just reproduces the main faults of standard econometric policy in exaggerated form.

By contrast we might argue that the major defect in standard econometric policy evaluation has been that it took insufficient account of the limited range of its data, and the possibility of exogenous shocks outside this range, so that, for example, in exercises applying optimal control theory, it claimed to predict the effects of policies that lay far outside the patterns of historical experience. However, most practical applications largely avoided these problems, since econometric policy modelling was (and still is) ordinarily used to extrapolate the effects of policy paths that have historical precedents into the immediate future.

7.3.2 Sims

Let us start by presenting Christopher Sims's vision. Gilbert (1987, pp. 17–18) wrote:

> Sims' (1980a) critique of standard econometric practice is based upon the assertion that the restrictions imposed in conventional macro-econometric models in order to obtain supposedly structural representations are incredible. This is partly because many sets of restrictions amount to no more than normalizations together with shrewd aggregations and exclusion restrictions based on an intuitive econometrician's view of psychological and sociological theory; because the use of lagged dependent variables for identification requires prior knowledge of exact lag lengths (and orders of serial correlation); and partly because rational expectations imply that any variable entering a particular equation may, in principle, enter all other equations containing expectational variables.
>
> Since the simplified and supposedly structural equations estimated in conventional models are incredible, the appropriate response, Sims argues, is not to simplify. In place of these erroneously simplified models, Sims proposes that we confine our attention to reduced form models for all variables which cannot be established (through Granger causality tests) to be strictly exogenous. These systems are to be estimated as vector autoregressions (VARs).

Spanos (2010, p. 237) made similar comment:

> Sims argued that substantive information in macroeconomics is often 'incredible', and 'cannot be taken seriously' (Sims, 1980a, pp. 1–2). Indeed, the only such information needed for empirical modeling is some low level theory as to which variables might be involved in explaining a certain phenomenon of interest, say *Zt*: = (*Z1t*, . . . , *Zmt*), and the modeling should focus on the statistical information contained in *Z*. He proposed the Vector Auto-Regressive [VAR(p)] model.

Furthermore, Spanos (2010, p. 236) observed that 'the first credible challenge for the Cowles Commission pre-eminence of theory perspective came in the form of a persistently inferior predictive performance of its multi-equation structural models when compared with the single equation (theory-free) data-driven model, known as the Autoregressive-Integrated-Moving Average [ARIMA(p,d,q)] model'.[20]

Epstein (1987, p. 6) argued that:

> Sims (1980a) doubts that identification of simultaneous behavioural equations in macroeconomics is practicable. Granger (1969)[21] denies that economic relations are really governed by simultaneity (see also Wold, 1954). Both authors refuse to allow the concept of an exogenous variable into their work. Their models mimic time series methods without pretending to have too much prior economic theory.

Here we shall focus on Sims's VAR approach.[22] Sims (1980a) differed vigorously from the Cowles Commission tradition. As noted by Epstein (1987, chapter 7, p. 205):

> He [Sims] wrote in recognition of a 'deep vein of scepticism' about the large models that he felt had surfaced within the academic economic community. He asserted that as representations of economic behaviour their 'claims for identification cannot be taken seriously'. He denied that their identifying restrictions were derived 'by invoking economic theory'. These objections seemed equally intended for the ostensibly Keynesian and the strictly monetarist econometricians. The implication for econometric modelling in his view was simple. By rejecting all identifying restrictions as 'incredible', all variables should then appear without lags in all equations. The category of exogenous variables does not exist for models constructed on this basis. With no prior information as to lag lengths, only a set of reduced form equations with identical lags for all variables could be estimated. He called this alternative style of econometrics a vector auto-regression or VAR.

Pagan (1987, p. 14) argued that Sims resurrected

> an old article by Liu (1960), which insisted that it was incredible to regard 'B' and 'C' of the system of structural equations ($By_t - Cx_t = e_t$) as sparse. The

argument touches a chord with anyone involved in the construction of computable general equilibrium models. If decisions on consumption, labor supply, portfolio allocations, etc. are all determined by the same set of variables, consequently, theoretical considerations would predict no difference in the menu of variables entering different equations, although the quantitative importance of individual variables is most likely to vary with the type of decision. Prescription of the zero elements in B and C therefore involves excluding variables with coefficients close to zero. In this respect, the action is little different to what is done in any attempt to model reality by capturing the major influences at work.

The approach advocated by Sims and his co-researchers, to paraphrase Pagan (1987, pp. 14–19), departs from the Cowles Commission methodology in two important respects. First, 'it denies that a priori theory can ever yield the restrictions necessary for identification of structural models', and, second, 'it argues that for forecasting and policy analysis, structural identification is not needed!' Accordingly, this approach, termed by Cooley and LeRoy (1985) 'atheoretical macroeconometrics', maintains that 'only unrestricted vector-autoregressive (VAR) systems, which do not allow for a priori classification of the variables into endogenous and exogenous, are admissible for macroeconometric analysis' (ibid.).[23]

Epstein (1987, ch. 7) argued that the VAR approach represents an important alternative to conventional large-scale macroeconometric models and has been employed with some success in the area of forecasting. Whether such unrestricted VAR systems can also be used in policy evaluation and policy formulation exercises remains a controversial matter. Epstein (1987, ch. 7, p. 206) wrote:

Sims does not give statistical evidence to support his statement that many identifying restrictions in macroeconomic models are invalid. However, he does give a theoretical example of a particular continuous time model that is formally unidentified if estimated with discrete data, a demonstration not greatly different from the cases of underidentification discussed by the Cowles commission. But in the absence of such rigorous economic theory to establish the unidentifiability of parameters, the identifying restrictions might better be viewed as the definition of the economic theory embodied in the model. As such, the econometrician should view them as hypotheses to be tested as critically as available data and statistical techniques will allow. At the same time it does not follow that putting every variable in a structural equation, as in VAR, is more acceptable theoretically than leaving a given one out. The criterion in each case is whether a reason can be supplied for the particular decision.

Furthermore, Klein (in Mariano, 1987, pp. 417–18) commented on the VAR methodology as follows:

To some extent vector autoregressions are associated in my mind with the concept that Koopmans introduced, 'Measurement without Theory.' I think

> that they are eventually going to be misleading from that point of view. I look at the problem in the following way: When we first put our models together, people said that the relevant test should be the random walk, or today equals yesterday. Then, after that became a not very severe test – after it was shown that that was not a good standard – people went on to the next more sophisticated criterion, today's changes equal yesterday's changes. Then they went to autoregression, then they went to ARIMA models; and now they have gone to vector autoregression. So I regard vector autoregression as being in this sequence of moving from the most simplistic model of testing, which we call the naive model, to a semi-naive model which is, in the present state, a vector autoregression. In all these tests we have noticed that the systems that represent 'measurement with theory' break down at turning points; they break down under unusual circumstances and they cumulate error fast. The vector autoregression is the first of such systems that doesn't seem to cumulate error very fast, a least at this stage of the process.

He (ibid.) went on to argue that

> the real test will come when we watch a vector autoregression try to handle something as complicated as the oil embargo of 1973, the Iranian revolution of 1978–1979 or what I call the Nixon NEP program in 1971–1972. My prediction is that it won't be very useful when we need it most. Our structural systems, I think, served us well on each of those occasions. And I think that some future critical situation will be the true point of distinction between the two. Under present conditions, given the period of time during which we have looked at the performance of vector autoregression and the macromodels that we presently have, I would conclude that in the short run they perform very nearly the same. Vector autoregression holds up better for the longer term than any of its predecessors – the ARIMA, the simple no-change, and so on. I think that we have to wait until we see a more crucial test, and I think the crucial test will not be so kind to the vector autoregressions. There are some parts of the vector autoregression structure that I find curious or bothersome. One is that all variables are endogenous. I think that is not a useful way to structure a system. Secondly, I haven't really gotten all the details, but I believe that not all the terms in the vector autoregression are used, and some zeros are, on a judgmental basis, placed here and there until the model is fine-tuned. I would have to look more carefully at the placement of these zeros before making further judgments.

The VAR methodology wouldn't be Klein's choice for the system to be used even if in the end

> we deliver equal predictive performance from vector autoregression and from the large-scale system, I would say that I prefer the large-scale system because it has more informational content. It handles more variables and it provides more information, and that is what users want. The criterion that I use for model selection is to say: use the biggest and most detailed system that can be well managed by human agents, together with our computers, and not lose on the accuracy of some of the principal aggregates, and that can deliver these additional pieces of information. (Ibid., p. 418)

Klein (ibid., p. 418) argued that VAR is not a 'bad system as a standard for comparison on some main aggregates, and that would be its main use'. The early work of Klein clearly had to deal with the characteristic uncertainties that confront macroeconomic model builders in this regard.

Malinvaud (1988, p. 208) argued that:

> More particularly pointed to macroeconomic modeling, the critique raised by C. Sims also concerns the specification stage, which is said to often involve 'incredible' hypotheses that unduly simplify what are known to be complex phenomena. One would overstate the critique if one forgot that, in all fields of science, useful hypotheses often are simplifications. But the critique deserves serious consideration.
>
> The positive proposals of C. Sims are also interesting in the present discussion. They suggest that econometricians should avoid introducing a priori restrictions and should concentrate their effort on a descriptive unconstrained study of the multidimensional stochastic process ruling the evolution of the main economic variables. Thus, these proposals recommend an approach that has much in common with the National Bureau empiricism and with R. Frisch's attempts at describing geometric properties of sets of points in the sample space, attempts that were considered as rather uninteresting by T. Haavelmo.
>
> Some of the writings of C. Sims and other econometricians working with him seem to argue for a complete replacement of the traditional macroeconometric methods by the new multidimensional time-series analysis they are promoting. Accepting to go that far would be tantamount to rejecting the probability approach. I had occasion to explain elsewhere why the arguments in favor of such a revolution cannot be accepted.[24] But, seen as providing a complement to present practices, the proposed analyses are quite valuable.

Furthermore, Malinvaud (1988, footnote 33, p. 208, italics added) pointed out that:

> One might, however, reflect on the lack of consistency between the various critiques now attacking macroeconomic practice. *While C. Sims argues for a more careful reference to the facts*, other economists are strongly stating sweeping conclusions based on very simple models that are much more incredible than current macroeconometric ones. What should we think, for instance, of studies of the role of monetary policy for economic stabilization when it is assumed that the price level instantaneously adapts to what is required for equality between the demand for money and the money supply?

To conclude our discussion let's quote Epstein (1987, p. 6):

> The principal difference with the Cowles approach is that it does not seem likely to develop a reliable theoretical base for the future. Perhaps as a corollary, these investigators do not emphasize that statistical inference in their work is highly contingent on the adequacy of asymptotic approximations to the true finite

sample distributions of estimators in models with lagged dependent variables. They retain the use of linear difference equations, similar to Tinbergen's final forms, but seem less concerned with the problem of model selection – or even hypothesis selection – in this framework than many other schools of econometric thought. The approach tends to stress forecasting and prediction with little regard for changes in underlying economic structure.

7.3.3 Leamer

Let's start this discussion by introducing Leamer's vision. Leamer (in Hendry et al., 1990, p. 178) wrote:

> I've been trying to produce a complete and coherent framework for thinking about the issues of data analysis. There is a great discrepancy between the popular theories of statistical inference and actual practice. What I've tried to do is to think about that discrepancy, to think about the issues raised by actual practice as compared to theory, and either to alter the theory to conform with those aspects of practice that I thought of as desirable or sensible, or alternatively, to make recommendations as to how practice ought to be altered, ought to be policed, and made more effective.
>
> Ultimately, I would measure success and failure by my effect on how people analyze data. A substantial success is unlikely to occur in any short period of time.

He (ibid., p. 179) went on to argue that:

> The traditional theory hypothesizes rather simple settings that may have generated data set, and indicates how one ought to respond to data that are generated by those simple models. In practice, people don't commit themselves to the kind of simple assumptions that are necessary to carry out that response. In practice, analysis with economic data normally involves fitting many different kinds of models, selecting from among them, and trying to convince a reader that the selection process makes some kind of sense. These specification searches are not easily accommodating with the traditional theoretical structure.

Furthermore, Leamer (ibid., p. 180, emphasis added) went on to explain why as a profession we do not seem to value empirical work very highly:

> One of the reasons is that we don't have standards by which we can judge empirical work. When you see a theoretical exercise, econometric theory or economic theory, you can admire the intellectual contributions which were required to produce that work. When you see an applied piece of empirical research in economics, it is very difficult to know whether it is creative, correct, or compelling. I think that the reason for this is that we don't have clear standards. And I think both David [Hendry] and I are aiming in the same direction in the sense that we would like to elevate the discourse of econometric methods to the point where there would be clear standards. Then we could say: This piece of empirical work is really well done and it is very convincing.

Pagan (1987; 1995) argued that assessing Leamer's methodology is more difficult than for Hendry and others, because of the lack of applications of his ideas. Pagan (1987, p. 9) observed that 'it is hard to infer the general principles of the approach without any classic studies showing how the approach is to work in practice'. Indeed, he (ibid., p. 9), noted:

> despite this qualification [. . .] Leamer's methodology [has reduced] to four distinct steps:
> (i) Formulate a general family of models.
> (ii) Decide what inferences are of interest, express these in terms of parameters, and form tentative prior distributions that summarize the information not contained in the given data set.
> (iii) Consider the sensitivity of inferences to a particular choice of prior distributions, namely those that are diffuse for a specified sub-set of the parameters and arbitrary for the remainder. This is the extreme bounds analysis (EBA) of Leamer and Leamer and Leonard. Sometimes step (iii) terminates the process, but when it appears that inferences are sensitive to the prior specification this step is only a warm-up for the next one.
> (iv) Try to obtain a narrower range for the inferences. In some places this seems to involve an explicit Bayesian approach, but in others it seems just to involve fixing a prior mean and interval for prior covariance matrices. If the restrictions in this latter step needed to get a narrow range are too implausible, one concludes that any inference based on this idea is fragile.

Based on the four steps, Leamer's methodology may seem 'to be just another sect in the Bayesian religion' (Pagan, ibid, p. 9). Chapter 9 will discuss briefly the debate in statistics concerning Bayesian procedures. Pagan (1987, p. 10) argued that Leamer's methodology is an 'exercise in Bayesian econometrics and the fourth step constitutes clearly the expression of Bayesian philosophy in Leamer's work. However, it can be argued that Leamer has produced, particularly in step (iii), an approach that can be interpreted in a "classical" rather than Bayesian way, and it is this which one tends to think of as the Leamer methodology'. Pagan (ibid.) went on to argue that 'the reasons for such a belief lie in the advocacy of such ideas in Leamer's two most widely read articles, Leamer (1983), Leamer and Leonard (1983), although it is clear from Leamer (1985) that he now sees the fourth step as the important part of his analysis'.

Furthermore, Pagan (1987, p. 10) has argued that much of this is epistemological and he doubts if it will ever be resolved. In practice, the Bayesian approach in econometrics

> seems to have been based on the difficulties coming from a need to formulate high dimensional priors in any realistic model, nagging doubts about the need to have precise distributional forms to generate a posterior distributions, and

> the fact that many dubious auxiliary assumptions are frequently employed (e.g. lack of serial correlation and heteroscedasticity in errors).

Although, in theory, perhaps all these doubts could be laid to rest, in practice the computational burden would become increasingly heavy (see Kennedy, 2003, ch. 13).

Leamer is not opposed to the estimation of structural models but emphasizes the even greater problem of model selection compared to the single equation context. His extended bound analysis finds the greatest variation in estimated parameters of interest obtainable from the different possible models with a given data set. He has advocated Bayesian methods in this context that are intriguing but he seems to replace the problem of model selection with the choice of prior distributions. Leamer differs from the Cowles Commission's approach by not emphasizing the development of critical statistical tests to reduce the number of plausible competing models. His time-series work demonstrates this strongly. Many of his most provocative examples of ambiguity in econometric inference are drawn from cross section models where fewer diagnostic tests are available.

McCloskey (1983, pp. 494–5) highlighted the importance of Edward Leamer's (1978) book, whose very title is an outline of rhetoric[25] in econometrics: *Specification Searches: Ad Hoc Inference with Nonexperimental Data*. McCloskey (ibid.) argued that Leamer's book is 'an exception to the general neglect of rhetorical considerations in economics. Edward Leamer asks what purpose the workday procedures in econometrics may be serving. Instead of comparing them with a doctrine in the philosophy of science he compares them with reasons that ought to persuade a reasonable person, with what really warrants assent, with in short, economic rhetoric'.

Furthermore, Sims (1979, p. 567) argued that

> there is a myth that there are only two categories of knowledge about the world – 'the' model, given to us by 'economic theory,' without uncertainty, and the parameters, about which we know nothing except what the data, via objectively specified econometric methods, tell us. [. . .] The sooner Leamer's cogent writings can lead us to abandon this myth, to recognize that nearly all applied work is shot through with applications of uncertain, subjective knowledge, and to make the role of such knowledge more explicit and more effective, the better'.

Indeed, to paraphrase Leamer (1978), traditional statistical theory is helpful for studying experimental data but either misleading or nearly irrelevant for studying nonexperimental data. Leamer's (1994) book is an important contribution to this process.[26] He argued that the gap between econometric theory and practice is very large; however, the main objective of econometric theory is to improve the practice rather than to narrow the gap. From Leamer's perspective, economists are trained to use mostly formal thinking and formal data analysis. He observed that economists would do better and more relevant work if they used informal approaches.[27]

Finally, let us note Malinvaud's (1988, pp. 207–208) comment on Leamer's (1978) book:

> At the beginning of his book, *Specification Searches – Ad Hoc Inference with Nonexperimental Data*, Leamer correctly states that 'specification searching' often occurs, based on the same data to be later used for estimation, that this activity is not recognized by present teaching, and that it is worthy of a systematic study intended at improving its methodology. He then goes on to note that the classical model of inference is not helpful for understanding specification searches, whereas the Bayesian approach yields insights. The book is indeed a proof that this approach is appropriate for dealing with a series of long-neglected questions. The lag in methodological research, which Leamer identifies, may have something to do with the way in which the probability approach was put to work by the Cowles Commission, as well as by most mathematical statisticians and econometricians at that time. Reference was made only to classical principles of inference, as if they necessarily resulted from the probability approach, which of course was not the case.

Readers acquainted with Malinvaud's (1980, ch. 2) econometric textbook (*Statistical Methods of Econometrics*) will remember that Malinvaud strongly argues in favour of the probability approach. Indeed, careful reading of chapter 2 shows that after arguing for the explicit specification of a stochastic model formalizing the prior knowledge about the phenomenon, Malinvaud explains what a Bayesian inference would be; classical principles of inference are then presented as providing ways of avoiding the difficult choice of prior distributions.

7.3.4 Hendry

Pagan (1987, p. 4) argued that 'the closest of all the methods to the "old style" of investigation is the Hendry methodology.[28] It owes much to Sargan's seminal paper, but it also reflects an oral tradition developed largely at the London School of Economics (LSE) over the past two decades'. Hendry has pursued a long-standing interest in the history of econometric thought because of the insights provided by earlier analyses that were written when technique *qua* technique was less dominant[29] (see also Gilbert, 1986a; 1986b; 1989).

We start by quoting Hendry's conception of the role of economics in empirical modelling. Hendry's (2004, pp. 759–60) vision is eloquently described in his 2004 *ET* interview:

> I studied economics because unemployment, living standards, and equity are important issues – as noted previously, Paul Samuelson was a catalyst in that and I remain an economist. However, a scientific approach requires quantification, which led me to econometrics. Then I branched into methodology to understand what could be learnt from non experimental empirical evidence. If econometrics could develop good models of economic reality, economic policy decisions could be significantly improved. Since policy requires causal links, economic theory must play a central role in model formulation, but economic theory is not the sole basis of model formulation. Economic theory is too abstract and simplified, so data and their analysis are also crucial. I have long endorsed the views in Ragnar Frisch's (1933) editorial in the first issue of *Econometrica*, particularly his emphasis on unifying economic theory, economic statistics (data), and mathematics. That still leaves open the key question as to 'which economic theory'. 'High-level' theory must be tested against data, contingent on 'well-established' lower level theories. For example, despite the emphasis on agents' expectations by some economists, they devote negligible effort to collecting expectations data and checking their theories. Historically, much of the data variation is not due to economic factors but to 'special events' such as wars and major changes in policy, institutions, and legislation. The findings in Hendry and Juselius (2001a), and Hendry and Pesaran (2001b) are typical of my experience. A failure to account for these special events can elide the role of economic forces in an empirical model.

Much of Hendry's research has focused on constructing a unified approach to empirical modelling of economic time series. His 1995a book, *Dynamic Econometrics*, is a milestone on that path. General-to-specific modelling is an important aspect of this empirical methodology, which has become commonly known as the LSE or Hendry approach.[30]

Gilbert (1986b) argued that Hendry reflects the commitment of English econometricians to the primacy of hypothesis testing. He especially recognizes the often tenuous nature of exogeneity assumptions in solving identification problems. Although a critic, he is ambivalent. Like the Cowles Commission group, he sees econometrics as valuable for examining the testable implications of economically interesting theories. Hendry's vision of the role of econometrics in economics is very clearly stated in Hendry (2004, p. 760):

> Econometrics is our instrument, as telescopes and microscopes are instruments in other disciplines. Econometric theory, and, within it, Monte Carlo, evaluates whether that instrument is functioning as expected. Econometric methodology studies how such methods work when applied.

Hendry's vision of econometrics is based on what Jan Tinbergen called 'kitchen-sink econometrics', being explicit about every step of the process. The process could be described as follows: It starts with what the data are; how they are collected, measured and changed in the light of theory; what that theory is; why it takes the claimed form and is neither more general nor more explicit; and how one formulates the resulting empirical relationship and then fits it by a rule (an estimator) derived from the theoretical model. Next comes the modelling process, because the initial specification rarely works, given the many features of reality that are ignored by the theory. Finally, ex post evaluation checks the outcome.

Pagan (1987, p. 4) summarized Hendry's methodology essentially in four steps:

> (1) Formulate a general model that is consistent with what economic theory postulates are the variables entering any equilibrium relationship and which restricts the dynamics of the process as little as possible.
> (2) Re-parameterize the model to obtain explanatory variables that are near orthogonal and which are interpretable in terms of the final equilibrium.
> (3) Simplify the model to the smallest version that is compatible with the data.
> (4) Evaluate the resulting model by extensive analysis of residuals and predictive performance, aiming to find the weaknesses of the model designed in the previous step.

The idea of interaction between theory and data is present continually in Hendry's methodology. However, Hendry doesn't discuss how the theory could be modified in the light of the data. Pagan (ibid, p. 5) argued that it is usually assumed that 'theory suggests which variables should enter a relationship (unless there are good reasons for believing otherwise), and the data is left to determine whether this relationship is static or dynamic (in the sense that once distributed from equilibrium it takes time to re-establish it)'.

With the benefit of the accumulated experience in estimating structural models, Hendry advocates extensions to non-linear disequilibrium models using panel data. He approaches model selection by seeing if a favoured hypothesis that is statistically significant is capable of explaining competing results. This so-called 'encompassing principle' is in large part a strategy for dealing with multicollinearity that tests the significance of the components of alternative structures that are orthogonal to the hypotheses of interest. Hendry's methodological position is well argued but it does not seem consistently evident in his applied work.

To conclude this short note on Hendry's methodology, we should point out that he does not emphasize the lack of existing distribution theory for proper hypothesis testing with small samples. Moreover, he frequently remains with a single equation linear difference model that employs transformations akin to principal components to eliminate collinearity in the sample. It is not always clear what economic theories are being tested with this procedure.

7.3.5 Malinvaud

Malinvaud has made important contributions in three fields: microeconomic theory, econometrics and macroeconomics. These contributions[31] range from theoretical econometrics and macroeconomic theory to applied project evaluation, from the formalization of basic concepts in macroeconomics and microeconomics to analyses directed towards policy planning and assessment, and from examples of pure statistical and econometric methodology to empirical studies. As Alberto Holly and Peter Phillips (1987, p. 273, emphasis added) observed:

> Econometrics owes its essence and much of its ongoing vitality to the infusion of ideas from economic theory, the development of appropriate statistical methods, and the quantitative recording of economic phenomena. Each of these elements is [. . .] important. [. . .] we contribute to this evolution [. . .] all too frequently with little concern for holistic issues. Few economists indeed have the knowledge, the scientific expertise, and the professional experience to speak out with authority on the subject in its entirety [. . .] Even fewer command the respect of colleagues and authorities in neighboring disciplines, like mathematical statistics. [. . .] Edmond Malinvaud has stood out from the rest of our profession as a scholar who is uniquely qualified in this regard. His writings influence every field of economics. [. . .] The scientific standards of his work set an example to the entire profession. And his recent evaluations of scientific accomplishments in quantitative economics have brought unity and direction.

Here we wish to capture Malinvaud's thinking about the crisis in structural econometrics, focusing on the methodological problems that have not attracted the attention they deserve. In his Ragnar Frisch lecture in 1980, given at the Fourth World Congress of the Econometric Society, Malinvaud argued that following Frisch's inspiration, we should ask ourselves constantly if the econometric methods in current use are adapted to the needs of economic policy.

As Malinvaud (1981, p. 1363) observed:

> The research program launched by Tinbergen, and taken by Klein, has led to the current abundance of macroeconometrics models and to the fact that these models have become the main instrument for studying macroeconomic policy [. . .] However, this success no doubt explains the large number of critics who are today attacking a methodology which has become so commonplace; it gives to the critics in question a pertinence which evidently must not be underestimated and which evidently should on the contrary stimulate the search for the improvements, revision or reorientations, whose possibility can easily be imagined.

As we mentioned earlier, the initial effects on econometrics came through the notorious Lucas Critique (Lucas, 1976), which implied that changes in economic policy would induce structural instability in the Cowles model, and through Sims's (1980a) observation that many supposedly structural econometric models cease to be identified under rational expectations hypothesis (REH). The route is different, but again we have the strong implication that conventional structural econometrics estimates mis-specified relationships using techniques whose validity depends on correct specification.

Despite the Lucas (1976) Critique and Sims's (1980a) VAR methodology, which have had a profound influence in academic circles and economic research institutes in the USA, INSEE (under the directorship of Malinvaud) continued to play a major role in the development of structural macroeconometric models in France in the late 1970s and 1980s.

Malinvaud (1981) argued that Lucas's RE approach in applied macroeconometric model building has sent the profession down a false trail and represents a 'retreat' from the 'scientific standards' that the Cowles Commission sought to establish. Malinvaud's position is strongly advocated in Fair's (2004) book, *Estimating How the Macroeconomy Works*. Indeed, as Fernandez-Villaverde (2008, p. 694) observed:

> Fair (2004) does not assume that expectations are rational. In fact, it is one of the very first things. He tells the reader (as early as page 4): 'It is assumed that expectations are not rational'. Instead, he includes lagged dependent variables as explanatory variables in many of the equations and finds that they are significant even after controlling for any autoregressive properties of the error term. These lagged variables may capture either partial adjustment, effects or expectational effects that are not necessarily model consistent, two phenomena that are hard to separate using macro data.
>
> Fair recognizes that his choice puts him in the minority in the profession, but that he sees several advantages in it. First, RE are cumbersome to work with. Without them, one can safely stay within the traditional Cowles Commission framework, since the Lucas critique is unlikely to hold. Second, Fair does not consider it plausible that enough people are so sophisticated that RE are a good approximation of actual behavior. Third, and related to the previous point, he finds his estimates a considerable amount of evidence against RE and little in its favor.

However, commenting on Sims, Malinvaud (1988, p. 208) wrote:

> C. Sims' writings must properly be understood, as a plea for a more conscious exploratory analysis of the data, before any model is specified. They then transpose to econometrics recommendations made for all fields of application by some mathematical statisticians who, following J. Tukey, now promote all kinds of unconstrained data analysis.

Some aspects of macroeconometric modelling have been somewhat neglected: the study of aggregation, the taking into account of non-conventional macroeconomic variables, and the need for more systematic testing. It is this macroeconometric research program which one tends to think of as the 'French Example'. Econometricians working with macro models must use variables representing expectations, attitudes, disequilibria and tensions, and for all these variables data are increasingly available. It can be said that Malinvaud, through his research career, has advocated that research into macroeconomic modelling should devote a part of its work program to the study of the introduction on these new variables.

Faced with the theoretical insufficiencies of macroeconometric model building, economists must obviously wonder how much faith we can put into macroeconometric models when they claim to describe the consequences of alternative choices concerning economic policy. Malinvaud (1981, p. 1368) argued that:

> A nondogmatic study of the diverse questions which economic policy in fact asks during its elaboration suggests on the contrary that the econometric models now in use are still too simple in many ways despite their large size.
>
> This is true even when it is a question of orienting monetary or budgetary policy in the most traditional sense; it is even truer for prices and incomes policy or for such global decisions as those concerning the shortening of the work week or the deregulation of foreign exchange.
>
> My own thoughts often led me to focus my attention on the representation of disequilibria and to be sensitive to the fact that our models are groping about for specifications which would be suitable in this respect. The presence of indicators of tension on the various markets is certainly frequent, and their role seems to be correctly rendered in most cases, although it is rarely put forth in the description of the models.
>
> Disequilibria concerning quantities are thus taken into account; but price disequilibria do not have the place which should be theirs. Ideas such as those of profitability or competitiveness certainly have an explanatory power when it comes to evaluating the dynamism of an economy or of one of its sector: creation of new firms, investments, employees recruiting, success in exporting or in domestic market obviously depend on this. Such notions are rarely put in the forefront by constructors of models. No doubt they haven't been stressed enough recently in economic theory, and the quantitative importance of their effects has not been brought out by the econometric work done on the behavioral laws in question.

Epstein (1987) argued that Malinvaud's approach to economic model building is perhaps the closest one to Cowles econometricians, partly reflecting his early participation in the Cowles project.[32] Like Haavelmo and Klein, he is concerned that theory should provide guidance, and to do this effectively, it has to be realistic. (Because the underlying theory is so unrealistic, he dismisses the Lucas Critique as irrelevant. Certainly, he agrees, policy changes – and other political changes, too – may have impacts on structural parameters that could change the character of the model. But the Lucas approach will not help at all in studying this problem, because it is based on hopelessly inappropriate assumptions.)

To grasp Malinvaud's view of the debate over the methodological foundations of structural econometrics it is important to recall here the economic and historical context of France in the 1970s and 1980s. Malinvaud has devoted a great deal of his work on disequilibrium unemployment. In the late 1970s, when he started working on this subject, the unemployment rate in France was 5 per cent; it rose to 10 per cent in the 1990s. Since the publication of his (1984c) book, *Mass Unemployment*, unemployment has increased in France and much of Europe.

We quote Malinvaud's reflection 20 years later:

> I was pessimistic in the years from the first oil shock to the middle 1980s for reasons that some of my writings of this period aimed at explaining. But I did not expect that mass unem- ployment would last so long. This is why I felt particularly desperate when, in summer 1992, I realized we were heading to a new wave of increase in unemployment. This is why Jacques Dreze and I then tried to instigate a reaction of European academic economists. We eventually published, with a few colleagues, our article 'Growth and Employment: The Scope for a European Initiative'. Well, it may still be too early for being affirmative about a worldwide recession. But there are two serious causes for worry: the situation in Japan and what I still hold to be an overvaluation of assets and of the long-run sustainable profit rates. (Krueger, 2003, p. 191)

Malinvaud's influence on the subsequent generation of French economists and econometricians is another illustration of his position. The driving force behind his economic research programme is the integration of economic theory, econometrics and statistics as a foundation for macroeconometric model building in which fieldwork plays a crucial role. Malinvaud has continued to produce an approach to macroeconomic model building that can be interpreted as belonging to the Cowles methodological structuralism. During Malinvaud's tenure at INSEE (1974–87), INSEE's models (particularly its best known macroeconometric models in the late 1970s and early 1980s, namely METRIC and DMS)[33] have showed that the building of institutional reality into a priori formulations of economic relationships (through fieldwork) and the refinement of basic data collection have contributed much more to the improvement of empirical econometric results than have more elaborate methods of statistical inference.

Furthermore, Holly and Phillips (1987, p. 281) pointed out that Malinvaud has not only emphasized the importance of theoretical models and statistical inference for serious and good empirical work, but he has also shown interest in a variety of methods of data analysis and the advantages of these methods for exploring the data in econometrics. Malinvaud wrote:

> It is at the exploratory stage that the method of data analysis can be used because they are essentially of a descriptive nature. I would be inclined to distinguish between econometrics and socioeconomic research. In econometrics most of the time we already have some definite ideas as to the specification of the data generating process, and therefore classical inference methods are appropriate. But very often in socioeconomic research, we face questions that are still of a purely descriptive nature, with little concern for generality. Then, in that case, data analytical methods are often preferable to the assumption of a model that has no particular foundation except that it is convenient and that you have software for dealing with data in the context of this model.
>
> Now it is true that it is also good to look critically, even in econometrics, at the results that have been obtained by traditional econometric methods, to see whether they will stand up to the test of rougher data analysis methods. Because if they don't, then there is cause for concern that there may be something wrong with the specification. Also there are cases in which we are unable to come up with a precise specification. (Holly and Phillips, 1987, p. 281)

Malinvaud retains his commitment to developing econometric models that can assist in devising economic policies. His critique combines a noticeable ambivalence about the economic relevance of the simultaneous equations model, while demanding the highest standards of mathematical rigour in its application. On the methodological level, Malinvaud, like the Cowles Commission group, stresses the need for a priori assumptions to allow construction of multi-equation models with large numbers of variables but small data sets. Furthermore, Malinvaud openly recognized the tension in the field by asking how the message sent by the Cowles Group to the world in the 1950s stands (in the 1980s) and whether a different one should replace it? Or should it simply be somewhat amended and supplemented? The problem of model selection is approached by Malinvaud in the same spirit as the Cowles group, by testing the restrictions of a given specification as much as possible with existing distribution theory to map out their compatibility with the available data. According to Malinvaud, these tests have objectively demonstrated the need for other kinds of structures, possibly using panel data to model disequilibrium effects, to generate forecasts and to guide effective intervention policies.

In his concluding remarks, Malinvaud (1988) highlighted the importance of sound economic theory, a good understanding of the economic system (through fieldwork), and a well conceived and more conscious exploratory analysis of the data set, before any model is specified. In this way, we could find new ways of making economic model building flourish. Furthermore, Malinvaud observed that during the previous three decades or so, both theoretical and applied econometrics had developed along interesting but more and more multifarious tracks. Malinvaud (quoted by Krueger, 2003, p. 193) stated that he could not

> pretend to have a good grasp of this diversified evolution. As a macroeconomist eager to learn from econometric results, I am particularly interested in what we can obtain from the analysis of individual data sets (cross-sections or panels) which are more and more numerous. The transposition has to face difficult aggregation problems. But mastering these problems is, in any case, a condition for important improvements in our macroeconomic knowledge.
> On the other hand, I deplore that the analysis of macroeconometric models along the lines explored by Ray Fair and Kenneth Wallis is not attracting more attention.

Commenting on Ray C. Fair, Malinvaud (1988, p. 201) wrote:

> The research of Fair into econometric methodology is part of a much more important project that falls mainly outside the domain of this paper, namely, to revise somewhat the present approach and practices in macroeconometric work, at stages of both modeling and policy analysis. This research[34] must be considered as a contribution of the Cowles Foundation to macroeconomics, in the same way as was the work of L. Klein on the American economy in the forties. But it incidentally also touched on some questions of econometric methodology, and more particularly on how to evaluate and compare the predictive accuracy of the models.

Researchers acquainted with Fair's writings and Malinvaud work about the confusion of ideas in macroeconomics must notice that Malinvaud does appreciate Fair's macroeconomic contributions.

As Diebold (1997, p. 6) pointed out:

> By the late 1970s, it was clear that Keynesian structural macroeconomic forecasting, at least as traditionally implemented, was receding. One response was to augment the traditional system of equations econometrics in attempts to remedy its defects. Important work along those lines was undertaken by R. Fair and J. Taylor (see e.g., Fair, 1984, 1994 and Taylor, 1993), who developed methods of incorporating rational expectations into econometric models, as well as methods for rigorous assessment for model fit and forecasting performance. Models in the Fair–Taylor spirit are now in use at a number of leading policy organizations, including the Federal Reserve Board and the IMF, as described for example in Brayton et al. (1997). They are an important step forward, even if the theory on which they are built remains largely in the system of equations tradition.

Furthermore, commenting on renewed interest for simultaneous equations, Malinvaud (1988, p. 202) wrote:[35]

> When Peter Phillips permanently joined the Cowles Foundation in 1980 after a visit in 1978, he had already contributed in various ways to

> simultaneous-equation econometrics, in particular to its small-sample theory. His work continues at Cowles. His aim for this theory is to determine exact distributions of various estimators in general cases, for instance, when using the instrumental variables method, limited information maximum likelihood, two-stage generalized least squares, and so on. Concurrently, he is developing workable and accurate approximations to these exact distributions.

Let us leave Malinvaud (1988, p. 208) to conclude the discussion:

> Looking back in 1983 to the work on econometric methods that was done at the Cowles Commission in its second decade, we may find that its inspiration was at times a bit uncritical. After thirty years of development and application of this work, we may on occasion take a less dogmatic position about some issues. But we so often rely on this work that its relevance need not be debated.
>
> A father cannot expect more than to see his son take up his business and find new ways of making it flourish. Cowles econometricians of the forties are truly the fathers of present day econometricians and, like successful fathers, have good reason to be proud.

7.4 APPLICABILITY OF THE MTC DIAGRAM TO THE DEBATE

As can be seen we have now travelled very far from the concept of absolute truth to a model and to the three conditions of its operationality. We explained these as three necessities for an econometric model to be operational. We shall now show how they interact, indicating that in present day econometric practice, they tend to undermine each other. This is the object of this section.

As discussed earlier, the coherence – theory – is one point of the triangle; relevance or applicability is another and measurement is the third. The theory provides an explanation of the economic problem or problems being considered. It will do so in terms of concepts, variables and relationships that must be applicable: they must be present directly, or discoverable beneath the surface, in the behaviour and institutions under discussion. But the plans, decisions and behaviour being described have quantitative aspects, both in prospect and when the results are in. So it must be possible to measure them without ambiguity, to a definite degree of precision.

We argued earlier that a good and useful econometric model has to have all three attributes of the triangle. It must be coherent theoretically; the theory must be able to explain the aspect or aspects of the economy under consideration, and must do so in a plausible and internally coherent way. In doing so it will define the central variables and relationships, and it will indicate which features of the system can be considered as given.

An econometric model must be composed of variables and relationships that are actually applicable, that make sense in terms of the daily life, the skills and abilities, of the agents being described. The concepts don't have to be the same as those used by the agents, but they must be translatable into those the agents use themselves to describe their plans and behaviour.

It is sometimes argued that economic theories do not have to be realistic; it only matters that they predict well. The case against this is presented in Hollis and Nell (1975). But even if this case could be made for economic theory, it clearly makes no sense when we are setting up an econometric model. The model can, of course – indeed, must – be *abstract*. But that is entirely different from being unrealistic. Anything unimportant or inessential can be left out. But things that don't exist or that are impossible cannot be added – as is unfortunately routine in neoclassical thinking.

And, finally, the behavior being modeled must be measurable. The relationships are mathematical; many decisions should be based on mathematical calculations. Many actions are carried out to a certain point and then stopped. Agents have to be able to measure, and so therefore do observers.

7.4.1 The Debate Reconsidered from the MTC Diagram Perspective

Let's recall here that the second diagram presented the three concepts as the points of a triangle, and we shall keep the idea of a triangle. But instead of representing the concepts as the points of the triangle, we propose to exhibit each as a circle, the three circles standing in a broadly triangular relationship (for the original MTC Diagram, see Figure 6.2 on p. 171). Then, if a concept is well-developed, we can show it as a large, well-formed circle; if the concept is weak or ill-favoured, it will be a small or ill-formed circle.

Theory must describe the behaviour of agents in a coherent way; the relationships must make sense, and, taken together, they must show how the economy works. The theory poses questions and proposes answers. It should tell us what is important, and what is not; what can be taken for granted, and what cannot. If the theory is well-developed, we can again show it as a large, well-formed circle. If the theory is simple or weak, it will be a small circle. If the theory is half-baked or incomplete we can show it as a broken circle.

Applicability or relevance means that the concepts – the variables and relationships of the theory – must make sense not only in terms of the aims and objectives of the agents, but also in terms of their powers and abilities. For an agent to be supposed to act in a certain way, it must make sense to believe that such an agent has the appropriate powers – and that those powers can be sustained in the context of the working of the economy. Then, if the variables and relationships have been carefully defined and drawn up on the basis of the actual practices of the agents, this can be represented by a large, well-defined circle. But if the variables are defined arbitrarily, or only in theory, without regard to the actual behaviour of agents, the circle must be shown as small or deformed.

If agents are assumed to make calculations and precise comparisons in quantitative terms, but the concepts that they use turn out to be impossible for observers to measure in practice, something must be wrong. Observers can always ask agents how they do it. But if agents cannot understand the concepts, and observers cannot figure out how to measure them, clearly the approach is defective. Again, if the conditions for measurement are good, the circle will be large and well-formed; but if the set-up is such that we do not know what we are measuring, or if we cannot tell reliably what measures attach to what variables, the circle will be small or deformed.

Ideally, an improvement in any one should enhance the other two. Redefining variables to improve their applicability should make them easier to measure, and should provide a better fit with the theory. Clarifying theory should improve understanding, which should make it easier to measure, and so on.

7.4.2 Using the MTC Diagram to Interpret the Debate

Let's look at examples. Start with the interaction between theory and data or measurement. (An example from natural science would be the development of the atomic clock.) The Keynesian revolution and subsequent development of macroeconomic theory led to the collection of macroeconomic statistics and the expansion and reorganization of national accounts. For a case of data collection influencing theory, consider the famous articles of Hall and Hitch (1939) and Andrews (1949) in England, and the work of Means (1935) in the USA; extensive interview data gathered from business leaders led to a reformulation of the theory of the firm, resulting in the mark-up pricing approach. Cases of theory influencing relevance/applicability generally mean the construction of operational definitions, or the creation of new variables; an extreme example might be the introduction of a new economic institution, such as the euro. (An earlier example would be John Law).

Conversely, we can have applicability/relevance influencing theory, as in the case of the rise of the modern corporation, where a new institution/set of new operational variables led to the development of new theories.

Finally, we have data/measurement influencing applicability/relevance, as when computers lead to the development of new data sets (e.g. in banking and finance), which in turn lead to institutional changes (new ways of moving money around, leading to new definitions of variables). And conversely, applicability/relevance can influence data/measurement as with the emergence of high-technology, which called for new sophistication in measurement.

But the suggestion here is that in the case of neoclassical theory and structural econometrics, an improvement in any one of the three points of the triangle will worsen the situation of at least one of the other two. For example, Lucas greatly clarified and tightened the neoclassical model; but to do this he had to attribute powers to his agents that are idealized and imaginary. Nothing in reality corresponds to them. Nor can his variables be measured. Suppose we start with relevance, and through interviews establish that businesses go about setting prices by fixing a markup on costs (Hall and Hitch, 1939; Andrews, 1949; Gordon, 1983). This cannot easily be made compatible with any neoclassical theory of the behaviour of the firm. Suppose we start with measurement, and study the cost accounting practices and the role of costs in balance sheets of firms. We shall run up against 'standard costs' right away; but this will be difficult to reconcile with either marginal or average costs as derived from a standard increasing/diminishing returns production function.

In Figure 7.1 we show the relationships in the same way as in Figure 6.2, which we take to represent the best of the Cowles work, in particular the position of Malinvaud.

We have theory (coherence) at the top, measurement (quantification) on one side, and applicability (relevance) on the other. Each of the three is represented by a complete circle of the same size. The circles are complete, indicating that each is fully developed – the theory is adequate and sufficient to the task, the processes for taking and analysing measurements meet the requirements, and operational definitions have been advanced for all the variables and relationships and investigation has confirmed that they are workable and appropriate. Each of the other circles supports theory, and theory provides and explains the meaning of the other two. Theory tells us what measurements measure, and why it is important; and theory defines the concepts for application and integrates them into a framework of meaning. The lines connecting the circles indicate influences – theory defines the possibilities and meaning of measurement, measurement supports theory (provides its quantitative aspect). Theory defines the relevant variables and relationships, and indicates what can be taken as given, and why; it defines the conditions

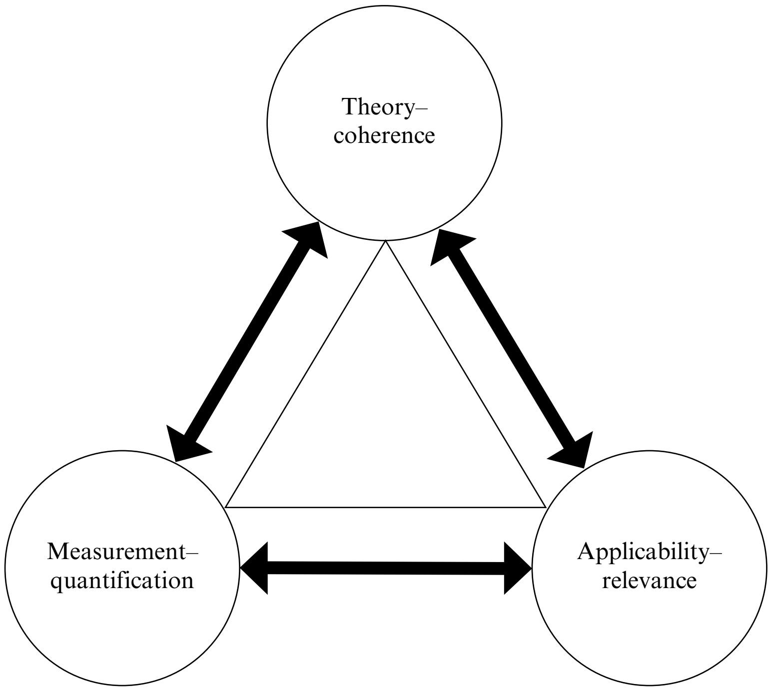


Figure 7.1 Malinvaud methodology

of application of these ideas. Of course, applicability and measurement interact; the definitions of the variables and relations tell us what kinds of measures will be appropriate, and the available measures and statistics provide guidance in developing the definitions of the variables. When these interactions are strong and mutually supportive, the connecting lines will be heavy and dark; when the influences run only one way, this will be indicated by arrows. When the influences are weak, dotted lines will be drawn. And when they are non-existent, there will be no lines.

Figure 7.2 shows the desirable relationships; the design of measurement not only supports theory – it is part of it. The conditions of applicability of the concepts are part of the coherence of theory, and vice versa. Applicability helps define the procedures of measurement. The three interpenetrate, but are still distinct, so the overlap in the Figure is only partial (see Figure 6.3).

But the three can't work so well together when neoclassical theory is organizing the project. If applicability and measurement are developed

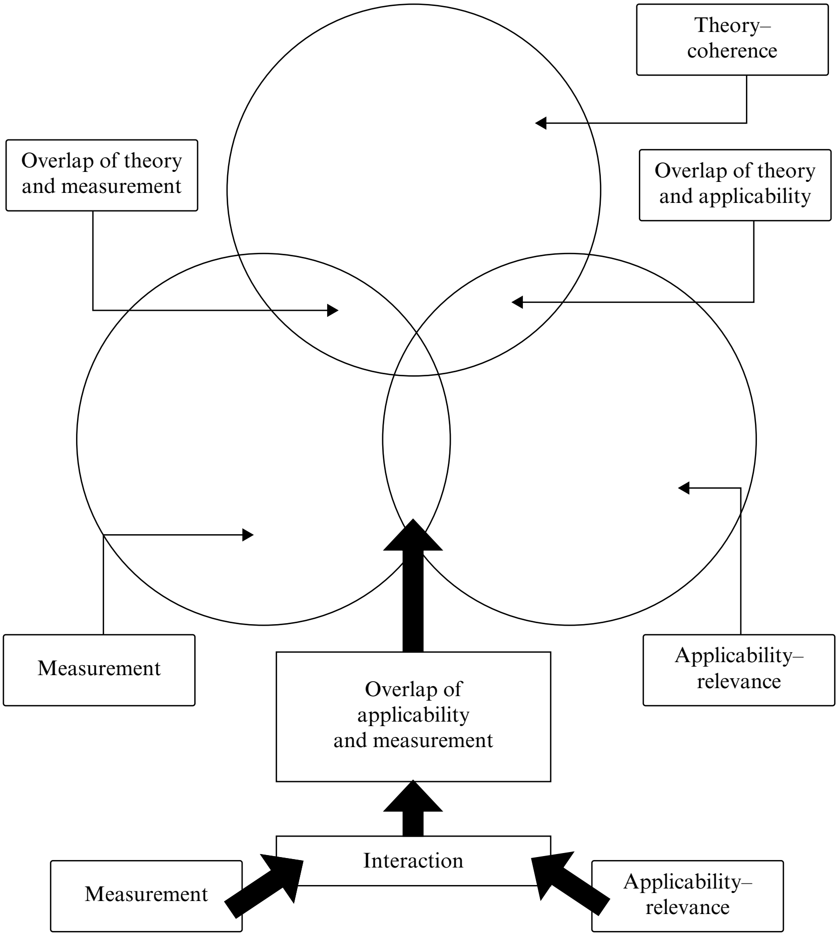


Figure 7.2 Haavelmo–Klein–Nell Unification Vision

and move closer together, as with Sims (1980a) and Granger (1969), theory will be reduced in importance and meaning, and will become more distant from the others. Figure 7.3 shows large important developments in measurement and applicability, so that the two are actually touching, with theory far away and only weakly connected.

On the other hand, if neoclassical theory is seriously developed and improved, as with Lucas, then measurement and applicability move further apart, are reduced in size and scope, and in fact become disconnected from each other. Each is defined by theory, and in each case the definitions make successful practice difficult (see Figure 7.4).

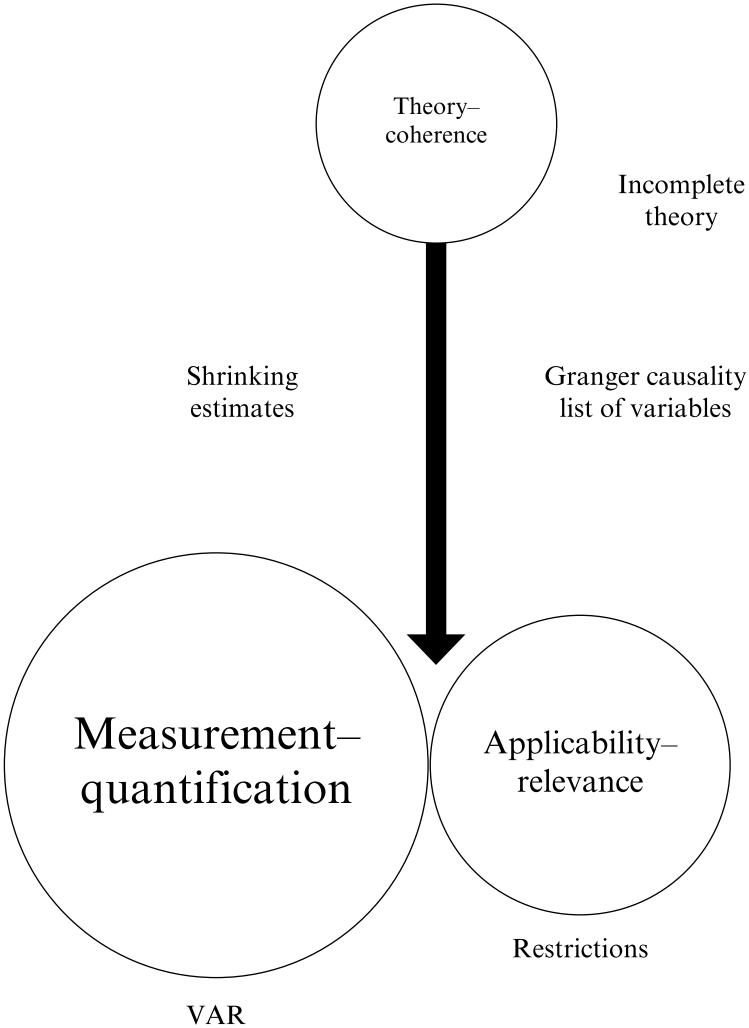


Figure 7.3 Sims methodology

The Lucas approach is an overall attack, designed to ensure adequate model selection or specification: it aims to revise and simplify theory, grounding all aspects, including expectations, in rational choice; and it proposes to collect and use statistics from many sources to ensure measurement. But there is no fieldwork, so there is no clear determination of what the numbers are really numbers of. There is no attempt to use interactive methods (Hendry) or fieldwork (Malinvaud) to ensure applicability.

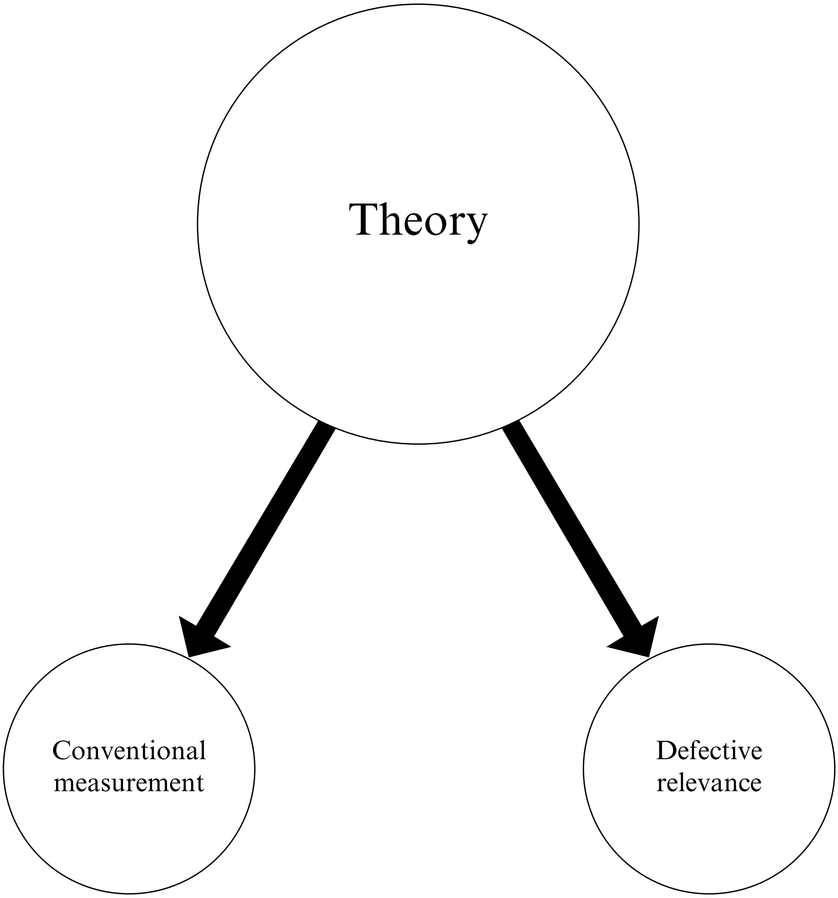


Figure 7.4 Lucas methodology

The implausibility and unrealism of the rational choice approach is not addressed at all.

Leamer headed in a different direction. He tried to improve the applicability of variables and relationships, by relating groups of inferences to prior distributions. In effect, Leamer substitutes the problem of determining the priors for the problem of model selection. He is troubled by the unrealistic aspects of neoclassical theory and does not seem to think the model selection problem can be solved; instead he draws on a variety of theoretical ideas to generate a group of models; these models should indicate the extreme bounds of the variables, and can be used to help select the priors. But this leads him to throw doubt on whether there

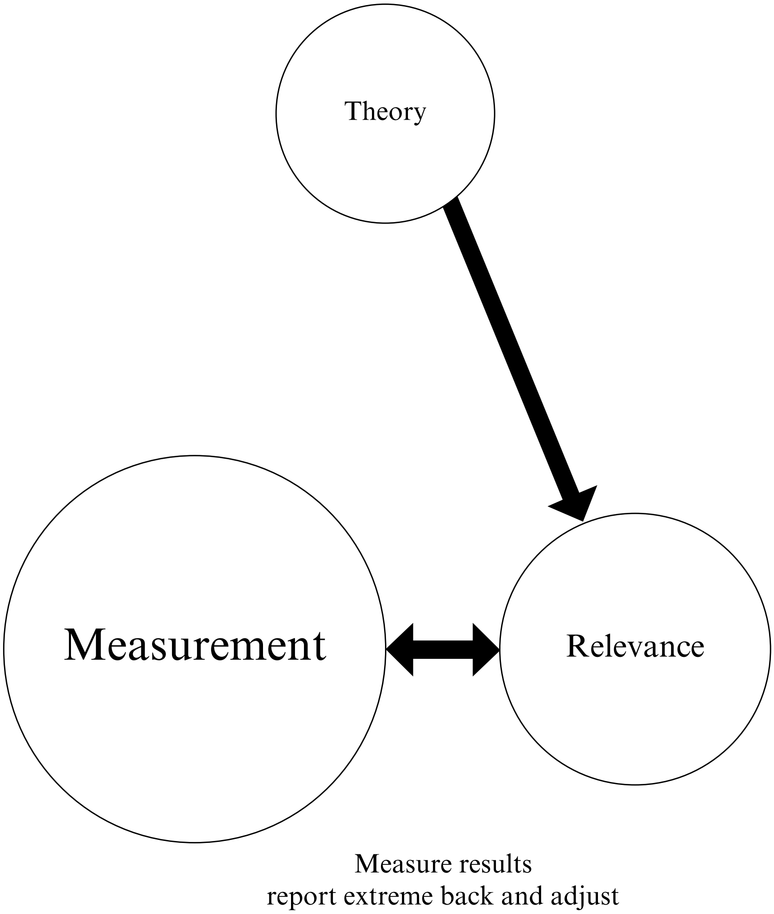


Figure 7.5 Leamer methodology

can be decisive tests or settled and unchallengeable measurements (see Figure 7.5).

Hendry makes a general attack on all three aspects of the problem. He simplifies (and moves away from) neoclassical theory, but he does not develop theory. However, he consistently addresses dynamic questions, while leaving the specification of the dynamics open; he does not try to impose a conventional approach. A distinctive feature of his work is that he engages in repeated interaction between model specification and statistical fitting; each is used to illuminate and improve the other. But this procedure also leaves the question of what is being tested – or fitted – open;

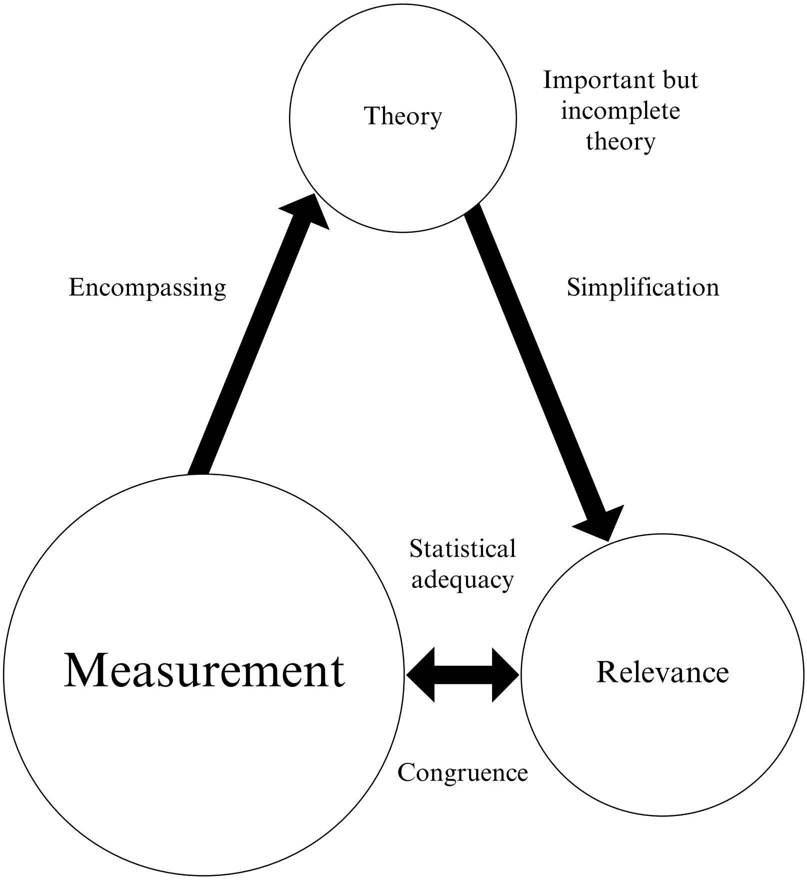


Figure 7.6 Hendry Methodology

what the results mean is ambiguous, because there is little guidance from theory (see Figure 7.6).

If great emphasis is placed on ensuring applicability of concepts and relationships, as with Leamer (and Hendry?), theory will be reduced in importance and become harder to interpret, and measurement will also become more problematical – statistical tests will be less decisive.

Consider an example. Suppose we have data on a branch of, say, the furniture industry. Suppose there are about 700 firms in the industry association, with a more or less log-normal distribution of firm size. We have data on prices, outputs, costs of all sorts (wages, salaries, materials and inputs, energy), interest rates, dividends, stock prices, productivity, and so on. We want to develop an econometric model of pricing behaviour to determine how the prices for the industry are set. We could use one the following approaches:

(i) Armchair empiricism This approach would start from a textbook or perhaps more sophisticated published model of firm behaviour, deriving the first – and second order – conditions for profit-maximizing. The resulting expressions would be used to generate simple specifications, which would be checked for their congruence with the statistical properties of the data. Then regressions would be run.

(ii) Fieldwork Start by thinking about the problem. How important is size? Do the largest firms dominate? What is the institutional set-up? In what ways does competition manifest itself? Realistic theory should be consulted, but textbooks should be approached with caution. (There is no harm, apart from wasting time, in looking at textbook models; in some areas they can be helpful. But, as a general rule, a pronounced focus on abstract optimizing is likely to miss the point.) Consult institutional histories and business journalists who cover the industry. Then arrange to interview key figures – the managers responsible for pricing – in both large and small firms. Try to get internal memos to confirm or disconfirm what they say; check what they say against published data. Look into the likelihood of innovations and entry.

Suppose we try to show the influence of demand on price as well. It will not be useful to posit general utility functions and optimize. A first step would be to consider the range of customers, in terms of their social class and ethnic background, and then note whether there have been important fluctuations in income and employment during the period under investigation. When prices fall, will new groups be able to move into the market? Will those already in the market 'move upscale' in their choice of styles? (See Nell, 1998a.)

7.5 ASSESSMENT OF THE CRITIQUES

There is some justice in the criticisms presented briefly above, but they have been overdrawn (Dagum, 1986c; Davis, 2000; Epstein, 1987; Klein, 1985; Malinvaud, 1988; Nell, 1998a; Phillips, 1983; Spanos, 1986; 1989). Moreover, there is a tendency to blame econometrics and econometric methodology for faults that may well be due to inadequate theory. Many deep questions about the interpretation of the Cowles model had already surfaced in the early period of the formation of econometrics. After years of often stormy discussion, they were largely set aside without being completely resolved. The latest critics in the 1970s and 1980s indicate a renewed concern with long standing basic problems of methodology (Epstein, 1987, ch. 4; Marschak, 1941; 1950).

After examination of the so-called debate over the scientific foundations of structural econometrics, it seems clear that there is a need for transforming structural econometrics. Thus an examination of the purely internal problems of Cowles econometrics leads naturally into a general concern with the scientific foundations of the whole discipline.

The objection that the 'true' structural relations are not directly observable is, however, not peculiar to structural econometrics. Indeed, it is a basic tenet of econometric methodology. The distinction between structural form (unobservable) and reduced form (observable), and the need to ensure identifiability, are both necessary to ensure that the observable arises directly from the underlying structure (and that this relationship is captured in the model; this is identical in spirit to the role of 'fetishism' in Marxian economics).[36]

In the standard econometric case, the parameters are unobservable and only the coefficients can be estimated, but the variables themselves are usually observable. (By analogy, if we were to consider Marxian economics, the variables as well as the relations at the structural level must be considered unobservable). Econometric methodology has indeed increasingly begun to deal with latent variables and also accommodating situations where the observed outcome is a censored version of the underlying events. So, again, in spirit these objections are not specific to the Cowles econometrics. But they need of course to be addressed properly by anyone embarking on empirical work.

It is time to sum up. What have we learnt from all of this debate? First, we have seen a review of the problems and procedures of model selection, together with auxiliary concepts such as exogeneity. Second, we have arrived at a pronounced recognition of the limits of econometric modelling. The much more critical attitude towards econometrics that prevails today is generally a good thing, although, as we will argue, there is a danger that the emergence of differing methodologies will be interpreted as a tacit admission of a complete failure of the discipline, rather than as constructive attempts (à la Klein) to improve it.

Let us turn to Sims's methodology to clarify our point. Sims's (1972; 1980a; 1980b; 1982a; 1982b; 1996) writings must properly be understood; as Malinvaud (1988, pp. 208–209) puts it:

> As long as they are not understood as a negation of the probability approach but as stressing the importance of a well conceived first exploratory phase in any analysis of data sets, these recommendations are healthy. They may expose statisticians to the risk of being dominated by computer specialists who know no subject matter of the field and think only of algorithms but we econometricians are too well aware of the importance of economic theory to be in danger of that.

We now conclude by reconsidering Hendry's methodology. The idea of interaction between theory and data is present continually in Hendry's methodology. However, Hendry doesn't discuss how the theory could be modified in the light of the data – and neoclassical theory cannot be modified in any fundamental ways (Hollis and Nell, 1975; Nell, 1998a). But post-Keynesian theory could be (for example Klein, 1950; 1985). And of course, this is basic for transformational growth (Nell, 1998a; 1998b). Nell (1998a) argued that conceptual analysis, understood here as a flexible search for conceptual truths, interacting with fieldwork, provides a method by which to approach economic model building. It will be argued later that this approach is superior to that advocated by pragmatism, which cannot give a coherent account of theoretical concepts, especially in relation to empirical work. It is normally assumed that theory and fieldwork, unless there are good reasons for believing otherwise, suggest which variables should enter a relationship, and the data are left to determine whether this relationship is static or dynamic.

Here, one could begin to discuss the fact that, from the earliest beginnings, all relationships were presumed equal. That is, all functional relations between economic variables were presumed to be grounded in rational choice, and therefore to be stable and reliable. Of course, they could be upset by exogenous forces, and some might be more often upset than others, but that would just be a matter of our ignorance in not knowing how to endogenize those exogenous forces. But we suggest that there may be intrinsic differences between relationships – some are grounded in structure, in duties, obligations, contracts, etc., while others may rest on nothing more than evanescent matters of fancy, or on intrinsically unreliable hopes for the future. Again, it is important to know which relationships are structurally grounded and reliable, which are ungrounded and unreliable, and why some relationships are enduring.

Granger (2004) has argued that the data generating process (DGP) is a myth: there is no special process or processes. It is just everyday life. Data is needed because firms must pay sales taxes, keep accounts, and know whether or not their books balance.[37] Households have to balance their cheque books. Banks must stay solvent. Yet, as we have seen, this means that, far from being a myth, the DGP is deeply embedded in the structure of the socio-economic system.

7.6 THE MTC DIAGRAM: A SYNTHESIS

Now let us look again at the 'methodological triangle-circle diagram' and explore the crucial dilemma that this appears to pose. As we have just suggested, when the theory is neoclassical, the three requirements for an operational model appear to be in irreducible conflict with one another! Let's explore this further, and ask: would these requirements still be in conflict when theory is built on strong foundations of field work and conceptual analysis?

Consider the following interpretation of what the econometric enterprise does:

1. It takes a reality (preferences, endowments, technology, values – *O* and *V* relations) that not only seems to be largely unquantifiable in its multidimensional complexity, but whose rules and ordinary practices do not appear to conform easily to the principles of mathematics, and forces it into a fictitious mould, made up of arbitrarily selected (and distorted) statistical data.
2. Then it asserts, with great certitude, that this fictional data set properly represents that previously-mentioned possibly unquantifiable reality, and can be drawn on to numerically realize a system of mathematical formalism – whose coherence, in turn, is contingent on its adhering to wholly idealized assumptions.
3. Finally, taking these arbitrarily selected data, and drawing on mathematical formalism, a model is developed that elaborates a structure and game plan designed to 'explain' or 'act' upon the reality on the basis of fundamental econometric hypotheses which simply assume or assert (in the face of common sense to the contrary) that the distributions are well-behaved, so that this entire process will be measurable after all, in concordance with a scientific method whose results, traditionally, are based on the regular repetition of physical phenomena – and are therefore impossible to validate in the realm of economics!

If this is how things are done, the enterprise is simply not valid in general. But this is not how it has to be, although if the enterprise starts from a neoclassical perspective, and proposes to specify the relations in line with standard theory, something like the above will result. We want to recall some points made earlier. Economic actions can be quantified because they are value relations. They are inherently quantitative, but the quantities and proportions actually involved cannot be postulated, but have to be uncovered by fieldwork. Idealizing assumptions have no place in this. Nor is there any place for unwarranted assumptions about probability distributions. But if we try, as econometricians, to impose a neoclassical framework, then our theoretical variables will not fit the data experience that the world offers us.

The neoclassical approach proposes one or another mathematical model of maximizing behaviour, calling for empirical research to come up with corresponding numbers. But recall the distinction between modelling behaviour and modelling structure. Structure can be modelled very accurately, because the relationships *are* mathematical, they reflect technology, accounting, contracts and other obligations, relationships that are judged and monitored and regulated by balance sheets. These depend upon what we earlier called the underlying O and V relations. So we can show precisely how structure can be maintained and extended; there is no need for the idealized assumptions mentioned above.

By contrast, behaviour is more difficult to model mathematically. For one thing, contrary to conventional thinking, many aspects of economic behaviour cannot be predicted. We cannot readily predict 'winning' and 'losing', although in repeated games, like baseball, enough statistics can be collected and the game situations are similar enough and repeated often enough that we can develop good odds. But many – perhaps most – situations of social and economic conflict are not repeated, and even when they are, the circumstances are not similar. We cannot reliably predict innovations; we cannot predict what will work and what will not. We cannot predict learning, or when people will change their minds. In a non-mathematical, humanistic sense, of course, we can and regularly do predict all of these things, in our daily lives, assisted by poets and novelists – and anthropologists and psychologists.

But these ways of thinking are not really amenable to mathematical formulation or statistical analysis, and this is where the idealizing assumptions find a happy home, making it possible to define variables that will (seem to!) fit the mathematical models. The calculations of these models can be presented as holding generally only when the agents are not only abstract but also idealized – precisely the neoclassical vice which renders its models inapplicable. For example, simple 'stimulus and response' models of behaviour – 'when price is x, demand will be y' – cannot be considered reliable. For one thing, learning by agents can always upset the pattern of responses, unless these are grounded in technology or obligations. Finally, observe that many ordinary economic actions – 'producing an output', 'offering a service', 'undertaking a job, or a sales campaign' – are the sort of activities that can succeed or fail under various circumstances. This is obvious, but important; it means that the stimulus could bring about the desired response, so that the action is begun – only to *fail*. Something happens and it is not carried through to completion. This does not happen in natural science; the tree is shaken and the apple falls. It does not get cold feet and suddenly decide not to drop. (Of course, the probability of such happenings could be estimated in many cases.) Standard models can only work if the data is forced into the appropriate mould, but fieldwork could help to suggest more flexible approaches.

Now let us look at this central difficulty from a different angle, relating the terms ‘coherence’ and ‘relevance’ to two principal approaches for the advancement of economics: the deductive approach of logico-mathematical formalism, and the inductive approach of collecting and analysing data. The methodology of econometrics, of course, calls for an interplay between these two, even though at times they may appear to be fundamentally at odds with one another. The former is the methodology employed to meet the model’s requirements of coherence and measurability, while the latter addresses the requirements of relevance and measurability.

Because much of the data of economic reality is not repeatable in terms required by scientific research, however, and because many aspects of that same reality obstinately refuse to observe the laws of mathematics, the difficulty of applying either approach in this field is not only tremendous in its own right but further compounded by the model’s mandate to combine them both in a single system. One holds in the theoretical realm, the other in the applied; it might even seem that they are mutually exclusive – and both also somewhat at odds with reality. Reality is not at peace with the assumptions of the deductive systems, nor does it readily give itself over to measurement.

Now, let’s state our claim in an extreme form, in which the conflict is reduced to an absolutely simple ‘Catch 22’: when a model based on the neoclassical tradition properly represents economic reality, it is not mathematical; and when it is mathematical, it cannot represent economic reality. Here ‘neoclassical’ implies methodological individualism and maximizing behaviour by idealized agents; a non-mathematical neoclassical analysis could be an account of an historical event or an institution – think of Friedman and Schwartz on the Federal Reserve.

So what exactly are we claiming? In order for a model to function according to its definition – to explain reality or act upon it – it has to be operational. That is, the theoretical underpinnings have to match the actual game plans of the world to which it applies (so the concepts of theory must be drawn by fieldwork from the social reality being studied). The catch lies in the fact that the elements of the basic theories on which conventional econometrics is based (the variables, the basic relationships) are not measurable in practice. The mathematics of the theories – for example, continuous differentiable functions, convexity, reversibility – do not match the discrete, frequently irreversible, often erratic patterns of economic behaviour. Moreover, central theoretical concepts like ‘utility’ have virtually no concrete content, and empirical counterparts or ‘proxies’ are hard to find, and seldom satisfactory. (‘Revealed preference maps’ have been suggested, but the actual preferences of real people or institutions do not seem to fulfil the axioms.) Even worse, because misleading, are conventional smooth, well-behaved production functions. They do not in fact represent the technology of mass production, let alone hi-tech, but the conventional methodology of 'estimating' them erroneously makes it appear that they do (see Chapter 11). ('Fixed coefficient' production functions, of course, are another story – but marginal productivity and marginal costs are central features of the conventional approach.) Most problematic of all is the assumption of 'rational' maximizing behaviour. Broadly speaking, theory pictures rational agents maximizing continuous differentiable functions; but the world operates with discrete technologies, follows socially defined norms and sums up its activities in balance sheets. On top of all this, there is the further problem, to be discussed in the next chapter: very often it is hard to justify assuming that our actual data is a sample drawn from a properly behaved distribution.

And so neoclassically-based econometricians find themselves facing the following Mexican stand-off: If a model employs no operational concepts, it cannot perform its function of explaining or acting on reality and therefore cannot be termed 'operational'. In fact it cannot, according to our definition, be termed a model at all. However, when it does fulfil its definition and explains reality or acts on it, it must be operational; but the kind of 'operational' concepts and relationships that can both be derived from fieldwork and fit into the neoclassical framework are not likely to be easily translated into mathematics. Indeed, there may be many qualifications, such as those Keynes suggests, that should be kept in mind. These may be special cases, real but not fully measurable – or if measurable, not generalizable – so 'scientific operationality' may remain unachievable. See Chapter 10 on fieldwork and proxies. Also see Chapter 11, on the craft economy, which we consider to fit at least partially into the neoclassical framework.)

An example, where coherence and measurability enter into contradiction: What could be more consistent than the general equilibrium model? Yet what could be less measurable than the functions of utility, demand, or production which it conceals? The ambitious objective is to give an account of the determination of all quantities produced and traded in an economy, along with their prices. Each good and its price will be identified by an index (say, i); then all goods and all prices on the set of markets in question can be represented by allowing this index to vary, say from 1 to n if there are n different goods and n markets to consider. Of course, the model will not tell us if there is one market for toothbrushes, or if there is one market for hard toothbrushes and another for soft ones. And even if it did tell us this, where would we go to find the statistical measurements of toothbrushes sold in diverse eras under diverse conditions?

Now consider starting from fieldwork, from a practical understanding. Suppose a body of toothbrush manufacturers decides to ask an economist to fashion a model permitting it to understand its particular market better and to supply predictions regarding its evolution. The economist will make a specific effort to produce statistical data and to propose an analysis of the principal mechanisms that regulate this market, drawing on the experience of the manufacturers. Then, with a little know-how, an operational model will be produced which will reconcile measurability and consistency. This will be partly because the objective is so much less ambitious that the reconciliation is easy: it is no longer a matter of giving an account of what happens in all markets, from toothbrushes to telescopes to wooden legs. It is, rather, a matter of taking just one of these, through direct interaction with the market agents, identifying the critical tendencies and issues, and afterwards assembling the measurable data and relying on a few constants, playing with it all until a picture emerges. However, obviously, a model of the toothbrush market has neither the theoretical power, nor the ambitious objective, of a general equilibrium model. But we could abstract from its particular characteristics, and, drawing on further fieldwork and market studies, extend the model to cover other similar markets. Then we might find, as we venture even further afield, that some important aspects of the model might have to be changed – in some models of pricing, for example, expectations of market growth might be very important. In others, costs will dominate, regardless of expectations. This, then, would itself become a matter of study – we would wish to know why. Perhaps we could define notions of 'innovation' and 'product cycle', and show how they appear in different phases of the product cycle. We would then need to explain why there is a product cycle in the first place, which would move us towards a discussion of technology and transformational growth. In this way we might build up, starting from fieldwork and proceeding by conceptual analysis, not so much a general theory of markets, as a set of well-grounded models, related to one another by theoretical hypotheses, all capable of being fitted or verified statistically, because measurability is established by fieldwork.

But not so quick! We have not yet explained how statistical verification can be justified. Recall our thumbnail history of the elaboration of the model in order to see the particular point where this difficulty emerges. The model was, we argued, created to arrive at a resolution of the problem of estimation by means of effecting calculations of the numerical values of those coefficients that could be defined as possessing the maximum likelihood based on the statistical data of the reality being modelled. Generally in this enterprise the statistical theory of estimation has been the method of calculation that has been employed, its justification accorded by the criterion of maximum likelihood. However, in order to achieve this objective it was technically necessary to equip econometric models with a probabilistic structure, and this structure could not help but profoundly amend its logico-mathematical foundation. In other words, to the traditional hypothesis relating to the mathematical form of the equations, probabilistic hypotheses were of necessity added, for, lacking these, the entire apparatus that permits and justifies the estimation of parameters might well collapse around the ears of the authors. According to this amended structure, the residual terms of the equations were now considered to be random variables endowed with a certain number of properties with regard to their probabilistic distribution. This adjusted structure, however, raised a whole new set of problems, to say the least. For the requirement of measurability ends up being met, but at the cost of well-grounded doubts about the pertinence or applicability of the hypotheses about the distributions, creating a very solid obstacle to claims of operationality.

The congenital flaw of econometrics, then, is that these crucial hypotheses, which permit and justify the estimation of parameters, are not generally amenable to verification. But the answer, we have already seen, is simple yet far-reaching. They can be approximately verified for the reliable relationships. They cannot be verified for volatile relationships. The implication is enormous, and changes the nature of the econometric enterprise: structural econometric models must be considered essentially open. They are only closed and solved for short-term practical purposes. But they are inherently open, because we cannot know the future.

NOTES

1. See Sims's 2011 Nobel Lecture (slide 39). Surprisingly, Sims goes so far as to suggest the importance of preserving respect for Tinbergen's and Haavelmo's projects.
2. Ray Fair's (2004) book, *Estimating How the Macroeconomy Works*, is the latest in a series of books by Fair that build, estimate, and apply his macroeconometric model to study the US economy. Fernandez-Villaverde (2008, p. 691) argued that 'Fair takes a more skeptical view of the ability of modern DSGE [dynamic stochastic general equilibrium] models – not directly, as it does not discuss those models in detail, but implicitly, through its adherence to the Cowles Commission approach'.
3. The reader is also invited to consult his webpage, http://fairmodel.econ.yale.edu/main2.htm, for details of Fair's models. For a good account of Fair's approach, see Fernandez-Villaverde (2008, p. 691).
4. For an account, see Nell and Errouaki (2006b).
5. By contrast, Godley and Shaikh (2002), Haavelmo (1958; 1989), Heilbroner and Milberg (1995), Hollis and Nell (1975), Klein (1982; 1985), Lawson (1997; 2003), Nell (1988; 1996; 1998a; 2004) and Malinvaud (1981; 1988) have argued that conventional economic theory (and policies) are increasingly irrelevant and out of touch. While the economic system continues to change rapidly, the dominant theories that guide macroeconometric modelling and economic policy have largely remained unchanged for the last 50 years.

6. For an account of Keynes's view of the statistical verification of business cycle theories, see Marschak and Lange (1940). For an account of Keynes's view on econometrics, see Pesaran and Smith (1985).
7. For further account, see Keuzenkamp (2000). His book provides valuable discussions of central philosophical topics (for example, induction, causation, simplicity, definitions of probability, alternative approaches to statistical inference, etc.) with many references to the works of leading authorities in many fields. Blaug observed that Keuzenkamp's main thesis is that 'econometrics cannot test, it can only estimate, says Keuzenkamp'. Blaug went on to argue that 'this is a thesis I find hard to swallow but such is the quality of his argument that he has almost convinced me of it (and I say this as one who is not dealt with too kindly in the pages that follow)'. (See Editorial Reviews in Amazon.com.)
8. Sims (2011) argued that Haavelmo defended Tinbergen against Keynes's argument which says that because 'Tinbergen's model contained "error terms", it could explain any observed data and therefore could not be used to test theories of the business cycle, contrary to Tinbergen's claims'. Sims pointed out that Haavelmo argued that 'economic models, in order to be testable, must contain explicit error terms, since they would not make precise predictions'. He went on to argue that economic models are testable, so long as they are formulated as probability models that make assertions about the likely size and correlation patterns of their error terms'. For an account, see Sims' 2011 Nobel Lecture.
9. Incidentally, Akerlof and Shiller (2009) reopened discussion of animal spirits in their new book. The authors are clearly correct. The complex psychological elements that underlie animal spirits are obviously powerful – outcome determinative – factors in the waxing and waning of the business cycle and in the course of economic development. They correctly emphasize the importance of confidence and trust, various alarms and fears, and stories people tell about their lives today and in the future. How could we find out about people's thinking if not through fieldwork? For further details, see Nell and Errouaki (2008a).
10. Three books on the history of econometrics have provided both background and inspiration for our work, namely Epstein (1987), Morgan (1990a) and Qin (1993). They have also provided a close examination of Haavelmo's probability approach in econometrics. Each book offers a different interpretation of the historical evidence and selects a different subset of that evidence as being the most important. Sometimes these different accounts are complementary, sometimes antagonistic. However, a serious critique of method and approach in the history of econometrics will have to take on the arguments of all three – with a central theme being their rereading of Haavelmo. For further details, see Nell and Errouaki (2006b).
11. Epstein (1987, p. 223) argued that profound problems in econometrics 'have not all received equal attention in subsequent research but each one has been fundamentally advanced in the last thirty years. Identification can be now ascertained for arbitrary non-linear restrictions on a non-linear equation system. Estimation by instrumental variables methods, notably 3SLS and 2SLS, avoids the difficult computations of maximum likelihood techniques. Finite sample theory has established important differences between asymptotic distributions of econometric estimators and test statistics and their behavior in samples with realistically limited degrees of freedom'.
12. Epstein (1987, p. 56) observed that in the autumn of 1941, 'Marschak formed an "econometrics seminar" with Haavelmo at the New School (NY); this attracted interested brilliant economists, statisticians, graduate students and instructors from the New School, Columbia, and the NBER'. Three rising stars, namely Haavelmo (who had been acquainted with Marschak at least since the 1938 conference at Cambridge), Modigliani (who was a student of Marschak and Adolph Lowe at the New School and finished his PhD thesis in 1944), and Abraham Wald, participated in the seminar. Haavelmo 'joined forces with Jacob Marschak at the New School and his ideas were so clearly compelling that the New School econometrics seminar soon agreed that

the least squares had to be replaced by some other method for econometric work'. The New School became an important econometric centre for exchanging ideas, and Marschak, drawing on friendships made in Europe, in Colorado Springs and in New York, contributed tremendously in the development of the econometric field. 'In 1942, Alfred Cowles successfully induced Marschak to accept a joint position as professor at Chicago and a research director of the Cowles Commission for Research in Economics starting in January 1943. Marschak at once planned to devote all the resources of the Commission to develop the work of Tinbergen in the light of the works of Haavelmo and of Mann and Wald'.

13. Even where the best statistical practices have been followed, however, it is important to point out that the present state of the econometric field would still support only modest claims for the stock of empirical results they have so far produced (Hammouda and Rowley, 1996; Nell, 1998a; Summers, 1991).
14. We cannot do justice to all the authors of that sort that have shaped the thinking of our profession, but the references to these authorities mentioned in this chapter grasp the essence of their methodological thinking. Other references are made throughout this chapter and the following chapters. Since the interest here is largely philosophical and methodological, the mathematical details are not covered. For an account of the mathematical details see Dagum (1986c), Dagum et al. (2003), Davis (2005a; 2005b), Errouaki (1990), Gilbert (1987; 1989), Nell and Errouaki (2006b), Pagan (1987; 1995) and Spanos (2007), among others.
15. For further discussion see Chapter 11 of this book.
16. The rational expectations hypothesis (REH) approach in applied macroeconometric model building is still in progress. A historical examination of it can only be highly tentative. This section will not attempt a full treatment of the subject. A number of important background essays and critical interpretations already exist, for example the introduction by Lucas and Sargent (1981) to their essential collection of readings, and the articles by Boland (1982), Chari (1999), Dagum (1986c), Dagum et al. (2003), De Vroey (2001), Epstein (1987), Lawson (1981; 1989; 1995a; 1995b), Lucas (1996), Malinvaud (1981; 1988; 1991b), Modigliani (1977), Pesaran (1988), Sargent (1992; 1996) and Sims (1982b; 1996). This literature has grown enormously and now covers an exceedingly wide range of issues in pure economic theory, econometrics and policy. For an in-depth discussion of Lucas's approach, see Vercelli (1991).
17. Although Muth's (1961) seminal paper introduced the concept of rational expectations in the neoclassical economic arena, the concept of expectations is fully incorporated in the writings of economists such as Marx, Keynes and Schumpeter. Epstein (1987, p. 194) argued that 'it was Marschak (1946) who anticipated the point of departure for Muth (1961) in a letter to Schumpeter in 1946. Although Marschak's letter comes remarkably close to expressing the REH he doesn't recognize the scope of its importance'.
18. Friedman (1968)'s famous presidential address to the American Economic Association, focused attention upon the apparent breakdown of the Phillips Curve relationship in the 1970s, suggesting to replace it with a 'Natural Rate of Unemployment' (NRU) – a concept later formalized in more detail by the new classicals.
19. Phelps's (1967, 1968, 1970) papers are considered to be the most important contributions to the theory of inflation and unemployment. The 1968 and 1970 papers studied wage setting and equilibrium unemployment when markets are characterized by frictions. Phelps's (1970) paper is an extension of his 1968 one and appeared in the famous monograph Microeconomic Foundations of Employment and Inflation Theory. Phelps (1967) analysed optimal demand policy when there is no long-run tradeoff between inflation and unemployment. Combined, these three papers contain the core of the new insights in Phelps's program.
20. Spanos (2010, p. 237) pointed out that 'the predictive success of ARIMA modeling encouraged several econometricians to challenge the then dominating pre-eminence of theory over data perspective, and call for greater reliance on the statistical regularities in the data and less reliance on substantive subject matter information'.

21. For an account of Granger's view of empirical modeling in economics, see Granger (1999).
22. Although this section draws heavily on Sims's overall work (Sims, 1972; 1980a; 1980b; 1982a; 1982b; 1996), we shall focus here mainly on Sims's methodological view as expressed in Sims (1980a; 1982a; 1996).
23. Cooley and LeRoy (1985), in their critique of this literature, argue that even if this approach can be applied successfully, it will still be of limited relevance except as a tool for ex-ante forecasting and data description (see for example Leamer, 1985). To paraphrase the authors, it does not permit direct testing of economic theories, it is of little use for policy analysis and, above all, it does not provide structural understanding of the economic system it purports to represent. Sims and others (Doan et al., 1984), however, maintain that VAR models can be used for policy analysis, and the type of identifying assumptions needed for this purpose are no less credible than those assumed in conventional or RE macroeconometric models.
24. See Malinvaud's (1984b) comments on Doan et al. (1984).
25. McCloskey (1983, p. 492) has made a similar point about Lakatos's (1976) book, *Proofs and Refutations: The Logic of Mathematical Discovery*. She praises Lakatos's (1976) book as an outstanding piece and cites it as example of work concordant with McCloskey's vision. McCloskey argued that Lakatos (1976) explores the relationship between various responses to anomalies and scientific progress. The book gives an account for a theorem in topology of the rhetoric of mathematics. It appears that some deep problems facing mathematics are problems of rhetorics.
26. Leamer's 1994 book is a collection of 26 influential essays (including his popular piece, 'Let's take the Con out of Econometrics') with an excellent and imaginative introduction.
27. We shall argue in Chapter 10 of this book that fieldwork can be conceived of as an important informal approach in economic model building. However, Leamer doesn't refer explicitly to fieldwork in economics.
28. Although this section draws on Hendry's overall work (Hendry, 1980; 1983; 1985; 1990; 1993; 1995a; 1995b; 2000; 2001), we shall mainly focus on Hendry's methodological and philosophical view.
29. For an account of Hendry's methodology, see Gilbert (1986b) and Hansen (1996).
30. Hendry is widely recognized as the most vocal advocate and ardent contributor to this methodology. His research has also aimed to make this methodology widely available and easy to implement, both through publicly available software packages that embed the methodology (notably PCGive and PCGets) and by substantive empirical applications of the methodology. Hendry's research has many strands: deriving and analysing methods of estimation and inference for non-stationary time series; developing Monte Carlo techniques for investigating the small-sample properties of econometric techniques; developing software for econometric analysis; exploring alternative modelling strategies and empirical methodologies; analysing concepts and criteria for viable empirical modelling of time series, culminating in computer automated procedures for model selection; and evaluating these developments in simulation studies and in empirical investigations of consumer expenditure, money demand, inflation, and the housing and mortgage markets. Since the early 2000s, and in tandem with many of these developments on model design, Hendry has reassessed the empirical and theoretical literature on forecasting, leading to new paradigms for generating and interpreting economic forecasts.
31. Malinvaud's 1969 microeconomics textbook (which was republished in English in 1972 as *Lectures on Microeconomic Theory*) and his 1964 Cowles-inspired econometrics textbook (which was republished in English in 1966 as *Statistical Methods in Econometrics*) were created as outcomes of teaching at INSEE School in Paris (ENSAE). His microeconomics textbook was written with a narrow conception of what we call microeconomic theory. As Krueger (2003, pp. 193–4) observed, 'his microeconomics lectures are presented data-free, and econometrics-free'. This is not unique to his textbook, but, in general, the way microeconomics is taught in the textbook seems to ignore all

of econometrics, in that very little evidence is presented to try even to describe the phenomena that microeconomic models are designed to describe. That is not so much the case in his macroeconomics book. His most recent book on macroeconomic theory was published by Elsevier in 1998–2000 in three volumes. However, as Malinvaud puts it (see his Web home page), at his age today, his contribution aims at capturing significant conclusions rather than bringing forward new results. Readers interested in further discussions of Professor Malinvaud's work and philosophy might begin with Holly and Phillips (1987) and Krueger (2003).

32. Malinvaud argued that the challenge of macroeconomic understanding was his first motivation for studying economics, but he found it too difficult for a long time. He argued that his main difficulty concerned the conception of a course on macroeconomic theory. He started to teach the course in 1957, and the book was published in French in 1981–82, then, much extended, in English in 1998–2000.
33. INSEE's METRIC Model is a good illustration of the ideas outlined above by Malinvaud. Indeed, the METRIC model built for the French economy shows particularly well the indicators of tension that have long played a significant and large role in the analysis of business conditions conducted in France. Furthermore, on the matter of the explanatory power of ideas such as profitability or competitiveness, the contribution of Courbis, who was trained in the teams charged with medium-range macroeconomic projections for the French economy, was significantly crucial. It is important to mention here the works of some scholars who contributed significantly to the development of macroeconometric models at INSEE, namely Fouquet, Charpin, Guillaume, Muet and Vallet for the DMS Model; and Artus and Sterdyniak among others, who were the architects/designers of the METRIC Model. For further details, see Artus et al. (1981) and Artus and Morin (1991).
34. Malinvaud refers specifically to Fair (1984)'s book *Specification, Estimation and Analysis of Macroeconometric Models*. Fair's book gives a practical, applications-oriented account of the latest techniques for estimating and analysing macroeconomic models. Fair points out at the beginning of his book that 'practical modelers seem to be taking less interest in the theoretical foundations in econometric and economic theory, while at the same time academic macroeconomists are paying less attention to the connection of their work to large-scale macroeconometric modeling'. He demonstrates the application of these techniques in a detailed presentation of several actual models, including his United States model, his multi-country model, Sargent's classical macroeconomic model, autoreggresive and vector autoreggresive models, and a small (twelve equation) linear structural model. He examines the difficult and often neglected problem of moving from theoretical to econometric models. He also examines optimal control techniques and methods for estimating and analysing rational expectations models. Anyone wanting to learn how to use large macroeconometric models will find this an essential guidebook.
35. Although the presence of nonlinearities in macroeconometric models was long ago recognized to be frequent, Malinvaud (1988, p. 202) observed that 'it is only recently that the methodology of estimation for systems of nonlinear equations was seriously examined. This is now part of the Cowles Foundation research program. Fair has determined feasible procedures for the computation of full information maximum likelihood estimates in large nonlinear models. Phillips has clarified the conditions for consistency of the estimators, studying in particular the interplay between non-normality of the error terms and nonlinearity of the equations. Ray Fair and John Taylor have studied estimation and solution of nonlinear rational expectations models. It seems that the work at the Cowles in the early 1980s has shown a renewed interest in simultaneous equations econometrics and witnessed a revival of the old tradition approach'.
36. Desai (1988) used this idea to characterize the transformation problem in econometric terms. For an account of methodological problems in quantitative classical economics, see Shaikh and Tonak (1994).
37. For an account of DGP or DGM, see Spanos (1986) and Granger (2004).

The rigorous notions of probabilities and probability distributions 'exist' only in our rational minds, serving us only as a tool for deriving practical statements.

Haavelmo (1944, p. 48, italics added)

The validity of these statistical tools depends itself on the acceptance of certain convenient assumptions pertaining to stochastic properties of the phenomena which the particular models are intended to explain; assumptions that can be seldom verified. In no other field of empirical inquiry has so massive and sophisticated a statistical machinery been used with such indifferent results.

Leontief (1971, p. 3, italics added)

The claim that econometrics, because it uses probability models without the kind of objective foundation such models can have in experimental sciences, is unscientific, is certainly incorrect in at least one sense. As we have seen, similarities in the personal probability distributions of individuals can create a basis for exchange of statistical results which is formally like supporting of results in experimental science. Whatever one calls it, it can be in principle a useful activity.

Econometrics does face special problems in setting professional standards for empirical work, however. The standards for setting up an experimental probability model – use of controls, randomization methods, etc. – and for reporting results have developed over many years.

Sims (1982a, p. 323, italics added)

The increasing discontent with empirical analysis in economics reached a crescendo in the early 1970s with leading economists like *Leontief (1971)* lambasting both economic theorists and econometricians for the prevailing state of affairs. *He was especially disparaging against deliberate attempts to enshroud the lack of substance under a veil of sophisticated mathematical formulations, both in economic theory and econometrics*. More specifically, he diagnosed a major imbalance between abstract theorizing and its empirical foundation and blamed the 33 'indifferent results' in empirical applications primarily on the unreliability of empirical evidence arising from *non-testable* probabilistic assumptions concerning errors.

Spanos (2010, p. 235, italics added)

8. Scientific issues in structural econometrics

We have suggested that much of the later work in applied macroeconometrics represents a retreat from the scientific standards the Cowles Commission sought to establish. Indeed, Stone (1978, p. 1, quoted by Swann, 2008, p. 43) pointed out that 'econometrics proper – meaning the use of econometric methods to improve applied economics – had not been advancing'. He went on to argue that 'the Cowles project was superior to the econometrics work of the late 1970s'.

8.1 ALCHEMY?

It goes without saying that all disciplines should improve with criticism. The difficulty in the specific case of structural econometrics, however, is that critiques, for many years, remained superficial. Indeed, Phillips (1983, pp. 314–15) commented:

> In recent years there has been increasing disquiet in the profession concerning traditional principles of econometrics. Much of the disquiet concerns methodological issues which in one form or another have troubled members of the profession since the early work of Tinbergen. But, while there is little that is new in the nature of the criticisms when they are carefully inspected, some of the voices in the present chorus are particularly censorious in tone and unedifying in content.

The theoretical core of econometrics, for decades, remained unquestioned, even unexamined. Critics offered no telling or insightful objections into what might be called, to use Leamer's expression, the 'technology' of econometric theory.[1]

Hendry (1980; 1993; 2000) refers to the current state of affairs in econometric methodology as alchemy three times. Seventy years ago, in his comments on Tinbergen's econometric studies for the League of Nations, Keynes (1939) described econometrics as a form of 'statistical Alchemy'. Swann (2008, p. 24) pointed out that what 'Keynes (1939) meant is that econometrics is trying to turn the base metal of imprecise data into the pure gold of a true parameter estimate'.[2]

Which aspects of alchemy did Keynes (1939) have in mind when referring to econometrics? Swann thought it most likely that Keynes was referring to the transmutation by which the base metals of imprecise non-experimental economic data are turned into the pure gold of a parameter estimate (Swann, 2008, pp. 33–5). On re-examining Keynes's critique, parts of it still stand – problems identified by Keynes, taken together, pose serious difficulties for applied econometrics, leading to serious doubts about whether parameter estimates were made of pure gold. Figuratively speaking, of course, economics as a subject matter is about how people turn base metals into things of higher value. So economic transmutation is feasible – physical transmutation may be a fallacy, but the pursuit of economic transmutation is not.

Economics is characterized by a sharp delineation between, on the one hand, theories expressed in conceptual language but hardly specified at all empirically, and on the other hand, empirical models that present analytical relations between measurable variables, few of which have clear economic meaning. Validation of theories is supposed to proceed along Popperian lines, but under the circumstances success is limited, to say the least.

We have set this picture aside, and replaced it with the MTC diagram. Conceptual analysis is deductive, but not in the conventional sense: it reveals conceptual connections and provides guidance. It helps us to define scientific variables and guides us in the search for lawlike relationships. We see a need for theory and observation to interact repeatedly, with theory guiding observation, and observation refining and correcting theory, so that they gradually focus on and define the crucial relationships. But there has long been tension between the supporters of theory and those who want the data to speak for themselves.

Morgan (1990a, p. 4) observed:

> Nineteenth century economists believed that mathematics and statistics worked in different ways: mathematics as a tool of deduction and statistics as a tool of induction. Jevons, who pioneered the use of both mathematics and statistics in his work, expressed both the status quo of those aiming at a scientific economics and his own vision of econometrics when he wrote: 'The deductive science of Economy must be verified and rendered useful by the purely inductive science of Statistics. Theory must be invested with the reality and life of fact. But the difficulties of this union are immensely great' (see Jevons, 1871, p. 26).

For Malinvaud (1980, p. 739), it is clear that both good theory and careful attention to the facts are needed:

> The art of the econometrician consists as much in defining a good model as in finding an efficient statistical procedure. Indeed, this is why he cannot be

> purely statistician, but must have a solid grounding in economics. Only if this is so, he will be aware of the mass of accumulated knowledge which relates to the particular question under study and must find expression in the model [. . .] we must never forget that our progress in understanding economic laws depends strictly on the quality and abundance of statistical procedure. Nothing can take the place of the painstaking work of objective observation of the facts.

This dependence of the data investigation on a good theoretical model is also pointed out by Haavelmo (1989, p. 15) in his Nobel lecture, who insists: 'econometrics has to be founded on theories that describe in a reasonably accurate way the fashion in which the observed world has operated in the past'.

8.2 THE PROBLEM OF MEASUREMENT

> A current critique of econometrics has been the idea that we are trying to build bricks from straw. The raw data we have are not accurate enough for use with advanced econometric methods. (Swann, 2008, p. 42)

Perhaps the most famous critique of this sort was from Morgenstern (1963), who devoted an entire book to examining the accuracy of economic data, arguing that economic data were not and probably could not be as accurate as econometricians seemed to assume. Likewise, Spanos (2009, p. 5) argued that 'data z_0 are marred by systematic errors imbued by the collection/compilation process and such systematic errors are likely to distort the statistical regularities and give rise to misleading inferences'.[3]

What does it mean – what could it mean – when economic data are reported to six or more significant digits?

> Griliches (1985) suggested four responses to Morgenstern's (1963) book. Firstly, the data are not that bad. Secondly, the data are lousy but it doesn't matter. Thirdly, the data are bad but we have learned how to live with them and adjust for their foibles. Fourthly, that is all there is – it is the only game in town and we have to make the best of it. (Kennedy, 2003, p. 169)

Arguably, Griliches (1985) just wholly misses the point. Desrosieres (2001, p. 343) has explained:

> Morgenstern is utterly dedicated to establishing a measurement system for economics that is just as rigorous as that of the other sciences. For this purpose, he examines the information provided by business accounts. He studies the status of errors – often regarded as falsifications or lies – that are to be found in these documents. Morgenstern distinguishes – for example, in balance sheets – between the items that are verified and identified without ambiguity (such as a

> cash position) and those that are merely estimated and shrouded in uncertainty, a practice justified by the need for prudence.

Furthermore, the tension between two forms of quantification – one derived from what Desrosieres calls 'scientific metrology', the other from 'business accounting practices' – has been examined by Morgenstern. Desrosieres (2001, pp. 340–43) defines the two approaches as follows:

> Metrological realism derives from the theory of measurement in the natural sciences that is complemented, in the social sciences, by the sampling method. The object to be measured is just as real as a physical object, such as the height of a mountain. The vocabulary used is that of reliability: accuracy, precision, bias, measurement error (which may be broken down into sampling error and observation error), the law of large numbers, confidence interval, average, standard deviation, and estimation by the least-squares method (see Stigler, 1986; Hacking, 1990). This terminology and methodology was developed by eighteenth-century astronomers and mathematicians, notably Gauss, Laplace, and Legendre. The core assumption is the existence of a reality that may be invisible but is permanent [. . .] Above all, this reality is independent of the observation apparatus [. . .]
>
> Business accounting is predicated on concepts of reality and proof that underscore its profound differences with the metrology of natural sciences. To begin with, the equivalence space is composed not of physical quantities (space and time), but of a general equivalent: money. Money allows the circulation of claims and debts (via bills of exchange); it serves to determine profits by measuring receipts and expenditures and by assigning a probable value to claims and debts [. . .] It should be noted that this subjective probability – used, for example, to assess a doubtful loan – is different from the frequentist or objective probability on which classical metrology bases its computations [. . .] Business accounting is a rich and dense social practice that seeks to achieve consistency and coordination in evaluations, actions, and decisions, either for a single player over time, or for several players whose relationships need to be regarded as fair and hence reproducible [. . .] double-entry bookkeeping plays a role similar to the repetition of observations in classical metrology. The requirements and tests involved in balancing the books are analogous to the regularities and normal distributions of repeated observations of the same object.

These two methodological orientations may both be characterized as realistic but each has different ways of verifying and articulating the substance of that reality and its independence of observation. Reality is often perceived to be self-evident. Statistics is compelled to reflect reality or approximate reality as closely as possible, but as Desrosieres (2001, p. 339) observed, this is ambiguous:

> these two expressions are not synonymous. The very notion of reflection implies an intrinsic difference between an object and its statistics. In contrast, the concept of approximation reduces the issue to the problem of bias or measurement error.

Desrosieres argued that the way producers and users of statistics converse about reality is determined by the fairly unconscious interweaving of several attitudes to reality, with specific constraints prevailing in different situations. For example, Desrosieres (ibid., p. 339) pointed out that 'the field of business statistics offers a representative spectrum of these possible attitudes to reality'. Furthermore, Desrosieres (ibid., p. 342) argued that 'official statisticians in charge of business statistics are specifically exploring the gaps between the methodology of business statistics and the metrological approach. One of these gaps is due to the heterogeneity of the population, which is so great that the largest firms have to be profiled individually using monographic methods quite different from the established statistical method. There is also the difficulty of defining and classifying the statistical units (establishments, enterprises, groups etc). Last, there exists a quantification system internal to the world of enterprises (double-entry bookkeeping). This system, which was conceived in the sixteenth century, pre-dates the age of statistical observations and is far older than the metrology of eighteenth-century astronomers'.

Furthermore, Desrosieres (2001, p. 345) argued that:

> the issues raised by the linkage between the two methodologies – one statistical, the other accounting based – were clearly visible in other circumstances: the establishment of national accounts, for example, in France in the 1950s and 1960s. National accounting has partly inherited the reality tests derived from business accounting: its variables were defined a priori; they were recorded in consistent, comprehensive, and theoretically balanced tables, where they were arranged in rows (transactions) and columns (agents). Disparate sources were reconciled to compile these tables. The final resemblance between national accounting and business accounting is that both tools were action- and decision-oriented: the national accounts were intended as monitors of macroeconomic policies, in the same way as the balance sheet and income statement provide guidance for the company executive. The accounts form a whole, explaining why the so-called reliability constraints are not identical to those of a pure metrological measurement of an isolated variable, whatever it may be.[4]

With this in mind, we discuss now the problem of measurement in economics. The problem of measurement of economic variables is frequently cast as a problem of error of measurement. As Desai (1976, p. 16) has observed:

> The problem of measurement is not only that of errors of measurement. It may also arise due to conceptual problems either in the nature of data collection or in the formulation of economic theory. These render the task of estimating economic relationships difficult. There is an additional problem, however, that the relationships we want to estimate are not directly observable in data. This is known as the identification problem and it has been at the heart of econometric discussion for many years.

He (ibid., p. 14) went on to argue:

> Economists work with data already collected and these data are the result of undesigned experiments in real life situations carried out by consumers, producers, workers, civil servants, etc. In econometric work, there is a tendency to use published data that are readily available without looking carefully into the definitions used or the method of collection, though these can be frequently shown to lead to biases in the estimated parameters [. . .] published data often do not measure variables that are economically meaningful.
>
> There are also delays in data publication and many subsequent revisions which plague any data user. Non-availability of certain types of information or a small sample often forces the exclusion of certain relations from econometric study.

There is a specific problem related to short-term forecasting: the availability of the data within the required time frame. Applied econometricians curse the delay of quantitative information, which requires them to spend a good amount of their time predicting the past! Suppose, for example, that a 3-month forecasting model operates with the actual values of certain explanatory variables. If these values, or even certain among them, are themselves only available in 3 months, it is clear that the model is unusable. The econometrician will then fall back on a model operating only with values available today – that is, the values of the variables 3 months earlier, and the forecasts will not be as good. Once again, it is the unavailability of suitable data that is to blame, and not econometrics as such.

Yet there is a tendency to sweep these problems under the carpet. Maddala (1998, p. 414, quoted by Swann, 2008, p. 53) reported that 'while he was preparing a new edition for his econometrics textbook, the reviewers suggested that he should remove the chapter on errors in variables because it is never used!' Maddala (ibid.) retorted that 'applied economists have to face the problems of errors in variables all the time'.

The problem of measurement is also connected with specification. Indeed, as observed by Desai (1976, p. 15):

> Our specification may dictate a certain way of measuring a particular variable. This problem occurs most frequently in the context of stock-flow discussions. The stock-flow problem points to the problem of the time unit of observations. Economic theory is cast in terms of continuous time and instantaneous rates of change. Most available data are in discrete time very often with high level of time aggregation.

A first problem is posed by the definition of the majority of economic magnitudes. It is clear that the verification or the refutation of a theory is dependent on the precision of the definition and therefore on the measurement of the magnitudes put into relations. Thus, one of the reasons for the relative weakness of econometrics cannot be directly ascribed to econometrics itself; it stems from the basic materials to which it is applied (Desai, 1976; 1988; Haavelmo, 1944; 1958; Hollis and Nell, 1975; Klein, 1950; 1957; 1982; Leontief, 1948; 1971; Nell, 1998a; Schultz, 1938; Shaikh and Tonak, 1994; Stone, 1954a; 1954b).

As described by Desai (1976, p. 14):

> there is also the much more serious problem that published data often do not measure variables that are economically meaningful. Thus, depreciation of capital stock either at the level of the firm or of the economy is measured by accounting conventions and does not approximate to the rate of physical or economic obsolescence. Economists are forced to use these statistics due to lack of any alternative series, but the testing of hypotheses is rendered difficult because the empirically measured variables do not correspond (though they may be similarly labelled) to economic concepts.

Indeed, Morgenstern (1963) has stressed both the problem of the definition of economic categories and the problem of measurement accuracy.[5] To paraphrase the author, definitions have been poor because economists didn't do empirical labour in the field comparable to what the physicists did before establishing a mathematical science. This position was reinforced by Leontief (1971, p. 6), who was also concerned by 'the lack of standardization in economic categories and data'.

A second problem, closely linked to the first, concerns the very frequent unsuitability between theoretical concepts and operational concepts (for example, Haavelmo, 1944; Hollis and Nell, 1975; Nell, 1998a; Shaikh and Tonak, 1994). Indeed, Malinvaud (1991b, p. 105) argued that 'on several occasions, whether it is a matter of unemployment, of revenue, of productive branches or of market structures, we have found that the theoretical concepts used for the abstract analysis of problems differed from the corresponding operational concepts used for observation or for applied economic work'.

Malinvaud (1991b, p. 70) offered, among many others, the example of durable goods, arguing that 'most of the purchases of immovable goods figure in consumption, according to the conventions of national accounting except for housing'. Shouldn't all durable goods be treated the same? Malinvaud observed that 'if all are treated as consumption, the reserves of immovable goods retained by households must not figure in the calculation of wealth. But if they are treated as wealth, we must define the consumption of the services of immovable goods, include it in consumption and subtract it as depreciation in the calculation of changes in wealth' (ibid., p. 70).

The frequent unsuitability between theoretical concepts and operational concepts can be seen in the methodological problems of the quantitative classical economics research programme.[6] Desai (1988, p. 3) argued that 'a serious difficulty in confronting classical theories with empirical observations comes from the fact that the variables (as well as the relations at the structural level) are unobservable'. Of course, in the standard econometric case, the parameters are unobservable and only the coefficients can be estimated, but the variables themselves are usually observable.

The objection that the true structural relations are not directly observable is not peculiar to classical economics, however it is indeed a basic tenet of econometric methodology. The distinction between structural (unobservable) and reduced form (observable) and the need to ensure identifiability (i.e. to ensure that the observable arises directly from the underlying structural model) are identical in spirit to the objection of fetishism (for example Marx, 1967, vol. 1, ch. 6).

The proposition is that there is an observable phenomenal level at which relations are juridically equal and voluntarily contractual, but when one penetrates to the underlying non-observable structural/real level, one will unmask the unequal exploitative class relations. This implies that looking merely at observable facts may be misleading. Sometimes this objection is put as saying that one must not be 'empiricist': the data should not totally dictate the model to be derived. One must draw on a prior theoretical framework to confront the data.

Shaikh's work in the late 1970s and 1980s on the methodological and empirical problems in quantitative Marxism was an attempt to develop a comprehensive and empirical framework for Marxian categories. He produced a systematic mapping between classical and national income account categories, which provided measures of the rate of surplus value in the USA, and made some preliminary estimates (for 3 sample years) of the size and direction of the net transfer between workers and the state (that is, of the balance taxes paid by workers and social expenditures directed to them).

The synthesis of his work was published in 1994 and was co-authored with Tonak. Shaikh and Tonak (1994) wrote:

> This book has been a long time in the making. The interest in providing an empirical framework that would correspond to Marxian categories dates back to 1972–73, when Anwar Shaikh first discovered Shane Mage's path breaking work and developed an alternate schema and an alternate set of estimates based on Mage's own data.
>
> In 1974 Shaikh came across Edward Wolff's working paper on input–output based estimates of the rate of surplus value in Puerto Rico. This added a new dimension to the problem. Mage's work emphasized the significance of the

> distinction between productive and unproductive labor, but it was restricted to only the value-added side of national income accounts. On the other hand, whereas Wolff's work was located within the more comprehensive double-entry framework of *input–output* accounts, it did not distinguish between productive and unproductive labor. This led Shaikh to attempt to develop a comprehensive framework for Marxian categories which made both distinctions *simultaneously*.
>
> The procedure that emerged in 1975 was essentially the same we used in this book: a mapping between Marxian and input–output categories illustrated by means of a continuing numerical example in which both total price (*the* sum of purchasers' prices) and the magnitudes of the aggregate value flows (total value and its basic components) were held constant, while the associated money forms became ever more complex as more *concrete* factors were considered. This allowed one to verify, at each stage of the argument, that the overall mapping was correct.

Desai (1988, p. 5) argued that we now have some solid evidence from Shaikh confirmed by others, that, quantitatively, the value price divergence is empirically very small.[7] Prices are proportional to values when these are calculated from input–output tables and cross-sectionally related across sectors. Shaikh's work has considerably advanced our knowledge about the size of the price value deviation problem in an actual economy. Thus what Shaikh has demonstrated is that what is needed in econometric methodology is a well-articulated (that is, identified) model that connects the phenomenal and the structural/real levels. This suggests that Shaikh's approach to macroeconometric model building supports the revival and rethinking of the Cowles Model.

But structural econometrics faces a serious problem in carrying out its programme; as we have seen, it may not be possible to move from the observable reduced form to unique estimates of the structural equations.

8.3 THE PROBLEM OF IDENTIFICATION

8.3.1 Definition

Kennedy (2003, p. 182) defines the problem as follows:

> a mathematical (as opposed to statistical) problem associated with a simultaneous equation system [. . .] concerned with [. . .] the possibility or impossibility of obtaining meaningful estimates of the structural parameters.

Kennedy (ibid.) explains that 'if you know that your estimate of a structural parameter is in fact an estimate of that parameter and not an estimate of something else, then the parameter is said to be identified. Identification is knowing that something is what you say it is'. As an example, Kennedy (ibid., p. 183) offers the classic paper of Working (1927):

> the case of a supply and demand curve for some good, each written in the normal fashion – quantity as a function of price. This along with an equilibrium condition represents a simultaneous system. The observations on quantity and price reflect the intersection of these two curves in each observation period. The positions of the supply and demand curves in each period are determined by shifting the true supply and demand curves by the amount of their respective disturbances for that period. The observation points, then, are likely to be a cluster of points around the true equilibrium position, representing the intersections of the supply and demand curves as they jump around randomly in response to each period's disturbance terms. The supply and demand curves have the same included and excluded variables, so that regressing quantity on price generates estimates that could be estimates of the supply parameters, the demand parameters or, or as is most likely, some combination of these sets of parameters.

There are two basic ways of describing the problem of identification:

> In general, different sets of structural parameter values can give rise to the same set of reduced-form parameters, so that knowledge of the reduced-form parameters does not allow the correct set of structural parameter values to be identified. This is what we call the identification problem.
>
> The set of equations representing the simultaneous equation system can be multiplied through by a transformation matrix to form a new set of equations with the same variables but different (i.e. transformed) parameters and a transformed disturbance. Mathematical manipulation shows that the reduced form of this new set of simultaneous equations (i.e., with a new set of structural parameters) is identical to the reduced form of the old set. This means that, if the reduced-form parameters were known, it would be impossible to determine which of the two sets of structural parameters was the true set. Since in general a large number of possible transformations exists, it is usually impossible to identify the correct set of structural parameters given values of the reduced-form parameters.
>
> Can one equation be distinguished from a linear combination of all equations in the simultaneous system? If it is possible to form a linear combination of the system's equation that looks just like one of the equations in the system (in the sense that they both include and exclude the same variables), a researcher estimating that equation would not know if the parameters he or she estimates should be identified with the parameters of the linear combination. Since in general it is possible to find such linear combinations, it is usually impossible to identify the correct set of structural parameters. (Ibid., p. 182)

So what to do? Kennedy (2003, p. 183) notes that the problem can be resolved if restrictions derived from economic theory, or other sources, can be put on the set of equations. For example, there may be outside estimates of parameters or of relationships between parameters, or knowledge of the relative variances of the disturbances or correlations between disturbances. Most commonly so-called 'zero restrictions' are invoked, saying that certain parameters and/or certain variables do not appear – have the value zero – in certain equations.

> Placing a [zero] restriction on the structural parameters makes it more difficult to find a transformation of the structural equations that corresponds to the same reduced form, since that transformation must maintain the restriction. Similarly, the existence of the restriction makes it more difficult to find a linear combination of the equations that is indistinguishable from an original equation. If the econometrician is fortunate, there will be enough of these restrictions to eliminate all the possible transformations. (Ibid., p. 183)

In general, how can we know whether or not a system of simultaneous equations contains enough restrictions to manage the identification problem? First, Spanos (1986, p. 615) comments that 'in practice the identification problem is usually tackled not in terms of the system of simultaneous equations as a whole but equation by equation using a particular form of linear homogeneous restrictions, the so called exclusion (or zero-one) restrictions'.[8] In other words, the task is made a little simpler by the fact that each equation in a system of simultaneous equations can be checked separately to see if its structural parameters are identified. Kennedy (2003, p. 185) argued that

> mathematical investigation has shown that, in the case of zero restrictions on structural parameters, each equation can be checked for identification by using a simple rule called the order condition. The latter requires counting included and excluded variables in each equation. This condition is only a necessary condition and not a sufficient one. Therefore it is recommended to check the rank condition.[9]

Summing up, the picture Kennedy (2003, p. 186) gives us falls well short of the early ambitions of the founders:

> If all the equations in a system are identified, the system or model is said to be identified. If only some equations are identified, only the structural parameters associated with those equations can be estimated; structural parameters associated with unidentified equations cannot be estimated; i.e., there does not exist a meaningful way of estimating these parameters. The only way in which the structural parameters of these unidentified equations can be identified is through imposition of further restrictions, or use of more extraneous information.

This extraneous information will have to come from outside the original data set, either from previously established theory, or perhaps from research activities that are outside the usual range of econometrics. The identification problem tells us that we cannot determine the structure of the economy from statistics alone. We already know this from our earlier discussion, and we will return to this theme in later chapters.

8.3.2 The Importance of Theory

Hollis and Nell (1975) argued that an identification problem occurs, in general, whenever an event is caused by as many behavioural forces as there are variables involved. This provides for solution points, but offers no information about how the different points were reached, thus giving no information about which functions are being 'traced out'. This can be solved only by finding the causes of changes in the observed variables; scrutiny of the data alone is insufficient. We have to look for the forces causing a shift in the initial functions. A regular connection between at least one of the forces and some further variable must be established. This requires a reasonably careful charting of the initial forces or, in short, theory. Without theory, data resulting from multiple simultaneous forces cannot even be examined for regularities or patterns (see Hollis and Nell, 1975, p. 123; also pp. 12, 58, 67).

This role for theory implies, as Boland (2000, p. 79) pointed out, that 'the problem of identification is logically prior to the problem of estimation of the parameters of the model (e.g., Johnston 1963 [1984], Goldberger 1964)'. Moreover, the problem of identification exists quite apart from the stochastic nature of econometric models, as Boland noted (2000, pp. 79–80). In regard to non-stochastic models, he (ibid., p. 80) meant

> only a specification of the form of the structural equations (for instance, their linearity and a designation of the variables occurring in each equation) [. . .] More abstractly, a model can be defined as a set of structures [. . .] a specific set of structural equations such as is obtained by giving specific numerical values to the parameters of a model.
>
> Identification [means] that, if the model is posited as being the hypothetical 'generator' of the observed data, a unique structure can be deduced (or identified) from the observed data.[10]

He (ibid.) continued:

> There are two ways that a model may fail to possess the identification property: either the model is such that no structure can be deduced or the model is such that more than one structure can be deduced from the same data.

The blind man can't adequately describe the elephant just by feeling the parts he can touch.

Hollis and Nell (1975, p. 85) pointed out that the successive problems of classifying the data as coming from a certain kind of source, specifying the appropriate variables and nature of the relations between them, and then identifying those relationships, arise only on the assumption that there are functional relations between the variables. These relations must be known to be lawlike, if they are to license what philosophers term 'counter-factual conditionals'. Unless, for instance, we may infer from a demand curve that, had the price been p, the quantity demanded would have been q, demand curves are not worth constructing. There is nothing novel about this; a projectible generalization is one that warrants the assertion of counter-factual conditionals for cases that never actually occurred. Since the concepts of identification and specification error are applicable only with the help of theory, as we have seen, we must be able to relate laws to the workings of theory.

Theory must be able to discount the many extraneous variables and accidents that seem to affect the phenomena it wishes to isolate. Marshall (1961, pp. vi–x) offered the great metaphor of a river, in which the 'deep silent strong stream', the main current, represents the true and major forces driving the economy, while the 'fluttering eddies' on the surface attract attention but are causally unimportant. The claim of neoclassical theory to embody general truths about the working of the markets depends on making this distinction; so does the testing of theories that require a solution to identification and specification problems; so does the soundness of long-term policy, designed to survive surface turbulence. The implicit recipe for making the distinction is that an apparent observed tendency is lawlike or projectible if it can be deduced from the theory of normal distribution and exchange or perhaps from an expanded version of basic theory; otherwise it is an eddy.

We can give Boland (2000, p. 81) the last word here:

> One of the implications of the priority of the methodological problem [the truth status of the form of the model] over the identification problem is that econometric studies are not substitutes for research in pure theory. [. . .] Many economists unfortunately confuse the sophistication of the statistical theory of econometrics with the sophistication of the economic theory upon which the econometric model is based. The fact is that the economic theory used in econometric studies is usually very primitive.

All too true, but it is not just a matter of being 'primitive'. The theories too often rest on the assumed 'rational' behaviour of idealized agents, thereby undermining the possibility of establishing real-world correspondences. Theory–coherence comes into conflict with applicability–relevance.

The difficulties of measurement, of unsuitability between concepts, and problems with the availability of data seem to be characteristic of economic reality (Haavelmo, 1944; Hollis and Nell, 1975). Even if these could be managed, econometric methods alone could not give us the true or correct model of the economy. However, if we knew that model, if theory could give us the specifications, econometrics could propose to estimate it. To do this, it will rely on a probabilistic approach – even though the theories are usually not probabilistic. The questions then arise, why develop such a sophisticated tool for treating such impure material? Do the probabilistic assumptions bring their own problems?

8.4 NORMALITY ISSUES RECONSIDERED

We argued in Chapter 7 that the simultaneous model was (and is still) open to a wider variety of conceptual problems than the single equation; it is not so easy to defend. More specifically, the hypothesis of the normality of the remainders, according to which the remainders e_t are random variables that follow a normal law, is very hard to justify and is often clearly not true. (However, this hypothesis is not necessary in order to obtain non-biased, convergent and efficient estimators of the true values of the parameters; the six other hypotheses are sufficient for that.) But it is very common to assume that error terms are normally distributed (Maddala, 1977); that is, error terms should be considered to be a bell-shaped continuous distribution which: (1) has only one peak (unimodal); (2) is the same shape on both sides of that peak (symmetric); and (3) has the property that the probability of picking very large or very small values at random diminishes as these values move further away from their arithmetic mean. The normal distribution is the one most extensively used in statistical applications in a wide variety of fields. Extensive tables have been prepared for this probability distribution. Many variables in practical life follow the normal distribution.

Because of this, Maddala (1977, pp. 29–30) holds that, while many variables in economics do not follow it, we can assume that the normal distribution is a valid approximation. Other probability distributions (the χ^2, Student's t-distribution and F-distribution) are derived from the normal distribution. The assumption of normality is quite strong but also quite popular and, unless there is a good reason to assume that the error terms are otherwise distributed, it is usually made. On the other hand, Kennedy (2003, p. 70) notes that 'while it is extremely convenient to assume that errors are distributed normally, there exists little justification for this assumption'. He quoted Tiao and Box (1973) to point out that 'belief in universal near-Normality of disturbances may be traced, perhaps, to early feeding on a diet of asymptotic Normality of maximum likelihood and other estimators'. He refers also to Poincaré, who long ago claimed that 'everyone believes in the (Gaussian) law of errors, the experimenters because they think it is a mathematical theorem, the mathematicians because they think it is an empirical fact' (ibid.). Furthermore, there is another justification for normality that could be rationalized by arguing that there are many omitted variables in any analysis, and, if the number omitted is very large, their effects will average out and give rise to a normally distributed error term. This result is due to the central limit theorem (see Desrosieres, 1999).

However, without a specification of the law followed by the remainders, the law followed by the estimators remains unknown, and any procedure of testing or prediction remains impossible. On the other hand, only the normal law, due to its numerous properties, allows us to reach our goal, which is to say the testing of results and the proposing of predictions. This hypothesis was therefore formulated only for practical reasons; the question then becomes: is it possible, after the fact, to justify it? Now the answer to this question is very likely to be negative for the volatile functions (Bonnafous, 1972, ch. 2; Errouaki, 1990).

Moreover, there are problems even with reliable functions. We have already indicated this above: it is not possible to consider that the value of total consumption for the year 1990, for example, results from a random draw within an infinite population.

However, an interesting point arises when we compare reliable social and economic processes with industrial ones. Calot (1967a; 1967b; 1995) argued that the success of the utilization of the normal law in many cases stems from the fact that it perfectly represents a phenomenon when the latter is the effect of a very large number of independent causes with additive random effects, such that the variability of each effect is weak with respect to the total variability. This is why we encounter the normal law often enough in agronomic, biometric and industrial contexts (industrial magnitudes relating to objects manufactured in series, where the variable factors are numerous: vibrations, temperature, conditions of supply and of manufacture). Nevertheless, we should not infer that the normal law, despite its name and numerous properties, has a universal character. For, as general as are the conditions enumerated above, they are obviously not always met (Calot, 1967a, p. 373). Furthermore, Mouchot (1996, p. 196)[11] considered it striking that Calot, administrator of INSEE and, as such, drowning in econometric models, did not think it wise to include econometrics among the areas of application for the normal law.

A quick review of the application conditions enumerated by Calot (ibid.) shows clearly that there is only one that is widely satisfied: e_t is in fact the effect resulting from a very large number of causes. But the other conditions may or may not be satisfied, depending on the circumstances.

The additive nature of the effects and the weakness of the variability of each have a priori no reason to be fulfilled. But some reliable functions – those for example relating output and employment or revenues and costs – will tend to be exactly the same as industrial conditions. The employment–output function rests on the same foundations. The case will be similar for households. There are a large number of independent factors, each being determined from outside, each having a small random impact – elections, wars and politics, general news, weather, media, fashions and fads, diseases and epidemics, earthquakes, tsunamis and Acts of God, inventions and innovations, and so on.

8.5 STOCHASTICISM, ERROR TERMS AND THE RANDOM DRAW

The validity of stochastic assumptions in econometrics will be discussed in Chapter 9, but here we may ask why stochastic models are so prevalent. According to Malinvaud (1980, p. 58):

> logically, we should insist that economic theory provides stochastic models that would apply directly to observed data. Statistical inference could then be made in the context of these models. In fact the theoretical models set up by economic science almost always imply exact functional relationships. In their different spheres the model of the competitive market and the elementary Keynesian model illustrate the fairly general point that theoretical representations disregard random fluctuations, yet these cannot be ignored in empirical investigations.

Indeed, as Malinvaud (ibid., p. 59) observed,

> this situation is not specific to economics. Exact sciences generally have developed mainly by way of functional representations. The fitting of observations was the aim of what was originally called the theory of errors, a very significant name. However, in economics the gap between theoretical pictures and observed facts is much wider than in the exact sciences, and the problems which arise in going from one to the other become much more important.

Recall that chapter 1 of Malinvaud's (1980) econometric textbook is devoted to a discussion of econometrics without stochastic models. He showed 'the usefulness to econometrics of a good understanding of questions which do not call for a stochastic model'. The first (brief) discussion of stochastic models then comes in chapter 2 (section 7, p. 58), where he points out that 'the transition to stochastic models presents hardly any difficulty'. He goes on to argue that 'the econometrician must substitute for the functional model given by economic theory a stochastic model that allows him to assess the statistical methods which he uses, and to interpret their results correctly'. According to Malinvaud, 'this formulation of the stochastic model is often called specification, and it is of great importance since it is basic to every study in econometrics'.

To explain why stochastic models are so prevalent – and to show the dangers in casual empiricism – Malinvaud addresses the law of demand (1980, p. 51):

> Let c_i be the consumption of a certain product by household i whose income is y_i. Suppose we know, for a given period, the values of c_i and y_i for a limited number of individual households. How can we deduce a law which allows us to determine the consumption of this product by any household at any time?
>
> The simplest approach would be to suppose that there exists a strict functional relationship between y_i and c_i, this relationship being independent of time or of the particular characteristics of each household. The model could then be written: $c_i = f(y_i)$.

It is not difficult to demonstrate the inadequacy of this assumption and this model. Malinvaud (ibid.) continues:

> Model [$c_i = f(y_i)$] must therefore be discarded since it is too rigid and simple. We must modify it by the inclusion of explanatory variables other than income, such as price, composition of household, liquid assets, etc. In this way we could give a more complete description of consumption. But it is to be feared that a purely functional relationship is still unsatisfactory, even when four or five explanatory variables are included. Two households with exactly the same income, composition, liquid assets, etc. will generally behave differently.

This sounds convincing; but to get an idea of what is at stake, consider two society ladies meeting at a New York dinner party, each wearing the same brand of new diamond necklace – or, even worse, the same designer dress! Our society puts a premium on individual self-expression: variety (and competition) in product choice is part of modern Western culture. Now consider Mennonite communities, where it is a norm that everyone should consume the same. Malinvaud's equation will be very happy there. Perhaps there will still be some variation, but it will be small – and it will represent divergences that should not exist. The point is that economic behaviour follows social rules; it will be reliable when those rules are clear, and violating them is costly (Nell, 1998a).

Therefore, Malinvaud (ibid., pp. 51–2) argued that:

> The most reasonable solution is therefore to take account in our assumptions of the fact that the factors determining consumption are partly unknown to us. They seem to some extent random and we can only hope to estimate their probable influence. We must therefore modify the model by the introduction of random elements. With a single explanatory variable, we could, for example, set: $c_i = f(y_i) + \varepsilon_i$ where ε_i is a real value obtained by random selection from a distribution whose characteristics are more or less precisely known. This random element ε_i will be called the 'error' [. . .]
>
> The assumption expressed in ($c_i = f(y_i) + \varepsilon_i$) results from the fact that the difference between the quantities consumed by two households with the same income appears to have all the characteristics of a random variable. It does not necessarily presuppose the existence of a specific chance mechanism, but it is admissible whenever our state of ignorance is well represented by the distribution of $\varepsilon_{i.}$
>
> Model ($c_i = f(y_i) + \varepsilon_i$) involves the function f and the distribution P of the ε_i. But it happens most often that neither f nor P is completely specified, and only their properties are given. Thus we may say that f is a linear function: $f(y_i) = ay_i + b$, where a and b are two unknown numerical constants. Similarly, we generally allow P to be independent of the value of y_i and to have zero mean, sometimes even to be normal.
>
> Formally, the model is defined by postulating ($c_i = f(y_i) + \varepsilon_i$) and by giving the classes to which the function f and the distribution P belong.
>
> The model therefore provides the logical structure on which the study of demand can be carried out.

Mouchot (1996, p. 192) admires the strength and simplicity of Malinvaud's argument. Desai (1976, p. 12) highlighted the importance of specifying the nature of the random error term as well as the deterministic part of the equation. But the question is: at what stage of the theoretical analysis should explicit statistical considerations be brought in?

Farjoun and Machover (1983, pp. 24–5) think that they are an afterthought:

> The traditional approach starts by looking for deterministic laws. Since such laws cannot apply to real-life prices, profits, etc., one invents idealized theoretical concepts, to which deterministic laws are believed to be applicable. Thus we have the ideal unit price of each commodity [. . .]
>
> The ideal quantities of the model are supposed to be deterministic approximations to the real statistical quantities. The latter are supposed to be obtained from the former by the addition of an indeterminate random or noise term [. . .]
>
> Likewise, the deterministic laws derived within the theoretical model are supposed to be approximate idealizations of real phenomena. A better representation of the real economic phenomena can hopefully be obtained by adding a random statistical error term to the deterministic equations of the model.
>
> Thus, the deterministic approach does not, in principle, deny that economic phenomena display in reality an indeterministic behaviour. But it hopes to capture this behaviour by super-imposing a statistical disturbance on a deterministic model. The probabilistic element is thus admitted at a second stage, as an afterthought.

Klein (1969, pp. 170–72) offered a comprehensive account of the literature concerned with the superimposition of probabilistic elements onto deterministic models. He held that it is not surprising that this approach has been favoured particularly by econometricians, concerned as they are with the measurement of real economic quantities; the deterministic models of pure economic theory are rather useless. But probability as an afterthought is not much of a model.

The introduction of a random variable corresponds specifically to taking into account the as yet unspecified extraneous factors. But as Hendry (1995b) has argued, this already poses problems. Hendry's (1995b) presentation of stochastic models in econometrics argued that there is a key difference between a fully controlled experiment described, for example, by a linear model, and a linear econometric model. The first can be represented schematically as follows:

$$\underset{\text{(output)}}{y_t} \quad = \quad \underset{\text{(input)}}{f(z_t)} \quad + \quad \underset{\text{(disturbance)}}{v_t} \tag{8.1}$$

where y_t is the observed result of the experiment when z_t is the experimental input, $f(.)$ is the relation between input and output, and v_t is the disturbance (which we hope is reliable) that varies between experiments carried out for the same values of z. This equation entails that if we have the same inputs, a repetition of the experiment will produce essentially the same outputs. The main thing is that causality flows from right to left in Equation (8.1). We can also say that Equation (8.1) is the procedure for generating data for y_t. It is this characteristic that proves the validity, for example, of the regression analysis between y and z.

Furthermore, Hendry (1995b) argued that in order for econometrics to reproduce the controlled experiment, we need data in which the outputs are in fact produced by the inputs, and therefore the model must coincide with the mechanism that actually produced the data. However, the economic mechanism is too complex to be modelled in a precise way, and all econometric models must be simplifications and are therefore false. (We don't think this can be said with certainty; besides which, simplification can – and should – leave the essentials in place, revealing important aspects of truth.)

Hendry (1995b, pp. 183–4) argued that we do not know how the data were in fact produced and since we do not control the economy, even if econometric equations can resemble Equation (8.1), there is in fact an essential difference, shown in his Equation (8.2):

$$\underset{\text{(observed)}}{y_t} \quad = \quad \underset{\text{(explanatory)}}{g(z_t)} \quad + \quad \underset{\text{(remainder)}}{e_t} \tag{8.2}$$

Now, the left side determines the right, rather than the inverse, as in Equation (8.1), and Equation (8.2) shows only that y_t can be broken down into two elements, $g(z_t)$ (a component that can be explained by z) and e_t (a component that is not explained). Such a division is possible even when y_t is not dependent on $g(z_t)$ but is determined by completely different factors, $h(x_t)$ for example.

From the econometrics viewpoint, $e_t = y_t - g(z_t)$ (referred to herein as Equation (8.3)) describes empirical models. A change in the choice or the specification of z_t on the right modifies the left side, and therefore $\{e_t\}$ is a derivative process. Contrary to the process $\{v_t\}$ from (8.1), $\{e_t\}$ from (8.2) is not an element that is drawn randomly from nature; it is defined by what remains of y_t after having extracted $g(z_t)$.

This presentation has the advantage of highlighting the possibility, for econometrics, of searching for better correlations, independently of any causality. It significantly relativizes Malinvaud's approach. We can explain this point by asking the following question: what becomes of this argument if, instead of considering the individual consumption function, we look at the macroeconomic consumption function $C_t = f(Y_t)$?

The random draw that generates the consideration of one household among the population of all households is not at issue; the macroeconomic consumption function at a time t does not fit this picture. Bonnafous (1972, p. 59) argued over 3 decades ago that a fundamental objection to the validity of the probabilistic linear model stems from the implicit hypothesis upon which rest all probabilistic interpretations of the estimation procedure. In fact, everything is understood as if the available sample were the result of a random draw among several hypothetical draws, random and independent, carried out in an infinite population or in a finite population with replacement. This conception appears to be in flagrant contradiction with the reality of an economic time series. The latter translates the evolution of a magnitude in a unique temporal and spatial context in the sense that there is found its exact duplicate nowhere else and at no other moment in history. (How much does this matter for reliable functions? And volatile functions are not valid anyway.)[12]

We argued in Chapter 6 that there is a world of difference between the applied and exploratory realm in econometrics (see also Leamer, 1978). The most important works in applied econometrics are still those that lead to the development of large models which are used to guide economic policies. These models, even when they are fairly detailed, cannot but call upon aggregate quantities whose values, unique, at a time t, cannot be considered to be drawn randomly from a very large, indeed infinite, population. Malinvaud (1991b, p. 384) is aware of this. To paraphrase him, our ignorance cannot be represented (well or badly). He argued that the imprecision of statistical data, always subject to error, obviously affects the precision of econometricians' work. The thing is known, but it is hardly taken into account in any explicit manner. The fact is that, unless by exception, the imprecision of statistics escapes measurement. The impression prevails, however, with today's econometricians, that the imprecision of macroeconomic data series relative to western countries rarely constitutes the principal cause of the difficulties encountered in the estimation of laws or the testing of hypotheses. According to Malinvaud, the complexity of the phenomena themselves and the multiple disturbing influences to which they are subjected are considered to be much more harmful.

Mouchot (1996, p. 194) argued that, in practice, certain models are accepted (except those mentioned below) while others are rejected; it is therefore good that the econometrician has at his or her disposal a tool for such decisions. In the end, no matter the refinements brought to the methods, this tool is, we know, the coefficient of correlation of the descriptive statistic: it is the proximity of the theoretical model to the confidence region of the points (C_t, R_t) that decides the validity of this model.

Mouchot (ibid.) observed that perhaps this proximity has nothing to do with any representation of our ignorance, since a perfect correlation can go hand in hand with an equally perfect absence of causality. Mouchot (ibid., p. 194) notes (as is well known) that it is always possible, by manipulation of the data, to obtain satisfactory adjustments on a statistical level. Indeed, Hendry (1980; 1993) provides an example: with error terms following an autoregressive process of the first order, a regression of P, implicit deflator of expenditures of consumption, on C, accumulation of the precipitation in the UK offers a spectacular adjustment as much on the level of past observations as on the level of forecasting. It is thus, remarks Hendry, stripped of all meaning to speak of confirming theories when we can so easily obtain fictitious results.

This leads us to another problem. First of all the function f is not specified. We know that this specification must be done before the econometric procedure, since this will decide whether to accept or to reject the function thus specified. Let's follow Mouchot (1996, p. 194) and take his example $f(y_i) = ay_i + b$ to illustrate the point. Suppose first of all that we are led to reject this specification; at least two paths are open to us. Either we can modify the form of the relation (we could attempt, for example, to test the model $\text{Log } c_i = a \text{ Log } y_i + b$); or we could attempt to introduce new explanatory variables. In the first case, we are assured of being able to find forms of function f that bring it closer to the confidence region of points; in the second, we are assured of improving the overall coefficient of correlation, whatever the new variables introduced (which does not, moreover, necessarily mean that the new model will be judged superior to the previous one). It is obvious that econometricians are not going to try just anything to obtain a better correlation; in particular, Malinvaud constantly insists upon the necessity of testing models stemming from economic theory (see also Haavelmo, 1944; Hollis and Nell, 1975; Klein, 1982). However, with the help of the great calculating power of computers, we can see the danger of finding explanations post-factum, which is to say more often than not ad hoc, with high correlations.

However, to paraphrase Malinvaud (1991b, pp. 445 and 467), in practice things are less pure and can even give rise to contestable abuses when the model is chosen on the basis of the data analysed, in such a way as to lead to results that look good – so much so that the econometrician may have refrained from mentioning all the preliminary attempts that had to be carried out. Without deserving, still, the accusation of data mining (see Leamer, 1978), the search for a suitable specification is often carried out in a manner very different from the image presented by econometric theory.

Let us now suppose that we are led to accept this specification. Malinvaud (1991b, p. 354) argued that it is in order not to overburden the language that we speak of accepting the hypothesis when we do not succeed in rejecting it. With this reservation in mind, we know that the real values $\hat{a}$ and $\hat{b}$ that were supplied by statistical induction will be considered as estimations of the true values: a and b.

Mouchot (1996, p. 195) observed that the problem here is with the word 'true'; in fact, we wrongfully extend to econometrics affirmations that have meaning only in other domains, such as that of the control of manufacturing. In this last case, the real average value of the diameter of factory pieces exists, even if it is unknown and if it is the subject of an estimation by random sampling. In the case of reliable functions, something analogous exists; it is costly – sometimes in monetary terms, sometimes in social terms – to fail to live up to the rules. The social or technological rules or design will provide us with the true values, what 'should be done'. But for the volatile functions it is not at all the same in our example, since the parameters a and b do not exist. Because of this objective identity between the sample and the population, to which reality can we reduce the concept of real value of a parameter that we are trying to estimate? (Introduce the notional problem of the cost of not following the socially defined rules governing reliable relationships and defining 'true values'.)

Thus the specification $f(y_i) = ay_i + b$ is at best only a (good) approximation of reality. To paraphrase Malinvaud (1991b, pp. 471–2), in classical mathematical statistics, as in the methodology that underlies the current work of econometricians, we admit that the random model offers an indisputable reference; it certainly operates with more or less unknown elements but, aside from that, we act as if we were certain of its validity. Now we have seen that this validity could be most often only approximate; even worse, this approximation has a somewhat conventional character. The solidity of the approach is open to question.

8.6 IDENTIFYING RELIABLE RELATIONSHIPS

Malinvaud constantly insists upon the following: first, develop a theoretical model, and only afterwards verify, using the econometric approach, the causality thus proposed. However, this verification, whatever the probabilistic equipment put to work, remains founded upon a simple correlation; and, because of this fact, it does not have, nor will it ever have, anything but the status of a provisional proof: the absence of correlation entails the absence of causality; the presence of correlation only allows for the possibility of causality.

We argue that this challenge is misdirected. There are plainly cases where quite solid results can be obtained, even though many desirable conditions may not be fulfilled. Neither consumption spending nor income fit the strict idea of a 'random draw', any more than employment and output. But all reflect the stability of well-established institutions and technologies, and are key parts of the socio-economic system. And propensities to consume and labour productivity have been reliably estimated over and over again, in many, many different environments. Moreover, shift factors for these relationships have also been estimated; good multivariate functions have been established. It is true that many of these have changed over time, and the functions have generally been somewhat different in different economies. But that is to be expected; that is why Nell (1998a) calls his approach transformational growth – as an economy grows, it changes. So we would expect an estimated function only to hold for a certain length of time, and then to change as the economy undergoes transformation (see Errouaki, 2003).

However, there are other relationships, important ones that are not at all reliable. Efforts to estimate these have generally failed, or at best have succeeded only in establishing fits that held for short stretches of time. Investment functions are notoriously unreliable, as are interest rates and the speculative demand for money, as well as the stock market and virtually anything.

The two cases of reliable and volatile are quite different. But most work runs them together. Nell gives an example (1998a, p. 58) citing the work of Ray Fair (1984), who derives 'multipliers' from the interaction macro-model as a whole (as discussed in Chapter 11). But such multipliers, estimated following Fair's method, are defective compared to traditional, well-grounded expenditure multipliers. The latter depend on purely reliable functions but in Fair's approach are mixed with the often worthless estimates of volatile relationships. The result apparently has some temporary value, but it is evidently not up to expectations. Many of the problems faced by traditional econometric models can be thought of this way: the models are a mix of reliable relationships that can be reasonably estimated and volatile ones that simply cannot. The result is an unstable mix of apparently reasonable calculations that mostly seem on target, with carefully estimated functions that turn out to be wildly wrong. What is needed is to separate the two.

On the other hand, Mouchot (1996, p. 197) argued that, since the 1980s, the large models of the leading French Economic Institute, INSEE, use the method of ordinary least squares only for the estimation of the parameters of their equations; they have thus abandoned the distinction between exogenous (certain) variables and endogenous (probabilistic) variables, the necessity of transforming the structural model into a reduced model discharging the problem of identification, and so on. They have come closer, in fact, to the simple correlation of the descriptive statistic. We must, moreover, note that the equations are preserved whatever the values of the coefficient of linear correlation R^2 (thus, in Mini-DMS, a model used by the INSEE, there was an equation whose coefficient of linear correlation R^2 had a value of 0.018!).

We have already said that all these hypotheses were necessary for developing assorted predictions with a confidence interval for their fulfilment. It is therefore finally on this level that we must judge the pertinence of their introduction.

8.7 THE FAILURE OF PREDICTION

Leontief (1971) famously asserted that in no other field of empirical enquiry has so massive and sophisticated a statistical machinery been used with such indifferent results.[13] Leontief wrote (ibid., pp. 1–2):

> An uneasy feeling about the present state of our discipline [. . .] has been growing in some of us who have watched its unprecedented development over the last three decades. This concern seems to be shared even by those who are themselves contributing successfully to the present boom. They play the game with professional skill but have serious doubts about its rules.

In the same vein, Feldstein (1984)[14] observed that one of the great mistakes of the past 30 years of economic policy has been an excessive belief in the ability to forecast. To top it off, Kennedy (2003, p. 363) visited wonderland:

> How can you possibly award prizes when everybody missed the target? said Alice. Well, said the Queen, some missed by more than others, and we have a fine normal distribution of misses, which means we can forget the target. Alice-in-Wonderland logic.

Even if economic forecasts are poor, there are none better, and perhaps a poor forecast is better than none at all. Klein (1984) provides a good exposition of how forecasts are used together with examples of their successes. Furthermore, Simon (1994) argues that although economic forecasting is bad in the short run, it is quite good in the long run, primarily because economic laws tend to dominate over long periods of time. (Forecasting falls into two main categories: causal forecasts and time-series forecasts. Only the first is dealt with here.[15])

Causal forecasting with econometric models works as follows: once the econometrician has provided the estimates of an economic model, the model can be employed to forecast the dependent variable, assuming that the associated values of the independent variables are given. It is this forecasting method, based on the *causal* interpretation of the economic model, that is referred to here by the expression 'causal forecasting/econometric models'.

Although the primary goal of structural econometrics is to provide good parameter estimates, the production of good economic forecasts is viewed as a goal of equal importance by many applied econometricians. Indeed, the use of econometric methods enables us to state in precise numerical terms the explanation of the phenomenon provided by the model and thus to improve our understanding of the economic facts. Although this explanation is necessary, it is not an end in itself, but should directly or indirectly help us to reach economic decisions. Before a final decision is reached, some forecasting must usually be done.

Now we turn to the issue of failure of prediction. To illustrate our point, let's follow Mouchot (1996, pp. 197–9) and consider the early 1990s recession in France, a recession that the vast majority of forecasters had failed to predict. Mouchot argued that the winter of 1992–93 saw France's deepest post-war recession. This focused the attention of economic agents on short-term economic analysis and on employment and unemployment statistics. This scrutiny highlighted some of the system's imperfections. He observed, for example, that while the 1994 recovery was forecast with great accuracy, the preceding recession had gone undetected until the last moment. Moreover, the job figures underwent many revisions. These problems signalled the need to adapt statistical methods as well as national accounting and analytical approaches to a French economy that had become more cyclical and more open to the rest of the world. The pressure from European institutions intensified with the start of the first phase of economic and monetary union. There was now an urgent need to ensure better comparability of EU member states' data, particularly for the aggregates used to track economic convergence. At the national level, the concepts of decentralization, deconcentration and administrative streamlining became ubiquitous. The new INSEE team (in Champsaur's era) formulated its policy – without breaking from that of its predecessors – by spelling out medium-term goals. Four basic priorities were defined: more international involvement, closer ties between the statistical system and business firms; adjusting the Institute's methods to a now more cyclical economy; and broadening the regional offices' range of activities. The medium-term goals were first implemented through the adoption, on 1 July 1994, of a new organization chart for the head office. The main change was the creation of a business statistics directorate. Its structure was clearly designed to assert a commitment to consistency in the business statistics system and to improve relationships with firms, both as survey respondents and data users (with an emphasis on fieldwork).

Mouchot (1996, p. 197) recalled that, during the summer of 1992, the predictions of French growth for 1993 were between 2.2 per cent and 3.4 per cent, while in October 1993 those predictions had fallen to between –1 per cent and –1.6 per cent. Moreover, a similar significant mistake had been made 5 years earlier, but in the opposite direction: there again, almost no forecaster had predicted the boom of 1988. The public obviously wondered what were the causes of such errors; so did economists themselves.

A story of forecasting failures of similar magnitude can be told about the USA in the 1990s. Again there was a recession that was only imperfectly foreseen; but in the US case the real failure came first in not foreseeing the strength of the boom, and, second and more importantly, in not foreseeing how far unemployment could be reduced without harming the economy. Indeed, the entire apparatus of policy analysis for inflation simply collapsed. Most major econometric analysts – with the outstanding exception of Robert Eisner – agreed that the 'natural rate of unemployment' was to be found at or above 6 per cent. When unemployment fell to 6 per cent, we could be sure that inflation would begin to accelerate. In fact, as the boom progressed, inflation fell rather than rose, and unemployment dropped below 4 per cent, not only with no sign of inflation, but amid increasing worries about the prospect of further deflation![16]

Three standard reasons for prediction failures have been suggested: unreliability of the data, errors of analysis, and the problem of structural changes. To these we add a fourth: inherent volatility. That shifts in the volatile variables are impending may be apparent; but the timing is inherently unpredictable. It is especially important to note that this volatility has been largely financial, and that these failures of prediction took place in a period of vast financial expansion. The previous econometric model failures – in the 1970s – concerned oil prices and the breakdown of the Bretton Woods system and its replacement by a system of flexible exchange rates, which introduced volatility into areas where it had not previously existed.

We have already dealt above with the question of unreliable data. It is certain that the definition and the collection of data require constant improvement, which can only lead to better predictions. A perfect example of this is the modification of the definitions of the monetary aggregates: the new aggregates allow us to better follow the situation; let us note nonetheless that this modification is rendered necessary by the changes in the monetary behaviour of agents. But let us look at the other two standard explanations, before returning to the question of inherent volatility.

8.8 CRISIS IN VISION

Unlike hard sciences, such as physics or chemistry, economics cannot proceed via a formula of replicable experiments: each economic event is unique in time, place and nature and the result of innumerable never-to-be-repeated sociological and factual variables. We may refer here to an analogy used by Nell (1998a, p. xxiii) to further clarify this important point:

> Atoms have no history and they don't behave at one time, in one era, in one way and then differently at another time and place. However, markets may do just that. They are social institutions, and institutions develop and change, historically. Therefore, if markets do change, then a theory describing their working may be true for one era, for one time and place, and false for another.

This observation poses a significant challenge to the terms of current debates on econometric methodology. The objectives of economic theorizing and econometric modelling have to be adapted to the changing nature of the economy, which, in turn, has to be understood more broadly than is usual in mainstream thinking. Unfortunately, the problem is that there is no room for such ideas in mainstream economics.

Furthermore, Heilbroner and Milberg (1995, particularly ch. 7) argued that the deep crisis that affects modern economic theory today derives from the absence of a vision. This term is defined as 'a set of widely shared political and social preconceptions on which all economics ultimately depends'. Heilbroner and Milberg attempt both to analyse this sad state of affairs and to explore the direction in which economic thinking should eventually move in order to regain its relevance.

There is a multiplicity of theoretical representations to which economic reality gives rise, and choices have to be made among this diversity; often, at least partially, upon ideological criteria. Heilbroner and Milberg, following Schumpeter, term this the 'pre-analytic vision' that motivates the development of theory. We have argued that this must include an explanation of how the socio-economic system can continue to exist, hence it must account for the basic reproduction system.

Mouchot (1996, p. 198) argued that the multiplicity of models could easily constitute one cause of error: for example, with the policy of supply followed once by French Prime Minister Édouard Balladur's government. However, this cause cannot be important, since in this case *all* the forecasters were wrong.

A possibly more important reason for policy errors may be found in the ageing of the general structure of the majority of (mostly neo-Keynesian) models. We think that this point raises three significant issues: (1) The basic models were developed during the 1950s and 1960s, a period marked by limited inflation, moderate unemployment, and low interest rates. Such models cannot give an account of problematics that appeared later, like the changing structure of business and household debt, the role of salaries in unemployment, or again the factors of competitiveness other than costs (research, education, organization of work, etc.). (2) The neo-Keynesian equations tended to represent production and productivity inadequately (Okun's law, Kaldor's laws), leaving the models mere 'black boxes' with partially indeterminate or poorly modelled behaviour regarding productivity, wages and prices. (3) The representation of behaviour generally tended to be too simplified: aggregation obscured the changing character of the heterogeneity of the agents (the division of poor and rich households has shifted, the ratio and character of small to large businesses has changed, new sectors have emerged, etc); a much too naive description of the formation of 'anticipations' upon which decisions are based; too unpolished an approach to monetary and financial behaviour, and the financial sector has grown markedly relative to the rest of the economy; moreover, it offers a whole new range of services; and a fixed and conventional statistical framework.

To paraphrase Mouchot (ibid., p. 198), it is not so much errors of analysis that are to blame, but rather a failure to adapt and upgrade the models used. It seems, then, that it might suffice to develop other, newer, types of models. This is, moreover, just what many econometricians are employed to do. However, the problem may be deeper still, since some go so far as to blame national accounting itself: it, too, was conceived in the immediate post-war period according to a fundamentally Keynesian pattern, several of its concepts imposing themselves one way or another upon the model builder; reciprocally, it is often not in a position to supply the elements that new types of models attempt to take into account (it was not conceived for this purpose). Indeed, Volle[17] (quoted by Mouchot, ibid., p. 199) argued that the 'old system no longer works. However, there is nothing with which to replace it. National accounting has been indispensable. We may need to rethink it completely. But as long as the new world economy is not clearly defined, this project will not attract people of imagination capable of realising it'.

We are faced with the same technical problem as that of the modification of the monetary aggregates; but defining new aggregates is otherwise obviously easier and infinitely less costly than attempting to reconstruct upon new foundations that gigantic tool which is national accounting.

Let us note, finally, that all the elements presented by Nell (1998a) amount once again, in fine, to changes in the characteristic patterns of behaviour by economic agents, which leads us to the final problem.

8.9 TAKING STRUCTURAL CHANGES INTO ACCOUNT

Mouchot (1996., p. 199) argued that 'the failure of prediction in the recession of 1993 basically came down to a failure to take into account changes in the behaviour of economic agents. The fundamental question is, therefore: is it possible to build models that guarantee this taking into account? Our overall answer is obviously no. Nonetheless, it appears necessary to distinguish at least two different issues'.

The first is the one where the changes of behaviour result from major institutional or technological changes, and will eventually be stabilised again on a macroeconomic level. It is this case specifically that Volle calls to mind in the previous section: let us wait for the new world economy to structure itself; we shall then be able to model it. . . at least, until the appearance of a new world economy. Is it possible to illustrate more clearly the fundamentally provisional character of models, their lifespan finally being only that of the economic structures that govern the kinds of behaviours being modelled? It is very clear that we must also model the development of these structures if we are to hope to have good predictions as long as they last. (This, of course, is what the theory of transformational growth (TG) sets out to do).[18]

However, we must not believe or let it be believed that a series of good predictions would show a definitive advance in knowledge of economic reality: this reality evolves and the correlations highlighted for a time will become inexact. For example, today, households play a more important role than they used to. We must therefore pay more attention to indicators of confidence (see Gherardi, 1993, p. 32, quoted by Mouchot, 1996, p.199). Furthermore, Gherardi (ibid.) argued that 'the French retain a very mercantilist view of the exterior environment: is someone going to buy something from us?' He pointed out that 'monetary factors explain much better the reversals of circumstances'.[19] In these cases, we see new links appearing that the model builder must specify and which will allow him a better prediction. Here again we can see the relevance of fieldwork in the construction of realistic forecasting models.

But there seems to be a built-in resistance to this kind of criticism. Even though the problem is recognized, there is a tendency to lose interest in it or minimize it. Thus to paraphrase Malinvaud (1991b, p. 383), practice leads us to think that this possibility is exaggerated in relation to the very real risk that, keeping in mind the complexity of the phenomena, the information contained in limited data series might be too poor. In the same way, he includes changes of behaviour among the causes of prediction errors in the models only in regard to studies of the anticipations of agents – and this comes in a critique (otherwise very detailed) of econometric modelling (Malinvaud, 1991b, ch. 16).

However, Malinvaud devotes only two pages to Lucas's Critique, which contends that the effects of a change of economic policy cannot be correctly predicted by a model developed before this change, since the change will modify the behaviour of the agents, and thus renders the old model obsolete. However, this is not the same issue; Lucas's changes in behaviour are due neither to the changing character of institutions, nor to volatility in financial and capital variables. His criticism, perfectly founded on an extreme version of neoclassical theory, is just as perfectly immune to realistic application: pushed to its extreme, it says that *any* innovation in any agent's behaviour (not just policy-makers) that affects other agents widely or strongly must undermine the pre-innovation model. So it forbids all neoclassical model building and therefore all forecasting. It is the reductio ad absurdum of intertemporal optimizing.

In parallel, and as a counterpoint, Malinvaud (1991b, p. 447) recognizes that the 'laws which are well established over a certain period become inexact during other periods, a case which reveals a certain fragility of the work of econometricians, as indispensable and competent as it is; and that the models actually used are of a principally Keynesian kind'. (Here we recall Goodhart's law, that 'any established relationship used as a basis for policy will sooner or later break down'.)

To conclude this section, consider a paradox: the results obtained by the econometrician appear to be:

1. well-grounded: the hypothesis of non-variance in the rules and preferences governing behaviour generally constitutes a good approximation of the reality; it recognizes the fact of socio-economic inertia; and it is this inertia that, during relatively long periods, gives validity to econometric predictions, thus authorizing important political choices. But also
2. irreparably provisional: either, most often, by the slow evolution of behaviour; or, sometimes, by a sudden and unforeseen change that reminds the forecasters just as suddenly of the contingent character of the dependencies upon which they were basing themselves – the recession of 1993 being a perfect example of this.

8.10 A DIGRESSION ON TIME SERIES ANALYSIS

Some of the critics of structural econometrics considered its flaws deep and irreparable; for example, assuming a normal distribution – or, indeed, for the most part, any other – could usually not be justified. Specification drew on a body of conventional theory that itself rested on implausible and unrealistic assumptions, and identification usually rested either on such theory or on ad hoc grounds that were often highly debatable, and frequently came from outside normal economic data.

These problems led to a serious lack of determinacy in the Cowles approach when faced, for example, with the choice of model or specification. Decisions had to be made that seemed arbitrary, or based on the preferences or pet theories of researchers. Cowles offers no full-scale or general solution to the model selection problem, and this ambiguity carries over and blends into the identification problem, where the difficulty may be even worse – at times there may be many conflicting solutions, or no solution at all! Some critics, observing that simple statistical projections based on time series provided forecasts as good as or better than those of large models, proposed abandoning the models for approaches based on analysing and projecting the properties of various time series. Finding the arbitrary decisions unacceptable, many critics turned to approaches based on time series analysis, such as VAR, thinking to approach the data without theoretical preconceptions, allowing the data to 'speak for itself'.

But before a data set can speak for itself, it has to be put into a format that will allow it to be understood: a time series has to be separated into the 'trend' and the 'cycle' – each of which has a different voice and a different message. That is, non-stationary series have to be reduced to stationary; if this is not done, t and DW statistics, and measures like R^2, will not work properly and are likely to give rise to spurious results. Furthermore, if the analysis is to be usable, it has to be applicable not only to the sample stretch of the series available in the data set, but also to unobserved stretches of the series.

And this means that, while time series analysis is important in itself, it provides no escape from the necessity of making strong and hard-to-justify assumptions. Just as the Cowles approach requires the very dubious assumption of normal distributions, to be able to get good results, time series analysis likewise has to rest itself on an unjustified and perhaps unjustifiable assumption, that of ergodicity (namely, that the properties of a short sample of the series will converge with those of the 'true' series as the length of the sample tends to infinity).

Of course, many time series will change character at some point. They will exhibit pronounced characteristics over a long stretch and then change and a new set of characteristics will emerge. There is a structural break in the series, although it may be hard or impossible to say at what exact point the break occurs. We may want to analyse the time series in a certain way, perhaps even treating it as ergodic (or, 'as if' it were ergodic) up to this point, but after the breakpoint it will have to be considered a different series, even though it consists of the same variables, in the same succession, collected or reported in the same way.

For example, retail and wholesale price series for most advanced countries in the nineteenth century shows sharp movements both upwards and downwards, with about equal variance, and a slight but marked trend downwards over the whole century. But the downward trend disappears as the twentieth century begins. Then a period of instability follows in which there is great divergence between countries, many experiencing dramatic inflation followed by total collapse. After World War II, however, the fluctuations settle down but very significantly change character; there is now a clear general upward trend, different in different countries, but weakly correlated; prices almost never fall and the cycle is now to be seen in the rate of change of prices, rather than in the levels.

Identifying the precise point, or even the period, at which the structural break takes place turns out to be difficult; there is no infallible method capable in general of giving a justifiable and unique result. Then there are questions about how the series is to be separated into trend and cycle, and the changed characteristics of each. The series after a break may differ in the trend, and the trend itself may change periodically, either regularly or randomly. The various characteristics of the cycle may change at a break – mean, variance, periodicity. But the point of the break might be different for the trend and the cycle; some characteristics may change before others, and there might be a succession of such breaks, where this succession might itself, for some purposes, be considered to constitute a time series.

So the time series approach has a set of problems analogous to those of structural econometrics. Ergodicity is as hard to justify as assumptions about probability distributions (but we would offer a limited defence based on the idea of a stable socio-economic system). Structural breaks cannot be identified precisely on purely statistical grounds; there is room for debate and, ultimately, judgement calls. Then, as mentioned, there are the issues around the question of the method of detrending, and these need further discussion.

Two kinds of stationary series can be distinguished: difference stationary and trend stationary. The first is established by repeated differencing: a stationary series is I(0); a series that needs to be differenced once is I(1), and so on. Differencing, of course, leads to information loss. When the trend is removed by applying a 'filter' – the Hodrick–Prescott, the Bandpass or some other – the trend data are not only preserved, but are 'smoothed' and can be graphed and used. But different filters smooth the trends differently. Moreover, the trend might be removed by another process, such as 'penalized splining' (Kauermann et al., 2010; Stock and Watson, 1999; Zarnowitz and Ozyildirim, 2005). As the diagrams at the end of the Kauermann, Krivobokova and Semmler paper make clear, the different ways of detrending (applied to macro series developed and analysed by Stock and Watson) lead to different results. Sometimes these differences are minor, but on occasion they can be major.

Indeed, in the process of taking out the trend, there is the question of selecting the way of smoothing it, or of presenting it as a succession of linear segments. This will sometimes simply be determined by the choice of filter, but there may be choices or modifications to be made along the way. Again, it does not seem possible in general to determine the right approach on the basis of statistics alone, raising the problem of model selection.

Even prior to all of this, if there are a number of non-stationary variables that may be important to the question at hand, it will be important to test for co-integration. This leads to a new nest of problems and opportunities, beyond our scope here, but it is an area in which it is generally admitted that economic theory is necessary to provide guidance. We would add that fieldwork and conceptual analysis might be even better.

Going back to time series, once the series has been determined to be stationary, in the effort to make the data relevant, it may be proposed to regress the variables on themselves, and at this point there emerges the question of selecting the appropriate lag structure. Different lags can lead to very different results, and there seems to be no decisive reason to prefer one lag structure to another. Indeed, the same can be said for the decision about which set of variables to include in the first place: there is no general criterion. That is to say, there is no reason to be drawn from statistics or from general methodology. Economic theory might provide reasons, of course, but that just brings us face-to-face with the model selection problem.

In other words, time series econometrics is good and interesting, and is needed to complement the Cowles approach (see Malinvaud, 1988; Spanos, 1989). It is indispensible, and has become highly sophisticated; it is able to handle important problems, and sometimes contributes insights that the Cowles approach could not have developed. But it is no more able than traditional methods to reach unique and unambiguous results based on statistics alone. (On the basis of our earlier arguments, we might add that combining statistics with neoclassical theory will not help.)

8.11 VOLATILITY

The volatility of variables and relations is based on the fact that they are grounded in expectations about the future, concern capital accumulation and capital accounts – capital items in the balance sheets of households and firms – and are monetary or financial rather than real parts of the economy. These relations are volatile because they cannot, in principle, be stable, since there is no way of knowing the future.

On the other hand, as Keynes repeatedly pointed out, very important and practical decisions have to be made on the basis of our expectations of the future. By definition, the value of a capital project, today, is the discounted value of the stream of future returns from that project. This requires estimating both how well the technology will perform and how markets are likely to develop. We can make educated guesses about these matters, but there is no way of knowing in advance.

This has been noted by econometricians, but they tend to see it as a general problem, rather than one linked to specific areas of the economy. It has been remarked that behavioural changes of agents sometimes either appear truly unpredictable, even after the fact (which does not prevent us from explaining them), or else do not seem to have to fix themselves within new macroeconomic structures. Sometimes this is considered part of the essential indeterminism of economics, which we have already indicated. Agents surely do know more than before and this knowledge opens up a new freedom of choice in their behaviours; the result is clear: a reinforced unpredictability, and this at all times.

All of this, econometricians know. Indeed, Hendry (1995b, pp. 173 and 174) argued that there exist, naturally, important differences between the social and the other sciences. These are reinforced by the rapidly evolving and non-stationary nature of economic behaviour. This leads to a discussion of the status of empirical observations in a non-stationary environment, another paradox of corroboration. Furthermore, Malinvaud (1991b, p. 383) has argued that the phenomena being studied may be modified between the observations of the first period and the last. Economists are increasingly sensitive to deep modifications that might be provoked by the transformation of institutions and of behaviours following some accident – or, somewhat differently, following a long tendency of history.

Why not carry these statements through to their logical conclusions and recognize that economic laws are in a state of permanent (r)evolution? This, indeed, is Nell's (1998a) position in the TG approach – with this proviso: for large parts of the economy, the laws are stable for long periods and changes accumulate slowly. But most practitioners of econometrics are likely to shy away from accepting permanent change for two reasons: first, their conception of the scientific aim itself leads them to hope to discover universal laws, following the model of Newtonian physics. This, we argue, is a philosophical mistake; econometric models, at their best, will not establish economic laws analogous to those of Newtonian mechanics, for the simple reason that there are no such laws to be found. What econometrics can do, however, is show us how the economy works, given its present institutional configuration – how it works, precisely and quantitatively (see for example Haavelmo, 1944; 1958; 1989). The economy does work; the socio-economic system does reproduce itself; there are regularities to be discovered (Chapter 3). The second reason is the fear – unjustified according to us – that the recognition of the provisional character of econometric correlations comes down to abandoning the entire econometric approach, even as it appears today to be the only branch of economics that tries to validate its results by a confrontation with the facts.

In effect, if we have criticized the econometric approach, it is because the theoretical tool often appears to us unsuitable for its object. Yet it remains the case that correlation on the one hand, and taking into account the multiple links between economic variables by models on the other hand, are indispensable to understanding today's economic reality. Using these to model the economy empirically, in various ways, drawing on a process of intelligent extrapolation is not only the best, it is perhaps the only way to understand in a quantitative way how the economy actually works and will work, even though it is based upon information that is poor, partial, and already partly obsolete – but is all we have at our disposal.

8.12 CONCLUSION AND SUMMARY

A reckless tendency on the part of certain econometricians leads them to conclude that a fairly rudimentary model can and does account for economic reality. This certainly merits an acerbic comment; but the deeper difficulties may be due more to a failure of vision on the part of econometricians in general, than to any flaw in the econometric approach per se. It was this kind of superficial thinking and evasive reaction to critiques that led Ragnar Frisch, after several communications to the World Econometrics Congress, to complain 'all that is playometrics, not econometrics'! Indeed, econometrics must have relevance to concrete realities; otherwise, it degenerates into something not worthy of the name; it becomes little more than playing games with functions and sets of numbers.

But 'playometrics' flourished. For decades, the conceptual underpinnings of econometrics were left unquestioned. Spanos (1986) pointed out that in the late 1950s and early 1960s philosophers of science began seriously reformatting the logical positivist approach (with its questionable tendency to favour observed data as the source of truth at the expense of theory), turning it into a more centred logical empiricist approach. Yet, in spite of this, no practitioners thought to criticize the outdated logical positivist core of econometric methodology. But in the case of economics, such a critique had been made nearly 40 years ago by Hollis and Nell (1975). They argued that neoclassical theories of economics were unsound and that they relied for defence on a positivist theory of knowledge, which was also unsound. Unfortunately, in the case of econometrics, even now many in the mainstream continue to believe that they are marching under the 'Popperian flag', sometimes also waving the 'Friedmanite banner of prediction'.

This brings us back to the forecasting issue. In spite of the hoped-for kinship of econometrics to the experimental approach (empirical verification of theoretical models), we noted numerous and important errors of prediction around the year 1993. This is one of many serious failures – yet not all of the econometricians' difficulties should be ascribed to them: rather, bad definition of magnitudes, difficulties of measurement, and availability of these measurements have to take much of the blame.

But it must be asked: are the power and the complexity of the statistical tools used by econometricians really adapted to their object? We can be doubtful: the basic pattern of a random draw of a sample in an infinite population does not correspond to the reality of time series; the status of the remainders is the subject of different presentations on the part of well-known econometricians; the very hypothesis of the normal law is itself questionable.

Questionable – in some areas, certainly, but perhaps not in others. For too much has been attempted. Some parts of the economy can be modelled; but other parts cannot. They can be guessed at, bets can be placed on how things will unfold (that is what the stock market does, among other things) and attempts to control can be imposed. But what happens in these volatile sectors cannot be predicted or modelled, except for short and unreliable periods. By contrast, the other parts can be modelled quite effectively, provided that the theory is itself based on actual practice, as understood through fieldwork; and there is some reason to think that the probabilistic approach will work rather well in those regions.

Despite this hopeful circumstance, the outcomes for the economy as a whole depend on the interaction of the reliable and the volatile parts, and it happens that predictions are very far from reality. The reason that more and more practitioners give to explain this is the report according to which the economic mechanisms are presently undergoing profound transformations and that the concepts, the models, and even national accounting (which articulates the set of numerical concepts available) are no longer adapted to these new mechanisms. That is surely part of the story, for we are seeing the development not of a 'new economy', but certainly of new sectors in the economy, and new relationships in old sectors. But a large part of the story can be traced to the fact that econometricians have so far not meticulously separated the volatile from the reliable parts of the economy, first modelling them separately, and then carefully putting them back together.

NOTES

1. Pagan (1987) provides a good account of what Kennedy (2003) called the wakening of the profession's interest in econometric methodology. Pagan (1995) is an update. Granger (1990) contains a selection of articles prominent in this major debate. Hendry et al. (1990) provides an instructive informal discussion of these issues. Hendry (1993) is a selection of papers tracing the evolution of the TTT (test, test, test) econometric methodology. Hansen (1996) is an interesting review and critique of Hendry (1993). Hendry (2000) analyses the effectiveness and validity of applying econometric methods to economic time series. Dharmapala and McAleer (1996) discuss econometric methodology in the context of the philosophy of science. And finally Stigum (2003) provides a semantic account of econometrics.
2. Swann argued that 'Keynes's metaphor captures exactly what has gone wrong with our over-reliance on econometrics in the second half of the twentieth century'. To use

Swann's expression, econometrics was seen as a universal solvent. According to Swann, 'of all the critiques to econometrics, perhaps Keynes's (1939) is the most famous, and arguably the most telling [. . .] Keynes – [the] finest social scientist of the 20th century, an accomplished mathematician and logician [. . .] could have easily mastered the technical side of econometrics, had he chosen to do so, but [. . .] argued from an early stage that he thought it a misguided sort of statistical alchemy'. See Swann (2008, chs 3 and 5).

3. Spanos (2009) argued that the 'discussion of the data in Moore (1914) gives enough clues to suspect that inaccurate data is likely to be another serious source of error contributing to the unreliability of any inference based on (1) [estimated model of Moore's (1914, pp. 62–88) 'statistical demand' curve for corn]. In particular, the averaging of different prices over time and taking proportional differences is likely to distort their probabilistic structure and introduce systematic errors into the data'.
4. For an account of the history of statistical reasoning and the issue of the realism of statistical production, see Desrosieres (1999).
5. Spanos (2006a; 2009) argued that the primary potential sources of error contributing to the untrustworthiness of evidence include: statistical mis-specification ('the statistical premises of inference are invalid vis-à-vis data z_0', Spanos, 2009, p. 5); inaccurate data (see Morgenstern, 1963); incongruous measurement (this occurs when 'data z_0 do not adequately quantify the concepts envisioned by the theory', see Spanos, 2009, p. 6 and also Desai, 1976); substantive inadequacy (this 'concerns the extent to which the estimated model captures the aspects of the reality it purports to explain in a statistically and substantively adequate way', Spanos, 2009, p. 6). Unfortunately, Spanos (2006a) tells us, 'very few' of the published applied econometric papers over the last 50 years are likely to pass the statistical adequacy test. This raises serious doubts about the trustworthiness of the mountains of supposed 'evidence' accumulated in econometrics journals during this period (see also Spanos, 1995). Indeed, as Spanos (2009, p. 5) puts it, 'in most cases the modeler is not even aware of all the probabilistic assumptions constituting the statistical model used as a basis of his/her inference. Even worse, statistical inadequacy is only one of several potential sources of error that could render empirical evidence untrustworthy'. For an account, see Nell and Errouaki (2006a).
6. Shaikh's interest in providing an empirical framework that would correspond to Classical economics categories dates back to 1972–73, when he discovered Shane Mage's path-breaking work and Edward Wolff's paper on input–output-based estimates of the rate of surplus value in Puerto Rico. Mage's work emphasized the distinction between productive and unproductive labour, but was restricted to only the value-added side of national income accounts. Wolff's work was located within the more comprehensive double-entry framework of *input–output* accounts but did not distinguish between productive and unproductive labour. This led Shaikh to develop a comprehensive framework that drew on both approaches and he worked closely with Leontief.
7. In correspondence, Shaikh pointed out that many people were instrumental in helping to overcome these and other related barriers. He observed that in the early 1980s their attempts to utilize input–output data were greatly hampered by a lack of computer facilities. Michel Juillard was working at the New School and at the NYU Institute for Economic Analysis on recasting US input–output and national income account data into a Marxian departmental schema. Katherine Kazanas's work focused on the impact of the distinction between production and nonproduction labour for the measurement of productivity. Julie Graham and Don Shakow provided crucial support in the manipulation of the input–output tables. Ernest Mandel and Dimitri Papadimitriou helped secure funding at various points. With the help of Eduardo Ochoa, Paul Cooney and Michel Juillard, Ara Khanjian created an input–output database and used the basic framework to measure and compare money and labour value flows in the United States.
8. For further details on the mathematical treatment of identification, see Spanos (1986, ch. 25; 1990a; 1999) and F.M. Fisher (1959; 1966).

9. If an equation is identified, it may be either just identified or over-identified. An equation is just identified if the number of identifying restrictions placed on the model is the minimum needed to identify the equation; an equation is over-identified if there are some extra restrictions beyond the minimum necessary to identify the equation (Kennedy, 2003).
10. Boland (2000, p. 81) has observed that 'solving the identification problem amounts to avoiding generality – in an algebraic sense – in a solution statement of a model. A solution is general when it remains invariant under transformations of the coordinates. Thus the uniqueness property of identification implies a lack of invariance'.
11. Mouchot's (1996) book discusses selected topics in philosophy of science, economic theory, limits of econometrics, critical account of rationality in economics, political economy and other economic issues and ideas. He offers a comprehensive account of economic methodology. We will draw closely on Mouchot. Instead of translating long quotes we will rather paraphrase the author and sum up his main arguments.
12. The question of reliable functions and volatile functions will be discussed below.
13. Leontief, in correspondence and in personal discussion with Errouaki, pointed out that economists use post-Einsteinian mathematics to solve pre-Newtonian problems.
14. Feldstein, former chairman of the US Council of Economic Advisors, was quoted by *Time* magazine (27 August 1984, p. 46).
15. Time series analysts tended to ignore the role of econometric explanatory variables and modelled time series behaviour in terms of sophisticated extrapolation mechanisms. The expression 'time series analysis' at one time referred to the Box–Jenkins approach to modelling time series in the context of forecasting. This method developed its own techniques and abandoned the Cowles structural estimation methodology, which was based on the use of explanatory variables suggested by economic theory to explain/forecast. It chose instead to rely only on the past behaviour of the variable being modelled/forecast. Not all forecasting methods can be neatly classified into one of the two categories structured here. A good example is the leading indicator approach. For further details, see Klein and Moore (1983).
16. See Eisner (2003); also discussion in Nell and Forstater (2003, pp. 107–15).
17. See Herzlich, *Le Monde*, 19 October 1993, p. 33, quoting M. Volle.
18. Nell's transformational growth moves in a definite direction, distinguishable from other directions, but we do not know what will eventually be found in that direction, or where we shall end up. For an account, see Errouaki (2003).
19. See Gherardi (Misère de la Prévision, *Le Monde*, 19 October 1993).

It is not to be forgotten that they [*theoretical models*] *are all our own artificial inventions* in a search for an understanding of real life; *they are not hidden truths to be discovered*.

Haavelmo (1944, p. 3, italics added)

I believe that econometrics can be useful. But as I have said, the possibility of extracting information from observations of the world we live in, *depends on good economic theory*. *Econometrics has to be founded on theories that describe in a reasonably accurate way the fashion in which the observed world has operated in the past* [. . .] *I think existing economic theories are not good enough for this purpose*. I have not said that I think existing economic theory is useless. In fact I believe it will represent indispensable building-blocks for a more general theory if we can ever hope to find one.

Haavelmo (1989, reprinted in the *AER*, Dec. 1997, p. 15, italics added)

9. Haavelmo and beyond: probability, uncertainty, specification and stochasticism

INTRODUCTION

Haavelmo (1944) provided the unifying foundations for present day econometrics. Although many have written about his contribution, his work has not been integrated within a more general philosophy of science. Since Haavelmo (1944), extraordinary advances have been made in econometrics. However, since the early 1990s the efficacy and scientific status of econometrics have become questionable. Not surprisingly, the growing discontent with econometrics has been accompanied by a growing interest in econometric methodology.

Spanos (2007, p. 2) has noted that the focus in the econometric literature since the early 1960s has been primarily on technical issues. But he doesn't think much advance has been made. He wrote (ibid., p. 3):

> The current state of the empirical foundations of economics [makes] Popper's (1934 [1959]) picturesque metaphor of piles in a swamp seem charitable, because a closer look at the published empirical evidence over the last century reveals heaps of untrustworthy estimates and test results which (a) provide a tenuous, if any, connection between economic theory and observable economic phenomena, and (b) facilitate no veritable learning from data; see Spanos (2006a).

He went on to argue (ibid., p. 8):

> From the perspective of the philosophy of econometrics, a central question in 20th century philosophy of science has been (see Mayo, 1996):
> How do we learn about phenomena of interest in the face of uncertainty and error? In particular, this raises several interrelated questions:
> (a) Is there such a thing as a scientific method?
> (b) What makes an inquiry scientific or rational?
> (c) How do we appraise a theory vis-à-vis empirical data?
> (d) How do we make reliable inferences from empirical data?
> (e) How do we obtain good evidence for a hypothesis or a theory?

Starting from the blueprint of Haavelmo, Spanos has shed light on a number of methodological issues relating to specification, mis-specification testing, and re-specification, including the role of graphical techniques, structural versus statistical models, crucial problems in statistics such as the likelihood principle and the role of conditioning (see Cox and Mayo, 2010; Mayo and Cox, 2006; Spanos, 2006b; 2006c; 2008), as well as philosophy of science including the problems of curve-fitting, underdetermination and Duhemian ambiguities; see Mayo (1996; 1997) and Mayo and Spanos (2008).

Here we shall look first at two interpretations of Haavelmo's work, namely those of Davis and Spanos. A principal aim of both Davis (2000) and Spanos (1989) is to argue that a retrospective view of the founding period can provide helpful insights into the weaknesses of the textbook econometric approach and suggest possible modifications.

Then we shall examine Los's (2001) rejection of Haavelmo on epistemological grounds and present a critical examination of his alternative approach expressed in his book, *Computational Finance*. He argued that there is serious epistemological doubt about the established practices of econometrics, of financial analysis, and of conventional, probability-theory-based statistics in general.[1]

To complete the discussion, an alternative grounding for statistical inference, provided by Foley (2005), will be examined. Foley (ibid., p. 5) offers 'an approach that repeats and amplifies the ideas of Laplace (1825 [1995]), Jeffreys (1939), de Finetti (1974), Rosenkrantz (1989), and Janes and Bretthorst (ed.) (2003), but at the same time proposes some significant innovations and modifications to their positions, including, especially, the elimination of the concept of underlying parameters in statistical models'. Foley (2005, p. 5) hopes that his Laplacian approach will contribute to 'dispelling the fog of confusion that surrounds the application of statistical techniques'. Although he does not deliver a direct message to econometrics, he (ibid., p. 5) argues that 'many widely employed classical statistical techniques which are viewed as objective can be interpreted as rigorous applications of the Laplacian theory with specific but intuitively persuasive prior probabilities'. Because the Laplacian conception provides the most coherent way to understand the logic of probability and statistical arguments, it could lead implicitly to a new foundation for empirical modelling!

These discussions have a crucial bearing on the problem of explicating the concept of stochasticism. The argument here will be that stochasticism requires an explicit modelling assumption – that the world is stochastic – which may well be false and should not, therefore, be taken for granted (Boland, 1977; 1982; 2000). Following this line of argument, the chapter will use Haavelmo's (1944) structure of econometrics and the Hollis and

Nell (1975) methodological framework to discuss how modern econometricians deal with stochasticism.

The chapter will bring together the main arguments of the authors and provide a coherent discussion of the four approaches within the MTC diagram. This will lead to re-examining the probability approach in terms of uncertainty and inherent unpredictability. A concern central in Haavelmo's work, namely the apparently inherent unpredictability of much economic behaviour, may have to be approached in terms of uncertainty and underdetermination, rather than probability. This echoes Keynes, and follows from the methodology of Hollis and Nell.

The Haavelmo approach, enhanced by some of the new work (Hendry, Spanos), is arguably sound and useful, so long as it is applied appropriately. But, of course, it cannot deal with inherent uncertainty. So the issue becomes: how to define the areas in which structural econometrics can be applied, and distinguish these from areas in which neither it – nor any other method – can provide scientific prediction (Nell, 1998a, ch. 3).

9.1 RE-READING HAAVELMO: TWO COMMENTARIES

Great thinkers can control their thoughts, but they cannot control how these thoughts fare after they have been made public. Some of the most important insights may not be noticed or properly appreciated until many years later. This is what Spanos (1989) and Davis (2000) both deplore in the case of Haavelmo's influence on econometric modelling.

While the simultaneous-equations approach to statistical modelling met with success, Haavelmo's methodological insights have not attracted the attention they deserve (Spanos, 1989, p. 405). But as both Hollis and Nell (1975) and Haavelmo (1989) argued, it is not so much the development of a methodology specific to econometrics that is required; what is required is a unified scientific methodology for economics in general, in which econometrics would not be separate, but would play a role coordinated with the rest.

Haavelmo (1958; 1989), Hollis and Nell (1975), Johnston (1963 [1984]), Klein (1982, 1985), Errouaki (1990, 2007) and Nell (1998a) have insisted on the opening of econometric methodology to fieldwork. They consider the fieldwork approach to be an additional attempt to bridge the gap between the various theoretical models proposed and a fixed unavoidable reality. This would serve to further set off the effect of the hitherto prevailing textbook methodology. Davis (2000) turns to Suppe's semantic conception for a better understanding of the problem of theory–data confrontations and applies Suppe's framework to Haavelmo's structure of econometrics. Davis sets out his argument in a framework developed by Suppe (1989),[2] from which we move to a conceptual examination of Spanos's alternative framework. Spanos (1989, p. 406) claims that 'the textbook methodology is really a less flexible version of Tinbergen's pre-Haavelmo approach. The textbooks have surprisingly little in common with the methodology in Haavelmo's 1944 monograph, apart from the probability language, in spite of the fact that Haavelmo is commonly acknowledged as having founded modern econometrics'. But several important elements of Haavelmo's approach 'have either been discarded or were never fully integrated into the textbook approach' (ibid., p. 406). Drawing on Davis, and combining these elements from Spanos with Hollis and Nell's approach, a case can be made for an alternative methodological framework.

9.1.1 Davis's Approach

As we saw, George C. Davis re-examined Haavelmo's (1944) article by drawing on Suppe's (1989) semantic approach to the philosophy of science,[3] which, he argued, demonstrates clearly that Haavelmo's structure of econometrics parallels closely the semantic account of the structure of scientific theories.[4] We have already provided an abbreviated version of Davis's (2000) discussion in Chapter 2, showing that Suppe's approach can be broken down into 'four main components: A) theories and phenomena as relational systems; B) experimental methodology; C) experimental testing and confirmation; and D) distinguishing between scientific and background domains' (Davis, 2000, p. 207).[5] Following Davis, we examined these four components of the semantic approach and showed how they closely mirrored Haavelmo's structure of econometrics.

Davis uses this approach to draw methodological insights from Haavelmo, especially in regard to model specification, but he seems to miss the key importance of realism. Davis (ibid.) suggested, 'the semantic approach interpretation of Haavelmo [can be] used to illuminate a central insight of his that could help improve the model selection problem'. Recall that the semantic approach presents theories and phenomena as relational systems. Starting from a phenomenal system – the buzzing, blooming confusion of life – the scientist must single out the relevant data and strip them of extraneous influences. That is, theories apply not to the raw data, but to such cleaned-up data systems, called 'physical systems' by Davis – a term we consider misleading. We are dealing with the economic aspects of social systems, so we shall refer instead to these as socio-economic systems. Such systems are parts or subsets of the phenomenal system where many aspects of the world are simply ignored and only certain variables and features of the phenomenal system are considered relevant. The next step, then, is to develop a 'theory-induced' socio-economic system, one in which elements in a chosen list of variables and features considered to be related in certain ways can be examined empirically.

This leads to the question of how to do empirical work, what Davis called 'experimental methodology', obviously more difficult in economics, since laboratory testing is not generally possible. Empirical work should yield measurements; it should result in quantitative relationships between the important variables of theory, and it should provide ways of testing theories. But there are difficulties. Observations, and thus raw data, come from the phenomenal system. Theories, however, apply to 'physical', that is, socio-economic systems, which are counter-factual. So the raw data have to be converted to data that correspond to the variables and conditions of the socio-economic system in question. But how – by what means and on what grounds – are we to transform raw data into data that can confirm or falsify a theory?

This is not easy. If it is not done properly, theories cannot be tested, because a disconfirming instance could be the result of the influence of an extraneous variable. Nor could the data be used to construct quantitative relations between theoretical variables; some of the data may be responding to variables and influences outside the theoretical system. So the raw data have to be adjusted to remove the extraneous influences; but how? There are no mechanical procedures; we must draw on many sources. Fieldwork will provide information, and common sense can help; we all know how to separate relevant from irrelevant factors. Conceptual analysis will be important, distinguishing essential from inessential matters. Theories from other domains in the background can be used; we can draw on sociology, psychology, anthropology and politics to single out non-economic factors and variables that may have an impact on economic decisions. Finally it may be possible to devise restrictions that should apply to the probability distributions of the variables, which is Haavelmo's suggestion, emphasized by Davis and developed in some detail by Spanos.

9.1.1.1 Distinguishing between scientific and background domains

According to Suppe (1989, p. 120), 'for any discipline, there exists a domain of discourse designed to address problems and questions within the discipline'. Davis (2003, p. 4) observed that 'the domain of discourse for economics is all decisions and outcomes influenced by economics. A domain of discourse can be partitioned into a background domain and a scientific domain. The background domain refers to the collateral tools, concepts, and facts that are taken as given [. . .] tools from other disciplines such as mathematics and statistics or concepts and facts from business law. The *scientific domain* refers to the tools, concepts, and facts that are used directly in answering the questions within the discipline'.

(The scientific domain in our case would include the propensity to consume, the productivity of labour, the multiplier, equations for money and so on, but would not include ideas related to marginal productivity or utility theory.)

Haavelmo (1944, p. 66, author's italics) makes essentially the same distinction between background and scientific domains: 'the requirement of a specification of the set of a priori admissible hypotheses *before* constructing a test forces us to state explicitly what we assume known beyond doubt, and what we desire to test'. He was also aware of the connection between partial formulations of background domain theories and the model specification problem. Haavelmo (1944, p. 74, author's italics) wrote:

> There will, therefore, in general be infinity of 'correct' theories. In particular, there might be various different systems [which] all lead to *identically the same* set of probability laws, i.e., *they are indistinguishable from the point of view of observations*.

Suppe (1989, p. 140) points out that partially formulated theories can potentially admit an infinite number of models: 'as long as any of [these systems] satisfies the criteria for acceptance, then the theory is deemed true'.[6]

All too unfortunately, this result is easily documented in applied econometrics.

9.1.1.2 Model specification

One of the most persistent problems in econometrics is the model specification problem. Simply stated, the model selection problem is the inability to choose between statistical models. Davis (2000, p. 221) reminds us that before 'any fruitful discussion of model specification can begin, the purpose of the model must be clear. Is the purpose of the model to test a theory or to explain a phenomenon?' As stressed in Haavelmo (for example, Haavelmo, 1944, pp. 7 and 14–17) and in the semantic conception of theories, a theory explains the counterfactual data of a physical system. According to Davis (2000, p. 221), 'a theory does not explain the observational data of a phenomenal system. Consequently, models designed to test theories can be very different from models designed to characterize phenomena'. Davis (ibid., p. 221) argued that 'Holland (1986) and Pratt and Schlaifer (1988) emphasized these distinctions and discussed their implications, yet their work is usually ignored in econometric methodology discussions (e.g., Darnell and Evans 1990, Hendry 1993, 1995a)'.

Davis (2000, p. 221) takes as his example testing consumer theory (see Chapter 3 above):

> The theoretical model generated from this theory is of the form
>
> $$q' = f(p'),$$
>
> where q' is an m vector of the theoretical quantities of goods consumed, p' is the m vector of corresponding theoretical price variables normalized by income.
>
> [. . .] the 'exact' theoretical equation may be written as an 'exact' observational relation $y_i = X\beta_i + \varepsilon_i$. Notationally, $i = 1, 2, \ldots, m$, y_i is the $N \times 1$ vector of the natural log of quantities consumed of the ith good, X is the $N \times (m+1)$ matrix of the natural log of normalized prices allowing for a constant, β_i is the $(m+1) \times 1$ vector of parameters for the ith equation, ε_i is the $N \times 1$ disturbance vector associated with the ith equation, and N is the number of observations. Simplifying to matrix notation yields $y = X\beta + \varepsilon$, where y is an $mN \times 1$ vector, $X = I_m \otimes X$ is an $mN \times m(m+1)$ matrix, β is an $m(m+1) \times 1$ vector, and ε is an $mN \times 1$ vector, I_m is an $m \times m$ identity matrix, and $\otimes$ is the Kronecker product operator.
>
> [. . .] assume that X is exogenous such that no other relations are needed other than those given by the [above] equation. Finally, assume the disturbances conditional on X are independent and identically normally distributed with mean zero and a constant contemporaneous $mN \times mN$ covariance matrix Σ, that is $\varepsilon|X \sim \mathrm{IIN}(0, \Sigma)$ [. . .] the joint conditional distribution for y is [then] $y|X \sim \mathrm{N}(X\beta, \Sigma)$ and the conditional expectation of y is [. . .] the counterfactual data the theory describes or $\mathrm{E}[y|X] = X\beta = y'$.

Next, Davis (ibid.) supposes that

> the *a priori* assumption on the disturbance distribution is wrong, so that, for example, the disturbance of each equation in the system actually follows a stationary first order autoregressive AR(1) process. Does this affect the economic theory proper in any way? Stated differently, do the laws of economic theory proper help in distinguishing between an autocorrelated and a non-autocorrelated disturbance?

Let's consider what economic theory proper says about these equations. 'Classical consumer theory generates four laws: Slutsky symmetry, adding-up, negativity, and homogeneity. For this functional form, these amount to linear restrictions on the parameter vector β' (Davis, 2000, p. 222), and Davis (ibid.) continues,

> from Haavelmo we know that prior to some assumption about the disturbance, the economic theory proper says nothing about the relationship between the

> observational data y and the theoretical data $y' = X\beta$. [. . .] by construction [we have] [. . .] an exact equation and the unobservable disturbance 'picks up any slack' between y and y'. [. . .] [T]he disturbance may be systematic, nonsystematic, correlated with the exogenous variables, nonstationary, or whatever is necessary to make [the] equation [exact] and it will still be technically consistent with the economic theory proper.[7]
>
> However, once a distribution is *assumed* [italics added] for $\varepsilon|X$, y and y' become connected through the distribution for $y|X$.

So what is the role of economic theory? Davis (2000, p. 220) asks:

> what does economic theory proper say about the states and sequences of states of the mean and variance of the distribution $y|X$? Because the mean of $y|X$ is $X\beta$, the theoretical restrictions on β will restrict the conditional mean's states and sequence of states. Yet the theory places no such restrictions on the conditional variance, because through the mathematics, the variance of $y|X$ (Σ) is the same as that of $\varepsilon|X$, which was arbitrarily assumed. Any variance is technically compatible with the economic theory proper.
>
> [However] when it comes to drawing valid inferences from the statistical model [. . .] the validity of the inferences depends on the validity of the underlying assumptions, especially the variance.

But Davis (ibid., p. 223) later stated: 'economic theory proper is immune to these disputes because what is being rejected is the assumption about the variance, which is not part of economic theory proper', and went on to suggest a thought experiment:

> imagine contrary to most present economic theories, an economic science where economic theories generate laws about more than one moment of either the joint distribution of y and X or the conditional distribution of $y|X$. In this economics it would no longer be possible to model these moments independently without being concerned about theory consistency and thus the number of models and viable procedures would be reduced. This appears to be what Haavelmo was calling for.

But, as Davis notes, this would require changes in economic theory or in the relation between economics (the scientific domain) and the background domain, which includes statistics and Haavelmo's experimental methodology:

> Yet as the example indicates, alternative statistical procedures and therefore models can be considered without any concern for violating the scientific domain. This implies there are insufficient intertheoretical links or constraints between the scientific domain and the background domain. (Davis, 2000, p. 223)

Davis (2000, p. 224) would like to see stronger 'intertheoretical links' – between statistical models and economic theory – and suggests specifying:

> some rather general properties all (i.e., statistical models) should satisfy if they are to be considered adequate [. . .] Models would be eliminated if they did not satisfy these general properties. The general-to-specific modeling methodology is an example of this approach as it requires that a model satisfy certain properties before it is deemed 'congruent' [Hendry, 1995a] or 'adequate' [Spanos, 1990b]. Some of these properties are homoskedastic errors, weakly exogenous variables for the parameters of interest, constant parameters of interest, and theory consistency. [. . .] Unfortunately, as is easily demonstrated [. . .] with the exception of theory consistency these constraints alone do not help in theory testing.

More generally, Davis (2000, p. 225) argued that:

> econometric methodological rules for restricting the class *S* of admissible models to some smaller 'acceptable' set such as *A* eases the model selection problem because such rules can eliminate an entire class of models, namely *A*, but this does not help eliminate theories. [. . .] While it may be required that all theory-testing models be adequate statistical models, all theories are not necessarily in the class of adequate statistical models.

In fact, Davis (2000, p. 225) has argued that the only way finally to eliminate theories is to strengthen their links with probability distributions. Davis pointed out that Haavelmo (1943a, p.1) was apparently aware of this (which we shall see shortly is strongly disputed by Los):

> if we want to consider a set of related economic variables, it is, in general, not possible to express any one of the variables as an exact function of the other variables only. There will be an 'unexplained rest,' and, for statistical purposes, certain stochastical properties must be ascribed to this rest, a priori. [. . .] [E]conomic theorists have, in general, paid too little attention to such stochastical formulation of economic theories. (Haavelmo, 1943a, p. 1)

Or:

> if we want to apply statistical inference to testing the hypotheses of economic theory, it *implies* such a formulation of economic theories that they represent *statistical* hypotheses, i.e., statements – perhaps very broad ones – regarding certain probability distributions. (Haavelmo, 1944, p. iv, author's italics)

Davis (2000, p. 226) observed that 'others have recently made similar observations (e.g., Birner 1994, Granger 1992, Stanley 1998, and Spanos 1990b) and there are a few cases where economic theories have been made truly stochastic and have implications for more than the first moment of a probability distribution'. He referred to McElroy (1987) and Brown and Walker (1995), who have demonstrated that

> additive, homoskedastic disturbance terms are theoretically inconsistent with the theoretical restrictions of cost minimization when the optimizing agent is aware of some factors unknown to the econometrician, which would seem to always be the case. Under some rather mild assumptions, they demonstrate that an additive disturbance term will not be homoskedastic but conditionally heteroskedastic. (Davis, 2000, p. 226)

But according to Davis (2000, p. 226), 'this approach on modeling optimizing behavior is far from being a meta-principle that economic theorists follow. However, as the semantic interpretation of Haavelmo demonstrates, this approach of specifying economic theories that have implications for more than the first moment of a probability distribution will help ease, if not solve, the model specification problem'. (But the implication here is that the world is really stochastic – that is, that probability distributions reflect the real state of affairs. Foley, among others, as we will see, considers probability subjective.)

Essentially, Davis (2000, p. 222) argues, the problem arises because economic theory proper says nothing about the variance of the disturbance term: 'the disturbance may be systematic, nonsystematic, correlated with the exogenous variables, nonstationary, or whatever is necessary to fit the observational data to the counterfactual data'. Within the semantic framework outlined above, economic theory proper (but which economic theory?) makes up the scientific domain, but it is the not-fully-formulated background domain of probability theory that keeps us from placing sufficient restrictions on the disturbance term.

Here, clearly, realism in theory might well suggest such links, as might fieldwork. Realism means accepting 'imperfections', not idealizing agents or their powers (rationality, mobility, etc.), placing agents in a setting that can account for their persistence over time, accepting uncertainty about the future, and most of all, understanding what agents are doing in their own terms, which is what fieldwork can tell us. It may be useful in developing theory to redescribe what they are doing in our own specially designed vocabulary, but this can legitimately be done only if we correctly understand what is going on in the agents' own terms.

But this is not part of Davis's approach. Davis is not concerned with how the terms of theory apply to the elements of the physical system or of the phenomenal world. Rather, he is looking for better connections between theories and probability distributions. Research into the properties of the implied probability distributions is a way of making our economic theories more specific. But what kind of research? A more specific theory says more about the observed world, and hence is more easily tested and, if found wanting, eliminated. Davis wants to promote a closer union between economic theory proper and the background domain of probability theory – but, while assuming the reality and objectivity of the probability distributions, he does not discuss the reality underlying economic theory. Davis simply does not take up questions about the realism of economic theory. Haavelmo, by contrast, repeatedly remarks on the importance of realism, and it is clearly central to specification.

9.1.2 Spanos's Alternative Framework

Spanos argues, in his 1989 paper, that except for the probabilistic language, the standard textbook methodology has little in common with Haavelmo's methodology as set forth in his 1944 article.[8] As Spanos says, Haavelmo's article 'became a classic much too early', meaning that it is widely cited but rarely read (Spanos, 1989, p. 409). Several important elements of Haavelmo's methodology are absent from the textbook approach. Spanos (1989, p. 406) suggests that reformulating these elements presents us with an alternative methodological framework that is superior to the textbook approach: 'primarily, this framework allows the structure of the data to play an important role without diminishing the importance of the theory'. In other words, this alternative framework, based on a re-reading of Haavelmo, walks a line between induction (allowing the data to play a role) and deduction (not diminishing the importance of the theory).

9.1.2.1 Textbook methodology

The econometric textbook approach is best illustrated by Johnston (1963 [1984]). Johnston (1963 [1984], p. 6, quoted by Spanos, 1989, p. 412) wrote:

> The essential role of econometric modelling is the estimation and testing of economic models. The first step in the process is the specification of the model in mathematical form [. . .] Next, we must assemble appropriate and relevant data from the economy or sector that the model purports to describe. Thirdly, we use the data to estimate the parameters of the model in an attempt to judge whether it constitutes a sufficiently realistic picture of the economy being studied or whether a somewhat different specification has to be estimated.

The textbook approach does pay lip service to Haavelmo's probabilistic framework,[9] but, according to Spanos (1989, p. 412), closer examination of the treatment given to statistical models 'reveals that probability theory enters their specification via the error term in a nonessential way to make more precise the pre-probability interpretation of such models as curve-fitting frameworks'. First, a statistical model is specified according to a pre-existing economic theory, and then probability theory is applied to the error term in order to improve the fit of the data with this model. The data have no more significant role to play than this.

Furthermore, Spanos (1989, p. 412) pointed out that the textbook approach is much narrower than that of the Cowles Commission. The latter, and in particular Koopmans (1937; 1945), were well aware of the importance of the principle of what Spanos calls 'statistical adequacy'. Although recognized, the notion of statistical adequacy (discussed in detail below) was never fully integrated into the textbook method because of a concern that any use of the information in the data beyond the theory constituted measurement without theory.[10] This concern, however, swung the textbook approach to the opposite extreme. The probabilistic structure of the data itself was incorporated neither into the specification of statistical models nor into the respecification stage – theory alone was relied upon: 'with the data being treated as an afterthought' (Spanos, 1989, p. 413). Spanos (ibid.) argued that 'the textbook notion of identification is also a purely theoretical issue. The problem is posed and solved with respect to some implicit reduced form without any reference to the data'. Although the importance of testing underlying statistical assumptions against the actual data has been gaining ground in econometrics textbooks, as Spanos points out, the discussion has tended to emphasize the symptoms without getting at the root of the problem.

9.1.2.2 Re-reading Haavelmo's methodology

Spanos (1989, p. 409) writes:

> In Haavelmo's approach, the procedure from a theoretical model to the estimated equations is considerably more sophisticated than the modern textbook approach of attaching white-noise error terms to theoretical relationships. According to Haavelmo, a theoretical model gains empirical meaning only when accompanied with some form of actual or designed experiment stating the circumstances under which the theoretical relationships are measurable.[11]

Haavelmo saw theoretical models as artificial, idealized descriptions of real phenomena, not as hidden truths to be discovered. Indeed, Haavelmo (1944, p. 3) observed: 'it is not to be forgotten that they [theoretical models] are all our artificial inventions in a search for an understanding of real life; they are not hidden truths to be discovered'.

This point of view itself leads one to be less attached to theory and to pay more attention to the data. Spanos also brings up the point that data in economics are usually obtained by passive observation, a point also made by Davis and recognized by Haavelmo. These passive data usually do not fit perfectly with theory. For example, demand and supply curves refer to the intentions of agents to demand or supply certain quantities at certain prices, but the data most often available are the actual quantities transacted at the actual prices. Haavelmo (1944, p. 7) recommended that 'one should study very carefully the actual series considered and the conditions under which they were produced, before identifying them with the variables of a particular theoretical model'. Spanos identifies four methodological elements presented by Haavelmo (but neglected by the textbook approach) for studying very carefully the actual data series. The first of these is the importance of what Spanos refers to as the Haavelmo distribution. This refers to the joint distribution of all the observable random variables for the entire sample period, and this is what must be taken into account for purposes of statistical inference.

The second element is statistical adequacy, mentioned above. It refers to the testing of the underlying assumptions of a statistical model against the data considered.

The third neglected methodological element pointed out by Spanos is the idea of a general statistical model, which takes its form from the structure of the data. It summarizes the information contained in the data and narrows down the class of admissible theoretical models. This allows for a distinction between a statistical model and the class of empirical models that can be derived from it. (In Davis's language, general statistical models summarize phenomenal systems observed through the data, while empirical models represent theory-induced physical systems.)

The fourth and final element unearthed by Spanos is the notion of specifying statistical models in terms of the random variables actually observed. As mentioned above, the standard textbook approach is to specify models in terms of the error term, fitting the data to a theory instead of allowing the data to restrict the set of admissible theories.

9.1.2.3 Reformulating Haavelmo

Spanos (1989, p. 410) argued:

> Haavelmo's powerful methodological intuition is most apparent in his discussion of the question of estimability in relation to the nature and structure of the data. He argued that there is nothing inherently problematical with time series data such as $\{q_t, p_t, t = 1, 2, \ldots, T\}$. The real problem is that such data do not refer to the agents' intentions, as represented by demand and supply schedules, but to actual realizations in the form of quantities transacted and the corresponding prices (see Haavelmo, 1944, pp. 26–39).

Furthermore, four chapters of Haavelmo's monograph constitute an advanced treatment of probability and statistical inference based on the works in the mid 1930s of Kolmogorov, Fisher, and Neyman and Pearson. Spanos (1989) pointed out that several important methodological elements in these chapters did not filter through the econometric textbook approach. He argued (p. 415):

> The pivot of this framework comes in the form of the Haavelmo distribution: the joint distribution of all of the observable random variables involved for the whole of the sample period [. . .] It constitutes the most general description of the sample information and demarcates the relevant information for modeling purposes.

The reformalization of Haavelmo's methodology is separated into two stages. The first stage consists of reducing the Haavelmo distribution to the general statistical model. This is done by postulating a set of probabilistic assumptions specific to the distribution of the random variables actually observed in the data sample. For this summarization of the sample information to be adequate, these probabilistic assumptions should be data-acceptable. The second stage of the process relates an adequate statistical model to a theory or theoretical model. The estimated form of the theoretical model is called an empirical model, and it is specified through a reparameterization (that is, a restriction) of the adequate statistical model in accordance with the theory.

In the context of the simultaneous equations model (SEM), the structural form can be viewed as the theoretical model, and the reduced form as the statistical model. To see this, we recast the simultaneous equations model according to Spanos's (1989, pp. 416–17) reformulation of Haavelmo. We will draw on his presentation and use his notation to avoid any unnecessary confusion:

$$\Gamma' y_t + \Delta' x_t = \varepsilon_t \tag{9.1}$$

where

$$[\varepsilon_t / X_t] \sim NI(0, \Sigma_\varepsilon), \quad t \in \mathfrak{I}$$

where Γ and Δ are subject to the usual a priori restrictions. The associated reduced form is

$$y_t = \Pi'_{xt} + u_t, \quad [u_t / x_t] \sim NI(0, \Omega), \quad t \in \mathfrak{I} \tag{9.2}$$

where

$$\Pi = \Delta\Gamma^{-1}, \quad \Gamma'\Omega\Gamma = \Sigma_t. \tag{9.2a}$$

In the present framework, the structural form is viewed as the theoretical and the reduced form as the statistical model. The latter entails reinterpreting Equation (9.2) as follows:

$$y_t = B'x_t + u_t, \quad t \in \Im \tag{9.3}$$

$$B = \Sigma_{22}^{-1}\Sigma_{21}, \quad \Omega \equiv \Sigma_{11} - \Sigma_{12}\Sigma_{22}^{-1}\Sigma_{21}, \quad \Sigma_{22} = \text{cov}(X_t),$$

$$\Sigma_{12} = \text{cov}(y_t, X_t), \quad \Sigma_{11} = \text{cov}(y_t). \tag{9.3a}$$

Assumption 9.1

(i) $D(y_t/X_t; \theta)$ is normal.
(ii) $E(y_t/X_t = x_t) = B'x_t$; linear in x_t.
(iii) $\text{Cov}(y_t/X_t = x_t) = \Omega$; homoscedastic.

Assumption 9.2

$\theta = (B, \Omega)$ are t-invariant.

Assumption 9.3

$Y = (y_1, y_2, \ldots, y_T)$ is an independent sample sequentially drawn from $D(y_t/X_t, \Omega)$.

Spanos (1989, p. 416) went to argue that:

> [t]his statistical model is related to the Haavelmo distribution $D(Z_1, Z_2, \ldots, Z_t, \psi)$, $Z_t \equiv (y_t, X_t')'$ via the following reduction based on the assumptions that $\{Z_t, t \in \Im\}$ is a normal independent and identically distributed (NIID) vector process:

$$D(Z_1, Z_2, \ldots, Z_t, \psi) = \prod_{t-1}^{T} D(Z_t/; \psi_t) = \prod_{t=1}^{T} D(Z_t; \psi)$$

$$= \prod_{t=1}^{T} D(y_t/X_t; \psi_1)\, D(X_t; \psi_2). \tag{9.4}$$

> The first equality follows from the independence assumption, the second from the identically distributed and the normality assumption ensures that ψ_1, and ψ_2, are variation free, and thus X_t is weakly exogenous with respect to $\theta = \psi_1$.

Viewed in the above framework, two important differences from the textbook approach become clear. First, the parameters ($\theta = (B, \Omega)$ in Equation (9.3)) of the reduced form (Equations (9.2) and (9.3)) are not simply transformations of the structural parameters; rather, the reduced form parameters are purely statistical, related to the moments of the random variables actually observed in the Haavelmo distribution. Instead of the reduced form being derived from the structural form, it is a statistical model of which the structural form is a restricted version.

Second, the error term (in Equation (9.3)) is defined in relation to the conditioning *information set* which is explicitly specified. The conditioning information set ($D_t = \{X_t = x_t\}$ and the generic scheme for Equation (9.3) become Equation (9.5):

$$y_t = E(y_t/D_t) + u_t, \tag{9.5}$$

where u_t is viewed as the nonsystematic component) is chosen in order to include all the relevant systematic information contained within the Haavelmo distribution, and the error term is defined as the remaining, nonsystematic information. In the textbook approach, the conditioning information set is chosen on the basis of theory alone.

Spanos (1989, p. 417) argued that

> the basic idea is that the modeller chooses the conditioning information set D_t so as to include all the relevant systematic information. The important departure from the textbook approach is that D_t is determined not only by the theory but also by the structure of the data. Any information contained in the Haavelmo distribution constitutes relevant information.

As Spanos (ibid.) observed, this is important because 'an inappropriate choice of the conditioning information set is the main source of misspecification'. Furthermore, he argued that statistical inadequacy of Equation (9.3) (Assumptions 1–3 being invalid) will be disastrous. If the reduced form is statistically inadequate, then the results of statistical inference used in the simultaneous equations model will be invalid.[12]

Spanos (1990a) asserts that, in the literature, the statistical assumptions underlying the reduced form are not tested, and the reduced form itself is rarely even explicitly estimated. Viewing the reduced form as a statistical model summarizing the Haavelmo distribution 'provides a coherent framework for all aspects of statistical adequacy: specification, misspecification, and respecification' (Spanos, 1989, p. 417). We now examine these in turn.

During the initial specification, the probabilistic assumptions are chosen explicitly to reflect the structure of the data. Various graphs of the data (time plots, histograms, cross plots, etc.) can be used for a preliminary assessment of the appropriateness of the assumptions. Certain problems, such as non-stationarity and non-ergodicity, can be detected right away, at the specification stage (see Phillips, 1986; Spanos, 1999).

Later, if there is mis-specification, the symptoms of this mis-specification can be related to the source of the problem, which is the underlying probabilistic structure. The theory does not yet come into play, because we are only dealing with the reduced form, which is a statistical model, and statistical models are sample-specific and have only statistical meaning. As Spanos (1989, p. 418) points out, it is futile merely to treat the symptoms of the problem:

> For example, nonlinearity might be detected because either the temporal structure or some mean nonstationarity in the data was ignored; not because there is an inherent (conditional mean) nonlinearity.

At the re-specification stage, Spanos's framework prompts him to redefine the statistical model so as to incorporate any relevant information contained in the Haavelmo distribution that was inadvertently missed by the original assumptions. To paraphrase Spanos, assume, for example, that a mis-specification test reveals that an initial assumption of temporal independence is not quite right. This means that past history provides some relevant systematic information that was ignored in the initial specification. A re-specification can be made to account for this by replacing independence with some kind of asymptotic independence that would capture the previously omitted information. Similar re-specification procedures can be undertaken if there is mis-specification due to other assumptions, such as homogeneity. Again, the important thing is that the re-specification is carried out in relation to the Haavelmo distribution – that is, in relation to the data. We are still working at the statistical level, and the parameters of this reduced form still have no theoretical meaning.

We have been speaking of the reduced form as a single model, but in actuality it represents a class of statistical models that have been specified in order to summarize the Haavelmo distribution. We now progress quite naturally to the alternative notion of identification implied by the reformulated framework. Spanos (1989, p. 420, author's italics) refines Haavelmo's notion of identification by separating it into statistical identification and structural identification:

> For a given set of (feasible) observations and a statistical model class, *statistical identification* refers to the estimation (choice) of a particular model, within this class, which constitutes an adequate summary of the sample information. For a given adequate statistical model, *structural identification* refers to the structural parameters [. . .] being uniquely definable in terms of the statistical parameters.

Seen in this way, identification is no longer a purely theoretical issue addressed without reference to the data, but is intimately related to the structure of the data as well as to the theory. It is the bridge that relates the data to the theory.

Once an adequate statistical model has been identified, the structural identification can be made. This process consists of a reparameterization, or restriction, which changes the sample specific statistical parameters into theoretically meaningful ones. Only this empirical model has any explanatory power, and only if it has been identified by restricting an adequate statistical model can this explanatory power have any degree of reliability.

Of course, this estimated structural form, this empirical model, must also itself be statistically adequate. This can be tested indirectly by testing the overidentifying restrictions, as well as directly by diagnostic checking. What is to be done if the empirical model is found to be statistically inadequate? Spanos (1989, p. 422) writes: 'in the context of the above framework, the procedure which suggests itself is to formulate realistic enough theoretical models so as to make the reparameterization/restriction simply one of theory'. He considers data-induced reparameterizations of the statistical model to be second-best solutions sometimes required by the absence of theoretical models, which are realistic enough. Each case must be examined separately, and the appropriate solution will depend on how large a gap exists between the data and the theory. Creative thinking, rather than rigid procedures, is called for.

This alternative framework – with its focus on the Haavelmo distribution, statistical adequacy, general statistical models and their specification in terms of the observable random variables instead of the error term – constitutes a more balanced approach than is found in modern textbooks. Whereas the textbook methodology placed a great deal of emphasis on deduction from theory, this reformalization of Haavelmo's methodology allows a much more important role for induction from the data without usurping the proper role of theoretical deduction. Spanos, in his 1989 paper and elsewhere, uses this alternative framework to meet some of the criticisms directed at the textbook approach.

9.1.2.4 The LSE versus the Textbook approach

Davis (2005a; 2005b) asks: are the Textbook and LSE camps at a tautological impasse – they don't agree, so they disagree? He thinks they can be reconciled somewhat, if the focus is turned towards the model re-specification problem and the lack of generally acceptable methodological guidelines for re-specifying an econometric model.

Economic theories do not provide complete empirical model specifications, and this was known long before the LSE approach[13] came on the scene. Empirical models require bridging assumptions between the theory and the data. Bridging assumptions can be classified in many ways. A useful demarcation is between bridging assumptions that are related and those that are unrelated to the economic theory. The economic theory related bridging assumptions (R) are those that are required in specifying a component of the model for which the economic theory being applied provides some information, albeit often weak information (for example, acceptable functional form class, variables or factors from which to choose). The economic theory unrelated bridging assumptions (U) are those that are required in specifying a component of the model but for which the economic theory being applied provides no information (for example, data span, data frequency, concomitants).

The textbook camp can be criticized by the LSE camp because re-specifications are achieved by capriciously altering assumptions in either R or U on a case-by-case basis and there is no overarching principle for ranking which bridging assumption set should be considered first and which elements within the set should be considered first. Alternatively, the LSE camp can be criticized by the textbook camp for usually ignoring R and focusing entirely on altering one element within U – the implicit static model assumption. That is, textbook econometricians recognize that economic theories do not provide enough guidance on model specification but object to the LSE approach because it systematically ignores much information the theory may say something about (for example, functional form or other factors) in favour of something the theory admittedly says nothing about (usually dynamics or concomitants) and then often claims that the theory is inadequate. For textbook econometricians, it is the inadequate representation of the theory that is seen as stacking the empirical deck against the economic theory.

Surely some progressive econometric methodology lies between the textbook 'case-by-case basis' and the LSE 'dynamics is everything' extremes for accessing and ranking bridging assumptions. But, at the end of the day, econometricians cannot solve the model specification problem alone. Davis (2000) argued that economic theories have very weak intertheoretical links with observational data and until some economic-theory-based methodological norms are developed for strengthening these links, no econometric methodology will solve the model specification problem in isolation.

This strikes us as a pretty good summary of a complex debate. But, again, we think Davis lets economic theory off the hook much too easily. Both Haavelmo and Hendry are more critical: it's not just that 'intertheoretical links with observational data' are missing. Rather, the theory is often downright misleading. It does not represent what actually exists in the socio-economic system, and it often suggests the presence of idealized agents or structures that could not possibly exist.

In the next section, however, we examine a different sort of criticism recently levelled by Cornelis A. Los in his 2001 book, *Computational Finance: A Scientific Perspective*.

9.2 LOS'S APPROACH

In the preface to his book, Cornelis Los offers a scathing critique of current modelling practices in financial economics (his field) and extends it to econometrics in general. At first glance, his argument seems similar to those of Davis and Spanos. Los (2001, p. 11) bemoans the fact that all current research starts with 'philosophical assumptions on the basis of which mathematical model philosophies are erected. Next, the real world data are then forced into these assumed abstract frameworks'. This 'top-down approach', according to Los, 'leads to misspecification, data inconsistencies, and model instability, and is therefore an unreliable foundation for decision-making'. Instead, Los (2001, p. 11) proposes to follow 'the logic of the scientific method, which requires that we let the complete set of financial data speak for itself, without prejudice'. Complaints regarding the forcing of data into an assumed framework, and proposals to let the complete set of data speak for itself, should sound very familiar after our examination of Davis and Spanos. Once again, here is someone criticizing an over-reliance on deduction from theory and calling for greater use of induction from the data.

But Los (ibid., p. 13) goes further than this. He takes aim not only at current practice, but at Haavelmo himself, speaking of the 'erroneous way Dr Trygve Haavelmo and the Cowles Commission in the 1940s and 1950s had defined theoretical econometrics, in particular, its "probability approach" and its "estimation" of simultaneous equation models, respectively'. Los (ibid.) says he has 'serious epistemological doubts about the established practices of econometrics, of financial analysis, and of conventional, probability theory based statistics in general'. He claims (ibid., p. 15) that Dr Rudolf E. Kalman, at a symposium in Greece in 1993,

> proved that there is very little, if any, scientific basis for Haavelmo's 1944 presumption of the empirical existence of Kolmogorov probability. The presumption of probability distributions [. . .] only detracts from the main task of model identification and can severely bias our conclusions.

He goes on to argue that 'starting from traditional fundamental analysis and using algebraic and geometric tools, [his work] is guided by the logic of science to explore information from financial data without prejudice'.

Further, his book has the 'unique feature that it is structured around the requirement of objective science: The geometry structure of the data = the information contained in the data'. Observe that this is a limited, entirely formal concept of information; substantive information, such as that from fieldwork, is not involved.

Los (ibid., p. 11) contends:

> Models are abstractions, often of mathematical nature, used to simplify our decision-making environment, since empirical reality is too complex and its complete analysis too consuming and expensive. Models must be realist representations of a reality that we find to be written in the language of mathematics, in particular of algebraic geometry.

In current research in financial economics, he says, all current expositions start with philosophical assumptions, on the basis of which mathematical model philosophies are erected. The real-world data are next forced into these assumed abstract frameworks; a 'top-down process' that leads to model mis-specification, data inconsistencies and model instability, according to Los. It also provides an unreliable foundation for informed financial decision-making. He (ibid., p. 12) goes on to claim:

> Most current finance models are simple models that, in top-down fashion, are derived from the axioms of economics utility theory and also from the process of non-arbitrage. The latter leads to measure theoretic notions of stochastic processes in general and of martingales in particular, under very restrictive and unrealistic assumptions of unconditional (wide and strict) stationarity and independence of the data points. That is, under the conventional i.i.d. (often Gaussian distribution) assumptions. In all these efforts, empirics does not lead the way, but has only been used to 'statistically corroborate' the way, in a game-theoretic fashion. These measure-theoretic axioms and subjective probabilistic theorizing are at the heart of the core of the current exact valuation models and have little to do with inexact empirical science.

By contrast, he claims to follow 'the logic of scientific method', and allow the set of financial data to 'speak for itself'. He contends that 'top-down reasoning models' often ignore the geometric structure and therefore the empirical information of the data. The geometric structure of many current exact valuation models does not fit the geometric structure of the empirical financial data. Put another way, he argues that the information content of the current exact valuation models often differs from the information content of the financial data. For example, a simply structured financial model, like a single equation, may have been postulated in a case where the multi-dimensional financial data requires a complex, multi-equation model. (For Los, this goes beyond the usual specification problem, because he thinks he can give a more precise meaning to 'requires'.) Along with a critical overview of computational finance, his book provides technical analysis of exact and inexact modeling based on real financial and economic data. For Los, the original data (the 'phenomenal system') may sometimes be exact but he argues that empirical data are more commonly inexact.

> Correspondingly, the result of our modeling efforts may be exact or inexact, as follows: Realization = building a model *S* from available exact data *D* [. . .]
>
> A 'realization of the data *D*' means that all the available (or more) data could have been produced by experimenting with the model. [The model generates the data; any additional data generated clearly belong to the data set.]
>
> Building a model *S* from available inexact data *D* is called identification. [The model generates some but not all the data, and also some that might not belong to the data set.] (Los, 2001, p. 20)

Paraphrasing Los, to identify the model, he first creates a complete, but still inexact, data set from the raw data set, by computing its first and second moments – that is, its means and covariances. This is useful, since linear models only use first and second moments for their identification. (By a linear model, he means a model linear in its coefficients only. The data may be non-linear – that is, they may be non-linear transformations of the raw or original data, such as powers and logarithms.)

Los (ibid., p. 21) argues that

> an identification of the data means that not all of the available data could have been produced by experimentation with the model. The data contain some unidentifiable and unexplainable noise, or uncertainty. The amount and the character of this noise depends on the modeling technique used, although there is a well known physical limit of unidentifiable, unexplainable empirical uncertainty, due to what he calls 'the energy granularity of the universe'.

He goes on to argue that 'data analysis is performed to identify models from inexact data. Based on the current state of knowledge of data analysis, which essentially uses linear algebra and geometry, the $n \times n$ covariance matrix of n data series, or data covariance matrix Σ, is its sole input'.

To paraphrase Los, once these inexact linear models 'are identified from the observed covariances, and the stationarity of these covariances is checked', they can be used to 'extrapolate from the inexact existing data set to an inexact future data set, based upon the observed inertia and homogeneity of the data set'.

Los (2001, p. 26) summarizes:

> Identification: is building a model from the available inexact data.
> Simulation: is experimenting with the model to produce new, additional data

> Extrapolation: is producing new data with a model beyond the reference period of the available data.

Los (ibid., p. 21) asserts that 'epistemic uncertainty is the uncertainty about the model induced by the inexactness of the data. In the context of financial modelling, epistemic uncertainty (= modeling risk) implies financial risk. This phenomenon of epistemic uncertainty, or noise, led Dr Kalman to formulate a second important mathematical modeling principle, namely, the principle of epistemic uncertainty: the inexactness of the data is expressed as uncertainty about the model'. A model can only be as exact as the data used for its identification. Furthermore, Los (ibid., p. 21) observed that:

> If the empirical data are inexact, but the model resulting from some mathematical manipulation is exact, we know that some prejudices and biases have been introduced by the research methodology and we must first find where and how these prejudices affect our research results.

The amount of epistemic uncertainty or modelling noise is not immeasurable. In fact, as Los (ibid., p. 22) pointed out, 'signal processing (e.g. sound) engineers have always measured epistemic uncertainty by the noise/signal ratio or by its inverse. The larger the noise/signal ratio, the more uncertain the identified model (= signal), and vice versa – the smaller the ratio, the more certain the identified model'. The importance of the measurement of the amount of noise or inexactness by the noise/signal ratio is derived from the following simple mathematical idea:

> Exactness = inexactness (noise) and mathematical continuity and limit, with inexactness (noise) $\rightarrow 0$

Quoting Los again, 'when the inexactness, or noise, in the data shows mathematical continuity, then, in the limit, when the inexactness (noise) vanishes, we end up with the exact modelling situation. Thus, in the limit and with continuity, model identification should result in model realization. Unfortunately, data are never continuous, since the universe isn't continuous. Even ostensibly analogue data are in essence discrete. There is a fundamental non-continuity and discreteness in data caused by the energy granularity of the universe' (Los, 2001, p. 22).

Los presents his argument clearly enough, but he introduces new terminology, and this can make it much harder to see what he is doing. The following may help:

An example of Exact Data: financial data, that is, accounting journal entries following GAAP.

An example of an Exact Model: two balance sheets, one at the beginning of the balance period and one at the end, plus the income statement during the balance period. Bookkeeping is the systematic recording of the monetary value of business transactions in a book of accounts.

The explanation of this is that financial statements – balance sheets, income and cash flow statements – are exact because the data in them (for example, the journal entries) are recorded in conformity with the Accounting Identity, which states that Assets = Liabilities + Equity. (Originally, Los says, this referred to book value, but now more often means market value.)

As for *Inexact Models*, such as those concerned with behavioural relationships, Los writes, (2001, p. 25):

> Behavioral relationships, like the relationship between inventory and sales, are inexact, because the available empirical data are do not conform to simple identities as in exact Newtonian physics [. . .] [as with] [. . .] Baumol's (1972) exact optimal square-root inventory model. [. . .] In fact Baumol's normative model is not a scientific model realized from exact empirical data or even identified from inexact empirical data. Therefore, it should not be surprising to find that this model doesn't fit the empirical data. [. . .] Such multidimensional inexactness can lead to spurious measured covariances between the four data series. The problem then becomes how to decompose such measured covariance data into exact systemic and residual unsystemic covariances.

Los offers a couple of examples of exact and inexact financial models: the valuation of Treasury bonds is an exact financial model, but graded corporate bonds are inexact.

As for methodology, Los argued that his book will not use the statistical term estimation, since that is based upon the assumption of stochastic, in particular, probabilistic phenomena. He proposes to demonstrate that the assumption of probability is extraneous to modelling, although it can be used for playing sophisticated statistical games. Kalman and Los established that modelling can be executed by only algebraic and geometric, that is, non-statistical methodologies.

Very often one encounters in statistics 'the assumption that the data are sampled from a universe ruled by a probability law, such as the Gaussian distribution, the Poisson distribution, the stable Cauchy distribution, the stable Levi, or the binomial distribution' (Los, ibid., p. 86).

But according to Los (2001, p. 86):

> the concept probability may not even be required as an explanation for a particular observable behavior. The measurement of relative frequencies can clearly not be extrapolated to probability laws without an extraordinary leap of faith.

> An act of faith or the setting of a level of confidence is unallowed in scientific research, which is based on data and – true or false – logic.

In the context of modelling, Los (2001, p. 86) claims, ‘a probability assumption does not assist or add to the linear identification of models from inexact data, since, as we have demonstrated, linear identification can proceed without it, using the first and second moments of data series as sole inputs’.

So, he asks, why are the theoretical concepts of probability law so widely used? Los (ibid., p. 86) argued that:

> the assumption of probability laws, to produce an abstract description of an actual relative frequency distribution, produces subjective game-theoretic constructions, like hypothesis testing (e.g. based upon the assumption of a normal or Gaussian distribution). Speculative exact asset valuations, that is, financial instruments like Black-Scholes priced options, or binomially priced options, use the concept of a probability law to introduce a closing limit argument to make a unique valuation possible, not because there is any evidence for it.

In other words, he argues, ‘scientifically unwarranted, but practically convenient assumptions are introduced to close the expansion series so as to produce unique speculative valuations, that fit some abstract theory, most likely the pet theory of the modeller’ (Los, ibid., p. 86).

Whereas Davis and Spanos see salvation in a return to Haavelmo, Los sees at best only irrelevance, and at worst subjectivity and bias.

9.2.1 The Los Critique

Los makes the familiar point that the data from which a model is to be constructed can be exact or inexact. In the hard physical sciences, or in areas of accounting in which the variables and their relationships are clearly defined, data can be exact, and hence so can models based on this data. It is in behavioural studies, like economics, that we encounter inexact data, because the relationships between behavioural variables are inexact. Behaviour, especially human behaviour, is not perfectly predictable. These relationships contain a degree of uncertainty, unsystematic residual noise along with the systematic information. It is these relationships, of course, that are problematic. Inexact data can lead only to inexact models.

In his exposition of inexact modelling, Los (2001, p. 65) argues that standard econometrics does not make full use of the information contained in the data. For instance, he writes: ‘an a priori distinction between ‘regressand’ and ‘regressor’ data variables is scientifically unjustified’ (ibid., p. 65). Again, he states the necessity of analysing the complete set of data in order to make this distinction properly. He (ibid., p. 80) complains that 'currently the choice of a particular linear combination (and projection) is still taught in conventional statistics as a subjective operation and a prejudiced choice of variables'. He claims that his own method, determined from the uncertain data alone, can objectively discover the true invariant number of independent equations of the model.

In the second of the two chapters on the analysis of inexact data, Los gives an example of an uncertain model in three dimensions. The data set upon which the example is based consists of three economic variables observed for Taiwan: the natural logarithms of its stock market index, its nominal GDP one quarter ahead, and its bank lending rate from the first quarter of 1986 to the third quarter of 1995. Los (2001, p. 80) plots these data on a 3D scatter plot and derives a quadratic equation for an exact ellipsoid based on the corresponding (3×3) data covariance matrix.

He goes on to plot the three 3-variable single equation orthogonal least squares projections. Each of the three projection planes formed represents the projection of one of the variables, the regressand, on the other two variables. In this particular case, the planes lie in three different directions, forming a kind of cone around the principal axis of the previously plotted ellipsoid. Again, Los (2001, p. 83) sees a demonstration of the fact that 'an *a priori* distinction between "regressand" and "regressor" data variables is scientifically unjustifiable, since each plane lies in a different direction, representing only part of the data'. In this case, therefore, a model with two independent linear equations seems preferable. The three planes intersect to form three rays, which all lie in the same direction. There are three ways of choosing two equations from the three formed by these rays, but the uncertainty inherited from the data does not allow us to choose between them: 'that is what complete modeling means: to provide a complete honest presentation of the epistemic uncertainty of the empirical data' (Los, 2001, p. 85).

This example given, Los briefly discusses his reservations about the probability approach ushered in by Haavelmo. Los (2001, p. 86) states that in statistics, the assumption is very often made that the data involved have been sampled from a universe ruled by a probability law: 'unfortunately, there is no scientific evidence that such an assumption is physically true, or can be physically true. In fact, there is much evidence that it is false'. He states that the establishment of probability laws for universes requires infinite continuous random data. This is physically impossible, says Los, since the universe is finite and discontinuous according to scientifically established laws of physics. What we observe are merely the relative frequencies of certain events, and as we stated earlier it is a leap of faith to extrapolate from this the existence of probability laws. If the use of probability law is neither necessary (since identification can proceed without it, as Los has demonstrated) nor justified (by the established laws of physics), then why does the practice continue? According to Los, the use of the theoretical concept of a probability law continues because it allows for the subjective game of hypothesis testing. The models can be closed and solved, and hypotheses can be shown to fit. In other words (ibid., p. 86), 'scientifically unwarranted, but [. . .] convenient' concepts are used to lend support to the pet theories of the modeller.

9.2.2 Diagnosis

An econometric model must be composed of variables and relationships that are actually applicable, that make sense in terms of the daily life, skills and abilities of the agents being described. The concepts don't have to be the same as those used by the agents; but they do have to be translatable into those the agents themselves use to describe their plans and behaviour. And, finally, the behaviour being modelled has to be measurable. The relationships are mathematical; many decisions are supposed to be based on mathematical calculations. Many actions are carried out to a certain point and then stopped. Agents have to be able to measure, and so therefore do observers.

Los supports the case for building realistic models, but it sometimes sounds as if he merely opposes excessive abstraction. This is a mistake. A model can, indeed must, be abstract (Krugman, 1997; Nell, 1998a). But that is entirely different from being unrealistic. Anything unimportant or inessential can be left out. But things that don't exist or which are impossible cannot be added – as is unfortunately routine in neoclassical thinking. Los is careful not to propose idealized agents, and indeed he opposes assuming probability laws precisely because they are 'idealizations' of the inexactness of the data!

Davis and Spanos, not to mention Haavelmo, would agree to a certain extent with the above critique. All these authors agree that the difficulty stems from inexact data, which reflect the intrinsic element of uncertainty or unpredictability in human behaviour. They all agree that standard econometric practice does not pay enough attention to all the information contained in the data, instead trying to 'fit' the data to pre-established theory. They all agree that this causes many problems like mis-specification, which can best be dealt with by allowing a much greater role for the data. (Hollis and Nell would add, and Haavelmo hints, that more realistic theory would improve matters.) So far so good; but then Los parts company with the others by rejecting the usefulness and validity of the notion of probability itself. How should we respond to this move?

First, note that Los is taking issue with the actual existence of probability laws in the world. He says that the established laws of physics rule out the possibility of probability laws, since these last require infinite, continuous random data whereas the universe is finite and discontinuous. To say the least, this is a huge jump; we do not think the established laws of physics are relevant here, and we don't think the characteristics of the universe are very solidly established. But we do agree that it is appropriate to challenge the actual existence of laws of probability in the world.

Yet Haavelmo never suggested that; indeed he argued that the probability approach was a way of obtaining results, rather than something dictated by the nature of the world. Los's claim confuses metaphysics on the one hand with epistemology and methodology on the other. Probability laws refer not to the actual nature of the world 'out there', but to the uncertain nature of our knowledge 'in here', so to speak. There is a certain degree of irregularity – inexactness, if you will – in our knowledge about the world; but there need not be (or rather, there is not to our knowledge) a corresponding randomness inherent in the universe itself. Haavelmo's claim is that the probability approach helps us to deal with this.

Even statements about the universe being finite and discontinuous are only probably true, in the sense of being conditional on our current level of scientific knowledge. Haavelmo himself is very clear about this distinction. He (1944, p. iv, italics added) writes:

> [S]tarting from a purely formal probability model involving certain probabilities *which themselves may not have any counterparts in real life*, we may derive [. . .] a statement about a real phenomenon, the truth of which can be tested.

For example (ibid., p. 9, italics added), a statement such as:

> The probability concerns the nature of human knowledge, i.e., epistemology, and hence affects our methodology, but does not have to say anything about the nature of the Universe, i.e., metaphysics. Haavelmo speaks of theory confirmation as occurring when 'Nature' has a way of selecting joint value-systems of the 'true' variables such that these systems are *as* if the selection had been made by the rule defining our theoretical model.

As mentioned before, probability laws, like all laws, according to Haavelmo, are artificial inventions that we use to help us understand the world, but they are not hidden truths to be discovered. It seems that Los (2001, p. 80) would disagree with this, as he speaks of his method objectively discovering the true invariant number of independent equations of the model.

Other problems can be found in Los. He seems to be taking the extreme position that no theory should influence the work of the model builder, that all information in the model should be derived from the data and the data alone. Yet, in the three-dimensional model discussed above, several questions arise. How did Los decide to use the stock market index, the nominal GDP, and the bank lending rate? Why construct a model using those three variables? Why not others? Why use the natural logarithms of the stock market index? Why use the nominal GDP one quarter ahead? Why lag certain variables and not others instead?[14]

A second set of questions arises concerning the structure of the model. From the data, Los proposes to establish a model with an exact set of relationships and a residual representing the inexactness of the data. But, as we know, the data will not establish this uniquely; there will be many different ways to construct this. How will Los choose between them?

Los (2001, p. 58) also states that once inexact models are identified, and the stationarity of the covariances checked, one can 'extrapolate from the inexact existing data set to an inexact future data set, based upon the observed inertia and homogeneity of the data set'. How can one assume that the 'observed inertia and homogeneity' will continue into the future? We have a provided an argument for this sort of assumption in Chapter 5, but it holds only for reliable relationships. By the same token, our position is that such things *cannot* be assumed for volatile relationships.

Consider an example of an 'inexact' relationship: the banking sector and the relation between loans and capital. Loans require backing or support from capital, and there is a 'normal' or sometimes legally required ratio. But in an expanding economy, pressure will develop on the capital requirements to permit more loans, and there will be an opposite tendency to contract in recessions. Yet it will not be possible to be exact about any of this. So far this seems a good example of an inexact relationship, with strong regularities but understandable deviations.

But when we look more closely, there is a problem: the extent of the deviations is not just a matter of being 'inexact' – that is, something that is somehow inadequately determined, like measuring by the naked eye the piece of wood needed for a repair. There is nothing 'inexact' about making loans, let alone about accounting for them on balance sheets, as Los himself affirms. The problem is that the capital requirements constraint does not actually constrain. In a boom, bankers are motivated – and pressured by competition – to lend excessively; we can speak of optimism and psychological factors. This sounds very much like 'inexactness' – how can we be exact about how optimistic we feel? But feelings are only part of the story, and maybe not even a large part. Our capital today depends on its expected earnings – and we *can* be quite exact about our forecasts and expectations. And if these tell us that our capital will increase in value – in fact, that it is going up right now – then more loans are justifiable, and the extent of the justified increase will be quite exact. The catch, of course, is that we cannot *know* the future, and our expectations of it depend on innumerable factors, including 'animal spirits'. Expectations are likely to shift suddenly, unpredictably, and sometimes strongly. The result is, in principle, unpredictable, and moreover we cannot even assign probabilities to the possible magnitudes. We cannot define probability distributions because we are facing volatile relationships. Not only do expectations change, but factors that formerly influenced expectations cease to do so and new factors of importance emerge. Forms of relationships can change, as well as magnitudes.

But suppose expectations are steady, because the period is one of tranquillity. Capital values might then be stable, and agreed. Could we then establish a relationship between capital and loans, inexact perhaps because of human error (miscalculated risk), but reliably inexact, so that the degree of inexactness could be projected into the future? This brings up a second problem of a different kind. The capital-to-loan ratio is not a pure market relationship; it is enforced by regulators. How effective will the regulators be? The banks, let us say, wish to lend, the regulators to restrain – who will tend to win out, and by how much? This is like predicting the winner of a game or a race. We know how to do this, and we do it for sporting events all the time, but we also know that when the two sides are more or less evenly matched, we can't ever be sure in advance. This is not inexactness: it is unpredictability.

The point is not that the questions put to Los have no answers, but that the answers must be derived from theoretical considerations as well as from the information contained in the data. Some pre-existing theory must be taken into account; an important question is, just what kind of theory? Haavelmo suggested that theory must be realistic. Hollis and Nell (1975), and especially Nell (1998a), have argued that theory must reflect conceptual truths and must be based on fieldwork. Such theories can be put to the test against the data, and modified in the light of the data, instead of being solely relied upon to shape the data to meet pre-existing conceptions.

There is certainly a problem with the current overreliance on theory, but Los suggests an over-reliance on data that would be equally pernicious. The problem may not be theory as such, but an over-reliance on individual maximizing theory. Many actual economic relationships simply may not fit the maximizing models. Fieldwork and clear thinking about the necessary presuppositions of economic activity may suggest better ways of theorizing. But this will require us to consider more closely the relation of theory to the world – and of probability to both!

9.3 FOLEY

Foley's (2005) book can be regarded as an attempt to emulate Richard Von Mises's (1957) book, *Probability, Statistics and Truth*. The subject of Von Mises's book is

> the quantitative concept of probability, which was first set down in a precise manner by Laplace, shortly after 1800.
>
> The essentially new idea which appeared about 1919 (though it was to a certain extent anticipated by A.A. Cournot in France, John Venn in England, and George Helm in Germany) was to consider the theory of probability as a science of the same order as geometry or theoretical physics. In other words, to maintain that just as geometry is the study of space phenomena, so probability theory deals with mass phenomena and repetitive events. By means of the methods of abstraction and idealization (which are only in part free activities of the mind), a system of basic concepts is created upon which a logical structure can then be erected. Owing to the original relation between the basic concepts and the observed primary phenomena, this theoretical structure permits us to draw conclusions concerning the world of reality. In order to allow a rationally justified application of this probability theory to reality, a quantitative probability concept must be defined in terms of potentially unlimited sequences of observations or experiments. The relative frequency of the repetition is the measure of probability, just as the length of a column of mercury is the measure of temperature. (Von Mises, 1957, p. v–vi)

Foley's book is an illuminating essay on the foundations of statistical inference written from a historical–methodological point of view. It sets out to present the Laplacian view of the concept of probability as the most suitable framework for reconstructing the foundations of statistical inference. A main theme of his book is that widely used statistical techniques, usually taught and interpreted as parametric and frequentist, are better understood as operational and Laplacian, because the logic underlying them is clearer in the operational Laplacian perspective, and supports their use in a wider range of situations.

To see what is at stake, consider Spanos's account of statistical induction and its underlying reasoning (Spanos, 2007, p. 18). He contends that in 1922 Fisher recast statistical induction, moving away from Karl Pearson's induction-by-enumeration in the context of an inverse probability (Bayesian) set-up, to a model-based induction drawing on a purely frequentist set-up. This called for two related innovations. 'The first was to replace the inverse probability approach, where priors and Bayes' equation are used to arrive at a posterior distribution, with a frequentist approach based on the sampling distributions of relevant statistics. This changeover is well-known and widely discussed; see Stigler (1986), Hald

(1998)' (Spanos, 2007, p.18).[15] There is still controversy over whether and to what extent the frequency definition of probability is circular (see Keuzenkamp, 2000, an issue that will be considered below). 'The second was to transform the primitive form of induction-by-enumeration, whose reliability was based on a priori stipulations, into a model-based induction with ascertainable error probabilities' (Spanos, 2007, p.18). Furthermore, Spanos (ibid.) argued that 'Fisher initiated a general way to quantify the uncertainty associated with inference by: (a) embedding the material experiment into a statistical model, and (b) using the latter to ascertain the (frequentist) error probabilities associated with particular inferences in its context'. The induction procedure proposed by Fisher (1922; 1935a; 1935b) bases the reliability of the inference on the 'trustworthiness' of the techniques used to arrive at the inference. Spanos (2007) pointed out that 'a similar form of model-based induction was proposed much earlier by Peirce (1878), but his ideas were way ahead of his time and did not have any direct influence on either statistics or philosophy of science' (see also Mayo, 1996).

9.3.1 The Frequency Interpretation of Probability

The frequentist interpretation of probability has often been challenged as 'circular', especially by Bayesians; the claim is that relative frequencies can only be defined by appealing to an intuitive concept of probability. A frequency only measures probability if it is based on a 'good sample'; a good sample or a typical draw is one that has a high probability of approximating the true distribution. Spanos (2007), however, argues that the circularity charge is misplaced.

The basic formal result invoked for the frequency interpretation is the strong law of large numbers (SLLN), a stronger version of the widely cited law of large numbers. Paraphrasing Spanos (2009, pp. 23–4) and using his notation, this theorem states that, under certain restrictions on the probabilistic structure of the process $\{Xk, k \varepsilon N\}$, it follows that:[16]

$$P\,(\lim_{n\to\infty} (1/n \sum_{k=1}^{n} X_k) = p) = 1.$$

This result is often invoked to define the frequentist probability of an event $A := \{X = 1\}$ via:

$$P(A) := \lim_{n\to\infty} (1/n \sum_{k=1}^{n} X_k) = p$$

Does the first equation above provide a justification of the frequentist interpretation of probability as given in the second? The issue often raised is that this justification is circular: it uses probability to define probability (see Lindley, 1965, p. 5). Spanos (2009, p. 24) cites 'notable mathematicians including Renyi who disagree; they draw a clear distinction between the intuitive description in the second, and the purely mathematical result in the first, dismissing the circularity charge as based on conflating the two (see Renyi, 1970, p. 159)'. (Spanos argues that 'a closer look at the first reveals that the mathematical theory underlying the result is that of a Lebesgue measure' (Spanos, 2007, pp. 35–7).[17]

We tend to think that the mathematical concept of frequentist probability ultimately rests on the intuitive concept. So while they should not be conflated, there is no problem in basing the first on the second. The real question is: what is the foundation for the intuitive concept?

Our argument is that the concept is necessary; we can't study socio-economic systems without it. Moreover, such systems could not operate without it. This is an argument based on conceptual analysis; this is not the place to develop it fully, but we can sketch the basic idea. Consider an argument designed to deny that any notion of probability was needed in order to describe a socio-economic system (which is more or less Los's position). Could such an argument consider all possible cases? Could we even know all the possible circumstances in which a probabilistic notion might appear? These would be infinite, yet we would have to consider them all if the argument were to rule them out. The best such an argument could possibly do, then, would be to conclude, after a lengthy survey in which probabilistic notions were successfully replaced in descriptions, that we probably don't need the concept of probability! Clearly this won't do. Moreover, it would be easy to extend this argument to show that, on the contrary, we probably do need such a concept, for both agents in a socioeconomic system and outside observers continually face potentially and/or actually infinite sets of data, and have to make judgements of the form, so many *a*s are *b*s. Such judgements are necessary to manage socio-economic systems (see Chapter 5); for example, each industry, each firm, must make judgements each period about how many items of equipment will need to be replaced. They must also estimate how many employees will die or retire and need to be replaced. In each case these are judgements about relative frequency, and they further depend on judgements that the *a*s and *b*s so far in evidence are properly measured and a good sample. Accordingly, we hold that probabilistic thinking along relative frequency lines is a necessary feature of the analysis of socio-economic systems.[18]

9.3.2 Foley's Approach

Foley (2005, pp. 4–5) tried

> to explain the analytical results reached by the theories of probability and statistics [. . .] from a unified Laplacian philosophical, mathematical, and scientific point of view. It reports the results of [his own] (and undoubtedly idiosyncratic) attempt to reconstruct probability theory and statistical practice on a consistent, logical, workable, and, above all, teachable basis.

He does not try to deal exhaustively with all possible objections to the interpretation proposed, but sets out the theories of probability and statistics in positive terms that are pedagogically effective.

Foley's (2005, p. 5) vision is at heart Laplacian, in that he sees

> probability statements as expressing the state of information of some particular observer of a system, not a property of the system itself. I will show that many widely employed classical statistical techniques which are viewed as objective can be interpreted as rigorous applications of the Laplacian theory with specific but intuitively persuasive prior probabilities. [The] first conclusion is that the long debate between classical and Laplacian approaches to probability and statistics is largely irrelevant from a scientific point of view, since they lead in practical situations to the same results. This reformulation also puts common practice statistical techniques on a transparent and logical foundation.

But at the price of postulating a highly idealized 'impartial observer'!

One chief aim of his book 'is to make explicit exactly what implicit assumptions about the informational state of the observer support frequentist analysis' (ibid., p. 2). Foley contends that in the case of classical statistics (or to use Foley's terms, the 'frequentist approach'), probabilities are interpreted 'as objective properties of the external world, which manifest themselves in the relative frequency of occurrence of different outcomes of random processes' (ibid., p.1). According to this conception, 'a change in the informational status of the observer can have no influence on the probabilities governing the phenomenon being observed' (ibid., pp. 1–2).

According to Foley (2005, p. 1), 'the classical approach flatters itself too much in this claim to be objective'. He rejects the claim that classical frequentist procedures are objective, arguing that subjective decisions enter into them; the observers must decide which observations data to count. (Yet it's not so simple. For while some such decisions might be purely subjective – for example, we like the idea of counting these – others could be a matter purely of logic – for example, you can't count that and not this – or they could arise from conceptual analysis – for example, those data are just what the theory means. Logic and conceptual analysis are both objective in a well-defined sense.) He wrote (ibid.):

> The most coherent way to understand the logic of probability and statistical arguments is to start from the Laplacian point of view that explicitly puts an observer in possession of explicitly defined information in the center of the theory. It is possible from this starting point to give a satisfactory and transparent account of frequentist methods and results. Basically, the argument is that frequentist methods are Laplacian methods derived from particular assumptions about the informational state of the observer.

This view places an assumed impartial observer in possession of explicitly defined information, at the centre of the theory. Foley (ibid., p. 81) argued that the 'probability language in a Laplacian sense reports the state of an observer's information relative to a system, rather than an inherent but not directly observable property of the system itself'. The reporting of statistical results will be in the form of a probability distribution, rather than through, as Foley (2005, p. 81) puts it, 'the imperfect and even incoherent concepts of hypothesis rejection, best estimators, or confidence intervals'. According to Foley (ibid.), 'the real force of Laplacian statistical reasoning is to remind us that it is impossible to report judgements about posterior probability distributions without making explicit the prior distribution assumed in the analysis'.

Foley presents what he calls the 'Laplacian interpretation' which 'sees probabilities as a description of the beliefs of an observer who has some specific information about the observed system'. Foley (2005, p. 1) argues that

> these beliefs may be influenced, or indeed, determined in some situations by the relative frequencies of events the observer has observed, but an observer can perfectly well have a probability system over an event which she knows will occur only once. A change in the observer's information will in general change her probability system. Since the Laplacian approach puts the observer at the centre of the formation of probabilities, it has a subjective element.

Furthermore, he contends that 'the distinction between subjective and objective is somewhat deceptive, calling our attention to Hegel's claim that subjective and objective are different aspects of the same dialectical unity.[19]

Foley (ibid., p. 53) sees

> [s]tatistical inference as the application of probability theory to situations where we believe that some features of a situation remain constant over many observations, so that we can learn something about a data-generating process by observing it many times.

The belief in constancy has to be based on objective information. He went on (ibid., p. 53) to argue that

> The typical statistical inference problem begins with some information in the form of a sample made up of observations of the data generating process. On the basis of this sample we are asked to infer some properties of process itself.

He also argues that 'approaches to this problem differ in two dimensions: they can be either parametric or operational, and they can be either explicitly Laplacian or classical' (ibid., p. 53). The parametric approach starts 'by assuming that the data has been generated from one of a class of models which are characterized by certain parameters. The problem of statistical inference in this approach is understood to be the task of making inferences about these parameters on the basis of the data' (ibid., p. 53). But as a general matter, the parameters are not directly observable even in principle, in this approach. In contrast, the operational approach 'makes assumptions only about in-principle-observable quantities. The problem of statistical inference in this perspective is the task of making inferences about possible further observations on the data-generating process from the sample of observations already available' (ibid., p. 53).

Foley (2005, p. 1) makes two claims at this point. First, he argues that

> the classical probabilist cannot avoid making 'subjective' judgments about what observations to count as arising from a given system, and hence contributing to the observed frequencies that characterize that system.

Again perhaps subjective, but not arbitrary; what should be counted will be the result of logic and conceptual analysis.

On the other hand, Foley (ibid., pp. 1–2) argues that the

> Laplacian probabilist wants to use evidence to convince others of the validity of her probability judgments, and thus is driven to seek *an inter-subjective consensus* on probabilities that has an objective aspect.

Foley's unifying statistical vision has thus opened a way for reconstructing new foundations for statistical inference. He wrote (2005, p. 55):

> The operational Laplacian posits the assumption that a future set of observations and the set she has made would be, in de Finetti's language, exchangeable. This means that she (and her reader) accepts a prior joint probability over all possible (finite) sequences of observations of the process that gives the same probability to any two sequences which are permutations of each other. As we will see, this is logically equivalent to giving two sequences of observations of the same length with the same number of hits the same probability. The scientist

> therefore condenses her data into two statistics: x, the number of hits, and $n - x$, the number of misses, or, equivalently, n, the number of observations she has made, and x, the number of hits she observed, or, equivalently, n and $p = x/n$, the proportion of hits she has observed. As we will see below, the assumption of exchangeability is logically equivalent to the statistical assumption that all the information in the observation is carried by the statistics n and x. Furthermore, the scientist adopts a prior which gives equal probability to any possible outcome of her experiment, that is, to any possible x from 0 to n. The assumption of exchangeability, motivated by symmetry, whose substance is the belief in the homogeneity of the behavior of the data-generating process over time, together with the uniform prior for x over the set $\{0, 1, \ldots, n\}$, and the data actually observed $\{n, x\}$, are sufficient (as we will show below) to derive a posterior probability over the uncertain quantity which is the number of hits, y, in any further sequence of m observations. When the future sequence of observations becomes very large, so that $m \to \infty$, the posterior probability distribution of the proportion $q = y/m$ depends hardly at all on the exact value of m.
>
> [I]f the scientist wants to be even more helpful to her reader, she reports this posterior probability distribution, either for arbitrary m, or in its asymptotic form for large m. It also turns out, for large m, that this probability distribution depends only on $\{n, p\}$, and can be very well described by simple formulas that depend only on $\{n, p\}$. In following this procedure the scientist allows the reader to answer any question of the form: given the data $\{n, p\}$, the assumption of exchangeability and a uniform prior over the number of hits in any experiment what is the posterior probability of seeing a proportion q of hits in a further m observations on the process?

In proceeding in this way, neither the scientist nor her reader need make reference to any in principle unobservable features of the world. Nor will the process try to construct or infer an unobservable parameter – instead, it will predict a new set of observable data on the basis of the present set.

Yet there may be questions. Foley rejects the structural econometrics approach that seeks to determine parameters; parameters are 'unobservable' in principle. Instead, from the data we have, we should seek to predict the properties of the next set of data that we shall obtain. But, in fact, very often we want to calculate the unobservable parameters; such parameters may be features of the data generating mechanism, or are derived from such features. We are interested precisely in going from the observable to the unobservable. Moreover, some parameters can be more or less 'directly observed', though not by econometric methods. An anthropological study of household spending habits, together with interviews and household financial records, could provide direct evidence of the household's propensity to consume. And engineers can provide direct estimates of the productivity of labour using certain equipment.

Foley has argued that we should not accept the classical presumption of an objective process generating the data; we do not know – we cannot prove – that such processes exist. Yet we argued in Chapter 5 that such processes *must* exist. It is true that economic data are not the results of laboratory experiments; the conditions that produced the data can't be controlled or repeated. Each instance is, in a sense, historically unique. What justification is there, then, for treating the data as if they were a sample drawn randomly from a population? But if this is not done then we have no grounds for applying the methods of statistics.

Our answer draws again on our fundamental idea that a social system exists only because it reproduces itself (or is a subordinate system that depends for support on such a self-maintaining system). To work with this idea we must first separate the basic or elementary self-supporting social reproduction system from other systems (weather, disease, the animal kingdom, geology etc). The variables have to be defined, background noise eliminated, and so on; that is, we move from the 'phenomenal system' to a 'physical system', or socio-economic system. But, by definition, this system will reproduce itself over time, allowing for two factors – expansion and evolutionary drift, both of which can be accounted for in various ways. Apart from this, the basic reproduction system will generate values of variables that reflect its unchanged nature, over a long time period, namely the life of the society.

If these are the values of a 'physical system' – a socio-economic system – they will be translatable into 'hard data', namely data from a well-defined system that exists and functions over time. Thus it will generate over time a population of data, and what we have, here and now, can be considered a draw from that population.

Of course, this system will be bombarded by impacts from the variables of other systems, some deterministic, others accidental. Productivity will be affected by disease (partly endogenous, perhaps), by earthquakes, by the weather (both still exogenous), and, of course, by human error. The truck driver, thinking of the smile of his girlfriend, misses the exit; the just-in-time delivery is late, the crucial contract cannot be met, and the deal is broken. The company goes into receivership. And these errors are largely exogenous (though errors may increase in times of intensity). How random are these impacts? How random the human errors? As we shall see, sometimes a case can be made for assuming a normal distribution; but at other times every effort should be made, every bit of information should be used, to define the shape of the distribution.

Foley contends that 'we decide', as a subjective matter, which data to count. But as noted, 'subjective' does not mean arbitrary or illogical; there must be reasons for counting some data and not others. So, the argument continues, the frequencies are not 'objective' features of the world. Probabilities do not characterize the world; they characterize the beliefs of observers. A nice move – and we agree in part – but it does not quite escape the problem.

For the Laplacian approach requires very effective, well-informed, neutral observers, whose activities and judgements are independent of time and the order in which the data are presented. Foley argues that we cannot prove the classical claim that objective processes exist generating the data; we disagree, and we note again that some of these processes appear to generate data of the form: so many *a*s are *b*s. These are relative frequencies, but we agree that many relative frequency probability statements do not necessarily describe the world, but are useful tools in analysing the world.

On the other hand, we argue, in turn, that Foley cannot prove that the necessary sharp-eyed, impartial observers exist. Suppose two observers look at the same data and the same processes, and calculate different probabilities. (This is a question about theory, not about what can happen in practice; of course this often happens.) Are they equally valid? Or shall we say that only one can be right, and that there is a procedure to determine which one? To put it in another way, must impartial observers facing the same observations come to the same conclusions?

To put it yet another way: the neutral observer distils the information available in her present position and settles on a 'prior', reflecting that information. When the information changes, she will change the prior appropriately. But this begs the question, which is: why and how does that information justify that prior?

If it does not justify it, then what the observer does is simply arbitrary. Moreover, it must justify it uniquely, for otherwise there will be a range of possible priors, and no grounds for choosing between them. Again the choice will be arbitrary.

On the other hand, if it does justify the choice of that prior, then the observer is simply irrelevant. The issue is decided by the process of justification, and this is a relation between the information and the prior; we need the logic and the details of this justification, but no observer is needed.

9.3.3 Foley's Unifying Statistical Framework

If operational Laplacianism is adopted as the approach to econometric modelling, the question that naturally arises at this stage is whether Foley's unifying statistical framework (FUSF) can tackle some of the problems raised in the debate over the methodological foundations of structural econometrics. However, we still need to bridge the gap between theory and practice, namely the development of macroeconometric models based on Foley's theoretical statistical framework.

We argued in Chapter 7 that Leamer (1978) attempted a systematization of the textbook methodology's illegitimate procedures using Bayesian techniques. In practice, Leamer's approach turns out to be much more difficult to apply, and his Bayesian methods seem to replace the model selection problem with the choice of prior distributions.[20]

There is also a strong emphasis in Malinvaud's econometric textbook on classical statistical inference and he only offers in chapter 2 a brief description of Bayesian principles of inference.[21] Malinvaud (1980, p. 64–5) wrote:

> Statistical inference, as carried out in the context of economic models, obviously depends on general principles of mathematical statistics [. . .] Different schools of thought give differing justifications for the same procedures, or propose methods for dealing with the same problems. Econometric literature shows traces of such disagreements [. . .]
>
> The aim of statistical inference is to make the model more precise, using available observations. The Bayesian School lays down the following approach. Knowledge of the phenomenon under analysis stipulates not only that the true structure ϖ_0 must belong to a space Ω but also implies that a distribution P over Ω defines the probability that ϖ_0 lies in such a probabilistic subset Ω.
>
> Most frequently, this probability does not result from a true random process; rather, it expresses certain a priori ideas about the likelihood of the different ϖ_0 in Ω. The object of statistical inference is then to determine how the available observations modify the (probability) distribution P which represents accumulated knowledge on the structure of the phenomenon under study [. . .]

He (ibid., p. 67) went on to argue that:

> In the so-called 'classical' theory, there are corresponding procedures which have the same objects but which are not generally of the same form as Bayesian procedures. Bayesian and classical procedures are not equivalent from the point of view of applications.

Furthermore, in an interview with Alberto Holly and Peter Phillips (1987), Malinvaud made explicit his view of Bayesian methodology:

> it depends on what you [Holly and Phillips] mean by Bayesian methodology. If by Bayesian methodology you mean the development of an analytical apparatus with conjugate priors and methods of computation of posterior distributions, then I am not sympathetic. Because the method seems too cumbersome for what it achieves; in particular, it relies too much on assumptions about the prior distribution. But if you mean by the Bayesian methodology an approach to the understanding of logical foundations of inference, then I am quite sympathetic. I think the classical methodology was introduced because the Bayesian methodology looked too cumbersome to apply directly and relied too much on hypotheses about prior distributions, whereas relying on such

> hypotheses did not fit with the idea that statistical procedures have to be objective if they are to be used in scientific research. So I am more sympathetic to classical methodology when it comes to actual procedures, but sympathetic to the Bayesian foundations of inference. (Holly and Phillips, 1987, p. 280)

Malinvaud (in Holly and Phillips, 1987, pp. 280–81) sees merit in the Bayesian methodology as an approach to understanding the logical foundations of inference. Here, Malinvaud is in agreement with Foley!

As stated earlier, one of the main aims of Foley's book is to complete the convergence of two methodologies (classical and Bayesian) in the context of a reformulated methodology that he calls the operational Laplacianism. Foley (2005, p. 130) pointed out the fragility of linear regression. He argued that 'linear regression makes very strong assumptions, which may not be met in practical situations' (ibid.). According to him, 'the problem with the linear regression model is that it imposes a linear relation among the variables in the Jeffreys prior itself' (ibid.). Foley argued that 'Jeffreys' prior represents the opinion of an observer who is completely ignorant of both the location and scale of fluctuation of the data the observer is about to analyze' (ibid.), and continued:

> The linear regression model is prone (highly prone) to two types of misleading result. Both of these problems arise because the linear regression model effectively averages out local correlations across the whole domain of the sample. Thus the regression coefficients report a kind of average co-movement of the variables which may not reflect the actual co-movement of the variables very accurately at any point in the domain. (Ibid.)

He reported three important points that show the fragility of linear regression:

> a strong but nonlinear relationship, may [not be found . . .]. If the relationship between two variables, for example, is positive in one part of the domain, and negative in another part, the linear regression model, in averaging out over the whole domain, may report no systematic relationship. Thus important covariation of relevant variables may be completely missed by the linear regression.
>
> Second, linear regression is very sensitive to correlations at a few outlying points of data. Because it dutifully averages co-movements of the variables over the whole domain, just one or two data points which show a large co-movement of the variables can dominate the linear regression results.
>
> The only general circumstances where the strong assumptions of linearity on which the linear regression model is based seem to be justified is when the data arises from small perturbations of a system. If the perturbations are small, nonlinear interactions in the system will be unobservable, and the linear model reveals the first-order interactions of the variables. Of course, what counts as a small perturbation in this context depends on the system being studied and the scientific judgement of the investigator. (Ibid.)

He concludes that it is 'dangerous to rely on the linear model uncritically, without at least checking for nonlinear co-movements of the data through non-parametric means such as the pixilation method' (ibid.); 'the modeller should, at a minimum, do inspection of scatter plots in two and three dimensions' (ibid.). We certainly agree.

Foley's attempt (2005, particularly chs 17 and 18) is mainly directed to reconsider the Gaussian linear model in the context of the operational Laplacianism view.[22] Spanos (1986, pp. 8–9) argued that 'in the context of the Gauss linear model and linear regression models the convergence of descriptive statistics and the calculus of probability became a reality (with Galton, Edgeworth, Pearson and Yule being the main protagonists)'. He observed that 'in the hands of Fisher (1890–1962) the convergence was completed and a new paradigm was proposed' (ibid.). Furthermore, he pointed out that 'the change from descriptive statistics to the probability theory approach in statistical modeling went almost unnoticed until the mid-1930s when the latter approach formalized by Fisher dominated the scene' (ibid.). He went on to argue that 'in the context of the Fisherian paradigm, the modelling of a probability is postulated as a generalized description of the actual data generating process (Hendry and Richard's term DGP or to use Granger's terms, DGM), or the population and the observed data are viewed as a realization of a sample from the process' (ibid.). Moreover, 'the distinction between the population and the sample was initially raised during the last decade of the nineteenth century in relation to higher order approximations of the central limit theorem (CLT), as developed by Bernoulli, De Moivre and Laplace. These limit theorems were sharpened by the Russian School' (ibid.).

However, Foley (2005, p. 107) argued:

> The historical approach to the smoothness/simplicity problem in data analysis has been to reduce complex observations to simpler statistics that can then be viewed as choosing a smooth probability distribution from a family. The most widely used such method for single variable data is analysis based on the Gaussian or normal probability distribution, a very smooth probability that depends on just two parameters, the mean and the variance.

He showed that 'like the case of Bernoulli trials, the one-dimensional Gaussian analysis can be completely developed without reference to unobserved parameters and an underlying population from which a sample is taken'. He went on to argue that

> we can follow the method used to analyze Bernoulli trials to derive Gaussian analysis from the stipulation that the sample mean and sample variance (or standard deviation) are fully informative statistics. Statements about

> population means and variances can be rationalized in terms of posterior distributions over further samples. Indeed, the limit of the posterior distribution for a further set of observations of size $m = 1$ as n tends to infinity will turn out to be the normal distribution.

In view of the apparent limitations of the textbook methodology (for example Spanos, 1986), any alternative framework should be flexible enough to allow the modeller to ask some of the questions raised earlier in this study. However, Foley does not examine the methodological foundations of structural econometrics, nor does he present a discussion of an econometric modelling methodology in the context of the operational Laplacian mould. It would be interesting if Foley were to develop the USF approach by assigning an important role to fieldwork and conceptual analysis, examining structure and paying attention to actual DGPs in order to bring about improved econometric modelling. There would be a place for discussion of Haavelmo's probability approach to econometrics: we think Foley's approach needs to be developed either to allow recasting of Haavelmo's structure of econometrics in the operational Laplacian mould, or it must explain what would take the place of that structure.

We accept Foley's strictures on linear regression. We also accept his criticism of the classical frequentist approach – in part. Yes, we very often do not know whether there is an objective data generating process, and indeed sometimes we may well believe that there is not. But sometimes we do know (see Allais, 1997; and Calot, 1967a; 1995). In such cases the frequentist approach is justified, and Foley's contention that choosing which observations to count introduces a 'subjective' element, which is not only weak but can be avoided by clear thinking and good empirical design. Of course, sometimes the subjectivism of the Bayesians may be well justified.

Foley's idea of the 'Laplacian' approach is subjective, but not Bayesian, and he wants it to be operational, and non-parametric. (The Laplacian has a subjective prior probability distribution over possible outcomes, but, unlike the Bayesian (Savage, 1954), does not have preferences over the outcomes of lotteries.) We think the estimation of parameters is desirable; and we regard both concepts of probability, the frequentist and the weight-of-evidence, as conceptually necessary to the analysis of socio-economic systems. In short, essentially the same probability calculus can have several different interpretations, Foley's and the classical, and perhaps the Bayesian, too.

The Laplacian approach is not something to which we necessarily object, but on the other hand it is not our position either. First, we do suggest that there are conditions under which a 'frequentist' position might be warranted, as a practical matter, for example the conditions outlined by Calot and Allais. Perhaps these are somewhat limited and special cases, but they might well be very important cases; indeed, we suggest just that. These cases would seem to be much less 'subjective' than the implied position of Foley's observer. Second, we emphasize Keynesian uncertainty, which results from the inherent volatility of certain relationships, and there does not seem to be anything like that in Foley. Third, it seems that he would agree with us that the world is not necessarily stochastic. He certainly does not seem to take the opposite position. On the other hand, he does not draw the kinds of conclusions we do from this.

Fourth, he does not consider the point that relationships between 'revenue' variables (household income and household spending, sales revenue, cost and profits, etc.) present less difficult problems in identification and specification than do relationships between individuated or decomposed variables, like prices and quantities in demand and supply functions. In the case of revenue variables, we know which way the revenue is flowing, whence and where; we generally know who the agents are and what they are doing. These are matters we can directly observe as payments are made; by contrast, when we are dealing with 'stimulus-response' variables, as with supply and demand functions, the observations are of 'singular points', where two (or more) functions intersect. We have no information about causality; it is difficult even to be sure that supply and demand functions *exist* (Chapter 5). To get estimates of these putative functions, we have to decompose the observed points into prices and quantities, and decide which function they lie on.

Two points follow. First, we think we have a superior notion of what 'operational' means, since defining that idea is the point of the methodological triangle-circle (MTC). Foley's idea seems to be pretty simple and based on traditional empiricism. Using the MTC should lead to a better understanding of 'operational', which means we might be able to suggest ways to carry forward some of his ideas of 'operational Laplacianism'.

His notion of 'priors' or 'prior information' again is pretty simple and traditional – the prior probability of an outcome is how much a (representative? rational? or just average ?) agent would pay for a $1 payoff, if the event were to happen. By contrast we don't limit our notion of prior information just to that which would fix a numerical probability in the form of a bet. We consider all kinds of prior information, such as that yielded by fieldwork, which allows us to establish the quality of our information, and also to provide the context for interpreting results. We also consider some outcomes inherently uncertain – we might make a bet on them, but the effect of losing the bet would be quite different from what we would do if we lost a bet on a reliable outcome. We would not necessarily feel that we would have to revise our thinking, or change our future behaviour if the Yankees, whom we favoured to win, lost the World Series, but if the productivity of a new process falls well short of our prediction, re-examination of both evidence and theory is called for.

9.4 THE MTC DIAGRAM APPLIED TO THE FOUR APPROACHES

Now we apply the methodological triangle-circle (MTC) diagram to the four approaches discussed above. This will show how they interact in each case and how they tend to undermine each other. We shall then summarize the main results.

9.4.1 Four MTC Diagrams: Davis, Spanos, Los and Foley

We argued earlier that coherence – theory – is one point of the triangle; relevance or applicability is another; and measurement or quantification is the third. The theory provides an explanation of the economic problem or problems being considered. It will do so in terms of concepts, variables and relationships that must be applicable: they must be present directly, or discoverable beneath the surface, in the behaviour and institutions under discussion. But the plans, decisions and behaviour being described have quantitative aspects, both in prospect and when the results are in. So it must be possible to measure it without ambiguity, to a definite degree of precision.

We argued earlier that a good and useful econometric model has to have all three. It has to be coherent theoretically; the theory has to explain the aspect or aspects of the economy under consideration, in a plausible and internally coherent way. In doing so, it will define the central variables and relationships, and it will indicate which features of the system can be considered as given.

We now turn to the MTC for each case.

9.4.1.1 Diagram for Davis

Davis shows little concern for theory, which therefore contributes very little to his analysis. Theory should be represented by a small circle with breaks in it – there is no guarantee here of theoretical completeness. Relevance is a little more important, but he overlooks Haavelmo's concern with realism. So it will also be a small circle. Davis's main concern is with measurement; he wants to specify just how to apply probability theory, and this he feels requires developing the links between economic

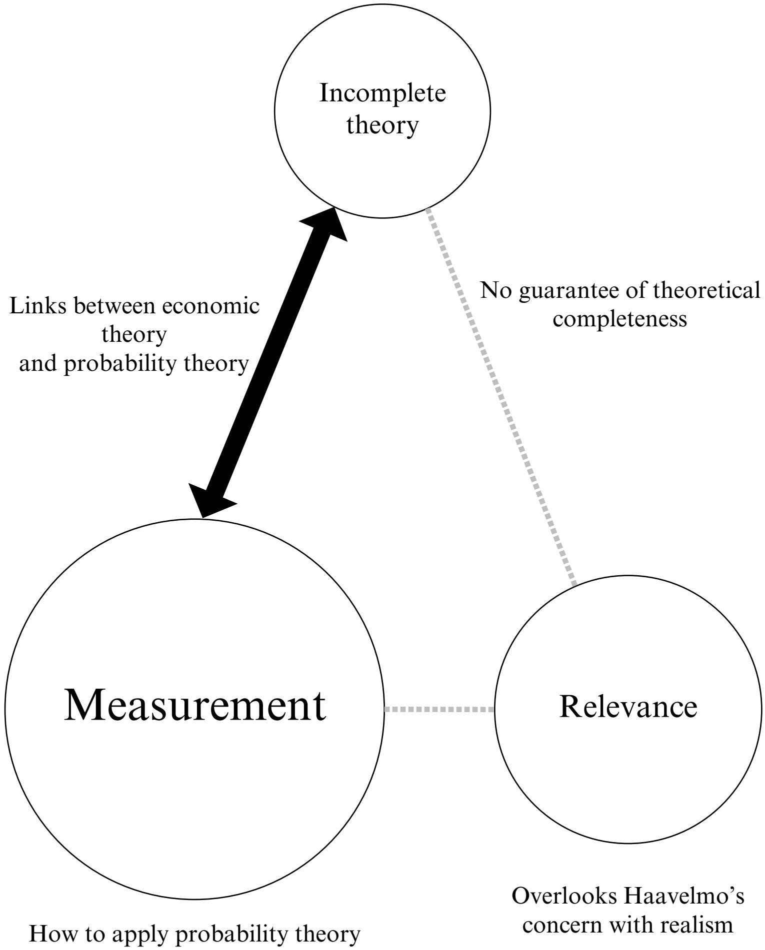


Figure 9.1 Davis's approach

theory – whatever it may be – and probability theory. So measurement will be a large circle, and the link between it and theory will be a heavy line with arrows in both directions. By contrast, the links between theory and relevance, and relevance and measurement, will be weak, shown by light dotted lines (see Figure 9.1).

9.4.1.2 Diagram for Spanos

Theory will be a small circle. Measurement will be large, and relevance will also be large, though smaller; measurement and relevance will be close

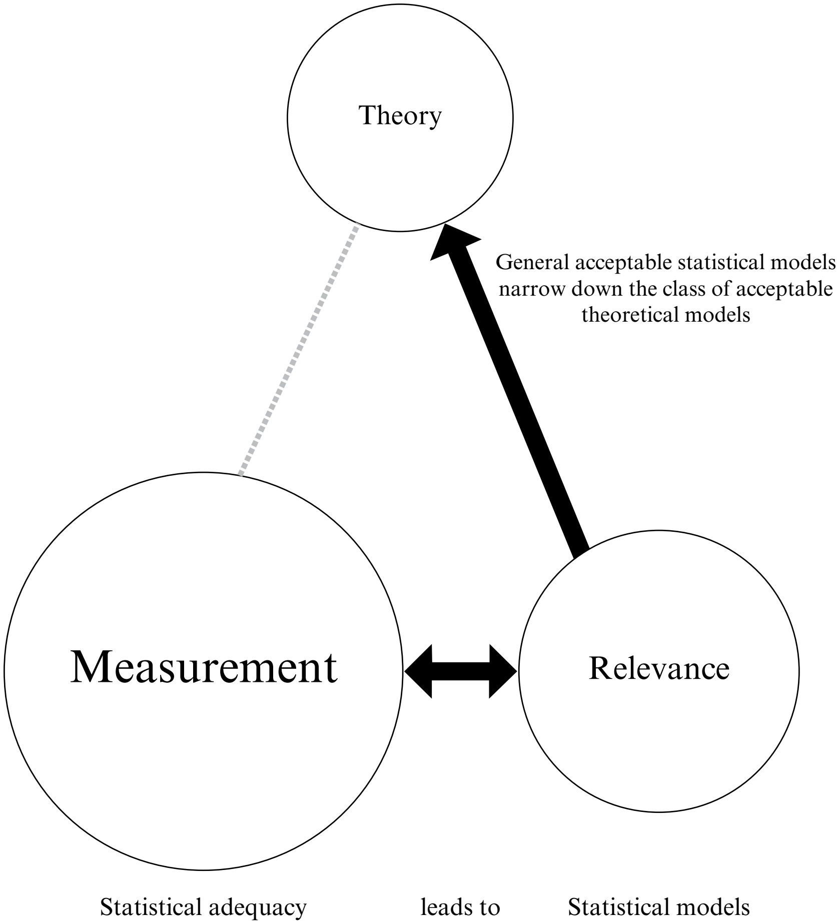


Figure 9.2 Spanos methodology

together. There will be a strong two-way arrow connecting them, as statistical adequacy (measurement) leads to statistical models (relevance). But the connection between theory and measurement will be weak, or virtually non-existent. A strong one-way arrow, however, will run from relevance to theory, since general statistical models narrow down the class of acceptable theoretical models (see Figure 9.2).

9.4.1.3 Diagram for Los

Theory will be small, and will not influence either of the other two. Both relevance and measurement will be large, close together and strongly

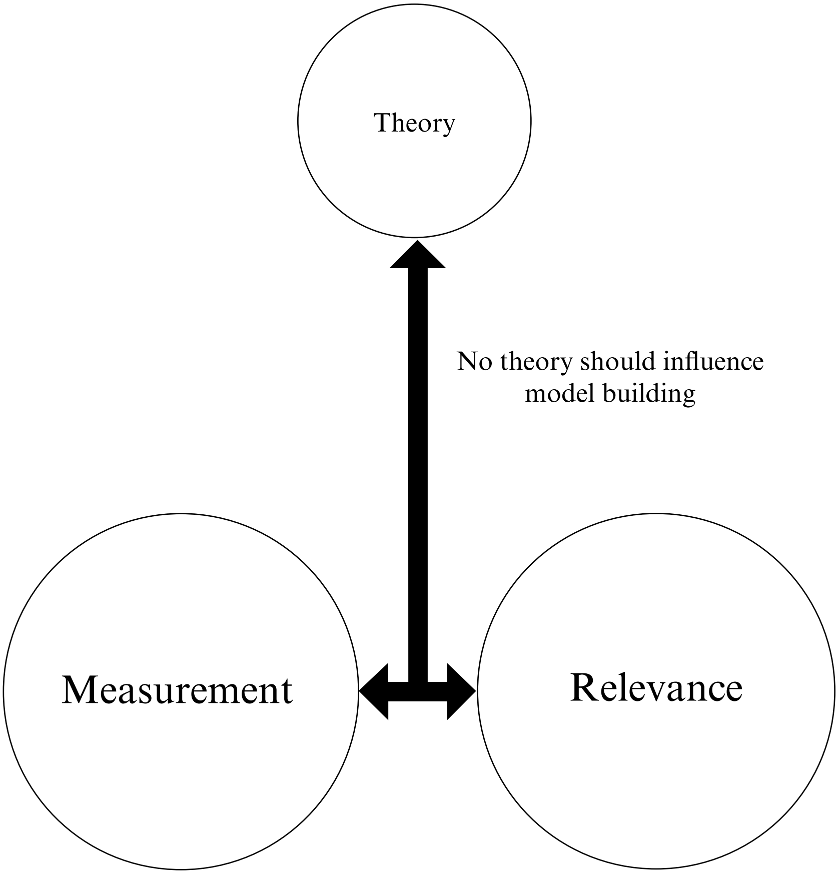


Figure 9.3 Los's approach

linked, with two-way arrows. But the relation to theory will be one-way, running from both measurement and relevance – actually from the two combined – to theory (see Figure 9.3).

9.4.1.4 Diagram for Foley

For Foley, theory provides the reasons for counting some data and not others, so it influences relevance/applicability. But relevance is important only if it leads to measurement; the two are mutually necessary. When both are achieved, present observables can be used to predict future observables (see Figure 9.4)

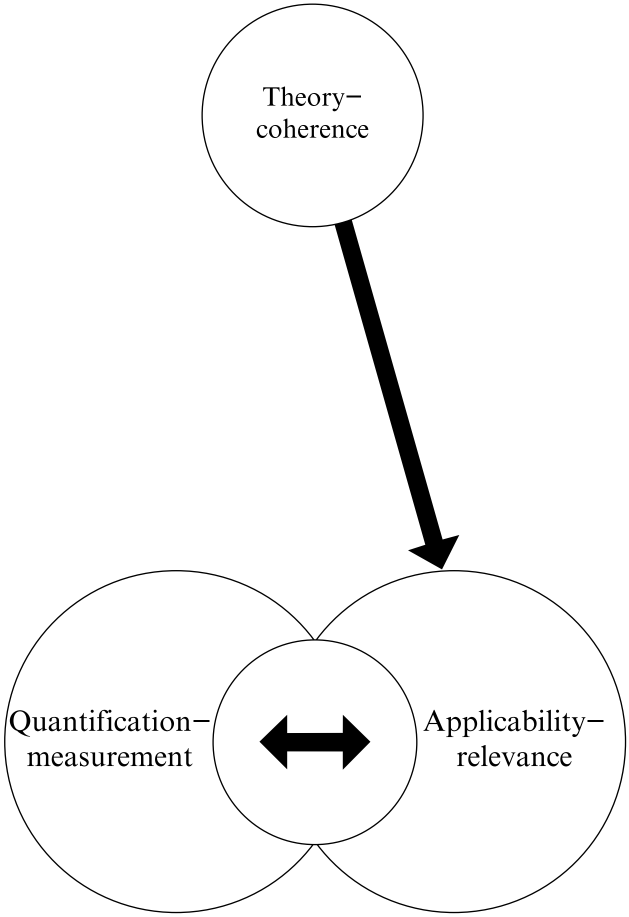


Figure 9.4 Foley methodology

9.5 STOCHASTICISM

For most mainstream economists, the world is a 'stochastic environment' (Boland, 1977; 1982; 2000). As Vernon Smith (1969, quoted by Boland, 2000, p. 151) eloquently stated: 'the quest for truth and validity is indeed a noble venture. However, the economist exists in a stochastic environment'. Granger (2004, p. 100), in the same vein, stated: 'the economy is effectively stochastic and so will be any relationship and thus it may not hold at times or hold only to a varying extent'. The reason, Granger (ibid., p. 97) argues, is that we do not know, perhaps cannot know, the data generating mechanism. The DGM 'is only an abstract concept used by econometric theorists. It is not considered an achievable structure or objective. Because it is so complex, there is even some debate about its existence'.[23] Economics is about decisions; the agents are the population – tens and hundreds of millions of them; and they make decisions, according to Granger (ibid., p. 96–7) 'largely independently, that is, without interactions between the agents'.

Chapter 5 suggests that this view of the world is quite misleading. We know that the socio-economic system exists and maintains itself, and we know quite a lot about how it works. The reasons for probabilistic models and methods must be sought elsewhere.

Stochasticism holds that realistic models must be stochastic, because the world is stochastic; not only does this view take too much for granted, but it leads to taking stochasticism for granted in most econometric models as well (Boland, 2000, ch. 8).[24] And this leads to trouble.

Stochos is Greek for bull's eye and, according to Boland (1982, p. 122), 'the word stochastic is based on the idea of a target and in particular on the pattern of hits around a target. The greater a given unit of target area is from the centre of the target, the less frequent or dense will be the hits on that area'. He (ibid.) went on to argue that 'there are then two worlds: the observed real world and the ideal world of the theory or constructed mathematical model. A theory (or model) is "true" when there is an exact correspondence between the real and the ideal worlds'. There are obvious reasons why, even when we consider theories alone, the correspondence will not be exact: there can always be errors of measurement, mistakes, exogenous influences and irregular or irrational human behaviour.

Stochastic models pose logical problems when we attempt to prove the truth or falsity of any given theory (Boland, 2000, ch. 8). But these problems could be conveniently bypassed when we aim only at building models that fit the data with acceptable degrees of approximation (Simon, 1979). In response, Haavelmo (1958) and Hollis and Nell (1975) might argue, if technically no model is ever refuted or verified, there could never be a chance to construe one as a refutation or a verification of a theory. How could we then ever improve theory by confronting it with evidence?

While reasons for missing the target may abound, they fall into two rough categories: (1) our model was false or logically invalid; or (2) there are random, unexplained variations in the objects we are attempting to explain or use in our explanation. It may thus be argued that a stochastic model is one that allows for movements of the target – that is, variations in the data. However, stochastic models may also follow from a methodological decision not to attempt to explain anything completely.

This has led modern 'stochastic models' to explicitly accommodate the stochastic nature of the correspondence. For example, we can assume that the measurement errors maintain the observations in a normal random distribution about the true values of the ideal world. This means that the correspondence itself is the stochastic element of the model.

Haavelmo (1943b, p. 1) already noticed the subtlety of this point, and warned:

> Personally I think that economic theorists have, in general, paid little attention to such stochastical formulation of economic theories. For the necessity of introducing error terms in economic relations is not merely a result of statistical errors of measurement. It is as much a result of the very nature of economic behaviour, its dependence upon an enormous number of factors, as compared with those which we can account for, explicitly, in our theories. We need a stochastical formulation to make simplified relations elastic enough for applications.

The world is thus not stochastic; it is the theories that should be formulated stochastically. Many methodological confusions in econometrics have been caused by this ambivalence or confusion about stochasticism or the nature of the error term (see Qin and Gilbert, 2001). It is clear, however, that it has to be the model that is stochastic rather than the world or the 'environment'. We can then assert that any test of a stochastic model is a test of the assumed correspondence as well as of the theory itself.

Asserting that the environment is stochastic may result in a serious danger of intellectual error (Boland, 1982; 2000; Haavelmo, 1943b; 1944; Hollis and Nell, 1975; and Nell 1998a), as putting the 'assumptions' of our theory beyond question, as stochasticism seems to presume, cannot be justified in any way. Considering the world as being necessarily stochastic is possible only if one assumes that one's model is true (and fixed) and thus that any variability of the correspondence is due entirely to the unexplainable changes in the real world. Thus, stochasticism can be seen as unjustifiably putting the truth of our theories beyond question (Boland, 1982, ch. 7; 2000, ch. 8).

9.6 PROBABILITIES AND ANALYSIS OF TIME SERIES

The probability approach (for example Haavelmo, 1944) seems to solve some problems, but seldom is attention paid to the logical structure that underlies the methodology.

Allais (1997, p. 6) offered a critical analysis of the concept of chance and of probability theories. He was looking

> for the fundamental factors lying behind the fluctuations of time series in uncertain conditions (particularly: the fluctuations of the residuals of the best empirically verified models), in the course of which he proved what he called the *T* theorem and introduced the *X* Factor, a new concept that represents the exogenous physical influence on time series.[25]

Uncertainty and probability 'are not in fact taken into consideration by the so-called mathematical theories of chance, he claimed. The models that these theories address, Allais (ibid., pp. 6–7) argued, 'are deterministic in nature, while, quantitatively speaking, of all equally possible configurations whose calculation is based on combinatorial analysis, they study only the mathematical frequencies of a set of particular configurations'. Allais concludes that 'no axiomatic definition of chance is actually conceivable' (ibid.).

Allais (ibid., p. 7) argued that the *X* Factor hypothesis posits that 'the fluctuations in time series observed in physical, biological and psychological phenomena are, to a large extent, the result of the influence, through resonance effects, of countless extraneous vibrations whose ubiquitous existence in our space is now well-established'. It is possible then, according to Allais (ibid.), to explain, 'to a large extent, the initially incomprehensible structure of fluctuations observed in a large number of time series, for example, sunspots or stock exchange quotations'. To Allais, 'these fluctuations reveal all the features of an almost periodic structure' (ibid.).

In addition, Allais (ibid.) argued, 'there is an almost periodic function for such a periodic structure'. This function 'is defined as the sum of sinusoidal components, some of which have incommensurable periods' (ibid.).

According to Allais (ibid.), the *T* theorem stipulates that, 'under very general conditions, the successive values of an almost periodic function are normally distributed'. So the 'deterministic vibratory structure of the universe can be shown to generate seemingly random effects, and what is commonly referred to as chance can result from the interaction of deterministic processes'.

When using the probability approach, we can state that errors of observation could reach a certain number; alternatively, we could assume that when the observation is repeatedly made, the errors will be distributed normally. If the average value of the observation is assumed to be the true observation, then the formal mathematical properties of such a normal distribution curve can be used to calculate the possibility that the observations will be incorrect in more than, say, 5 per cent of the observations, thus giving a measure of the potential damage resulting from incorrectly accepting a fit. Indeed, the ability of probabilistic approaches to economics to provide a basis for formalizing essentially arbitrary decisions regarding the choice of confirmation or disconfirmation criteria may be a primary reason for their promotion in economics. The formalization of unavoidable arbitrariness gives some economists a feeling of achievement! However, if we cannot justify either the assumption that errors are normally distributed, or the supposition that the observation errors are random, then probability presumptions may be problematic. If there is a justification, we have a useful approach; if not, we are courting disaster.

For some practical problems, then, especially where the assessment of benefits and costs presents no difficulties, the use of a given probability distribution to characterize the occurrence of errors can have its own merits. However, following Swamy et al.'s (1985) argument, if the distribution of errors is unknown, we may raise more questions than we could answer. Consideration of Haavelmo (1944) within the Hollis and Nell (1975) framework makes us realize that if we want to reach justifiable conclusions in the presence of errors in observations, we had better be very cautious and not rely on the statistical evidence alone.

9.7 CONCLUSIONS

The world is not stochastic; it is open, and its direction and development uncertain (Nell, 1998a; 2004). Not in all things, of course. But there are whole areas where we do not know what will happen because we cannot foretell the future. On the other hand there are areas where we understand quite well how things work. But in these areas there are often random elements, although this randomness may arise, as we have seen, from the interaction of deterministic processes. We write our models – expressing our theories – in stochastic form, in order to deal with the complexities of the world.

To say the world is open means much more than saying it is random. Parts of it may be random, others determinate, but some may be uncertain. That is, some markets and economic processes may unfold in one way or another, but we cannot tell in advance which way they will develop – even though, when they do move, they will move in a determinate path. This is not because we do not have the data or the insight, but because the direction of development has not yet been determined. In certain areas, there may simply be no knowledge to be had – yet. Agents may not have made up their minds yet; innovations may still be in the making; processes that might go one way or another have not yet arrived at the critical juncture.

In these circumstances, not only does it not make sense to test predictions, it may not even be reasonable to look for relationships. They may simply not exist. In other areas, of course, we may have a very good idea of what is going on, and relationships may be stable and predictable.

In other words, uncertainty is inescapable, but it is not necessarily ubiquitous. Economic relationships differ in regard to uncertainty; some relationships seem quite reliable. We know a lot about them, and can easily check our knowledge in a number of ways. We can describe these relationships; we understand why they hold. However, there are data uncertainties, and the relationships themselves may be disrupted by accidental or interfering factors, where these interfering factors can reasonably be presumed to be unsystematic. In these circumstances probabilistic methods help us deal with these matters, and using them we can establish reliable numerical relationships. (Employment and output, consumption and income, the circulation of money, and expenditure and employment multipliers are examples.)

By contrast, other relationships are simply inherently unreliable. We know the variables are connected; we understand why there might be causal pressures. But we cannot measure the magnitudes, and sometimes not even the direction, of these influences. We can list the factors influencing investment, for example, or the stock market; but which factors are more important, and even the nature and direction of the influence, may vary from time to time. Nor can we tell in advance when the nature of the influence will change. Probabilistic methods are no help where there are no relationships to be found. On the other hand, they can help to sift out the reliable relationships that are drowning in the general noise.

So on the one hand we find reliable, well-grounded relationships, resting on contracts, obligations and commitments, and on the other hand we see all sorts of speculative and evanescent proposals, many of which will come to nought. (But a few of them may very well drive the whole system!) Employment and output, consumption and income, and the multipliers all depend on the existing structure of the economy, grounded in property and contract, reflecting technology and social habits and obligations. These matters change only slowly. To these relationships it will be reasonable to apply the theory of probability. Precisely because the variables and relationships are reliable it will be reasonable to suppose that some of the conditions for the classical linear regression model may be plausibly assumed. The problems of traditional econometrics (summarized in the mnemonic MALTHUS) should be manageable; and it should be possible to use the well-developed tools of econometrics.

In the case of reliable variables embedded in reliable relationships, some of the classical assumptions (Kennedy, 2003) about the data generating process may hold, or may not be seriously violated, for example in the case of short-run employment and output at the margin. We have already discussed Allais's contention in his *T* Theorem that 'a large number of interacting cyclical fluctuations, from divergent sources and with incommensurable periods, will generate an almost periodic function whose successive values over time will be normally distributed' (Allais, 1997, p. 7). We can refer to Calot (1967a, 1995), who suggested a perhaps related set of conditions for a normally distributed random variable. He argued that a normal distribution of random errors – deviations from the true value – would arise from a large number of independent impacts, from exogenous causes, which are additively random, and where each has a small variance relative to the total variance. He suggested the example of an industrial process – machining a part.

These conditions can be considered plausible in regard to the reliable functions. In addition, the reliable functions can reasonably be considered to be represented by simple linear relationships with a disturbance term, and there is no reason to suppose that the error terms might be correlated. However, reliable relationships may be related by simultaneous equations, and some of the independent variables in these relationships might be linearly related. But these are precisely matters that the Cowles approach was designed to cope with.

The volatile functions are another matter altogether. Stochasticism is the claim that probability analysis can be applied to the volatile part of the economy, and this is what is not possible. Investment and the stock market depend on our expectations of the future – but these expectations are not well grounded, and cannot be. We do not and cannot know the future of markets, nor the future of technology. If we truly knew the next invention, knew it in detail, we would have made it. But even then we would not know if it would make money. We simply do not know what will happen or what will work. New information will lead some of us to change our minds one way, others another way. Expectations and valuations will shift. There are no grounds here for stable relationships. The probability approach may be some help, but not much, because the uncertainty is not only inherent, the degree of uncertainty is inherently large.

In the search for a balanced methodology, the probability approach is crucial. Estimates of reliable relationships will have to deal with errors in variables, errors of measurement, ordinary mistakes, and all kinds of accidents, including the more or less random side-effects of other deterministic systems as they impinge on the economy. The methods of probability analysis provide ways of handling these issues and arriving at estimates of the 'true' relationships. We know that there are true relationships, because reliable relationships are based on contracts, property, laws, social obligations, and the like. Fieldwork and conceptual analysis will confirm the fact that such relationships exist.

But the fact that probability analysis helps us manage the problem of estimation in these cases does not imply that probability laws characterize the world in general. And it especially does not imply that probability analysis will be any help in dealing with volatile relationships. Volatility in our expectations of the future is not probabilistic (although it may be compounded by errors in variables, errors of measurement, etc). Volatile relationships arise because uncertainty is built into the patterns of behaviour of economic agents who are making portfolio decisions in the light of their expectations of the future. The economic world is not itself probabilistic, but parts of it are inherently uncertain.

NOTES

1. Los claims that 'Dr Rudolf E. Kalman, at a symposium in Greece in 1993, demonstrated that there is very little, if any, scientific basis for Haavelmo's 1944 presumption of the empirical existence of Kolmogorov probability', but Los does not give us the details. Hic Rhodus, hic salta!
2. Dagum (in correspondence with the authors) draws their attention to the need not to confuse Frederick Suppe (1989) with Suppes et al. (1989), and observed that there are two other versions of the semantic view. Patrick Suppes presents a philosophical account closely connected to the representational theory of measurement documented in the three volumes of *Foundations of Measurement* (Krantz et al., 1971; Suppes et al., 1989; Luce et al., 1990). The second is Bas van Fraassen, whose constructive empiricism (e.g. van Fraassen 1980) focuses on ontological issues. Chao (2005) presents a good discussion of Suppes (1967; 2002) and Suppes et al. (1989).
3. According to Davis (2000, p. 206) 'the semantic approach is chosen mainly for three reasons. First, it is much more explicit than Popper, Lakatos, or Kuhn in defining the relation between a theory and a phenomenon. Second, it is well suited for inquiries into the foundations of particular theories. Third, the semantic interpretation of Haavelmo helps illuminate the importance of some of his less emphasized methodological insights'.
4. For further details see Chapter 2.
5. A fuller and more detailed semantic account of econometrics can be found in Stigum (2003). Also a very interesting mini symposium on the semantic approach to econometric methodology with contributions from three authors, namely Davis, Cook and Chao, was published in the *Journal of Economic Methodology* (March 2005), see Davis (2005b).
6. Hollis and Nell, however, would consider this a case of affirming the consequent. Dagum in correspondence and in personal conversation with Errouaki complains that the semantic approach has not paid sufficient attention to the question of whether the conceptual definitions of the variables in the theory correspond to conceptual definitions of the phenomenal system. What happens when these diverge? How much can they diverge without the theory introducing distortions?
7. Davis (2000, p. 222, footnote 7) argued that 'both Hendry (1995a, ch. 7) and Spanos (1986, ch. 22) show how rewriting serially correlated disturbance models in terms of lagged variables in the conditional mean can be used for testing if the disturbance

process is actually autoregressive (through common factor restrictions) or if the conditional mean is just misspecified'.

8. Spanos (1989, pp. 407–408) also observed that 'the textbook approach differs from Tinbergen's approach in two important aspects. Firstly, Tinbergen's experience with empirical modeling made him aware of the fact that theoretical models rarely fit the observed data unchanged and he proposed modifying the theory in order to bridge the gap. Secondly, Tinbergen allowed the data to determine certain aspects of reaction functions such as the dynamic structure'. For further details on Tinbergen, see Morgan (1990a) and Epstein (1987).
9. For further details on the textbook methodology, see Spanos (1986; 2007). Spanos's (1986) econometric textbook proposed a modelling framework for econometrics, named probabilistic reduction, and overlaps closely with the error statistical account in Mayo (1996). To paraphrase Spanos, the most surprising overlap is the sequence of interlinked models aiming to provide a bridge between theory and data. The main difference is one of emphasis. Spanos (1986) provides a more formal and detailed account on what a statistical model is and how its validity vis-à-vis the data is secured using thorough mis-specification testing and re-specification, in the context of the F–N–P (Fisher–Neyman–Pearson) statistical framework.
10. Spanos (1989, p. 413) argued that 'the principle of statistical adequacy was not fully integrated within the simultaneous-equations approach essentially because of the perceived outcome of the well documented debate between Koopmans (1947) and Vining (1949)'. The issue was that any use of the sample information beyond the theory constituted measurement without theory. Neither Koopmans nor the Cowles group endorsed such a position. Spanos (1989) pointed out that 'by making the specification of statistical models as a matter of theory only, there was no room for accommodating the probabilistic structure of the data in the original statistical model or allow a role for it at the respecification stage. The main emphasis was placed on estimation with the data being treated as afterthought'. For further details on the Koopmans–Vining debate, see Epstein (1987), Morgan (1990a) and Hendry (2004).
11. Haavelmo's experimental methodology was discussed in Chapter 2. Haavelmo stressed the fact that observed data in economics are usually of the type collected by passive observation and not the result of experiments on the artificially pure, simple and stable conditions assumed by the theory. For further details, see Davis (2000). We do not think observation should be 'passive'; active participation provides direct knowledge of economic relationships – working on a job helps in understanding productivity.
12. Spanos shows that the initial discussions of the absolute income hypothesis (AIH) rested on a statistically inadequate model, invalidating both the initial reasons for supporting the AIH and the widely accepted reasons for later rejecting it, and also invalidating Kuznets' contention that the APC is constant. By contrast, according to Spanos, Hendry's error-correction model passes the relevant tests (see Spanos, in De Marchi and Gilbert, 1989).
13. For an account of the development of British econometrics between 1945 and 1985, see Gilbert (1986a; 1988). For a discussion of the LSE approach, see Gilbert (1986b, 1987, 1989).
14. Yet Los is surely correct on the technical matter that failing to appreciate the distinction between regressors and regressands can blind us to the full implications of the data. The traditional textbook methodology must be faulted on this point.
15. For an account of the the rise of statistical thinking in the nineteenth century, see Porter (1986).
16. The first SLLN was proved by Borel in 1909 in the case of a Bernoulli, IID process. For an account, see Spanos (1999, pp. 476–81).
17. Spanos himself refers several times to a 'truly typical realization', for example on p. 9, in connection with developing the frequentist argument.
18. There is another concept of probability, enunciated by Keynes, and related to the Bayes and Laplace ideas. 'Between two sets of propositions, there exists a relation, in virtue

of which, if we know the first, we can attach to the latter some degree of rational belief. This relation is the subject matter of the logic of probability' (Keynes, 1973, vol. VIII, pp. 6–7). Quoted by Carvalho (1992, p. 56), who continues, 'the theory of probability is thus part of epistemology. It does not deal with events or material processes as such but with propositions'. 'Probability is the study of the grounds which lead us to entertain a "rational" preference for one belief over another' (Keynes, ibid., p. 106). Probability, in this sense, is likewise necessarily part of the way we think about socio-economic systems. Each step of the reproduction/exchange/distribution process must be carried out on the basis of knowledge that is incomplete and uncertain. The reason, of course, is that the data at hand are only a sample, so it has to be determined whether or not they constitute a good sample. Decisions, however, have to be taken; consequently, the evidence has to be assembled and weighed. Then the propositions with the highest evidential weights will be preferred as the basis for beliefs and action.

19. Claude Levi Strauss (1958) in anthropology, Sartre (1976) in philosophy, Bourdieu (1984) in sociology, Mintzberg (1973) in management and Nell (1998a) in economics have also remarked on the complexity of the distinction between subjective and objective, which faces any researcher who carries out fieldwork. The fieldwork approach puts the observer at the centre of the conceptualization process; yet given the active mind, conceptualization must have a subjective element (subjective but not arbitrary). Conceptual analysis based on fieldwork will provide the essential assumptions and definitions on which model-building should be based. In order to construct the kind of models that will enable economists to understand the way the system works, we need to start from conceptual truth, fleshed out by understanding from the inside, and then to develop stylized facts by interpreting statistics in the light of fieldwork. Second, fieldwork can give us a picture, for example, of markets in operation, of institutions that organize production and sales, and the way work is structured. Although institutions and practices are intangible, such a picture will be objective. Fieldwork has to understand the society or sector being studied in its own terms, and translate those terms into the observer's language. This is a subjective process, but it aims at an objective outcome.
20. For a comprehensive account of Leamer's econometric thinking, see also Leamer (1983; 1985; 1988; 1994).
21. It should be noted that Malinvaud's (1980) econometric textbook referred once to Laplace and devoted a short historical note to him. He pointed out that 'Laplace showed that every unbiased linear estimator is asymptotically normal when the number of observations tends to infinity, and that the asymptotic variance is minimal for the least squares estimator' (Malinvaud, 1980, ch. 5, p. 194). For further details on Bayesian econometrics, see Zellner (1985).
22. Foley (2005, ch. 18) discusses operational Laplacianism in the multivariate model. He argued that the results found in the case of the Gaussian linear model with one dimensional data could be extended to a derivation of all the statistics commonly used to evaluate multivariate regressions. He shows that 'the classical regression model, including its t-statistics, can be rigorously interpreted on an operational Laplacian basis as adopting a prior that assumes all the information in a sample is contained in its size, mean and covariance matrix and assigns the generalized Jeffreys' prior to possible samples' (Foley, 2005, p. 128). According to Foley, 'the parameters of the Gaussian regression model, the regression coefficients, can be interpreted as measurements derived from a further large sample of data from the same system, and the posterior probabilities over these statistics can serve any purpose that posterior probabilities over the parameters themselves would' (ibid.). Foley observed that, for example, 'the t-statistics on regression coefficients reflect the uncertainty the finite sample leaves as to the sign of the corresponding regression coefficient in a much larger sample' (ibid.).
23. Granger (2004, pp. 97–8) argued that 'the DGM is only an approximation to the generator of the economy which has to describe the process by which each decision is made, and so inherently unobservable. Even if one could observe the consumption of

every good, for example, the amount of information would be overwhelming and so it is usual and sensible to aggregate. The data available to econometricians for analysis, particularly in the area of macroeconomics, has undergone a number of reconfigurations, such as temporal and cross-sectional aggregations, seasonal adjustments, and various filters and redefinitions. It follows that any model built can only hope to be an adequate approximation to the DGM'.

24. An account of stochasticism and convincing tests in economics is available in Boland (2000), where the testability of economic models has been a central issue. While some philosophers have made extensive statements on the question of testability, the basis for making methodological decisions has hardly ever been examined in modern economics. Boland examines the way a practising model builder deals with the question of testability and similar methodological questions in modern economics. However, he also provides a brief theory of stochasticism in modern economics (ch. 8, pp. 51–4) 'mainly to show that stochasticism involves model building since it requires an explicit modelling assumption which is possibly false', and thus concludes that 'stochasticism should not be taken for granted' (ibid.). He argues that 'recourse to stochasticism does not eliminate the logical problems met by models used to test neo-classical economics' (ibid.).
25. Based on Allais's 1988 Nobel Prize lecture published in *American Economic Review* late in 1997. We also draw heavily on Allais (1954; 1977; 1983).

PART III

Structural econometrics in its place: mapping new directions

It's important *to go out and discover the facts for yourself.*

Coase (1937, italics added)

The building of institutional reality into a priori formulations of economic relationships and the refinement of basic data collection have contributed much more to the improvement of empirical econometric results than have more elaborate methods of statistical inference.

Klein (1960, italics added)[1]

Knowledge of the 'institutional realities' is, of course, valuable in all areas. *In a study of cost–output relationships in coal mining this author felt it necessary to don a safety helmet and get to the coal face in the narrow and twisting seams of the Lancashire coal field in order to see at first hand the nature of the production process before sitting down to peruse the statistics at the regional headquarters of the National Coal Board.* Similarly *in studies of scale, costs, and profitability in road passenger transport and of cost–output variations in a multiple-product firm the author spent time at each firm talking to accountants and managers to study their accounting and decision processes before extracting the relevant data by hand from the firm's records.*

Johnston (1963 [1984], p. 500, italics added)

A widespread problem in the economics profession is 'armchair empiricism', the idea that empirical work can be done sitting in a room with a computer, messing around with a data base. *The empirical economist doesn't have to know anything about the world, about the way things are actually done, 'know', that is, in the sense of having direct, intimate acquaintance* [. . .]

Conceptual theorizing must be based on and embody empirical work (here in the sense of fieldwork), which will tell us the identifying characteristics of the objects under study. The common belief that conceptual truths are supposed to make it possible to understand the world by just thinking about it has the true relationship exactly backwards. *On the contrary, to do pure thinking, to theorize about the world, it is also necessary to investigate the world.*

Nell (1998a, pp. 96–7; italics added)

A central question of interest to both scientists and philosophers of science is, *How can we obtain reliable knowledge about the world in the face of error, uncertainty and limited data?*

Mayo and Spanos (2010, p. xiii, italics added)

There is no applied science if there is no science to apply.

Houssay[2]

10. Conceptual analysis, fieldwork and the methodology of model building

INTRODUCTION

At the outset we complained that neoclassical economic theory provided the ontological basis (the rational individual) and the corresponding individualistic methodology of the modern econometrics that has come to replace the Cowles Project. The result is that neoclassically-based econometrics fails to develop any insight into deep structures – it interprets whatever it sees as individuals choosing with some degree of (perhaps bounded) rationality. It simply relates observables to one another, putting choices and actions together into equilibrium patterns.

We have proposed a new vision that puts methodological institutionalism in place of methodological individualism. We argued in Chapter 1 that Hollis and Nell (1975) had already both exposed and explained the methodological deficiencies of modern econometrics before they had become widely realized. Moreover, Hollis and Nell, and later Nell (1998a), suggested a way of fixing the problems. The founders of econometrics, Haavelmo and the Cowles econometricians, held a vision of the real world – expressed in the initial stages of the Cowles Project that laid the foundations for the beginnings of econometrics in the 1940s. This vision provided a perspective that was ontologically incompatible with contemporary econometrics as it developed in the 1970s and 1980s.

We make three observations at the outset.

First, Deirdre McCloskey (1996, p. 17) pungently describes the unfortunate situation facing economics and the methodology of economic model building today. She claims that it is the search for a machinery of perfectibility in what is called modernism that has led to what she calls the three vices: statistical significance ensures real significance; mathematical proofs of existence are scientific; and the two together can be used for social engineering. The first she attributes to Klein, the second to Samuelson, and the third to Tinbergen. Believing in these three has led modern economics to become a boys' sandbox game, where methods are wrong, and produce wrong results, but the game continues to be played because it is competitive. Basically, the approach claims to implement

Francis Bacon's new science in which 'the mind itself be born from the very outset not left to take its own course, but guided at every step, and the business be done as if by machinery' (Bacon, 1620, quoted by McCloskey, 1996, p. 13).

McCloskey (ibid., p. 99) claims that the three vices stretch a long way back into the intellectual history of the West.[3] To paraphrase: Klein's idea is Aristotelian and Baconian, based on the view that nature will speak for itself if you torture it on the rack. Samuelson, on the other hand, is Pythagorian and Cartesian, guided by the notion that sitting and thinking hard in a warm room, you can solve the world's problems. Finally, Tinbergen's idea is Platonic and Comtean, the notion that you can engineer society the way you can engineer a bridge.

In the hands of some of their less sophisticated disciples, their brilliant ideas of the 1940s have ended as boys' games. As McCloskey (1996, p. 18) puts it,

> The boys [today] are the intellectual grandsons or even now great-grandsons of Klein, Samuelson and Tinbergen. By now the sand-castles are very tall, and many careers have been spent building them, though strictly inside the sandbox.

Second, Swann (2008, p. ix) proposes a new direction and a new attitude to applied economics. What he calls vernacular economics, as Blaug puts it, 'is nothing less than an omnibus of fact-gathering techniques that economists have neglected far too long'.[4] Swann's book merits quoting:

> The advance of econometrics from the early days of the Econometric society to the present has been a massive intellectual achievement by some exceptionally clever people. But scientifically speaking, it has been problematic. First, besides the impressive methodological advances, the practical results from the use of econometrics are often disappointing. Second, the mainstream economists' preoccupation with econometrics has displaced other techniques of applied economics. Economics has treated econometrics as a universal solvent, a technique that can be applied to any economic question that is sufficient in itself, this is a serious error.
>
> *So what is the new direction in applied economics referred to in this book's subtitle?* The advance of economic understanding demands that economists learn to respect and assimilate what I shall call *vernacular knowledge of the economy, knowledge of the economy gathered by ordinary people from their everyday interactions with markets*. Such vernacular knowledge may sit uncomfortably with the formal models of economists [. . .] But no wise economist should discard the vernacular, because it offers insights that can never be found in formal analysis alone. (Swann, 2008, p. ix, italics added)

Swann's purpose is to persuade mainstream economists to take the vernacular seriously and to explain what can be learnt from paying careful attention to it. His critique of econometrics is very constructive and is not directed at econometrics as a tool in itself.

Swann is, however, critical of the idea that 'econometrics is the only proper or rigorous applied research tool, a universal solvent which can be applied to any economic problems' (Swann, 2008, pp. xi–xii). He argues that 'the beauty of econometrics is that it holds out the hope of quantifying the relationship between variables without fieldwork, without the need for a detailed exploration of the causal link' (ibid., p. 76). However, practical results from the use of econometrics are often disappointing. Drawing on Mayer (1980) and Summers (1991), Swann (2008, pp. 39–40, italics added) noted that

> Mayer (1980, p. 18) made a similar comment. As the econometric technique had its weaknesses, economists should accept that economic truth is not always written in the form of equations, and we should be open to other methods. *Summers (1991, pp. 129–30) argued that formal econometric work has had rather little influence on thinking about substantive questions.* Instead, the empirical work that had truly been influential was actually based on very different methodological principles – again the 'vernacular' of this book. And *Summers (1991, p. 146) concluded bluntly that it is easier for the researcher to develop technical bravura than it is to 'make a contribution to knowledge'*.[5]

We strongly agree with Swann's vernacular economics and with his contention that econometrics is not – and never can be – a universal solvent for all economic puzzles and problems. Should it be put in its proper place? Yes, certainly. However, we shall argue that, viewed from a different angle, econometrics will be applied only if it is applied properly – namely, if its variables are scientific variables and if the relationships it is examining are scientific relations, as we have defined them in earlier chapters. They must be derived from fieldwork and the vernacular by conceptual analysis.[6]

Third, Nobel Laureate Bernardo Houssay once said, 'there is no applied science if there is no science to apply'. We can adapt that to economics: economics will be used ('be applied') by professionals only if there are theoretical models that can be referred to with real data ('a science to apply'). Economics is useful only if theory is illuminated by real data. The core of traditional economic theory does perhaps contain some results whose character does not depend on the precise magnitudes of different relationships. If economics is to achieve real usefulness as a science, then we have to learn as much as we can about the size of economic parameters (Swann, 2008, ch. 2).We suggested in Chapter 3 that these parameters may not be like the great constants of the physical sciences. The constancy of parameters in economics depends on the stability of the structure of the socio-economic system. In chemistry and physics the objective is to measure parameters and constants, and once that has been done it is done for good. In economics, these parameters and constants may change over time (see Nell, 1998a). Keynes believed that any parameters that might be measured in a particular study at a particular time would not apply to the economy in the future (see Keynes in Moggridge, 1973, pp. 296–9). In effect, Keynes is saying that even if econometrics can turn nonexperimental data into parameter estimates, they cannot be 'real' parameters – a very simple but very important point (Swann, 2008, ch. 2). If there are no standard parameters in economics, they are at best local approximations applying only to a given time and place (Nell, 1998a).

Many modern econometricians share this view and would be ready to accept that the parameters of an economic model are not great constants,[7] but are instead useful simplifications in trying to make sense of the world (Swann, 2008, ch. 2). As a result, the inappropriate pursuit of quantification in economic models would reduce, rather than increase, their value in economic analysis.

In what sense, then, is economics an applied science? To put it in another way, can it be applied in the same way as the natural sciences? Consider an example from the history of economic thought. Smith and Marx were theorists and empirical economists at the same time. There was no division of labour between theory and empiricism in their time, or, if there was, they at least straddled both fields. But their empirical analysis could be described as informal. It had more substance than the empirical bases of many modern-day theorists, but did not claim any comparability with empiricism in the natural sciences. The main advances in the economics of the nineteenth century were contributions to theory (Swann, 2008, ch. 2).

Looking a little closer at what Houssay said, we see that it actually suggests the idea of a virtuous circle. In fact, we can identify two such circles. The first is between pure economics and applied economics. Pure economics informs applied research and data collection, and the fruits of this applied research feed back into the construction of better pure economics. If all data used in applied economics were collected by the applied economists themselves, this would be a complete virtuous circle. Then there is a second virtuous circle. This is from applied economics to fieldwork. Here, conceptual analysis based on fieldwork will provide the essential assumptions and definitions on which econometric model building should be based. Fieldwork will be of greater use to us and, combined with conceptual analysis, produce better applied economic models, which in turn will reward us with more useful data.

It seems that in neoclassical economics neither part of this virtuous circle is in very good repair. The link from economic theory (pure economics) to applied economics is quite weak, and the reverse link from the results of applied economics back to economic theory is weaker still.[8] We argued in the Introduction to this book that the original vision of the founders of the econometric society was of econometrics as a union of economic theory, mathematics and statistics, and that this econometric method would be applied to real data to estimate real economic relationships. We showed in Chapter 6 that this vision is reflected in the MTC diagram. Theory, application and measurement must all interact; from fieldwork and the vernacular we derive concepts, which are developed into theory by conceptual analysis, and then applied and measured, providing data and statistics, which can be used to generate new ideas and new questions. As we noted in Chapter 6, the model provides an interactive approach. The pioneering econometricians like Frisch, Tinbergen, Haavelmo, Leontief, Klein, Stone and others were equal to this challenge, but as early as the 1950s, mathematical economic theory was splitting apart from what we now call econometrics, breaking up the interaction.

The gap between mainstream economic theory and empirical economics has since become quite wide. Theory was only of limited use to the applied econometrician, because theoretical restrictions were not very relevant to the sorts of practical data that were actually available. Econometrics could only rarely provide convincing tests of hypotheses from economic theory, so theorists started to lose interest in applied econometric results. Theory did not meet the standards required by the applied econometricians, and econometric tests did not meet the standards required by theorists. The division of labour into theory and econometrics without common standards led to a breakdown in the virtuous circle. Turning to the second part of the virtuous circle, the pioneering econometricians could keep the circle in good repair because they took such care to understand the data they worked with and where they came from.[9] Later applied econometricians have tended to be more specialized, and have concentrated their energies on refining their techniques, and spend less of their energy in dialogue with the real world and other disciplines. This specialization without common standards has damaged the virtuous circle (Swann, 2008, ch. 2).

Economics is not yet and may never be applied in the same way as the physical sciences, because it does not have the same standard parameters (see comment by Allais, December 1993[10]). We shall argue in this chapter that this fact has very important implications for the rethinking of applied economics and the methodology of model building. This chapter draws heavily on Nell (1998a).[11]

10.1 CONCEPTUAL TRUTHS AND EMPIRICAL OBSERVATIONS

Let's recall here that the methodology of scientific economics adopted the traditional empiricist's view of the mind as the passive recipient of sense impressions, organized by definitions and analytic truths. Sense data provided the basis of our understanding of the external world, the building blocks out of which the edifice of knowledge was constructed. These were classified and manipulated by means of analytic truths, such as those of mathematics, forming the building blocks into patterns and structures which pictured the world – that is, were isomorphic to it.

Sense data were passively recorded; the structures were built to conform to external reality – the structure of knowledge, even the logical structure of propositions, mimicked the structure of the world. Knowledge was recorded, it was not created. We argued in Chapter 5 that in the picture of the economy sketched by neoclassical theory, the minds of economic agents play no role. We also argued in Chapter 1 that the formulae follow from the axioms of rationality – the axioms, in turn, are taken as given. This vision of the passive mind, however, is no longer acceptable philosophically. The underlying theory of perception has been shown to be inadequate. In economics, in particular, truths of reason provide us with a map of the relationships between agents and the material world – in economic terms, between rational choice and production.

The argument throughout this section is twofold. First, conceptual truths provide a basic framework for understanding the structure of human social systems. Such a structure, in turn, provides the setting in which behaviour takes place, a setting that limits and conditions behaviour. Finally, rationality guides behaviour, but rationality works through, and must be understood in terms of, conceptual truths. In economics such truths provide a framework, a set of guidelines, telling us how to construct theory and to build models to picture the world adequately. Second, conceptual analysis based on fieldwork will provide the essential assumptions and definitions on which model-building should be based. In order to construct the kinds of models that will enable economists to understand the way the system works, we need to start from conceptual truths, fleshed out by understanding from the inside, and then to develop stylized facts by interpreting statistics in the light of the fieldwork.

Many contemporary economists appear to have drunk deeply from a concoction best described as a pragmatist approach to methodology, although in its rhetorical form it borders on the postmodern. It seems to have been considered satisfying because it apparently supports and explains conventional practices, and helps to defend at least some aspects of neoclassical theory against competing theories and critics. But the recipe for the pragmatist brew retains important residues from earlier empiricist distillations, and may not be as digestible as it seems at first. A more satisfactory philosophical blend, while mixing in aspects of pragmatism, raises important issues for theories of market behaviour.

The most convenient potion mixes Popperian falsification with Lakatos's sociological account of knowledge (see Lakatos, 1970; 1978). This brew leads to a defence of conventional theory on the grounds that it works; that is, it guides policy, and seems to be empirically satisfactory in a broad way, so that the hard core deserves to be protected. On the other hand, competing theories are required to meet the falsification test, since, not being established, they cannot claim exemption for their hard cores. This they tend to fail. (The approach, however, leaves general equilibrium theory unprotected – it has no empirical content and generates no falsifiable propositions.)

A variant of this approach, likewise pragmatist but operating with a weaker criterion of falsification and rejecting Lakatos's sociology, holds, along with John Stuart Mill, that economics is a separate and inexact science (Hausman, 1992). It is separate because it can identify and study the chief causes of the principal phenomena that interest it (broadly, wealth), but it is inexact because its generalizations and laws are subject to a long list of ceteris paribus clauses. Yet it is a science, because the (as yet poorly understood) variables alluded to in these clauses are, in principle, identifiable, reliable and capable of refinement. Nevertheless, this approach ultimately rests on the hypothetic–deductive model of explanation. It requires a criterion for accepting/rejecting generalizations, yet it adopts a pragmatist stance toward the problem of induction (namely, that the problem can't be serious because science works).

But, as philosophy, the methodology of Popper–Lakatos and related approaches can be shown to be flawed; it both draws on and at a crucial point denies the concept of the active mind. The rules and maxims of the active mind must be self-justifying; but the Popper–Lakatos approach cannot justify itself, since it rejects the only kind of conceptual analysis that could provide a justification. Yet conceptual analysis derived from the active mind is exactly what is needed in developing economic theory.

In particular, such analysis allows us to understand the relationships, between agents, institutions and the material world in an economy system, providing an account of structure. Structure, in turn, is the setting for behaviour; behaviour has to be seen in a context that defines not only opportunities and limitations, but also commitments and expectations. With these in place, the role of rationality for the individual agent can be addressed. One aspect is instrumental: the rational agent seeks to choose the most advantageous option among those available. But another is procedural: the rational agent carries out his or her commitments in the most appropriate way. And finally, rationality can be both critical and imaginative with respect to ends and objectives.

10.1.1 Conceptual Analysis and Method

Conceptual analysis can provide guidance in adapting these general points to particular cases through empirical fieldwork (not library studies). As a consequence, conceptual analysis of fieldwork can then put together the real patterns of behaviour and motivation, in the context of the available and actually operating technology, ways of working, making and doing things. Such conceptual analysis may be concerned with deconstruction, a literary analysis taking apart the reported picture, discovering concealed meanings and hidden agendas, both on the part of observers and the observed. As argued by Coase (1937), 'it's important to go out and discover the facts for yourself'. Coase developed his ideas about the nature of the firm during a year of visits to firms throughout the USA. The resulting view of the economy gives rise to an account of value, competition and markets that differs from the mainstream. Moreover, it supports the view that history cannot be properly studied by equilibrium methods, and that economic analysis is likely to be different in different historical eras.

Truths of reason – a priori truths – which result from reflection on the processes of understanding, can tell us about the world. In Kantian terms this would be to claim that a priori truths can be synthetic. But too much should not be made of this traditional formulation. The argument here is not about the history of philosophy, nor is it about particular puzzles in reasoning about reasoning. It is, rather, that a stronger conception of the role of reason may help both to clarify philosophical issues in economics, and to underpin a better methodology.

This is a strong claim that many might view with suspicion. But it is not so implausible, as a sneak preview of our argument – in nutshell form – will show. Consider the opposite statement: 'truths of reason can tell us nothing about the world'. This is certainly not an empirical generalization; if true, it must be a truth of reason. But it is an informative one. It tells us that a long tradition in philosophy and certain contemporary research programmes in economics (for example, the Austrians following von Mises, 1957) are quite wrong. If true, therefore, it tells us something about the world; so it is false. Since the negation is false, the original statement must be true.

Truths of reason do not tell us about the world in the same way that empirical propositions do, but they are both informative and indispensable.

They are not independent units whose truth or falsity is determined separately from other propositions. Truths of reason are embedded in our conceptual framework; they are part of our theory of the world. They tell us how to look at the world, and what to look for, by telling us in general terms what must be there, and what cannot be there. They are guidelines, or maps; not detailed maps, but outlines. They give us the framework of theory by laying out the meanings of the basic terms. Fundamental philosophical propositions do tell us something – to take a negative example, the positivist claim that 'all general propositions are either analytic and a priori or synthetic and empirical, or meaningless' is neither analytic nor empirical, nor meaningless. If it were valid it would have to be a conceptual truth, able to tell us something about the nature of thought. It is not valid, however, and so the statement that it is not is an informative conceptual truth.

The same point can be made with regard to the problem of induction and to the problems besetting the principle of falsification (PF).[12] In one sense, the pragmatist is correct: scientists have nothing to fear from the problem of induction. But that does not justify a casual dismissal. It follows logically from the premises of empiricism, so the fact that it is not a practical problem shows that empiricism is flawed – and that is a conceptual truth. In the same way, the reason underlying the failure to find a justification for PF is that such a justification would have to be a conceptual truth in the strong sense that empiricist philosophy wishes to reject. But if there is what amounts to a priori knowledge in this sense, then PF is not, as it stands, valid. For in this case there must be some knowledge that is scientific and tells us about the world, but is not falsifiable by empirical tests; so PF, alone, does not do its assigned job. The realm of such scientifically valid knowledge may well be marked out by a set of conditions that include among them a version of PF, but this set of conditions will itself be developed by philosophical reasoning, and so will be a priori, even though it will have to take account of the actual practices of the various sciences.

10.1.2 The Pragmatic's Reply

Empiricists and pragmatists will reply with Quine that 'no statement is immune to revision': any truth can be revised, including the laws of physics, even the laws of logic, if we are prepared to make enough changes in our system of thought (Quine, 1965; Nell, 1976a). The image is that of a web or network, that covers experience, rather than corresponding to it. The problem of induction vanishes, because general statements are not verified or falsified – we decide, on the balance of the evidence and in the light of our interests, when to accept them and what status to give them.

When a problem arises – for example, when some statement appears to be falsified – it is 'up to us', in the light of our interests, to decide what changes to make. We can continue to hold the statement in question to be true, for example, and make other adjustments in the system. We are not compelled to reject the apparently falsified statement. And just as we are not compelled to reject statements falsified by events, we are not compelled to accept statements verified by them, either. Not even statements verified analytically have to be accepted. Of course, to reject an analytic truth would require extensive conceptual revision, but, according to the pragmatist perspective, such a revision is, in principle, possible. In short, there is no conceptual truth.

If true, this would of course have to be a conceptual truth. It clearly is not an empirical claim; how could it be confirmed empirically? It would be necessary to canvass all truths, to see if any were conceptual. But the set of all truths would have to contain the proposition, 'this is the set of all the truths there are', the truth of which cannot be known until all members of the set have been determined to be true. Since it is itself a potential member of the set, its truth must forever be undetermined, and the set of all truths can never be completed. The proposition 'There are no conceptual truths' could be falsified, of course, by finding a conceptual truth, but that is precisely the point at issue. There are plenty of candidates; the claim is that what appear to be conceptual truths are actually not.

What is inescapable, however, is that if the argument succeeds, it generates a conceptual truth of precisely the kind it denies. (For a similar problem in the arguments of Quine and Morton White, see Nell, 1976a.)

One aspect of this view is worth special attention here. Although there are no conceptual or a priori truths, on this view, there are certainly degrees of difference. There are statements that are primarily to be examined conceptually, and others that are obviously primarily empirical. But no statement is purely one or the other. All statements, all arguments, are at least a little of both. That is what it means to say that our statements are all part of the network of knowledge, and all face the 'tribunal of experience' together. Thus the pragmatist has no problem with the claim that seemingly conceptual truths tell us about the world; that will be granted. The problem comes in holding them to be necessary.

Yet even here the pragmatist is obliged to agree that to reject or revise a conceptual truth – humans are (potentially) rational animals, humans have free will – or a law of physics – action implies reaction – will be more difficult and call for more extensive reworking of the rest of the system than maintaining, say, 'all swans are white', in the face of the discovery of black swans. Black swans will have to be considered a different subspecies; this will require revising the criteria for belonging to a species. Besides the ability to interbreed, the criteria will now have to include colour. It might take some work to make sense of this; perhaps the easiest course would be to reserve the word swan for the special case of white members of the species. Making revisions in our systems of thought is not easy; we seldom see such projects outside the development of science.

To revise 'Humans are (potentially) rational animals', however, would be an undertaking of a wholly different order of magnitude. 'Potentially' is added here to Aristotle's proposition – and it will be argued that the need to add this qualification is itself a conceptual truth. One cannot be rational without learning language, problem-solving and social skills; the potential is ingrained. But it will not become actual except as a consequence of social processes.

Suppose we proposed to include Anne Rice's vampires as humans, on the grounds that they were derived from humans – as some of her characters have suggested. We would have to revise our biology: now not all humans can interbreed. Not all humans are mortal; not all humans need water and vegetables. Sunlight and garlic are harmful to some humans. Some humans have telepathic and psycho-kinetic powers. This begins to call for revision of physics as it applies to humans.

Some humans can fly, unaided. This raises questions of politics: should vampires be allowed to vote, to have citizenship? The ethical question is central – are some humans intrinsically evil or amoral? And how does amoral relate to evil? As for rationality, it is evident that vampires are partly rational – we can talk to them – but their behaviour makes it clear that their rationality extends only to choice of means. Their ends are not rational, even irrational; and while they acknowledge, they do not accept, the force of reason.

Still, they are mythological, fictitious. Take real cases where rationality is limited or unattainable. Do we accept those with Down's syndrome, and other mental shortfalls? It is ambiguous; we do not treat them as legal persons. They cannot vote or manage property. We can love them, even interbreed with them, but they cannot participate fully in human life. We accept them, but only so far. And we draw the line at those who are brain-dead; they are only vegetables. Nor will we accept talking chimps; not at all.

As even these simple and partly whimsical examples show, the criteria for being human are various and complex – an indefinite list of definite descriptions, to use John Searle's terms. But rationality and animal nature are central; when violated even in part, the ascription of humanity is put in question, or reduced in degree. To change this – to ascribe humanity, for example, in the absence of rationality – would require changing our conceptual framework in many different areas. Exactly what aspects of rationality are excised? What will be the implications for human speech, for government, for law, property and contract, for family life? And each of these changes might, in turn, plausibly require further changes in other areas. The pragmatist assumes that these changes can be successfully made, and that the further revisions called for can also be successfully made – and that any still further revisions will be increasingly minor and eventually peter out. In other words, the chain of revisions will be finite; it will come to an end within a reasonable period.

(As a thought experiment, imagine modifying our conception of humanity to include a group, in all other respects the same as us, but whose behaviour is purely and wholly instinctual. They never reason, never weigh alternatives, never argue, think or plan. They act only on instinct. Now – including them – describe what human beings are, what human institutions are, family life, the basis of ethics and morals, the chief determining factors in human history, the relations between the sexes, and so on. In each area the revisions will surely set up consequences for related areas.)

It does not seem plausible in the case of a major revision that the chain of required changes will be finite; certainly pragmatists have not argued the case in detail. What if, instead of coming to a stop, the chain of revisions continued endlessly? At each step, the proposed change could be saved by making revisions – that is the pragmatist's claim – but each such set of revisions would call for further revisions, in other fields. These in turn could be made, but then there would be still further revisions in fields even more distant, without end. Consider also: perhaps eventually the chain will require further revisions to statements already revised once. So there are two problems: first, the chain might go on forever; and second, it might double back on itself, requiring further revisions to those already made. Either of these possibilities would suggest that the original statement was not revisable in the way attempted.

However, the fact that pragmatism allows that different kinds of revisions differ in degrees of difficulty does make possible a sort of compromise. To put our project in terms acceptable to pragmatism: a conceptual truth could be understood as one that could not be revised without upsetting a vast range of subjects, generating an infinitely long, and possibly backward-folding, chain of revisions. Such truths will apply to the world, and are the ones that it will be most difficult and complicated to revise.

Instead of necessary and contingent statements, we would have a gradation by degrees of difficulty in revising. The most difficult, requiring the longest chains of revisions, would correspond to necessary truths; the easiest, to contingent. In between, however, there would be many grades, from those whose revision, while not utterly unthinkable, presents daunting complexities, through those whose seemingly easy revision turns out to involve unexpected awkwardness, to those that are in fact simple to revise.

Such a gradation may in fact be rather plausible; the blunt distinction between necessary and contingent may well be an oversimplification. Some statements and concepts do seem more deeply embedded in our framework of thought than others. This approach captures that. It also allows for an interpretation of conceptual truths as the most deeply embedded, ones that could not be revised without an endless chain of further revisions.

10.1.3 Conceptual Truths in Economics

In the same way, the boundaries of economics and the conditions for meaningful and, separately, for valid economic statements will be marked out by philosophical reasoning. Theory is not merely a process of unpacking the implications of arbitrary or useful assumptions. Even theoretical propositions in economics, as close cousins to truths of reason, can be derived from non-arbitrary assumptions, in a process that is not unlike philosophical investigation.

Such basic philosophical statements will be about economics, and how it applies to the world; hence, indirectly, they will be truths about the world, even though a priori. However, a priori in relation to economics should not be understood in the same way as to philosophy. In the latter case it is fully general; an a priori proposition must, in some sense, be self-evident, or rest on presuppositions that cannot be denied, and therefore must be presumed. Rationality in economic behaviour is an obvious example. We have argued in Chapter 1 that Hollis and Nell (1975) offer a summary account of such propositions (see also Nell, 1976b; Hollis, 1995; Nell, 1998a).

Economics is about agents choosing to take jobs, to invest, to produce certain lines of goods or services, or purchase various products. Choices must be made by weighing the costs of alternative strategies in the light of the ends to be achieved; this is the traditional province of rationality in economics. The format of rational choice expresses a conceptual truth that the optimizing procedure determines to be the best choice. But while this is the most readily apparent case of conceptual truths, certain others will prove more central to our argument. To understand them, we must remember: economics applies to the activities of human agents in the world; it therefore presupposes the general principles of the physics of material objects and the biology of the human race.

So there will be other important basic truths. Human reproduction requires two partners; so if Robinson Crusoe is to provide a paradigm for economic analysis he will need Ms Friday. The isolated individual cannot be the basis for economic analysis. More generally, human life is built around social relations; and economics, in particular, depends on property, for exchange is the exchange of ownership rights. Rights, in turn, depend on contracts; that is, promises that give rise to obligations. But that depends on authority: contracts must be enforced, which requires judgements and settlements of disputes. Hence there must be rules and institutions. Agents in turn interact with the world; they are able to affect and alter material objects in various ways – which is production. How they do this is the province of technology. Thus the economic activity of agents rests on two foundations: their relations with each other – institutions and their relations with the material world – and technology.

These activities take place regularly, organized through institutions. The forms according to which agents behave depend on the institutions that define everyday roles: family, household, job. These provide grounds for agents' expectations about each other, in terms of their respective roles, as determined by birth, education or appointment. Understanding, discriminating, forming expectations and making informed choices all require skills and training, which are passed along from one generation to another through institutions. Rationality itself is learned; only the potential for it is inbred. Such activities also require current material support, which comes from interaction with others. Institutions develop and channel human skills, but the limits to what people can do, and what they need as support, ultimately rest on human biology.

The activities of investing and producing require engineering skills, based on technology, which sums up the various ways agents can affect the material world and bend it to their purposes. But in doing so, agents, being themselves material objects, must also alter themselves and their relationships. Technology is not simply a set of ways agents can affect the world; to affect the world, agents must also alter themselves and their institutions.

Technology ultimately rests on principles elaborated by physics and chemistry, just as institutions reflect the limits and possibilities implied by biology. 'Ultimately' is important here; neither physics nor biology determines any economic relationships. But the portrayal of economic relationships must be *consistent* with the basic principles of each. For example, it may be convenient and, for some purposes, reasonable, to ignore intermediate products and the depreciation of capital goods. Many models do, including famous and influential ones like Hicks's *Value and Capital* (1939). For example, intermediate products are treated, following Pigou, as a lake, fed by current factor services and drained by current output (Hicks, 1939, p. 118), while depreciation is mentioned briefly in the discussion of income (ibid., pp. 187, 196). The need for replacement and its implications – that replacement depends on production and implies limitations on the pattern of exchange – are not explored at all (Nell, 1998a, p. 93, footnote 16).

But no general principles can be derived from such special models. Action implies reaction; any materials, anything manufactured, will eventually be used up or worn out, and have to be replaced. Energy is used up, materials wear out, no supplies last forever; the second law of thermodynamics ensures that energy will always be costly (Georgescu-Roegen, 1971). Similarly, for some limited purposes, agents may be assumed to be indefinitely long-lived. But nothing essential can rest on this assumption; human agents are at least as mortal as they are rational, and in practice more certainly so (Nell, 1998a, pp. 93–4).

General equilibrium theory provides another important example. Equilibrium is defined without reference to the level of real consumption necessary to provide agents with the support and training to enable them to carry on and reproduce. Hence an equilibrium position may be one in which some or all agents could not survive and rear children (Rizvi, 1991). For some limited purposes, again, this may be a convenient simplification. But nothing general can be concluded. Moreover, adding an account of the consumption necessary for agents to function creates problems for the theory: no equilibria may exist, and if any do they may not be stable. If a model cannot account for the training, support and replacement of its agents, the approach is surely flawed (Nell, 1998a, p. 94).

Conceptual truths in economics, then, trace the general forms of the relationships holding between economic agents, on the one hand – that is, economic institutions such as firms and households – and between agents and the material world, on the other hand –that is, technology, bearing in mind that agents themselves are part of the material world.

10.1.4 Interpreting Conceptual Truth

To claim that there can be a priori knowledge of the world does not imply that we can sit in our armchairs and figure out the ways African markets differ from those in Latin America. Such specific matters are never a priori. Truths of reason provide direction to research; they tell us where to look and what kinds of things to look for. They tell us about the shape of the world; they don't give us facts – they outline the possibilities and the limits.

To understand this better, consider the opposite position. Conceptual truths, according to empiricism, are conceived to be analytic – that is, trivial. They cannot provide information about the world and are little more than a system of classification, resulting from the manipulation of stipulated definitions. However, on this view, definitions, in turn, must themselves be arbitrary, since if they were not, there would be true or necessary definitions, which would not be empirical, but would not be analytic either (Johnson, 1933). (Think of Socrates' arguments over the correct definition of justice in *The Republic*). Thus it can be argued that analytic truths turn out to be the consequences of decisions, which are ultimately undetermined stipulations. In the end, there is no a priori knowledge, since conceptual analysis is simply drawing out the consequences of arbitrary classifications. However, the problem of induction stands in the way of empirical general knowledge. Since analytical knowledge and empirical knowledge are the only two possibilities – if the first is trivial and the second unattainable – empiricism is in danger of collapsing into universal scepticism.

By contrast, an approach built around conceptual truths provides a role for reason in the formation of theories. Most textbooks reject the idea that there might be truths of reason and embrace empiricism or pragmatism. The reason may lie in a belief that anything known a priori must be fixed and immutable. Moreover, because such truths are necessary and we know them, we impose them on others. Since we have found the truth, we cannot in good conscience permit others to remain in the dark shadows of error and ignorance. It is our duty to enlighten them, with bullets if need be. This is wholly absurd. Conceptual truths, like any others, can be understood and stated fully or partially; they can be known in depth or only approximately. They can also be mis-stated, or understood incorrectly. They can be developed, as the implications and connections between their terms are drawn out. Particular versions of a conceptual truth may be approximations that can be improved by further analysis, just as particular theorems in mathematics can be deepened and improved. A simple example: $2 + 2 = 4$. This can be understood at very different levels, depending on the conceptualization of number; moreover, the proposition has developed significantly in the last half century, as number theory has developed. The proposition is true; but the meaning of the numbers has changed! In the same way, Aristotle provided philosophical arguments for 'man is a rational animal'. The truth of this has not changed, but our understanding of both rationality (for example, Kant, game theory) and of what it is to be an animal (for example, evolution, genetics) has deepened greatly. A priori knowledge of the world requires examining the world, too. Just because knowledge is a priori, does not mean that anyone has privileged access to it, or that the conclusions cannot be criticized, disputed, or revised.

10.1.5 Conceptual Truths and Armchair Empiricism

Reason is not confined to the manipulation of arbitrary definitions; it can be concerned with establishing the correct definitions, definitions that capture the essential characteristics of the objects under study. 'Essential', in turn, is not a matter of mysterious essences; essential characteristics can be understood in two ways: on the one hand, as related to the ability of something to persist, to stay in existence, to remain 'the same' – that is, itself, during processes of change. The essential characteristics of something, in this view, are those characteristics that must be implied when we make reference to it. These characteristics are implied, because if they were not present, the thing would not exist, or would not be able to maintain itself in existence.[13] On the other hand, essential characteristics can be understood as those necessary for a thing to be re-identified by an observer as the same thing at a different time or place. Again, these are characteristics that must be implied when we make reference to the object (Wiggins, 1980; Nell, 1998a, p. 95 and ch. 3).

These two perspectives do not differ, for our purposes, where material objects are concerned; but when the objects of study are themselves agents, then the distinction matters, for the essential characteristics are the agent's identity.

It's easy to see the significance of this. As mentioned in the quote from Nell (1998a) on page 352, a widespread problem in the economics profession is armchair empiricism, the idea that empirical work can be done sitting in a room with a computer, messing around with a database; the empirical economist doesn't have to know anything about the world, about the way things are actually done – 'know', that is, in the sense of having direct, intimate acquaintance. Labour market economists don't have to experience job line-ups, get laid off, do temporary work, or work on shop floors – or even interview those who do. Monetary economists don't have to process mortgages. Or car loans. Or handle portfolios, or manage banks. Price theorists don't have to work in sales, or do the shopping. Anyone can become an authority; no experience is necessary. But if theory is based on real definitions – that is, on essential characteristics expressed in conceptual truths about the world – then to develop and understand the foundations of theory, the theorist would have to know the world in precisely that intimate direct way. Conceptual theorizing must be based on and embody empirical work, which will tell us the identifying characteristics of the objects under study. The common belief that conceptual truths are supposed to make it possible to understand the world by just thinking about it has the true relationship exactly backwards. On the contrary, to do pure thinking, to theorize about the world, it is also necessary to investigate the world.

By contrast, pragmatism leads to armchair empirical work. There is no need to distinguish the essential characteristics of an institution from its accidental properties, because there are no essential characteristics. No such distinction can be drawn. There is no need to investigate the inner workings of a system, because inner and outer are just a matter of the observer's position, an accident of perspective. You don't have to try to understand what really happened – in Dallas, or in 1929 – because nothing really happened; it's all a matter of what explanation works best, for us, now, in the light of our present needs. And we can revise the story later. What the numbers are numbers of is simply a matter of what we choose them to be of. It's Humpty Dumpty's theory of meaning.

10.1.6 Back to Method in Economics

Humans are (potentially) rational animals, animals are mortal, humans have free will, action implies reaction. These are all rather traditional a priori propositions. The first and third are true because they are undeniable, which is to say that an attempt to deny them would end up instantiating either them or propositions which imply them. (To argue against the first would be to exhibit rationality; if the third were false, the argument would not matter, since what everyone believes would be determined.) The second and fourth are examples of natural necessity, and follow from fundamental features of the natural world. If they were contravened, our ordinary notions of material objects and biological life would require extensive, arguably endless, revision.

It may be admitted that in some broad sense they are true, yet denied that they can be of any use. All are so general, so independent of context, that they describe nothing. But that is the point; they are not scientific laws, or generalizations. They are guides to thinking tools for developing theory.

All four are stated here very loosely. When amplified by related conceptions, they are capable of providing a basic framework for economic theory. That is, they provide guidance, a way of formulating the subject. Consider each, noting the implications for economics.

The minimal sense of rationality is instrumental, finding the best means to given ends. This provides a basis for economic calculation, which is certainly prescriptive, and can under appropriate conditions be used descriptively. But the concept will be broader than that – the notion of rationality that is undeniable implies much more. Much more: an extensive and Kantian line of argument holds that rationality implies the obligation to tell the truth, clearly fundamental to contract, and therefore to exchange, and also to the formation of expectations in economics (see Hollis, 1995; Nell and Deleplace, 1995; O'Neill, 1994.) At the very least it implies an ability to reflect on, weigh, refine, judge, and choose among ends, which, in turn, implies the ability to develop criteria by means of which to discriminate among ends. In other words, rationality is more than the mechanical application of criteria and algorithms to questions of choice: it is active. The rational mind defines problems and creates the tools for solving them.

In conjunction with our animal nature, this has an important implication. Rational agents are mortal; they are born and will die and have to be replaced. When born, however, they are unable to function rationally. They not only need support; they must learn to think and to speak. Babies are not born with language. Nor can they acquire language on their own. There is no such thing as a (fully) private language. But without language there is no rationality.[14] The need to learn to think and to behave is a consequence of being rational, in the fully-fledged sense that rationality will guide action. Consider: to the extent that actions are governed by instinct, a newborn animal does not have to learn, or needs to learn less. But if actions are instinctual, they are not governed by reason. Hence, if rationality is to govern action, action cannot be governed by instinct, and will therefore have to be learned. Thus human rationality presupposes (minimal) social relationships.

To this mix, now add free will. This implies that predicting actions, even rational ones, cannot be the sole basis on which theories stand or fall. Active choice, reflection, and innovation are always possible. Situations can be reinterpreted; new forms of behaviour can be invented. Any motivation to choose the best among the givens is also an invitation to find or invent something still better.

Since human agents are animals – in fact, mammals – material support will be needed in order for their rationality to function. Being an animal implies the need for subsistence; the agents of an economic system must be fed and otherwise supported. Material support, in turn, must be provided by processes governed by natural necessity – the elementary laws of physics. That 'action implies reaction' implies that when anything is made by material effort or processes, other things will be used up. Cutting dulls the knife; sawing wears down the sawteeth; tools wear out and must be replaced. To be sustained over time, a production process requires replacements, which in general will have to be produced by other processes (Hollis and Nell, 1975).[15]

What does this tell us about the correct method for economics? First, it must begin with conceptual analysis. And the starting point must be to establish the form of an economic system, namely the relations between agents, and between agents and the material world: institutions and technology and their interaction. Then we must ask how a system can continue to exist: that is, how it can support itself. We cannot refer to an economic system in a general manner unless its existence is stable and continuous. That is, it must be able to support itself, and continue to function. This will provide the concept of the system – that is, that which will remain the same while its properties and characteristics change. To put it in terms of Popper's situational logic: before the logic of what to do can be studied, we have to understand what the situation is – that is, what its continued existence depends on – and what can and cannot change without it becoming a different situation.

Such an approach implies, for example, a critique of the idea of an isolated human agent. Such an agent – Robinson Crusoe – could not reproduce himself.[16] Second, it implies material activity, again an a priori truth – a priori at least with reference to economics; a social system uses up energy and material goods, so it must replace or reproduce them in order to continue. Any natural endowments will be used up, and will have to be replaced. It therefore presupposes the general principles of physics and chemistry as these apply to the ordinary practices of life according to which we make and use material objects, tools, furniture, machinery, and so on.

A social system is based on human agents; these are born, grow, mature, learn the skills of living and so on, and finally die. Human agents are not born like Venus, fully developed; there are no independent, mature babies.[17] Economics presupposes the general principles of biology as they apply to the human race – and also in regard to farm animals, crops, and so on. For a social order to continue to function, it must replace those who have aged, or who are no longer able to carry out their duties. As new people are born, they must be cared for, socialized and prepared for their roles in later life. To feed the active members of the system as well as the children who will replace them, the society must manage crops, domesticated animals and so on. A social system is based on specialization of function and division of labour; hence production implies exchange (Hollis and Nell, 1975, ch. 9).

Notoriously, economic agents are assumed to function rationally: they adopt means to achieve ends; they form expectations of the future on the basis of present evidence; they order their preferences consistently; and in general, they try to make the best of their circumstances. Moreover, in the struggle for existence, those who fail to make the best of things tend to lose out to those who succeed. Different theoretical approaches will present these points differently, but some form of active rational behaviour appears in every account. But pragmatism and, even more strongly, the philosophical position suggested here have implications for understanding rationality and its role in economic behaviour. We have seen that concepts and categories are theory-laden, meaning that events and perceptions are not passively recorded, but must be actively interpreted. We must decide how to follow the directions and how to treat the results, what to accept, revise or reject; nothing is finally settled; new results may upset old truths at any time – but exactly what is upset will depend on our interpretation, guided by conceptual truths, which we also have to interpret and apply. Rationality, therefore, must be open-ended, able to learn and to innovate; it can never be mechanical, the calculated response to a foreseen stimulus. Purely instrumental rationality – as in neoclassical economics – sits uneasily in the company of the active mind. This will prove important. The purpose of conceptual analysis here is to spell out the priorities and map the logical geography of the relationships. The object is to identify the forms that any human social system must display, and to classify, provisionally, the different types of system.[18] Defining the form means showing how the system can support and maintain itself, how goods are produced and distributed, how roles and duties are assigned and authority is determined, and what is likely to happen if these relationships break down. It establishes the nature of the rational mind and outlines the place of rationality in economic activity.

10.2 FIELDWORK

Fieldwork is scholarly work that requires first-hand observation, recording or documenting what one sees and hears in a particular setting. It has long been regarded as the mainstay in anthropological research, and we shall present the essential ideas by distilling here some key insights from anthropology and management. The main thesis of this section is that to understand and sometimes even to discover the truths of reason, it is necessary to investigate the world, and especially, perhaps, to investigate investigating.

The purpose here is to move from the very general level to the study of a particular society and economy. This jump cannot be made by collecting some statistics and trying to fill in the general categories developed by conceptual analysis. First the general categories have to be adapted to the particular case; but that has been done by the people of the particular society themselves! We, the observers, have to discover how this adaptation has taken place, in the history and development of the society. This requires what anthropologists call fieldwork.

10.2.1 Defining Fieldwork

The first generation of anthropologists, studying mostly people under colonial rule, had tended to rely on locally based missionaries and colonial administrators to collect ethnographic information, often guided by questionnaires that were issued by theorists from 'back home'. In the late nineteenth century, important ethnographic expeditions were organized, often by museums; and as reports came in, academics would set out the findings in comparative frameworks to illustrate the course of evolutionary development or to trace local historical relationships. Contemporary ethnography is based almost entirely on fieldwork and requires the complete immersion of the anthropologist in the culture and everyday life of the people who are the subject of study (a relevant contemporary example is Ho, 2009).[19]

In anthropology, Malinowski (1922) is credited as being the most important figure in the development of the modern fieldwork tradition, through his study of the Trobriand Islanders of New Guinea. Equally important contributions were made, however, by Radcliffe-Brown, Evans-Pritchard, Morgan, Taylor, Benedict and others to this tradition of anthropology. Jarvie (1967) claimed that all schools of anthropology emphasize that fieldwork stands at the centre of the subject. Malinowski and Radcliffe-Brown, who thought anthropology was a science, placed the same emphasis on fieldwork as does Evans-Pritchard, who denies that it is a science.

More recently, Rice et al. (2004, p. 1) described fieldwork as generating

> [a] multitude of entanglements, emotional, financial, professional, intellectual or ethical. It is by talking and writing about these experiences in the field that we become familiar with the experiential core of social anthropology, the richness, complexity and contradictions of relationships. The data produced through these often compromised and compromising encounters is ultimately transformed into an authoritative academic text, and these articles seek to elucidate the process through which raw experience has been translated into vehicles for the production of ethnographic knowledge.

Fieldwork is scholarly work that requires first-hand observation, recording or documenting what one sees and hears in a particular setting – a rural artisan community, a city market place, hunting and gathering with a highland tribe, or the plush interiors of a corporate head office.[20]

The quality of results obtained from fieldwork depends on the data gathered in the field. The data in turn depend upon the fieldworker, the worker's psyche, level of involvement, and ability to see and visualize things that any other person visiting the place might fail to notice. The more open a researcher is to new ideas, concepts and things that they may not have seen in their own culture, the better will be the absorption of those ideas. Better grasping of such material means better understanding of the forces of culture operating in the area and the ways they modify the lives of the people under study. Anthropologists have always been taught to be free from ethnocentrism, the belief in the superiority of one's own ethnic group.

A researcher has to approach people without preconceived notions about the various institutions under study. Relying on previous literature is useful to introduce the researcher to the people and their culture. But the forces of evolution are at work on cultures and societies just as they apply to biological organisms; as a result, the existing literature may already be outdated. The researcher must gather as much information as possible personally. The collection of 'contemporary' ethnographic data serves to portray the current trends and is invaluable for studying culture change over time.

A fieldworker spends a great deal of time in the field, observing people. As Thomas (2004, p. 150) has reminded us, 'social scientists are privileged in being able to ask direct questions of the objects they study. Physicists are not able to interview their atoms; if they could, would they be able to remove some of Heisenberg's uncertainty?' But they would have to treat the answers with great caution.

Effective fieldwork depends on qualities that one is born with or must develop through intensive work. Malinowski (1922) is the perfect example; he never had any formal training in fieldwork research yet his work is considered as among the best of all time. The first hurdle a researcher faces is approaching people who may be suspicious of his intentions, who are different in background and whose values and customs are different. A fieldworker can face rejection, so must be strong in mind and convincing enough to persuade those being studied to allow the worker to come and live and work among them. There are things people say and things people mean; a researcher must be able to read between the lines, because nobody wants to present a bad picture about his own community.

Fieldwork requires tremendous concentration; there will be distractions to overcome. It is all about focusing on the object of the study. Since the fieldworker may be far from home, finding company and intellectual stimulation may be difficult. One has to be self-motivated. Fieldwork is more mental than physical; it stretches one to the extremities of mental and physical endurance. Diligence, patience, hard work and the ability to withstand bad tidings make a good fieldworker at a personal level, and the ability to understand processes, insight and visions make one good at the academic level. Anybody who combines both is a great fieldworker, one whose account may well give a reasonably complete and true picture of the people studied. Good work ethics, both in the field and out of it, are an essential part of a good fieldworker. Nothing should be done that destroys the faith which the community under study has put in the fieldworker. Of course, the purpose of the study, and whatever its advantages are, should be made clear to the population under study. Permission, where necessary, should be obtained from the appropriate authorities. The fieldworker must be discreet in presenting sensitive information as results in his report. Good work ethics lend credibility to the researcher, and ensure respect and recognition from among the group he has worked with. They also lay a good foundation for future researchers coming to work with the same people and in the same area.

Mintzberg played a crucial role in the popularization of fieldwork in management. He published his first book in 1973.[21] This pioneer work established his reputation worldwide as a major figure in the field of management and ethnography of organizations. Mintzberg adopted a method that had hardly ever been used in management research: direct and structured observation (fieldwork).[22] This method requires the researcher to follow the steps of each of the general managers no matter what activity they are doing. He must carefully note the slightest action, recording the amounts of time spent on each and entering all the data on a grid, which is later to be used to do breakdowns and calculations, make comparisons, and so forth. The tremendous amount of work that Mintzberg put into the findings earned him the title of leader of a new school of management: the descriptive school, as opposed to the prescriptive and normative schools that preceded his work. The schools of thought derive from Taylor, Fayo l, Urwick, Simon, and others who endeavoured to prescribe and expound norms to show what managers must or should do. With the arrival of Mintzberg, the question was no longer what must or should be done, but what a manager actually does during the day. Mintzberg's discoveries and deductions appeared to be a veritable revolution.

To sum up, fieldwork means finding out what people actually do, how they actually think and behave, and what they mean when they say something. Fieldwork has not been widely discussed or widely employed in economics – but it has been there right from the beginning. Adam Smith visited a pin factory, and observed it closely. This led him to explain how the division of labour worked. But, in general, economists have not done much fieldwork.[23]

In view of the importance of Adam Smith's example, why are economists reluctant to give prominence to fieldwork? There are exceptions: the intuitionalists did it; and much industrial organization is based on fieldwork, as is a good deal of labour economics (Andrews, 1949; Bewley, 1999; Blinder, 1998; Commons, 1968; Edwards, 1979; Florence, 1972). Work on the 'informal economy' provides a good contemporary example (Portes et al., 1989). Surveys of consumer confidence (survey research centre, Oxford surveys, conference board, INSEE) reflect fieldwork, but most so-called empirical work today is based on number-crunching (Nell, 1998a, p. 101).

Fieldwork calls for participation: to know the meaning of a social practice, it is necessary to experience it in some way. It may be possible to gain an understanding imaginatively, or through discussions with participants; and it is certainly not necessary to participate in every aspect. But participation ensures that the observer directly experiences the social practice, and can check the meaning and appreciate the nuances by asking other participants. The object is to get beneath the surface, to contrast actual behaviour with the 'official' view, and to relate language and description to behaviour (McCloskey, 1983; 1985a). It draws on the method of 'Verstehen' a method that economists tend to regard with suspicion, although it was central to the work of the German historical school. Indeed, this suspicion seems unwarranted; there is widespread appreciation for realism among economists – at least those who reject Friedman's extreme position. Even Blaug (see Nell, 1998a, chs 3 and 4) refers with approval to realism, for example in his comments on Hicks, who regarded it as central.[24] Yet 'realism' can be verified only by fieldwork.

In economics, fieldwork is necessary, for example to tell us the real relations in a corporation, as opposed to what the table of organization says; it is needed to tell us what really motivates people, as opposed to what they say motivates them, or what we – or the corporations! – think should motivate them. It can tell us how prices are actually fixed, and what was paid as opposed to ways of concealing profits; what is the difference between income and income defined for tax purposes; what inputs are really necessary; what is really work, as opposed to sophisticated shirking; what consumers really want, as opposed to what they have been induced to want – or whether such a distinction can be drawn. Fieldwork can give us a picture of markets in operation, of the institutions that organize production and sales, and of the way work is structured – as seen from the 'inside', and balanced against the 'official' picture, for both – and the contrasts will be part of the truth.

Without fieldwork, our numbers and therefore our statistics will give us a distorted picture of the world. Without fieldwork, we cannot know the operating rules in our economic institutions, or the true motivations of agents. Mayer (1993) gives the example of time inconsistency theory, in which a game theoretic analysis demonstrates the case for a rule-based rather than a discretionary monetary policy. In this approach, the central bank is assumed to generate inflation in order to trick agents into overestimating their real wages and therefore work effort. As Mayer points out (ibid., pp. 64–5), the statistical evidence suggests strongly that Fed policy has been anti-inflationary during most of its existence. The only exceptions were during wartime. This could be supported even more strongly by reading the records of meetings of the board of governors and the open market committee. Further, even if the Fed had an inflationary bias, the reason for this bias might be quite different than that assumed by time inconsistency theory. That theory rests on an attribution of intentions to an institution, the Fed an attribution made without considering the available evidence, or doing the fieldwork necessary to gather and evaluate new or better evidence. A different but even more extreme case is provided by Lucas's (in)famous claim that 'involuntary unemployment is not a factor phenomenon which it is the task of theorists to explain. It is a theoretical construct which Keynes introduced in the hope that it would be helpful in discovering a correct explanation for a genuine phenomenon: large-scale fluctuations in measured, total employment' (Lucas, 1987, p. 354; see also the commentary in Rosenberg, 1992, pp. 77–8). Even minimal fieldwork will establish that 'involuntary unemployment', in the normal sense of the term, is a fact, and, moreover, one in need of explanation. Further (historical) fieldwork will show that the character of employment in leading industrial countries changed from before 1914 to after 1945. The legal, regulatory and institutional arrangements changed.

Fieldwork does not result in scientific theories, let alone covering-law explanations (if there are any such!). As we shall see, two types of fieldwork can be distinguished. One kind can give us a carefully drawn picture of institutions and practices, general in that it applies to all activities of a certain kind in a particular society or social setting, but specialized to that society or setting. Although institutions and practices are intangible, such a picture will be objective, a matter of fact independent of the state of mind of the particular agents reported on. Approaching the economy from a different angle, another kind of fieldwork can give us the state of mind of economic agents – their true motivations, their beliefs, state of knowledge, expectations, their preferences and values. These results will also be matters of fact, but they will be records of the subjective states of the agents reported on – their feelings, attitudes, beliefs, preferences and values. Fieldwork is reporting, but it is at the same time an exceptionally sophisticated reporting, because it requires the observer to penetrate the disguises of key roles in society and the economy. This requires careful judgement, since the mask will usually display a partial truth.

10.2.2 Fieldwork and Structure

Structural fieldwork investigates the economy by looking at relationships in production, exchange, and distribution – such as the linkages between sectors or agents, for example; technological and legal interdependences (input–output relationships, interest on capital, wage or salary contracts); or relationships of status and authority, as in comparing the positions of property or wealth-owners and the property-less in various sectors. Fieldwork establishes the linkages between these features of the system and ranks them in importance; it is concerned with gathering and interpreting statistics, but also with the character of technology, with job titles and descriptions, contracts, chains of command, responsibilities, and so on. Objects of study will include roles (producers and consumers, suppliers of labour or of savings and wealth) and institutions (firms and households).

The study of households will raise the question of the position of families and the kinship system as holders and transmitters of wealth, as well as consuming units and suppliers of labour: does this make them also the unit of social classes? Indeed, can we usefully distinguish classes – that is, classes of families – by their holding or not holding income-earning wealth?

But before thinking about classes, fieldwork should distinguish the two kinds of institutions examined in Chapter 3 – one kind that runs the world, and the other that prepares people to hold positions in the institutions that run the world (Nell, 1996). That there must be these two follows from the fact that humans are mortal; if institutions are to continue to function, properly prepared people must be available to succeed those who currently hold the positions. Hence – as we saw in Chapter 3 – there must be institutions that prepare them: families, schools, churches, training programmes, apprenticeship systems, and the like. People to replace those currently running the world must be born, raised, socialized, educated, and trained for their roles in later life. In a broad and metaphorical sense, the two kinds of institutions represent a demand and a supply of suitably prepared individuals. The institutions that make up the world of practical affairs demand replacement personnel, while the educating and socializing institutions – families in the first instance, then schools and training programmes – provide the supply (including the supply of those to replace the present managers of socialization).

Studying this second kind of institution leads naturally to a study of the products of socialization, the different social types and personality profiles that the system turns out. Warriors, priests, shopkeepers, bureaucrats, engineers, explorers, farmers, rabbis all differ in attitudes as well as in skills. Many of these attitudes are learned, the results of nurture; however, some are inborn, deriving from nature. But even some of these latter may be the consequence of social processes, deriving from the gene pool established by the rules and customs governing partner selection in the marriage system. But the study of the types produced by the system's socialization and educational processes is not the same thing as examining the states of mind of people in their day-to-day lives. As we shall see, this is a separate enquiry, a different kind of fieldwork.

To obtain a picture of the whole, the two kinds of institutions must be put together. It must be shown, first, how each works, and then it must be shown how the two fit together. The roles and activities that we called 'running the world' are interdependent; they produce goods and services for each other, in the process consuming the very goods and services they produce. These must be shown to make sense taken together; the different aspects of society mesh, join to make up a culture. Economics will contribute by exploring whether the linkages and connections are mathematically consistent and stable. Along with this it must be seen whether the socialization institutions produce appropriately prepared replacements for those occupying the positions of society. A particular form of this is the class society, in which those occupying the leading roles form families that produce a new generation prepared to take over those leading roles (and who will, in turn, contract marriages that will produce the following generation), while those in the lower roles likewise form families that will produce future occupants of the same lower-level roles.

But all this does not take place according to a plan, nor on the other hand does it happen by accident. The running of capitalist society – its production, exchange and distribution, and the filling of its jobs and positions – are all coordinated by the market. But the actions that people take in the market are carried out in pursuit of self-interest; each agent is free to choose among a variety of possibilities, and does so in the light of material advantage. Workers may choose jobs, employers their workers, consumers their goods, producers their target markets.

Adam Smith spoke of a 'system of perfect liberty' – ideally, that is; in reality the agents all face various constraints. But in such a system, even ignoring the constraints, market outcomes will not in general be those intended by the market participants. Some will be winners, others losers, and there will be many who are disappointed at least in part. And while the market coordinates activities, balancing supplies and demands, no one has specifically acted with the intent to bring about such coordination. It comes about as an unintended consequence. Sometimes the market fails, and rather than coordination, it brings about a breakdown: depression or inflation. To understand this requires putting all the pictures together. In a sense, the final objective of fieldwork in economics is to give us a practical picture of the working of the market.

10.2.3 Fieldwork and Behaviour

The second kind of fieldwork concerns motivation, attitudes, preferences and other subjective influences on behaviour, given the context – laws, customs, technology, and so on. It is an exploration and mapping of the chief features of the states of mind of the agents, picturing such states as are likely to affect behaviour. It is not, however, personal biography: the issues concern the subjective influences on economic behaviour, typical economic behaviour. Personal histories may well be illuminating, but they are relevant only insofar as they shed light on economic decisions and actions.

These studies can be complicated by the fact that people are not always truthful about their states of mind, and, worse, even if they try to be, they may fail because they are unaware of their own motivations or attitudes, or are subject to self-deception. (In regard to economic questions: where preferences reflect officially discouraged prejudices, for example, the true preferences may not be acknowledged. Also, people frequently understate the extent to which they are motivated by money, and often hold false beliefs about their own and others' wealth, sometimes stubbornly clinging to expectations they know will never be fulfilled.)

To map the actual states of mind of agents is to study people, who are social products and have been prepared for certain roles, acting in the roles which they have assumed or to which they have been appointed (which may or may not be the ones for which they were prepared). There will be mixed loyalties, conflicts and uncertainties, and very often contradictory and unreliable reports will have to be reconciled.

What such a mapping will show is how agents see the world, how they value its various aspects, and how they plan strategy and tactics in regard to economic activities. In particular, it will show their understanding and motivation in regard to the market.

10.2.4 Modelling Behaviour and Structure

These two aspects of the economy, roughly its structure and the typical motivations and behaviour of its agents, give rise to two lines of analysis. The first will show the linkages and connections between economic institutions, making it possible to calculate various relationships. The second will examine motivation and strategy in various contexts, showing how these can explain behaviour. There is an obvious sense in which each needs the other as a complement: structure without behaviour is lifeless, behaviour without structure has neither basis nor focus.

Neoclassical models analyse behaviour in specific ways. Instead of drawing on fieldwork, to define motivation and set the problems of choice in well-described institutional context, agents are considered abstractly and presumed to be rational and to choose freely. This, then, leads to models that exhibit a particular kind of market behaviour, which we can call a 'stimulus-response' pattern. We argued in Chapter 1 that these models are strongly behavioural, paying little attention to structure. The context of action is abstract; the questions concern what an agent, usually a 'household' or a 'firm', would normally do, acting under the influence of an assumed motivation and calculating rationally, when presented with various stimuli. It is assumed that the actions in response to stimuli are successful – a harmless assumption, when it is households making purchases, but question-begging, when it is investors introducing a new technology. Given the behavioural assumptions, reaction patterns to such hypothetical stimuli are constructed, and from these sets market functions are aggregated. Equilibrium market positions are then determined by solving the market equations on the hypothesis that behaviour will be adjusted as stimuli move, until the markets are cleared.

The paradigmatic models of neoclassical economics are almost exclusively behavioural, but they adopt a particular form in which to model behaviour: that of stimulus and response, giving rise to the characteristic problem of 'inexact' laws or generalizations. But the stimulus-response approach is appropriate only for describing agents who are understood as having given motivations and values. The agents must also be understood as having given knowledge of the world; they do not learn or innovate, nor do they experiment with interpreting the stimuli they receive. The neoclassical approach therefore adopts the passive picture of the mind. Yet such models also rest on an assumed but largely unexamined structure, the context in which stimulus and response take place. But the structure of an economy implies the presence of agents who must be understood as having active minds, for it requires active minds to interpret and apply abstract rules in concrete situations.

We argued in Chapter 6 that a model can be said to have two aspects, or to be composed of two kinds of elements. On the one hand, there is the purely formal part, and on the other, there is the interpretation that clothes the formal skeleton with meaning. The formal part of a model consists of an algorithm in some formal calculus.

Behaviour takes place in a social context, one element of which will be transitory, others permanent. Regular or repeated behaviour will depend chiefly on the latter. But such permanent features of the social setting must themselves be reproduced physically, if material, or must be reproduced in the actions and behaviour of agents, if, like rules and customs, they are intangible. This suggests that enquiry must proceed along two related fronts, delving into institutions and into technology. But it also calls for two different kinds of fieldwork, one exploring structure (the enduring features of the social context, whether material or institutional), the other looking into behaviour itself (whether reflecting institutional imperatives or individual choice).

When behaviour is the object of study, the existence, the characteristics and the positions of those whose behaviour it is must be taken as given. It is here, in connecting behavioural functions to agents as they are assumed to exist, that the subjectivity of the approach lies. It has rightly been pointed out, in answer to the charge of subjectivity, that the variables of behavioural models in economics refer to publicly observable acts. Hardly anything could be more objective. The theory of demand has choices and market prices as its variables: both are observable, open and publicly verifiable; the theory of supply refers to inputs, prices and outputs, all likewise public and observable.

Nevertheless this misses the point, which is that none of these is observable except in connection with some actual agent; but actual or observable agents are rarely similar to the ideal types postulated by the model. The real significance of 'subjectivity' lies here, in the fact that acts, however public they may be, are always someone's acts; that is, they belong to a subject. The identity of the action – what exactly was done on a given occasion – depends on the intention of the agent (Wiggins, 1980). A theory of behaviour must therefore always predicate its behavioural functions of some agents or kind of agents. For acts done in the real world to correspond to the actions of theory, the agents of the real world must correspond in all essentials to the agents postulated by theory. But behavioural theory tends to concentrate attention on the way agents with assumed knowledge, abilities and desires make decisions and affect one another's actions, neglecting the question of what these agents are and how they and their characteristics are brought into being and maintained – the subject of Chapter 3.

Structural models show how the economy maintains and reproduces itself. But it will not do so in exactly the same way every time – agents with active minds will see to that. Market adjustment will confront agents with characteristic problems. Whoever solves these problems will be rewarded – at the expense of those who don't. Competition in the market will judge the innovations and reward the improvers, while discarding the failures and punishing the losers and laggards. Over time this will lead to changes in the way the market works; as they adapt to their altered market environment, the agents – households and firms – will take on new characteristics. The system evolves.

Instead of describing the behaviour of agents, a structural model shows the rules governing behaviour, the methods and procedures of production, the legal and property relationships. These may describe systems such as capitalism (Bharadwaj and Schefold, 1992; Sraffa, 1960) or feudalism (Nell, 1968; 1992c, chs. 12 and 13); or they may describe a particular institution, as in a flowchart showing the working of a production process, or an organizational table for a firm. What is shown is not tangible, perceptible, or 'objective' in the same way. But this does not imply that these rules are any the less real or objective. The intangibility of structure does not imply its subjectivity. The duties of, for example, the president of the United States are not a matter of subjective preference. A proposition stating them is not a 'value judgement' or an 'expression of feeling'. It is a proposition stating a fact, albeit one of a different kind than those the natural sciences examine.

Structural models represent intangible, immaterial relationships. They show rules and formulae; methods of production, like the entries in cookbooks, are recipes, and the rules of distribution are just that: rules. Organizational tables show the structure of a company, its accounts show its balance of profit and loss. Structural models show relations between agents, and between agents and the world, but what they give us is a blueprint, an outline, a pattern which has to be instantiated. And this may be done well or badly. Rightly or wrongly.

There is an important difference in focus here compared to neoclassical thinking.[25] Both are concerned with intangibles, but the latter's concern is with states of mind that are properly ascribed to individuals, whereas structural models relate to features of institutions. As we saw earlier, this calls for a focus on roles, duties, and norms rather than preferences, wants, and desires.

At this point, an important distinction must be made between three kinds of economic model (Hollis and Nell, 1975, ch. 5). The most basic models are structural and analyse reproduction – that is, the way the system can maintain itself or expand. These must be based on a conceptual analysis of the institutions of the economic system as determined in fieldwork. Input–output models, for example, show the basic exchanges that have to be made to keep the system running; they exhibit the possibilities of investment, consumption, export, and they show the structure of interdependence, the wage–profit trade-off, and so on. The analysis can be formal and abstract; institutions can be simplified, but the essential features cannot be distorted.

But such models say rather little about behaviour and tell us almost nothing about what is likely to happen in various markets. A second type of model, therefore, can be designed to explain and predict behaviour. But it cannot be based on the stimulus-response framework, for the reasons just presented. This second type of model must situate behaviour in structure and must draw on the results of fieldwork to make assumptions about motivation, rule-governed behaviour, and cession procedures, including optimizing. Economic agents may plausibly be assumed to pursue their self-interest, although not to the exclusion of all other motives. Such models will certainly assume rationality – humans are rational animals – but the concept of rationality will be broad, and will include propensities to learn and to innovate. Agents have active minds. They draw up plans, and then execute them. Planning does not entail implementation; commitment does not entail fulfilment. Implementation occurs in stages, and actions reach climaxes at which they may succeed or fail. At any point actions can be re-examined, commitments can be revised, and at any point things can go awry, simply because human action can always fail. An irreducible residue of uncertainty resides here.

As noted, such models will have to draw on simplified results from structural analysis, to present the context in which behaviour takes place. Agents must be properly situated, and have access to the means to act. They can be assumed to pursue their self-interest in markets. But self-interested behaviour does not mean generalized rational choice. Agents pursuing their self-interest must be considered in their actual circumstances, facing the options that exist for them. To add to those options something abstractly possible, but not actually a present option, is to introduce an irrelevancy into a predictive model. Such matters should be studied in programming models. Moreover, the self in question is the product of a family/kinship/educational system, now acting in a role in a production organization, and also holding a position (breadwinner?) in a family household, which will produce the next generation of agents. The actual motivation of an actor will develop out of the interaction of these various components of the self.

The choice set facing an agent is composed of those options between which the agent should choose, given the responsibilities of his or her position. That is, these are the choices they are supposed to make, are empowered to make, and have the skills and information to decide on. The agent can be expected to do a good job choosing among these options; to consider others might be beyond the agent's capabilities or powers. We can assume an agent will do the best job possible – that is, will maximize among routine options – when doing so is implied by his responsibilities, meaning it is normal and expected of him, and not to do so would waste resources needed for other activities. This can be described as self-interested behaviour, or role-based maximizing, and is a proper foundation for models of economic behaviour. (A powerful solvent of hypocrisy, too. We are rightly sceptical when people claim to have no economic motivation: as Deep Throat said, follow the money.)

But such routine maximizing is not rational choice. There is a division of labour issue here: the agent is an actor, that is, the agent makes choices and carries out the resulting actions. Actors are trained to act; households not only buy, they consume. Firms choose factors and inputs; but their main activity is producing. Management consultants and interior designers, on the other hand, are specialists in choosing and evaluating. They have studied the opportunities, know the possibilities and circumstances in detail, and have mastered the methods – and pitfalls – of making optimal choices under various kinds of constraints. Their job is to examine all the options, assess the constraints, rethink the possibilities, and then optimize or, at any rate, lay out and rank the best courses of action, spelling out the likely implications of each. By contrast, the actor's job is to make routine choices and get on with the programme.

Once agents are properly understood, and placed in their appropriate circumstances, important questions can be addressed. What will happen in the business cycle? Will inflation intensify, will unemployment rise or fall? Such models can also be developed for particular markets or sectors. The actual practice of the economy in question must be known if it is to be modelled accurately. Fieldwork is therefore essential to success, and the lack of attention to systematic fieldwork by economists may help to account for the generally poor record in predictive econometrics.

Finally, there is a third kind of model, quite different from the others, with a different conceptual foundation. These were called 'programming' models in Hollis and Nell, and termed 'instrumental' by Lowe (1965). They are not predictive, rather they determine what the best course of action would be for given agents in given circumstances, and moreover they allow for a reconsideration of the givens, since they make it possible to determine how much would be gained by shifting the constraints. Such models provide the natural and proper home for the narrow means–ends concept of rationality. If the agents do not do what the model calls for, it is the agents who are to be criticized, not the model, assuming that the model correctly represents the circumstances, and the goals and motivation of the agents (including the possibility that the agents might innovate or otherwise change their circumstances or goals, in line with the shadow prices determined by the model).

The conclusions of a rational choice model have an extraordinary power. They represent what ought to be done in the given conditions – not what should be done morally, but rationally. The model tells us the right, proper, sensible, best thing to do in the circumstances. Agents in the given conditions who do not act in accordance with the model may be considered foolish.

These models embody the optimizing interpretation of rationality, and thus generate the concepts of scarcity and opportunity costs. But they are prescriptive rather than descriptive. Such models are tools for the active mind. And, like any tools, they can be used in more than one way. The narrow, calculating concept of rationality may determine the best choice from a given set, but it is also a means by which an agent may analyse how to improve on the givens in the situation. Moreover, calculating rationality does not necessarily characterize agents or their behaviour; the actions that follow from a model of rational choice will only be descriptive if and when the agents accept the results prescriptively. Rationality is not a dispositional predicate, like nervous or stolid. Behaviour is rational in the required narrow sense only if it embodies or rests on an appropriate relation between means and ends. But this cannot be adequately judged only from outside; agents may rethink their goals, may reorder their ideas of short run and long, may wish simply to try something new on the chance that it might work better, and so on. For behaviour to be judged rational – carrying out the rational choice model – the model's relationship between means and ends must be that intended by the agent. The agent must accept the logic of the model, for if the agent rejects the model for good reasons (or even for bad ones, so long as they are reasons) it will not be descriptive or predictive.

There are thus deep-rooted problems in the assumption that rational choice will govern behaviour according to the stimulus-response approach. Agents will only behave rationally – that is, act in accordance with the dictates of a rational calculation – if they accept that calculation, which means that they must agree that the problem posed is the one they, in fact, face, and that it is posed in a manner that will yield the results of most use to them. The choice must be within their powers – or they must have commissioned the study! They must accept the choice variables and the constraints, and agree to decline to try to shift the constraints. Otherwise, they are entitled to dismiss the calculation as irrelevant. Hence a rational choice model must be based on realistic assumptions. To be descriptive, maximizing models must be closely tied to the roles and circumstances of agents.

However, this implies that outcomes will be specific and sensitive to the choice of assumptions, as in linear programming and operations research. But the neoclassical models seek to use rationality as the foundation for making universal claims; as a result, the models are highly abstract and decidedly unrealistic. It would be reasonable, therefore, for agents to reject such models and insist on developing calculations that are closely based on their immediate conditions.

This leads once again to the prescriptive power of maximizing, which now becomes a reason to reject descriptive and universal models based on maximizing. Given a model specific enough that agents would accept it, its conclusions might be used to show the agent how those conditions might be altered in their favour. In other words, the model might be used as a guide to innovation. Far from yielding a universal result, then, rational calculation might give a very particular answer, and some of these, at least, could become part of an effort to change the givens of the problem. Maximizing models are not a good foundation for equilibrium behaviour; they are just as likely to suggest change and deviations from the norm!

On this basis, some conclusions with respect to method can be provisionally sketched. The basic idea is to develop structural analyses, and then to consider the behavioural options. First, it is necessary to develop a picture of the basic structure of the system. That requires understanding the technologies in use, the organizations that use them and how they are controlled, and the way the human population is supported and enabled to reproduce. This means gathering the relevant information and devising structural models at various levels of abstraction. Reproduction models provide the foundations; they show what the system is, and how it works; they provide the blueprint, so to speak.

Once these are in place, behavioural questions can be considered: very little can be said about what will actually happen until behaviour is specified. Two general types can be considered. On the one hand, predictive models can be set up, for the system as a whole, or for various subsectors, down to individual agents. These models must be based on well-grounded assumptions about the circumstances and motivations of the actual agents. This depends on a good account of the technology, rules and institutions, including the situation and motivations of agents, which must come from fieldwork. Agents must be in a position to act, which means that they must occupy an appropriate place in the structure. Given the result of fieldwork, behavioural patterns can be developed and the course of the economy through time can be projected.

On the other hand, prescriptive, rather than descriptive, models can be devised to consider the ways in which the actual performance of the system can be improved, from various (possibly conflicting) points of view. Again, these can be developed for the system as a whole, or for subsectors, down to individual agents. If aspects of economic behaviour can be improved or better results obtained, programming models will indicate where and how this might happen. Programming models show possibilities for innovation and learning. They may also indicate, for particular agents, how competitive strategies could be improved. In turn, such changes may affect the basic reproduction/expansion conditions, leading, quite possibly, to changes in rules and institutions, which would have to be ascertained by more fieldwork.

10.3 CONCLUDING REMARKS

10.3.1 Background

We have argued that in economics, conceptual analysis of fieldwork can then put together the real patterns of behaviour and motivation, in the context of the available and actually operating technology, ways of working, making and doing things. Such conceptual analysis may be concerned with 'deconstruction', a literary analysis taking apart the reported picture, discovering concealed meanings and hidden agendas, on the part of both the observers and the observed. An important part of this will be uncovering the presuppositions of the concepts and activities reported by fieldwork. Or – the programme of economics – it may accept the picture, and set out to construct models that will show *how the system works* in various ways, including how it may fail to work and break down.

We argued earlier that fieldwork has not been prominent in economics, though there have been exceptions (for example, the Institutionalists, work in industrial organization, labour economics and informal economy, and more recently in development economics). But most so-called empirical work today is based on number-crunching. Haavelmo (1958, 1989), Klein (1982), Klein (in Mariano, 1987) and Johnston (1963 [1984]) hinted implicitly at the relevance of the fieldwork approach in econometrics. An econometrician coming cold to a study would run the risk of very slow progress with much searching through inappropriate formulations. The aforementioned authors emphasized the importance of knowledge of the institutional realities, and suggested that developing institutional realities (obtained through fieldwork) into well-grounded formulations of economic relationships and refinements of basic data sets would contribute much more to the improvement of empirical results than more elaborate methods of statistical inference.

Fieldwork in economics is necessary, for example, to give us a picture of markets in operation, of the institutions that organize production and sales, and the way work is structured – as seen from the inside, and balanced against the official picture, for both – and the contrasts will be part of the truth. Without fieldwork we cannot know the operating rules in our economic institutions, or the true motivations of agents. Conceptual analysis based on fieldwork will provide the essential assumptions and definitions on which model building should be based. In order to construct the kinds of models that will enable economists to understand the way the system works, we need to start from conceptual truths, fleshed out by understanding from the inside, and then to develop stylized facts by interpreting statistics in the light of fieldwork (Nell, 1998a, ch.3).

These can be further developed on the basis of published statistics (adjusted in the light of information uncovered in fieldwork), and the models can be tested, revised, and so forth. Verification and falsification have a place here; not a privileged place, but a role to play nevertheless. They are not decisive, but they are useful (see Nell, 1998a, part II).

10.3.2 Examples of Conceptual Analysis of Fieldwork in Economics

Alan Blinder's (1998) book and Truman Bewley's (1999) book are good illustrations of smart fieldwork in economics that Nell has advocated since the publication of his (1998a) book. Blinder of Princeton University and his graduate students visited 200 American companies, to find out why managers are slow to raise and lower prices.[26] However, Bewley's (1999) study grew from small beginnings. Seeking inspiration for theoretical models of wage rigidity, in 1992 he arranged a few interviews with businesspeople.

Indeed, although economists have posited many theories to account for wage rigidity, none is satisfactory. Bewley (1999, p. 430) argued that 'the views of business people and labour leaders suggest a morale theory of wage rigidity'. He basically revives methods used by institutional labour economists, mostly in the 1940s and 1950s. He argued that although these authors did not focus on wage rigidity, there have been some recent questionnaire studies of the issue that reinforce findings reported in his book. Bewley (1999, p. 430) wrote:

> [K]ey questions are whether the theory is consistent with rationality and whether it can be developed formally. Crucial aspects of the theory are that productivity depends on employees' mood, that workers with good morale internalize their firm's goals, and that pay cuts impact both mood and identification with the employer. None of these aspects is closely connected with rationality, which, in economists' usage, has to do with striving to achieve given objectives rather than with the selection of objectives or with the psychological capacity to accomplish them, matters central to morale. Nor does there seem to be useful way to discuss formally the choice of objectives.

During the recession of the early 1990s, Bewley explored the puzzle by interviewing over 300 business executives and labour leaders as well as professional recruiters and advisers to the unemployed. His book provides a new vision and much complementary background knowledge about how experienced people in the field see the employment relationship and what is actually crucial. As stated earlier, knowledge of this sort is all too rare in economics. Bewley's truly impressive work can serve as a role model for the relevance of the conceptual analysis of fieldwork. The usefulness of Bewley's insights suggests that the theory of wage rigidity must be reconceptualized on the basis of fieldwork.

Commenting on Bewley's book, Howitt (in Bewley, 1999, jacket blurb) wrote:

> Bewley's argument will be hard for conventional macroeconomists to ignore, partly because of the extraordinary thoroughness and honesty with which he evidently conducted his investigation, and the sheer volume of evidence he provides. Although Bewley's work will not settle the substantive debates related to wage rigidity, it is likely to have a profound influence on the way macroeconomists construct models. In particular, the concepts of morale, fairness, and money illusion are almost certain to play a big role in macroeconomic theory. His demonstration that there exist in reality simple, robust behavioural patterns that cannot plausibly be founded on traditional maximizing behaviour also raises the prospect of a more empirically oriented, more behavioural macroeconomics in the future.

Indeed, Bewley's findings contradict most theories of wage rigidity and provide fascinating insights into the problems businesses face that prevent labour markets from clearing. Furthermore, Bewley (1999, p. 468) has argued:

> The subject of economics has an enormous impact on everyone's life, and yet the discipline lacks the status of a real science, follows rather than leads ideological trends, and sometimes indulges in fanciful theoretical representations of reality. Many branches of economics are not anchored in empirical knowledge, probably because the subject originated as part of moral philosophy and is still regarded as having to do more with thinking than with observation. This attitude is compatible with the field's dependence on easily accessible statistical data, which, though essential, are also inadequate. Often it is not clear what they measure, and without this knowledge they can be used to support almost any contention. How can the unemployment rate be interpreted without knowledge of what it means to be unemployed? What sense can be made of wage data without knowing the impact of workers on pay raises and cuts? Empirical knowledge means systematic experience with the object of study, and this can be had only by taking responsibility for data collection.

Furthermore, Bewley (ibid., p. 13) wrote 'my conclusions are no doubt influenced by time and place and by current fashions. Nevertheless wage rigidity has been an enduring phenomenon. It was even mentioned by Malthus in 1798'.

Economists today typically do research using econometrics and mathematical modelling. These techniques have much strength, but share the weakness of distance from individual economic actors' (for example, Swann, 2008, pp. 29–32) view of econometrics as triangulation. In contrast, fieldwork research allows direct contact with them, yielding several advantages. Fieldwork can improve economic research, for example, by drawing largely on interviews as in the NBER/Sloan program and Bewley's study, or by drawing on common knowledge, as in Swann's vernacular economics. Desrosieres (2001, p. 350) pointed out that during the period 1995–99, INSEE annually devoted a one-day seminar to business statistics.

Many fieldwork insights can be translated into the language of econometrics or theory. It is possible that economists using only those methods could have generated the same insights, but in fact, they didn't. Fieldwork offers a new source of inspiration, one that is complementary to more conventional methods.

Many economists remain sceptical of qualitative research, fearing that it is not objective, replicable or generalizable. Econometricians ask what are the standards for good fieldwork, saying that, in econometrics, they know to look for identification and specification issues, but what are the analogues in fieldwork? How is it different from journalism? The trouble is, they have not read the literature on fieldwork. Furthermore, there is a tendency to think that while econometrics requires years of training, fieldwork research is easy. It's not. It's just as important to pay as much attention to careful research design and sample selection as to quantitative research.

Let's review some recent cases of effective economic field research, where researchers have asked people directly about their objectives and constraints. Renee et al. (1996) wondered why many professionals complain about long hours, yet few firms offer the option of short hours. In talking with lawyers, they learned that partners found it difficult to decide whom to promote in order to maximize their incomes. 'A partner saying about an associate, she does really good work, but I wonder, does she like money enough?' That is, he wanted to know, will she work really hard? These comments and others implied that the senior partners used work hours as a proxy for the propensity to work hard. This insight led the authors to build a model and collect survey data that suggested that reliance on these observable proxies led to incentives to work inefficiently long hours.[27]

Susan Helper (2000) thinks that fieldwork allows exploration of areas with little pre-existing data or theory. Indeed, she wrote:

> I started my dissertation research thinking I would look at automakers' make/buy decisions. But when I started interviewing and reading trade journals, I

> realized that important changes – not reflected in the existing literature – were occurring on the 'buy' side. US automakers were moving from adversarial deals (in which they 'would steal a dime from a starving grandmother', one supplier said) to 'voice' relationships in which they worked with suppliers to improve performance.[28]

Furthermore, in her 1991 qualitative study Helper argued that information exchange and commitment were important determinants of supplier performance. One finding was that voice relationships were associated with more cost reduction, but only if complementary policies were adopted (see Helper, 1991; 1999).

Helper (2000) observes that, because of fears about the unreliability of field methods, some economists get ideas from the field but do not discuss their fieldwork in their published articles. But understanding the setting can help explain differences in findings between cases by making clear the mechanism by which variables are linked. For example, Helper argues that while Lazear (1996) found that a move to piece-rates increased profits at the auto-glass installer, Freeman and Kleiner (1998) found that a change away from piece-rates increased profits at a shoe manufacturer. She goes on to argue that understanding the production process at the two firms is key to making sense of these results: while both papers found that productivity was higher under piece-rates, time-rates at the shoe firm facilitated the introduction of a new production process that brought reduced inventories and faster new-product introduction.

For Lazear (1996), fieldwork provides vivid images that promote intuition. His work on the change from time-rates to piece-rates at Safelight Auto Glass is one of his most-cited papers, because everyone can imagine workers working harder to install windshields once they're on piece-rates, an image easier to remember than the regression coefficients.

Zucker et al. (1998) used fieldwork to show that the number of gene-sequence discoveries was a good proxy for intellectual capital in biotechnology; their regression results were consistent with intellectual capital being the main determinant of the location and growth of biotechnology firms.

Ichniowski et al. (1997) consider that fieldwork facilitates the use of the right data. The interviews helped them to determine that steel finishing lines had homogeneous technology and that there were enough such lines to allow econometric investigation of the impacts of innovative human resource policies unconfounded by technology or industry differences. They conducted fieldwork and visited 45 plants to collect production data. They interviewed managers to ensure compatible measures across plants and observe what human-resource practices were in place.

Udry (2003, p. 1) noted that development economics has benefited from a rich tradition of field research. Within this broad tradition there is a huge variety of methods, from short qualitative studies to large-scale surveys:

> Typically, empirical work in economics relies on existing data. However, it is becoming more common in development economics to complement existing data with relatively short, often less structured visits to the field site in order to clarify aspects of the data, to better define the economic environment, or to collect limited amounts of complementary data. For example, ICRISAT hosted and provided institutional support for a series of visiting scholars during the collection of the Village Level Surveys. This proved to be a relatively inexpensive mechanism that generated an important sequence of insights regarding economic institutions in India.

These are recent studies, but recognition of the need for this sort of work goes back a long way. Jevons (1871, quoted by Swann, 2008, p. 15) felt that 'economic theory on its own would not make economics into a science'. It had to be supplemented by systematic collection and analysis of real data (Swann, 2008, p. 1). Stone (1978, p. 2, quoted by Swann, 2008, p. 15) pointed out Marshall's assertion that 'economics proper involved both theoretical reasoning and a thorough study of facts. Only by combining these activities could the economist disentangle all the complex causes found in economic activity'.

10.3.3 The MTC Diagram, Conceptual Analysis and Fieldwork

Figure 10.1 shows the MTC diagram's relationships to fieldwork and conceptual analysis. Fieldwork interacts with applicability/relevance on the one hand, and with measurement/quantification on the other. Fieldwork counts and measures, and gathers data of all kinds; but it also develops understanding of the concepts, ideas, values and norms guiding and regulating the activities being modelled. Fieldwork, in turn, delivers these concepts and norms to conceptual analysis, which then develops them into theory. And that in turn will suggest new questions and new directions for fieldwork.

Finally, the black lines forming the triangle connecting the three aspects of the model represent the *O* and *V* relations that hold the socio-economic system together. *O* relations provide us with the concepts, laws, norms and descriptions – the powers and responsibilities of agents, the characteristics of variables – that govern practice, that appear in contracts and documents. They therefore connect theory with application. *O* relations, in turn, must be expressed in *V* terms: what is ultimately owned is the insurable interest, which is value. *V* relations are by definition quantified. The V relations, in turn, must be specified by theory. So *O* and *V* relations connect all three aspects of the model.

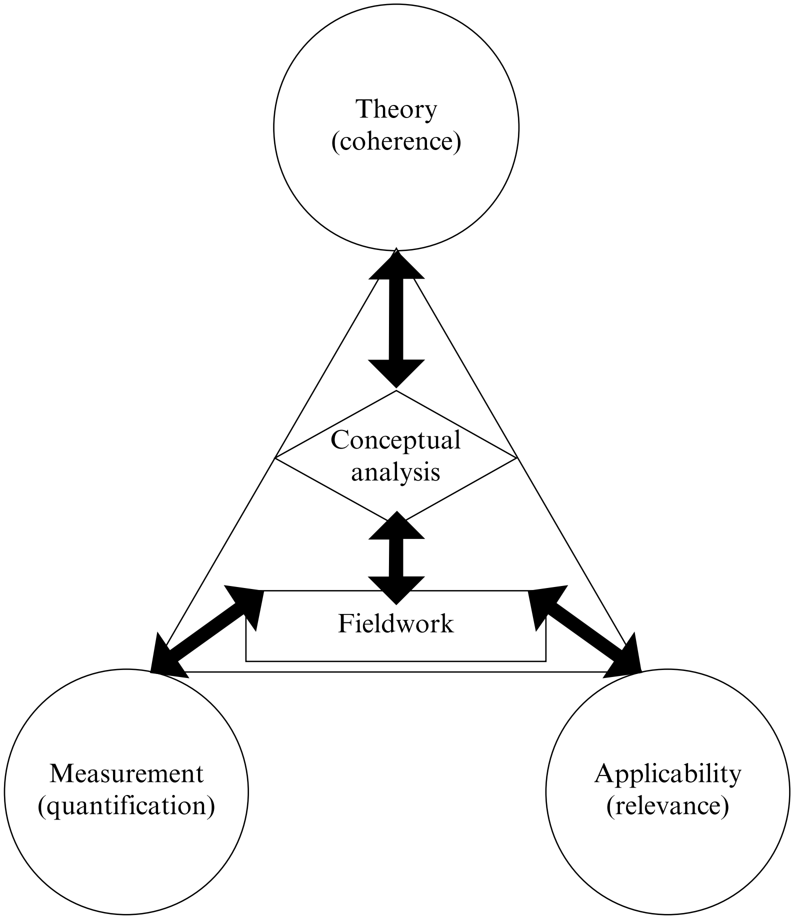


Figure 10.1 MTC and the methodology of economic model building

Our vision, expressed in Figure 10.1, which also reflects the vision of the founding fathers of modern econometrics, is that the combination of theory, mathematics, statistics and other data provides the best approach to the 'disentangling of complex causes' noted by Swann (2008, p. 15). As described by Frisch (1956, p. 302), 'this vision involved a combination of mathematical tools, an understanding of theory and the use of statistical data. Fifty years later we are still far from achieving Jevons's dream of a real empirical science of economics'. Indeed, Frisch (1956, p. 301, quoted by Swann, 2008, p. 15) has already observed 'how difficult it was to turn Jevons's dream into reality'. By using fieldwork in conjunction with conceptual analysis, we hope to avoid what Friedman (1991, p. 36, quoted by Swann, 2008, p. 6) expressed elegantly when he observed that the use of mathematics and econometrics in economics had progressed beyond diminishing returns to 'vanishing returns'.

The main conclusion is that all three levels – conceptual analysis, fieldwork and model-building – interact. Each can help to extend and develop the others. No single criterion governs all. Each draws on precepts and practical maxims peculiar to itself, but each provides assistance to the others, and in some measure each is necessary to the others.

NOTES

1. Quoted by Epstein (1987, p. 119).
2. Bernardo Houssay received the Nobel Prize for Medicine in 1947 for his 'discovery of the role played by pituitary hormones in regulating the amount of blood sugar (glucose) in animals'. This epigraph was quoted by Federico Mayor Zaragoza, former Director General of UNESCO and President of the Foundation for Culture of Peace, in a speech at the special session on Nobel Day, World Life Sciences Forum, 8 April 2003, Lyon, France.
3. Milberg (2007, p. 210) pointed out that the work of McCloskey should be taken seriously as a methodological critic. He argued that 'McCloskey, the lone neoclassical [. . .] sought to reform the profession by overcoming its "vices" – eliminating useless mathematics in theory ("blackboard economics") and the abuse of the notion of "significance" in empirical analysis, and generally abandoning an objective theory of truth in favor of consensus. Often the case for the "better" economics fell back on claims of greater realism' (ibid., p. 207). He went on to argue that 'this claim, however, contradicted the postmodernism epistemology – that knowing is possible only in a relative or discursive sense' (ibid., p. 207). For an account of postmodernism in economic thought, see Milberg (1993; 2004).
4. See the jacket blurb in Swann (2008).
5. As for empirical work, Summers (1991) offers a disturbing critique of recent macroeconometric papers, pointing out that virtually all sophisticated studies fail to establish or support any general position – that is, they fail to convince. Econometric studies almost never replicate results, and new work seldom or never builds on accepted earlier findings – unlike empirical work in the natural sciences (Mirowski, 1992). By contrast, the empirical work that is convincing to the profession is looser, less precise, and more historical.
6. Our approach is a striking parallel to Marschak's call for interviews with businessmen in order to clarify specification of the investment function. For an account, see Epstein (1987, p. 179).
7. See the discussion between Hendry, Leamer and Poirier summarized in Hendry et al. (1990).
8. For an account, see Carro (1981).
9. Leontief in particular wrote at length about engineering processes and engineering data that he and his associates used in their input–output work. Leontief was not just an econometrician. His understanding of the necessary adjacent fields meant there was no communication breakdown from a lack of common standards.
10. In December 1993, the leading French newspaper *Le Monde* devoted a special issue to the following question: 'is Economics a Science?' J.P. Dupuy characterized economics as 'science in delay', G.G. Granger spoke of 'a blind science' and M. Henry suggested

that economics is an 'aerialist knowledge'; A. d'Autume spoke of economics as a 'science enriched', Malinvaud spoke of economics as 'disciplined knowledge', A. Orlean spoke of 'dynamics knowledge'; and Allais thinks that economics in its current situation cannot be considered as a science. For further details on the scientific status of economics, see Mouchot (1996, Introduction).

11. Chapter 10 is a modified and condensed version of Errouaki (2007) and Nell and Errouaki (2008a).
12. Popper and his followers among economists accept the covering law approach. To cope with the difficulty, Popper introduced the principle of falsification (PF). Rather than trying, impossibly, to verify general statements, he argued that the implications of theories should, instead, be subjected to the test of attempting to falsify them. A general statement or a theory cannot be verified, but a falsifiable implication of such a statement or theory can be confronted with the evidence (Nell, 1998a, p. 77). For an in-depth examination of methodology and falsification, see Nell (1998a, pp. 76–87).
13. Conceptual truths are particularly important in the analysis of social systems, since they regulate the thinking of the agents as well as the theory building of the observers. The problem of 'rationality' for Popper is that the ascription of rationality to human agents is a conceptual truth, of exactly the kind for which he can find no place in his philosophy. It is a priori that human agents are rational animals, and also a priori that they have free will and hence can choose an irrational course of action.
14. That there are no private languages and that rationality requires language are conceptual truths, which have been extensively explored – and disputed – in contemporary philosophy (Nell, 1966; Strawson, 1959; Winch, 1958; Wittgenstein, 1956).
15. A good deal of effort has gone into trying to find or define processes for which this is not true – that is, processes that are self-sustaining and produce their own replacements. Ricardo suggested corn – which is its own seed, and provides the support for the labour that grows and harvests it. Knight offered the parable of the 'Crusonia' plant, which likewise supported its tenders and reseeded itself. Notably both are *biological*, rather than mechanical processes; the growth is brought about by internal causes that are unrelated to the economy. Both are admittedly fictional. If there is specialization and division of labour – if agents specialize in what they are relatively best at doing – then every production process will have to be resupplied by other processes. Hence production implies some form of transfer of products, or rudimentary exchange.
16. Crusoe was a shipwrecked slave trader! The wreckage washed up on shore, and he was able to retrieve dried food, tools, knives, firearms, gunpowder, and many other things. So he was not thrown on his own resources – he had the tools and equipment of European civilization at his disposal. Using these, he was able to make Friday his servant – a relationship of domination, not exchange between equals. Economic calculation certainly served him well, but he was a well-trained, not an abstract, individual, and both his training and his endowments were products of a complex economy; cf. Hymer (1980).
17. As a thought experiment, imagine that babies were born fully developed. The result would be a society without education, training, apprenticeship or socialization. What kind of learning would be necessary, or possible? How could there be innovation? How could people adapt to changes? Anthills and beehives do not innovate.
18. We have already argued that the existence of 'synthetic' truths of reason implies that we will have to know something about the world in order to develop basic theory. 'High theory' cannot be done in isolation, relying on abstract postulates and mathematics. Even more upsetting to the conventional wisdom, armchair empiricism will not suffice either, for empirical studies will have to inform conceptual ones.
19. Ho looked into the everyday experiences and ideologies of Wall Street investment bankers, the everyday world of investment banking before the crisis. She describes how a financially dominant but highly unstable market system is understood, justified and produced through the restructuring of corporations and the larger economy. She delves into the roots of excessive risk-taking. She worked at an investment bank and shows

that bankers' approaches to financial markets and corporate America are inseparable from the structures and strategies of their workplaces; their mission is the creation of shareholder value, but their practices and assumptions often produce crises instead.

20. Bourdieu played a crucial role in the popularization of fieldwork in sociology. He sought to connect 'his theoretical ideas with empirical research, grounded in everyday life, and his work can be seen as sociology of culture' or, as he labelled it, a Theory of Practice. His contributions to sociology were both evidential and theoretical. Bourdieu's work continues to be influential. His work is widely cited, and many sociologists and other social scientists work explicitly in a Bourdieusian framework. One example is Yves Carro (1981) in economics. For an account of Bourdieu's vision and methodology, see Bourdieu (1984; 2005) among others.
21. It was based on his PhD thesis at the MIT Sloan School of Management. The thesis title is in itself significant: *The Manager at Work – Determining his Activities, Roles and Programs by Structured Observations*. The thesis was based on an idea shared by a professor at MIT and a senior manager in a company: they wanted to study the latter's work. It grew into a systematic observation and description of five general managers, about whom we know nothing more than the fact that they were 'efficient' and that they were subjected to the constant presence of Mintzberg, for one week each, every minute of their working day.
22. *The Economist* magazine (16 January 2009) pointed out that 'Mintzberg found that managers were not the robotic paragons of efficiency that they were usually made out to be. The pressures of his job drive the manager to be superficial in his actions – to overload himself with work, encourage interruption, respond quickly to every stimulus, seek the tangible and avoid the abstract, make decisions in small increments, and do everything abruptly'. The tremendous amount of work that Mintzberg put into the findings earned him the title of leader of a new school of management, the descriptive school, as opposed to the prescriptive and normative schools that preceded his work.
23. The NBER Project on Industrial Technology and Productivity was begun in 1994 with funding from the Alfred P. Sloan Foundation. It has three intertwined objectives. First, it seeks to foster research on the fundamental determinants of productivity improvement. Second, it encourages economists studying these issues to supplement their traditional theoretical and empirical research methods with direct observation of business firms and conversations with managers and workers. Finally, the project provides a framework for communication among economists, researchers from other academic disciplines, and policy-makers.
24. Given his approving stance towards realism, one might expect Blaug to be rather well-disposed to the proposed 'Cambridge revolution'. He approves of the classical economists and disapproves of attempts to treat them as precursors to modern marginalism. He strongly supports Keynesian economics and regards it as high practical. He is antagonistic to mainstream general equilibrium theory, regarding it, on the one hand, as too abstract to be of any practical use, but on the other as providing a misleading understanding of competition. All these points have been made at one time or another by supporters of the post-Keynesian approach. Yet Blaug rejects the Sraffian treatment of the classical economists, and wrote a furious, and some would say unfair, critique of *the* New Palgrave, which provided a modest forum for some neo-Ricardian views. Why? Perhaps because he regards marginalism as a serious and at least partly successful attempt to understand markets – a view that finds support here, subject to the proviso that the markets in question are historically specific.
25. Neoclassical analysis not only emphasizes behaviour, it takes a rigorous stimulus-response approach. Classical theory, by contrast, tends to analyse structure, adding along the way some, often ad hoc, behavioural assumptions, which may or may not involve optimizing. Each approach has strengths and weaknesses. For a comparison of neoclassical behavioural and classical structural models, see Nell (1998a, pp. 121–4).
26. *The Economist* magazine (15 August 2002) observed that 'surprisingly few economists visit the pin factories today. An exception is Alan Blinder of Princeton University for

his 1998 book, *Asking about Prices: A New Approach to Understanding Price Stickiness*. Blinder and his graduate students visited 200 American companies, to find out why managers are slow to raise and lower prices. The national bureau of economic research (NBER) went a step further. It launched its own "pin factory initiative", dispatching scores of economists to car makers, razorblade manufacturers and genes splicers, in a hunt for the source of America's productivity growth. The official statistics are not much better at measuring productivity. Back in 1987, the year he became a Nobel Laureate, Robert Solow complained of a "productivity paradox": the computer age was to be seen everywhere except in the productivity statistics. By the late 1990s, the statistics finally seemed to get it, apparently confirming a productivity miracle thanks to information technology. Since then, however, downward revisions have put the miracle in doubt. Paradox, miracle, or mirage? Little wonder some economists want to go out and check for themselves'.

27. Quoted by Susan Helper (2000).
28. See the website on fieldwork in economics, www.sticerd.lse.ac.uk/FIELDWORK/. Helper's (2000) paper is a one-page document.

In my approach, *I have insisted that there must be a theoretical basis for equation specification, and there must also be a close correspondence with reality*. There must be forecasting tests. *I think many of the present generation of researchers are not careful with forecasting tests and are not careful with reality, but are over-impressed with pure theory-spinning*, that isn't going to lead to significant improvements in the system [. . .] *I adhere to the view that a system that is not well conceived will not stand up under severe forecasting tests*.

Klein (in Mariano, 1987, p. 416, italics added)

[good] theory [. . .] rests on the consideration of a hereditary link, invariant in time and space, between the present and past evolution. [. . .] human societies, within very different contexts [. . .] behave in a similar way. Thus, the general study of our conditioning by the past may be founded on this basis, and the hereditary and relativistic formulation [. . .] may be used in numerous applications in all fields of the human sciences.

Allais (1997, p. 8)

I have long endorsed the views in Ragnar Frisch's (1933) editorial in the first issue of *Econometrica*, particularly his emphasis on unifying economic theory, economic statistics (data), and mathematics. *That still leaves open the key question as to which economic theory*.

Hendry (2004, pp. 759–60, italics added)

We cannot predict the future because the future will never be as before. We can prepare for it because, far from being inscribed in a book of destiny, *the future is uncertainty, bifurcation, unpredictable creation*.

Prigogine (1995, italics added)[1]

No complex system is ever structurally stable.

Prigogine (1974, p. 246, quoted by Vercelli, 1991, p. 43)

Stability is destabilizing.

Minsky (1986)

The difficulty lies, not in the new ideas, but in escaping from the old ones.

Keynes (1936, p. viii, quoted by Swann, 2008, p. 216)

11. Working with open models: lawlike relations and an uncertain future

INTRODUCTION

Throughout earlier chapters we assessed the critiques of structural econometrics, and we examined its foundations. We found the critiques important but lacking, and argued that economic laws – relationships between scientific variables – could be defined for economics, but only for limited historical periods. There are indeed lawlike relationships to be discovered, although, unlike the laws of natural science, they are limited and bounded by history and geography. Moreover, they can change as a result of changes in institutions and technology. (But the processes of such change can themselves be explored.) And some relations must always be inherently volatile.

Conceptual analysis and fieldwork provide a basis for defining the variables and hypothesizing the laws, while structural econometrics offered a method of estimating those laws, provided they were relationships of the reliable sort. We proposed the MTC as a methodology: fieldwork to establish relevance/applicability by coming to understand the concepts, rules and norms by which practice is guided; conceptual analysis to weave those concepts into theory; measurement and statistics in numbers that match the concepts of the (practiced-based) theory, making it possible to estimate the parameters of the lawlike relationships.

Econometrics must be informed by theory, but the theory cannot be abstract and axiomatic; on the contrary it must rest on conceptual analysis and fieldwork. Econometricians must know what they are talking about and must know it well enough to think it through and draw out all the presuppositions and implications. Then the different relationships that seem to be involved must be sketched out and separated into those that are or seem to be reliable and those that are inherently volatile; together, of course, with those that seem to be somehow made up of both reliable and volatile components.

We've put fieldwork and conceptual analysis at the centre of our proposed approach. Now it's time to look at some examples of the way this can help develop useful theory and improve econometric model building. Let's start with a focus on unemployment and inflation, two highly charged, politically sensitive issues, but ones where nearly everyone has some relevant direct experience – there is plenty of vernacular information to draw on.

11.1 UNEMPLOYMENT AND INFLATION

11.1.1 Some Implications of Fieldwork

Does unemployment reflect aggregate demand, or does Say's law hold? Is there 'involuntary unemployment'? The Keynesian idea is that unemployment exists because firms will not hire labour that is available and willing to work at an acceptable wage, because the goods and services they would produce could not be sold in current markets. There is a 'shortage of demand' in relation to available capacity and labour, and because of it workers are unemployed involuntarily. But given I and G and interest rates, C may be such that this 'shortage level' of demand will be 'equilibrium', in the sense that there are no forces operating to correct it. Interest rates may be under no pressure to fall, investment will not rise in the face of underutilized capacity and, in the absence of active policy, G will be steady. With unemployment and hard times, households are likely to cut back, so, if anything, C will tend to drift down.[2]

Mainstream economics, however, must argue that there is no such thing – there cannot be any such thing – as 'involuntary unemployment' in equilibrium, where equilibrium reflects optimizing subject to constraints. It is a consequence of the 'theorem of the alternative' that if the product market is in equilibrium, then, if a factor has a positive price, it must be fully utilized; if it is not fully utilized, its price must be zero. So in equilibrium there cannot be unemployed labour if wages are positive.

The mathematical point is correct, of course; but it certainly does not prove that there is no involuntary unemployment. It could be taken to mean, for example, that markets are generally or commonly in disequilibrium, but lack strong corrective forces. What appears to be unemployment is due to extended job search, or to unanticipated fluctuations in productivity. Many economists subscribe to such views without realizing the damaging implications. The mainstream theoretical concept of equilibrium rests on too many far-fetched assumptions to be operational; it must be proxied by something simple and practical. That has tended to be a stable position of the economy, in which the level of employment and output holds steady in the short run, and develops along the trend rate of growth in the long run. There are no forces pressing for change. But if such a position can be interpreted as a disequilibrium – because unemployment is 'high', even though wages are not falling – then what is to be the proxy for equilibrium?

The mathematical point could be taken to suggest a more radical position (and one we think is correct), namely that values are not determined by scarcity; that is to say, that prices do not result from the equilibrating interaction of relationships that depend on maximizing subject to constraints. This may sound shocking to some, but it has the support of Maurice Allais (1997, p. 7, based on his Nobel Lecture, 1988):

> Any theory whatever, if it is not verified by empirical evidence, has no scientific value and should be rejected. [For example] contemporary theories of general economic equilibrium are based on the hypothesis of general convexity of the fields of production, a hypothesis which is disproved by all the empirical data and leads to absurd consequences.

Yet the commitment to scarcity thinking and optimizing sustains the widespread belief among economists that most unemployment is voluntary; yet this idea has never been held widely among any other groups. Indeed, to deny the reality of unemployment (and its costs to families) may be considered a sign of ignorance of how the world works; it certainly suggests that fieldwork has not been done.

Now consider the causes and nature of inflation. Many claim that inflation is caused by excess demand driving up prices and/or wages. (This of course implies that inflation cannot co-exist with unemployment.) Others consider inflation to be a collapse in the value of money resulting from an excessive supply. Finally, observers of households and industry argue that inflation – persistently rising prices – is due to rising costs of business and/or rising costs of living. (But this might be considered just a description, not a contribution to explaining anything.) Again, the idea that inflation is due to excessive issuance of money is probably more common among economists than in the general public. Most people know that costs and demand have something to do with inflation.

It is our position that these views – that there is no involuntary unemployment, that the driving force behind inflation is chiefly issuing too much money – cannot be sustained in the face of serious fieldwork. It is true that under the conditions of the craft economy both views were reasonable – not exactly Say's law, but there was a (weakly) stabilizing price mechanism (flexible prices and inflexible employment), while excessive issues of money, lowering interest and stimulating spending, might well drive up prices in the short run. Today, in parts of the developing world, the conditions of the craft economy can still be found. So there is some justification for the fact that these ideas are still widespread. Moreover, many economists doubt that there is a single principal explanation for unemployment or inflation; instead, we should expect multiple causes, so Say's law and monetarist explanations could play a role. On this view, the important job would be to assess the respective contributions of these and other possibilities. By contrast, in our view, good fieldwork will rule out these positions (and very likely others as well) for advanced economies.

11.1.1.1 Fieldwork provides direct knowledge

We argued in Chapter 10 that fieldwork is serious investigative work; it is a kind of detective work to find out what people are really thinking and really doing, bearing in mind that they may be trying to deceive not only you, the observer, but also themselves. (We go to church almost every Sunday, and give generously to charity.) Fieldwork calls for looking at budgets and accounts, checking statistics, studying court records, government documents, contracts, administrative rules, normal procedures, and what people believe to be the norms and traditions of society. It requires talking to people to find out how they understand and interpret the everyday procedures of their lives – what do their actions mean, what do the numbers mean? This must be done with a scientific attitude, not with the aim of reaching a pre-ordained result. Every effort has to be made to control bias and lay bare unconscious presuppositions, points well understood by anthropologists. The result should be a rough picture of how things work.

So, let's take on the big issues. Keynesians argue that fluctuations in aggregate demand are endemic (and do not necessarily indicate a disequilibrium), and that, in the modern world, business adapts to these fluctuations by adjusting employment. In the nineteenth century, fluctuations in demand led chiefly to changes in prices, followed only later by smaller changes in employment (Marshall and Marshall, 1879). Employment is chiefly determined by what happens in the product market. Traditional theory asserts Say's law, that once the supply side of the economy is properly specified, it will automatically generate the appropriate level of demand. Fluctuations in final demand are not the cause of variations in employment. On the contrary, according to the 'real business cycle' approach, variations in employment reflect short-term variations in productivity, usually not matched by appropriate wage adjustment.[3] On this view, of course, employment is determined chiefly in the labour market, rather than responding to the demand for goods and services.

To begin with, consider the question of whether some or most unemployment is 'voluntary' or 'involuntary', where the latter means that the worker would be willing to take a job even at a level of pay below the current market rate. This calls for interviews with the unemployed themselves, collecting not only statements, but their stories, then interviewing their previous employers, employment agencies and prospective employers, to discover why employers have downsized or laid off workers and what would make them willing to start hiring again. The results of course would show that many unemployed workers (during hard times probably most) are willing to work at or below prevailing wages, have spent time and effort looking for a job, and can't find one because business hiring is slow. We know this from Keynesian macroeconomics, obviously; the point is that it can be confirmed by a completely independent body of evidence gathered by fieldwork. Furthermore, most of us know it as part of 'vernacular knowledge' – we know people who were laid off and who had a terrible time finding another job. We know businesses that have had to lay off workers, an action both sides would have preferred to avoid.

In the same way, we know from ordinary macroeconomics that declining sales, due to declining aggregate demand, causes businesses to lay off workers. Demand does not always stay at the level of full employment; it fluctuates and this causes changes in employment, layoffs in bad times, extra hiring in booms. Again, interviews with managers, checking personnel records, and interviews with union officials will confirm this. Moreover, most of us from personal experience know of a shop or business that has had to shut down or lay off employees because of sagging sales. The alternative explanation offered by 'real business cycle' theorists, that employment and output levels fluctuate because of technology and productivity 'shocks', will not fit with what personnel managers say, nor will it accord with what the engineers say. Layoffs may increase but the actual technology has not changed.

Now consider the claim that inflation – rising prices and money wages – is caused by the excessive issuing of money. First – issued to whom? And what do they do with the money? If they just sit on it, how can it affect prices? But if what they do with it is what affects prices, then inflation is not just a matter of money – it depends on the way the money is used.

Next, we all know from personal observation and experience that businesses raise prices when their costs go up, and workers, households and unions call for pay rises when the cost of living goes up. Again, this is vernacular knowledge, but it can be supplemented, expanded, made more precise and generally extended by fieldwork examining how and why businesses respond to higher costs, and how households react to higher costs of living. Suppose there is an external shock, for example the price of oil is doubled. These two relationships alone would be enough to make up a wage–price spiral. And credit could be expanded without necessarily issuing more (high-powered) money. There is no excuse for continuing to claim that inflation is always or chiefly or predominantly caused by excessive monetary issue.

11.1.1.2 Fieldwork and explaining inflation

Indeed, mainstream economics has not done a good job with inflation. For that matter, it has not done well in general, integrating the monetary side of the economy with the real. The problems of the large econometric models in the 1970s were less in estimating than in specifying relationships and model structure, particularly the relationships between the real and the financial aspects of the economy – very closely related to the reliable/volatile distinction. This was particularly true of production; fixed coefficient models held up well, but neoclassical marginal productivity models turned out to be deeply flawed – they purported to estimate production, but actually captured cost identities (see pp. 412–14). Yet the estimations of many particular relationships often proved sound; parts of the models held up throughout. These were the reliable functions – household consumption spending on various categories of goods and services, labour productivity in the different sectors, import propensities, multiplier relationships – all of which generally came through OK.[4] In these functions the 'targets' are well-defined – we know what the process is aiming at or trying to achieve – so there are good reasons to expect certain mean values. And the forces or pressures that bring about 'misses' are also well understood; we know what gets in the way of achieving the targets, or causes deviations.

But monetary and financial relationships did not fare so well. Investment and interest rates proved troublesome, and the stock market elusive; but inflation was the killer. The big models tended to get it seriously wrong. For the models were built around the Phillips curve, or the *NAIRU* (non-accelerating inflation rate of unemployment), which made it difficult to accommodate 'stagflation' – meaning the simultaneous development of unemployment and inflation, including cases where, as inflation increases, stagnation deepens, and vice versa.

Yet this should not have been such a surprise. The Phillips curve is not robust econometrically; it does not test well and, indeed, Nancy Wulwick (2001) has argued in detail that the original version would not pass elementary methodological scrutiny today. Many different versions have been estimated, and the results are not generally compatible. Neither the reasons for target levels nor the forces causing deviations have been clearly specified and well-defined. The original version rested on the view that all inflation is demand-driven; lower unemployment reflects a spending boom that puts greater pressure on markets, so prices/wages tend to be driven up. The cost of lower unemployment is higher prices. Early Keynesian thinking considered this reasonable, even though the evidence was not very strong. Moreover, it was not clear that this established a relationship between changes in demand (unemployment) and inflation. The change in demand and employment might be permanent, but the corresponding change in prices might be a one-off, the consequence of adjusting to a higher level of capacity utilization.

But critics like Friedman and Phelps shifted from Keynesian to pre-Keynesian thinking, and argued that causality ran from inflation to unemployment, through the labour market, and further that the trade-off was only valid for the short run. Instead of arguing that lower unemployment means higher demand pressure, bringing about inflation, the critics contended that higher inflation (caused by increases in the money supply) leads to temporarily lower real wages, leading business to hire more labour. But money illusion is only temporary; labour will soon realize that real wages have been lowered and will demand higher money wages, perpetuating the inflation, but leading unemployment to rise once again to its natural level. So reducing unemployment markedly by policy must lead to a permanent rise in inflation; but the unemployment rate would not remain permanently lower. It would rise again, to a 'natural' rate. In the same way, if it were too high, it would eventually fall to the 'natural' rate. At this 'natural' unemployment rate, however, there would be no tendency for the inflation rate to either increase or decrease. This rate of unemployment was christened the 'NAIRU' (see above).

From our point of view, this entire line of argument is defective: it has two flaws, each of them fatal. First, it assumes that changes in the money supply, controlled by the Federal Reserve (in the USA), will move nominal aggregate demand more or less in the same proportion. Careful study of how the Fed actually works shows that it does not have that degree of control over money (Moore, 1988; Nell and Bell, 2003; Nell and Forstater, 2003; Wray, 2003); and money does not govern nominal demand (even the correlations are unreliable, and critics contend that Monetarist causality has never been adequately explained (Nell, 1998a; Robinson, 1980). Second, it is based on the neoclassical picture of the labour market. Fieldwork should tell us that, over most of a modern economy, 'marginal products' in the required sense cannot be identified; that is, outputs cannot be identified and attributed in the required manner. Moreover, marginal costs are constant or falling; employment is adjusted to sales or to inventories. The neoclassical falling demand curve for labour simply does not exist. Conceptual analysis then says that the model of the economy must show employment being determined by the level of demand in the product market in conjunction with the output function. These relationships are measurable. The neoclassical labour market is conceptually defective. Let's sketch an alternative.

Instead of a Phillips curve or a NAIRU, consider a wage–price spiral (Nell, 1988; 1998a). There are two relationships here (Flaschel et al., 2008, ch. 3). The first shows the ability of workers, unions and households to push up wages in response to a rise in the cost of living. If the cost of living goes up by x per cent in period 0, the wage will be driven up by ax per cent in period 1, where $a \geq 0$.

$$(dw/w)_1 = a\,(d\pi/\pi)_0,$$

where w is the money wage and π is the level of prices (different from the Π used earlier, which reflected the value of money, so took money wages into account).

The second equation shows the response of business to a rise in labour costs. If wages rise by x per cent in period 0, then business will impose a price increase of bx per cent in period 1.

$$(d\pi/\pi)_1 = b\,(dw/w)_0$$

so

$$(dw/w)_1 = ab\,(dw/w)_{-1}$$

and

$$(d\pi/\pi)_1 = ab\,(d\pi/\pi)_{-1}.$$

(Example, $a = 2/3$, $b= 3/2$. Steady inflation, but every period the real wage falls.)

When $ab = 1$, the system exhibits steady inflation; when $ab < 1$, inflation converges; and with $ab > 1$, inflation is explosive. Each of these relationships is solidly grounded in institutional practice. Businesses have to defend their profits, and households their standard of living; in each case an uncompensated rise in costs will create difficulties in meeting obligations. Each therefore will seek to pass along the rise in costs. But their potential success in doing so, indicated by coefficients a and b, cannot be assumed a priori, but must be examined case by case.

The effects of this can be seen by looking at the accounting identity,

$$Y = (w/\pi)N + rK.$$

When differentiated, this gives us

$$dr = wN/K\pi[d\pi/\pi - dw/w].$$

The rate of profit increases or decreases according to whether price inflation is greater or less than money wage inflation. The impact of inflation is on distribution, as in Kaldor's approach, and can therefore be expected to generate feedback effects on aggregate demand.

Notice that we are here modelling both money wages and prices on the basis of normal costs, costs of living for households, and costs of producing for firms. Each is assumed to try to pass along any increases in such costs. The equations shown here simply model the basic idea; by contrast, fieldwork suggests that the equations to actually estimate the inflationary process should contain additional terms for technological change, for feedback effects from changes in distribution and from changes in inflationary expectations (Rose effects and Mundell effects).

A more problematical issue concerns whether the two equations could also be written as reflecting demand pressures, rather than costs. The wage equation would then show the money wage rising with demand pressures as these caused deviations from the normal level of unemployment (NAIRU), and the price equation would show prices rising with the deviation from the normal level of capacity utilization (Flaschel et al., 2008, ch. 3, esp. 3.2). But we think the wage–price spiral is largely a cost-driven spiral; the chief effect of demand on inflation comes through driving up the demand-sensitive prices of primary products. When this does not happen, even very strong demand pressures do not generate much inflation, vide the late 1990s in the USA. (Of course, this is an empirical identification problem.)

In contrast to either version of the wage–price spiral, the monetarist story rests on an assumed 'demand for labour' curve, derived from an assumed production function with diminishing marginal products. These concepts have no grounding in contracts, and both appear to be inconsistent with mass production technology. The supposed reaction of business to inflation – hiring more workers – rests on this labour demand curve; we have argued that no such curve exists. Furthermore, the decisions to hire, offer labour and produce are typically made on the basis of assumed 'mistakes' about the level of the real wage by one or another party. In the short run, these mistakes supposedly generate a Phillips curve; but the system eventually returns to its natural position, where the long-term real wage is given by the marginal product at the natural rate of unemployment. In our view, neither the Phillips curve nor the 'natural rate' exist.

What are the benefits of our suggested approach (bearing in mind that it's not so simple, as the equations to be estimated in an actual study will contain a number of additional terms)?

1. A wage–price spiral model can accommodate stagflation easily; when price inflation is greater than wage inflation, the real wage falls from period to period; this reduces consumption spending, and

aggregate demand falls, so unemployment rises, bringing a tendency to stagnation – if anything, this would weaken the ability of labour and households to pass on the cost of living. But while this might weaken wage inflation, it would not necessarily weaken the ability of firms to pass along increases in costs. Hence we have stagflation.

2. We can generate an explanation for the apparent success of the Phillips curve. Consider a case of inflation triggered by an external shock, say an increase in oil price. But in this case assume that unions are strong and firms relatively weak in their ability to pass on cost increases. So wage inflation will be greater than price inflation, and this will lead the real wage to rise, stimulating consumption, with the result that aggregate demand increases; so unemployment will fall as inflation progresses. Inflationary expectations will develop, which will also encourage demand. The faster the inflation, the stronger the demand stimulus, so inflation not only leads to a rise in employment; accelerating inflation leads to faster increases in employment. In short, we have an inverse relation between unemployment and inflation. This looks like a Phillips curve, but it is not one. Both of these 'pass-along' relationships depend on there being a broadly positive relation between real wages and employment, running through the effect of real wages on consumption spending, and impacting aggregate demand through multiplier re-spending. By contrast, mainstream theory operates with a labour market based on supply and demand curves, according to which real wages are *inversely* related to employment and output. This, in turn, results from the assumption that there is a 'well-behaved' aggregate production function – which brings us to another controversy! But this time the issue is not so much facts about how the world works, since at first glance empirical studies appear to support the production function. Instead we face a conceptual tangle that has to be unravelled.

11.1.2 Examples of Conceptual Analysis

11.1.2.1 Production functions

Production functions are fundamental to conventional theory; prices, for example, are supposedly set in the light of variations in marginal costs, and real wages are thought to reflect variations in marginal productivity. Marginal productivity, in turn, together with factor supplies, supposedly determines the relative shares of labour and capital, and, in competitive conditions, their prices (rates of return). But when it comes to facing facts, the neoclassical story looks like a fairytale. Evidence from interviews, together with observation of business practices, suggests that prices are set by reference to the development of markets, the possible or actual actions of competitors, and to cover normal costs. But marginal costs do not generally play a role. As for real wages, physical marginal productivity is either impossible to measure (think of white-collar work, or service industries), or appears as a matter of fact to be constant over a wide range, so cannot play much of a role (Nell, 1977; 1992c). Even worse, marginal products are often inferred from cost data. Mainstream economists simply assume that there are marginal products; they exist, although we may have difficulty in measuring them. But do they? What exactly does it mean, to say that the marginal product of labour in a certain industry or service exists?

To claim this, it must be assumed that the processes of production in a modern economy (including mass production and high-tech industries) can be modelled by a continuous production function with positive first and negative second derivatives – that is, showing diminishing returns.[5] Theory requires that different points on the function represent different technologies, the consequences of substitution. Varying intensity of use of given plant and equipment – output changing with the level of employment – does not generate points on the theoretical function, as Hicks (1932, p. 20) pointed out. But this is what we get from empirical data. Moreover, studies of costs in mass production economies tend to show that costs are either constant over a large range, or that they tend to show economies of scale and fall. Evidence goes back to the Oxford Studies of the 1930s and has been confirmed over and over again (Hall and Hitch, 1939; Andrews, 1949; Gordon, 1983; Lavoie, 1992). This indicates that it would be more accurate to assume constant or increasing returns. Increasing returns are frequently found in network systems, which are widespread in high technology firms.

Nevertheless, the economics profession appears to be wedded to neoclassical production functions with diminishing returns, generating downward-sloping demand curves for labour, and rising marginal cost curves for firms.[6] Why such support for a fairytale? One reason is that it appears to be easy to 'estimate' such functions empirically, using simple regression techniques, and that the functions so estimated seem to have a good fit, and to be statistically significant. This is a good example of what is wrong with a great deal of mainstream econometrics.[7] When looked at carefully, what is actually being measured seems to be quite different from what the investigators think they are estimating. The evidence actually matches a linear cost function, and is mistakenly used to estimate a constant returns production function. Felipe and Adams (2005) re-examine Douglas's initial calculations, using his data set and conclude that all the aggregate Cobb–Douglas function regression captures is the path of the value added accounting identity according to which value added equals the sum of the wage bill plus total profits. The Cobb–Douglas form is simply derived as an algebraic transformation of the identity (ibid., p. 430).

This critique goes back to Phelps Brown (1957), but was developed more fully by Herbert Simon (Simon and Levy, 1963; Simon 1979), and later by Anwar Shaikh (1974; 1980). Shaikh (1980, p. 92) wrote:

> these (supposed) empirical results do not, in fact, have much to do with production conditions at all. Instead [. . .] when distribution data (wages and profits) exhibit constant shares, there exist broad classes of production data (output, capital, and labor) that can always be related to each other through a functional form which is mathematically identical to a Cobb–Douglas 'production function' with 'constant returns to scale', 'neutral technical progress' and 'marginal products equal to factor rewards'.

Essentially, if shares of wages and profits are constant, the estimation will almost always 'work'; that is, a Cobb–Douglas can be fitted and the elasticities will measure relative shares. Examples have been given using data sets in which there is no marginal variation, or which have been generated by completely different processes (for example, dots spelling out the letters of the word HUMBUG), yet with constant shares, production functions can be fitted, and generally seem to have all the desirable properties. (The procedure will also work for the CES (constant elasticity of supply) and other functional forms, though perhaps not quite as well.)

The basic point can be stated simply; differentiate the income identity with respect to time:

$$Y = \mathbf{w}N + rK,$$

where $\mathbf{w} = w/\pi$, the real wage. Then

$$dY/Y = [\mathbf{w}N/Y]d\mathbf{w}/\mathbf{w} + [rK/Y]dr/r + [\mathbf{w}N/Y]dN/N + [rK/Y]dK/K.$$

Let $\alpha = rK/Y$ be capital's share, so that $(1–\alpha)$ is labour's share. Then we have

$$dY/Y - \alpha[dK/K] - (1 - \alpha)\, dN/N = \alpha(dr/r) + (1 - \alpha)(d\mathbf{w}/\mathbf{w}).$$

Two empirical assumptions, both more or less stylized facts, come into play at this stage. The first is that shares are constant, and the second is that wages grow at a steady rate while the rate of profit stays constant (features of the Victorian equilibrium). (All that is necessary is that the weighted average be constant.) Then the right-hand side of the above equation will be a constant, call it λ and we have

$$dY/Y = \lambda + \alpha\,[dK/K] + (1 - \alpha)\,dN/N$$

which can be integrated into a Cobb–Douglas (see Temple, 2006).

Note that λ, the weighted average of wage and profit growth, may be a function of time, which is usually interpreted as technical progress. We have assumed it to be constant here, but if there is a time trend, a functional form will have to be specified. If the form chosen is incorrect, this will undermine the estimation.[8] But a good or 'correct' choice (a non-linear form or 'adjustments' to the capital stock) will capture the variation in λ, and improve the fit of the 'production function' (Felipe and McCombie, 2005, pp. 473–4).

Interestingly, constant shares are not necessary if the estimation is being made with wage and profit data, as Nell pointed out in 1977. An estimation made from cost data could provide an apparent fit if the data were derived from fluctuations in wages and profits arising from a wage–price inflationary spiral, of the kind we have just examined.[9] In such a spiral, the data will fulfil a condition that appears to imply that the rate of profit equals the marginal product of capital. From the well-behaved neoclassical production function we can derive an expression for the wage–profit trade-off condition:

$$d\mathbf{w}/dr = -K/N.$$

This is derived as follows. Start with:

$$Y = Y(K, N) => y = y(k).$$

from Euler's condition

$$\mathbf{w} = y(k) - ky'\,(k) \text{ and } r = y'\,(k).$$

Then

$$d\mathbf{w} = y'dk - ky''dk - y'dk = -ky''dk, \text{ and } dr = \mathrm{y}''dk$$

so

$$d\mathbf{w}/dr = -k; \text{ that is, } dr/d\mathbf{w} = -N/K.$$

This is a feature of a constant returns to scale production function, and is basic to marginal productivity theory, since it is necessary if paying factors their marginal products is always to add up to output.

But this relationship *also* characterizes the income identity, since

$$Y = (w/\pi)N + rK,$$

which, when put in per capita form, differentiated and rearranged, gives

$$dy = kdr + rdk + d\mathbf{w},$$

and this implies that the rate of profit equals the marginal product of capital,

$$r = dy/dk, \text{ if and only if } d\mathbf{w}/dr = -k, \text{ that is, } -K/N.$$

However, going back to the accounting identity, breaking the real wage into the money wage and the price level, and differentiating, we can derive

$$dr = wN/K\pi[d\pi/\pi - dw/w].$$

The change in r depends on the difference between the rates of price and wage inflation, the central issue in our model of the wage–price spiral. Rearranging and recalling the expression for the differential of a ratio,

$$dr/d(w/\pi) = -N/K.$$

This can be rewritten as

$$Nd(w/\pi) = -Kdr.$$

This means that value is conserved when wages decline and profit increases, or vice versa; a change in the distribution of income does not alter the amount of the income being distributed. Arguably, this is an essential property of a capitalist economy; and, since it follows directly from Euler's Theorem, it is an essential property of the neoclassical production function. Yet data generated by Keynesian inflation – a wage–price spiral – will also exhibit value conservation! That is, the data so generated will meet this same condition, and therefore will 'fit' a production function, even though there may have been little or no actual variation in output, and no marginal products exist.

This misidentification has appeared over and over again, in the best places, usually without the problem even being recognized. It begins to look something like an intellectual scandal – but unfortunately, it is not the only case of misidentification due to an uncritical assumption that standard theory must be correct.

11.1.2.2 Conceptual analysis and the money supply

Let's go back to the money supply. Monetarists and many of their supporters have argued for years that central banks deliberately expanded the money supply so as to create inflation, in order to bring down unemployment. But fieldwork at the time, and transcripts of meetings since, made it clear that the Fed and the Bank of England did no such thing. First, they did not have sufficient control over the money supply to set and hold to a target rate of expansion (Goodhart, 1989; Moore, 1988; Nell and Bell, 2003). Second, what they actually tended to do, most of the time, was to fix the overnight interest rate, and this is not consistent with attempting to meet a quantitative target. Central banks in the modern era do not and cannot fully control the money supply. This is partly because, ever since World War II, monetary institutions have been arranged (in the USA and the UK) so that when the government spends, it creates money, adding to the money supply; while when it collects taxes, it reduces the money supply[10] (Goodhart, 2003; Nell and Bell, 2003; Nell and Forstater, 2003; Wray, 1998). And it is partly because banks are in business to make money, to respond to market incentives, that they lend money and expand the money supply in a boom, and contract in a slump. Such procyclical behaviour poses a problem for policy-makers.

In short, fieldwork tells us that at least to some extent the money supply is endogenous. Conceptual analysis shows that this may be significant. Let's take two simple general relationships, one concerning money, the other pricing and distribution, and consider what endogenous money implies.

The quantity equation is:

$$MV = \Pi Y$$

and the aggregate markup equation is:

$$\Pi = kwN/Y$$

where M is money in circulation, V is velocity, Π is the price level, and Y is aggregate output; w is the money wage, N is total employment, and k is the aggregate markup.

Differentiating the quantity equation and dividing by it gives

$$d\Pi/\Pi = dM/M - dY/Y.$$

Treating the markup equation the same way gives

$$d\Pi/\Pi = dw/w - d(Y/N)/(Y/N).$$

The first seems to tell us that inflation is due to the failure of central bankers to control the money supply; the second that it is due to pressure from the unions or due to excessive tightness in labour markets.

Now consider the implication of the fact that the money supply is endogenous: for simplicity write:

$$M = wN.$$

Then,

$$MV = \Pi Y \rightarrow wNV = \Pi Y, \text{ or } \Pi = V(wN)/Y.$$

But this is the markup equation with V in place of k! If we make the same substitution in the markup equation we work back to

$$Mk = \Pi Y.$$

So $V = k$. The quantity equation appears, in effect, to be a disguised form of the markup equation. Joan Robinson (1980) charged that monetarists had no independent definition or measure of 'velocity', but this suggests that 'velocity' reflects or depends on the distribution of income. On reflection this is not so far-fetched; if M is endogenous, it will depend on spending, which in turn is strongly influenced by distribution.

As a result we can say:

1. When money is endogenous, the money supply will adjust to the level of employment, and will tend to be proportional to the wage bill.
2. The concept of the velocity of circulation is not well defined in mainstream thinking. But the markup equation is well defined. When money is endogenous, the quantity equation and the markup equation turn out to be equivalent.
3. So changes in the 'transactions demand for money' turn out to reflect changes in the markup. For example, if price inflation outruns wage inflation (as it did in the 1970s), the general markup will increase, and the (apparent) 'demand for money' will be reduced (Nell, 1998a).

The monetarist account of the behaviour of modern money is far from the truth. Nor is their account analytically coherent, for they have never been able to provide a precise, quantifiable definition of 'velocity'. But the problem is not only one for monetarists; many Keynesians have also worked with a 'demand for money' function. Even a researcher as sophisticated as Hendry has uncritically accepted the 'demand for money' framework, for use with data in which money wages are not moving closely with prices – implying changes in distribution (Hendry, 1995a, ch. 16). The preceding account, even if oversimplified, suggests that we might benefit from a closer look at velocity in relation to aggregate demand theory (Nell, 2004).

11.1.2.3 Multipliers and the quantity equation

Income can be paid in money and shown to equal the value of output in circulation:

$$Y = W + P = W_k + W_c + P_c + P_k = Y_c + Y_k.$$

W is the total wage bill, P total profits, n is the coefficient indicating labour per unit output, c and k are subscripts indicating the consumer goods sector and the capital goods sector, respectively.

Now let us assume that the labour coefficient will be the same for every subsector in consumer goods. Then

$$W_c = Y_c - P_c = W_k(1/(1-\mathbf{w}n_c) - 1), \text{ a simple multiplier relationship.}$$

Since

$$W_k = P_c,\ Y_c = W_k(1/(1-\mathbf{w}n_c)).$$

Next, assume that the labour coefficient will be the same in all subsectors in capital goods, and further assume that the machine tool subsector is vanishingly small. Then the first subsector receives P_c $(=W_k)$ in revenue from its sales of capital goods to the consumer sector. It withdraws $\mathbf{w}n_kP_c$ to repay its loans, and spends $(1-\mathbf{w}n_k)P_c$ purchasing its replacements and new capital goods from the second subsector. This second subsector will withdraw $\mathbf{w}n_k(1-\mathbf{w}n_k)P_c$ and spend $(1-\mathbf{w}n_k)(1-\mathbf{w}n_k)P_c$. The resulting sequence, taken to infinity, will sum to $(1/\mathbf{w}n_k)P_c$. But this is Y_k, since $\mathbf{w}n_kY_k = W_k = P_c$. So we have:

$$Y = W_k(1/(1-\mathbf{w}n_c) + 1/\{\mathbf{w}n_k\}) = W_k(\{\mathbf{w}n_k + (1-\mathbf{w}n_c)\}/\mathbf{w}n_k(1\mathbf{w}n_c)),$$

that is $Y = W_kV$.

Y is income expressed in real terms; the right-hand side shows the sum required for circulation, in units of account, multiplied by the sum of the multipliers for the two sectors, showing how that sum circulates. This expression may be considered the 'velocity of circulation'.[11] Velocity, then, consists of a sum of two multiplier expressions, each of which is based on distribution – the real wage and productivity. Hence velocity will reflect the average markup, as has been noted before (Nell, 1998a).[12] Each of the multipliers in the expression for velocity sums up a process of re-spending that traces out the pattern of vertical integration for a sector. And the process of circulation as a whole depends on the condition that $W_k = P_c$. But that condition also underlies each step of the circulation in the consumer goods sector. In the same way, the condition $P = I$ underlies the vertically integrated circulation in the capital goods sector. This shows that a sum of money equal to W_k will 'circulate', monetizing all the transactions in the economy.

This provides the foundation for a precise account of the quantity equation, giving a mathematical foundation to the idea of endogenous money. When there is a supply function for the money article, it will relate the amount M to the real unit costs of that article at varying levels of outputs, and that cost will be the value (cost of production) of money, $1/\Pi$. Then, from the above, we know that the amount of money required for successful and complete circulation is equal to W_k. From this we can derive the Quantity Equation by substitution, for when we have a given amount of money, equal in value to W_k, so that,

$$M/\Pi = W_k$$

(where M is the amount of the money article, and $1/\Pi$ is its value), then it follows that

$$MV = \Pi Y.$$

This equation will also hold for pure credit money, issued by banks, so long as such money is generally accepted and comes with a pre-determined value – even though there is no supply function based on cost-minimizing. When its value is given, the quantity of M adjusts to the conditions of circulation; it is endogenous. The circulation described here will monetize all transactions in the economy, and return to its starting point, ready for the next round. This account explains 'where the money to pay profits comes from', a question posed by Marx, Wicksell (1898), many Keynesians, some monetarists, and the French 'Circuit School' (Nell, 1967; 1998a; Arena and Salvadori, 2004).[13] But the main point is that this establishes that monetary analysis based on the quantity equation and the theory of aggregate demand are essentially one and the same. The implication is that understanding money requires understanding the long term pattern of changes in an institution that has developed more or less independently of our understanding of it. This has consequences for modelling financial markets as well as for inflation.

11.2 UNCERTAINTY AND 'ANIMAL SPIRITS'

11.2.1 Implications of Rational Choice Theory

Rational choice theory obscures – worse, in some ways obliterates – any distinctions between the behavioural functions. All are supposedly the results of constrained maximizing in the face of parametric variations. Of course, in some cases, the background parameters will be well-known; in others, they may not be. Nevertheless, the behaviour is understood to be planned; it is all forward-looking, with different degrees of ignorance and risk. All economic decisions are seen as choices in a plan over time. No current activities rest straightforwardly on today's obligations and current knowledge; today's activity was planned yesterday or earlier. All future economic activity will follow a plan that starts today and stretches out to the horizon. Consider the basic macroeconomic variables. Consumption is held to be planned over the lifetime of a household – saving is planned for old age and to leave bequests, and the pattern of consumption will include investment in human capital. Investment by business will be planned over the lifetime of a firm or of a plant and equipment. Money is held in the light of what may happen up to some horizon, as are other assets; a job will be taken now, in the light of a choice not only between work and leisure, but between work now and work in the future. Firms are likewise supposed to offer employment in the light of decisions to produce now or in the future. Plans are based on calculations starting now and ranging over the indefinite or even infinite future. What is expected in the future determines what we do now. No economic activities depend only or even primarily on the circumstances of today – except in so far as those circumstances determine our views of the future. (As we shall see, this is not abstracting from what we see around us; it is an idealization of the world, and provides a very different picture of decision-making than we get from fieldwork.)

11.2.1.1 The case of the missing present

Intertemporal rational choice theory presents a very curious picture of the world, in which the actual circumstances of the day – the way things are right now – has no influence on the course of events, except in relation to expected future circumstances. What we do is determined as a trade-off between the present and the future – the difference between the present (prices, incomes, interest and profit rates, etc.) and the future matters, but the present itself doesn't. Everything depends on what is expected in the future; but when the expected future arrives, it will no longer have much influence! Since all behaviour is guided by intertemporal optimizing, everything is planned, the plans are made on the basis of expectations and, of course, these expectations are appropriately discounted. So household demands for goods and supplies of labour, on the one hand, and business demands for labour and supplies of goods, on the other, will be set in the light of the relationship between the expected future prices, interest rate and so on, and those of the present. The present – the complex of current prices, interest rates, output, profits, employment and so on – by itself determines nothing.

Yet in our own lives we constantly feel that the way things are now, right now, has an enormous influence on what we do. We have contracts to honour, we are under obligations, we owe debts, we are expected to meet the duties of our social positions, and live up to the expectations of our families and friends. These pressures exist regardless of what we think the future holds, especially in the light of the fact that, whatever else we may think of the future, we are quite sure that it is uncertain. Vernacular knowledge and fieldwork tell us that many, perhaps most, economic decisions about what we do today, and also tomorrow, are based on the data we have at hand today. We know what we are currently doing most of the time and our expectations about what we are doing are usually reasonable. It is different when we look ahead. We know that the future is uncertain and that very often the factual basis of our expectations is such that we cannot have much confidence in them.

The mainstream approach takes a good point – that the present depends on the future – and carries it to an unreasonable extreme. Practical reasoning, backed by fieldwork and econometric evidence, suggests that household consumption (and saving decisions) involving the spending of current income (as opposed to income from capital), will be made largely on the basis of current circumstances, current knowledge and current obligations. Similarly, decisions to take jobs or to offer employment are largely dependent on current circumstances – pending sales and orders in hand – not on anticipated and uncertain future events. Of course, current circumstances also rest on expectations – that agents will finish what they have started, that the exercise of skills will result in the right result, that norms will be followed, and so on. These expectations, in turn, will give rise to still further expectations on the part of other agents, etc. But such expectations are grounded in the socio-econonomic system, and sometimes in specific further contracts or promises. They are tethered to the working of the system; we can call them 'tethered expectations'. By contrast, expectations of future markets or future profits, or of the future movements of asset prices, are not grounded; there is usually nothing or very little on which to ground them. They are floating free, wholly or partially, and can be considered 'floating expectations'.

Decisions and activities grounded in current circumstances tend to be reliable. The corresponding expectations are 'tethered'. But that leaves the future open. As a direct consequence, spirit, morale, exuberance and depression, what Keynes called 'animal spirits', make up an essential aspect of economic activity – in production, consumption and marketing – which is well recognized in business studies, but usually overlooked in economic theory. When animal spirits are high, so that managers and workers throw themselves into a project with gusto, productivity will improve, deadlines will be met, quality will rise. This is not surprising: we are all familiar with this phenomenon in daily life, in sports, in competition generally. Business people know it well.

But rational choice theory assumes that the parameters of activity are fixed. We are supposed to make choices between activities that have precisely defined outcomes. It is assumed that how we feel about these choices has no relevance. But in fact this is almost never the case. Outcomes depend on the morale and energy with which economic activities are carried out. This is part of the volatility in the economy. Of course, in some activities, energy and exuberance only matter a little; in others they may matter a great deal. Activities of the first sort will tend to be reliable; those of the second, volatile.

In general, we can say that there are three states of animal spirits: exuberance, neutrality, despondency. Neutrality is inherently unstable: it tends to drift either towards exuberance, if things are going well, or despondency, first passing through boredom, if things are the same time after time, and nothing new happens. These states are contagious. If some producers are exuberant, others will catch the feeling. If there is despondency among some, it will spread. The contagion will tend to follow a sigmoid path, a well-known finding both in studies of diseases and in social psychology.

In activities connected to reliable relationships and involving reliable variables, we can sometimes detect a major swing in mood towards exuberance or despondency, in which case we can introduce an adjustment. In the absence of a clear-cut pattern it would be reasonable to provisionally apply a normal distribution of positive and negative feelings, and work with standard stochastic methods. Effectively this assumes that positive and negative animal spirits will balance out, and that the mean value can be taken as representative.

But in the case of volatile relationships and volatile variables this will not do. (Examples are: investment and interest rates; investment – the MEC – and anything, the stock market and growth, or employment, inflation and unemployment, the price level and money.) The very existence of a volatile relationship, as well as its form and strength, may depend on the state of animal spirits in the market. Variables such as investment, the price level, the quantity of money, an index of the level of stock prices can move unexpectedly and for reasons that can only be ascertained later.

Optimism and pessimism should not be thought of as lying on a line, so that to move from one point to another it is necessary to traverse the points in between. On the contrary, the economy can jump from one extreme to the other. Such switches represent changes in the general mood of the economy, and have wide-ranging effects. Besides such switches there can be changes in strength of optimism or pessimism; these will have less pronounced effects, but may still change the character of relationships. Changes in optimism/pessimism will affect the volatile relationships and variables, but they will generally have only limited effects on the reliable ones. For example, it has long been known that pronounced optimism – high morale – in the labour force can improve productivity. 'Consumer confidence' is an indicator of optimism, and encourages consumer spending. These effects strengthen existing relationships, and marginally – but importantly – increase (or in the case of pessimism, reduce) coefficients. But changes in optimism/pessimism can completely change volatile relationships. Investment can go from a strong negative relation with interest to none at all; the price level/inflation can go from a clear negative relation with unemployment to none at all or even a positive one. The index of stock market prices can begin to move in apparent independence of the fundamentals in the rest of the economy.

Why do optimism and pessimism move in waves? Partly because moods are contagious; this factor will operate especially in markets where participants know each other or are in close contact, as in financial markets. But there is another, more practical reason: large movements of a market in one direction or another can be self-fulfilling, at least over a significant stretch of time, whether or not such movements are consonant with the 'fundamentals'. This will tend to happen when the expectations are such that they generate behaviour that will fulfil those expectations.[14] (We expect prices to rise, so we all try to buy now, hoping to get into the market before the rise; as a result, prices are driven up.)

11.2.1.2 Expectations and confidence

In the craft economy, expectations had to be adaptive. Not enough was known about how markets adjusted, or about the causes of booms and crises, or the pattern of cyclical behaviour (which was not studied in statistical detail until the early twentieth century; see Burns and Mitchell, 1946) for businesses or households to form their expectations guided by statistical regularities, let alone to form them in accordance with a theory. The best guide to the immediate future of the market appeared to be the recent past, modified in the light of long-term experience, and perhaps the current direction of change of key variables. In short, adaptive expectations.

But sometimes there are no good grounds for changing expectations, for example of sales or price, from what they have been to some other specific values; yet, on the other hand, there are signs either that all is not well, or that a groundswell is building for an upswing. Expectations will not change, since there are as yet no grounds on which to base a determination of new values of the variables, but the confidence with which they are held will decline, since in each case premonitions of change are experienced. In craft conditions, such premonitions must remain vague; when conditions do change, in a way that is evident to all, then the shift in expectations will be sudden and general. This will help to bring about the rapid collapses and booming upswings characteristic of the era.

By contrast, in mass production, and especially in high-tech, there will be a great deal more information and detailed theories on which to base planning. Expectations can be formed in greater detail, and the emerging signs of change – leading, current, and lagging indicators – will provide statistically precise reasons for the confidence with which these expectations are held. On the other hand, the greater flow of information has also made it clear that the cycle is underdetermined in many respects. In particular, exactly when it will change direction cannot be foreseen with reliability. Reasons that seem to be equally good can be advanced for different views. A distribution of beliefs could be expected, of more or less normal shape; but would it be centered around what will turn out to be the 'correct' turning point? Then those who are too early in anticipating a downturn, for example, would lose profits they could have made; those who are late will take losses they could have avoided. But if there were a general skewing to the early side, this could tend to speed up the downturn; if to the late, it might slow it down. Since this suggests that beliefs about the turning points can be self-fulfilling, it may not make sense to talk about a 'correct' belief. In any case, while Average opinion tends to be better than expert on many things – 'The Wisdom of Crowds' – it seems unlikely that the future of the economy is one of them.

Now let us tie this down to the monetary value of capital. As we saw earlier, in discussing *O* and *V* relations, the capital an agent owns is not the factory, it is the value of that factory. But that value is not the value of the funds that agents invested in it or raised in order to buy and equip it;

rather it is what the factory, now, is worth. And this is not, as is often mistakenly thought, what it will fetch on the market, what it can be sold for. It is what it *should* be sold for – otherwise we would never be able to talk about fire sales, or cases of selling below value. The value of the factory is the discounted sum of the reasonably and correctly anticipated returns from the operation of the plant in normal conditions. The anticipated returns have to be tethered as much as possible.

But we are going to see that the expectations that determine the value of capital are not very well tethered; we are going to have to try to model 'floating expectations'!

11.2.1.3 The present and the future

An essential feature of capital is that it is forward-looking; the value of capital today depends on the stream of returns it is expected to generate in the future. So the current position depends on present values, discounting the expected future streams of returns. But the expected future, in turn, depends on what has proved successful in the present. Nor is this mutual dependence of present and future confined to capital. Investment, the growth of capacity today, depends on the expected expansion of markets in the future, but future markets develop because of what is done today. Our view of the value of education or schooling today depends on the expected development of our careers in the future; while our expectations regarding the development of our careers in the future, in turn, are a projection from the value of education today. Thus

$$\text{Present} = f(\text{expected future}), f' > 0$$

$$\text{Expected future} = F(\text{present}), F' > 0.$$

The first says that present capital values will be higher, the higher are the expectations of returns in the future; this is an implication of the Keynesian 'marginal efficiency of capital' (Keynes, 1973). The second says that future returns will be expected to be higher, the higher are the values in the present, other things being equal (ibid.). This is Keynes's convention that we agree implicitly to project the circumstances of the present into the future, except where we have good reason to think otherwise.[15] Notice that this depends not only on our knowledge of what is happening now, but also on our knowledge of our state of ignorance – we need to know what we don't know, so that we can judge how costly our ignorance may be, and where to look for information. But what about the areas where we don't even know that we don't know what is going on?[16] It is this last area that may cause the most dramatic shifts.

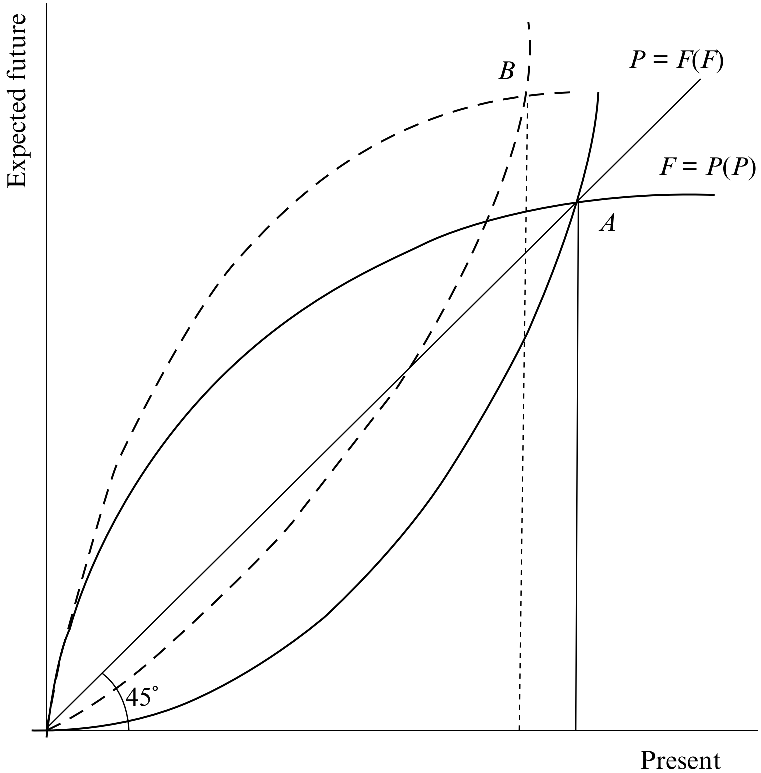


Figure 11.1 The CP and the MEC connect the present to the future

Of course, other things are not equal over the cycle: we can expect an optimistic view of the future during the upswing, and a pessimistic one in the downturn. We can also expect there to be a tendency for 'reversion to the mean', when values have been 'unusually high' or low over an 'unusually long' period of time. Unfortunately, even when such a reversion occurs, there is no way to say when it will happen. Nor is defining the relevant 'mean' without problems. Finally, as we shall see, the prospective yields have various degrees of risk attached: they cannot all be treated the same.

The interaction of future and present is (potentially) a determinate system, with two equations, or sets of equations, and two unknowns: the present, and the expected future value of assets (Figure 11.1). Besides depending on each other, each will also depend on various other parameters. These relationships could be linear or non-linear. If the latter, there could be multiple equilibria. If the former, there might be only a single equilibrium, but it will be partially unstable in a simple sense, since both functions have the same sign for the slope.

To examine this more carefully, measure the average expectation of prospective yields on the vertical axis, and present capital values established in the markets on the horizontal – ignoring default risks and maturity dates for the moment. We shall assume a known and stable rate of interest (alternatively we could measure $(1+r)K$ on the horizontal axis). The capital stock, its productivity and the labour force are given, but aggregate demand is variable. As demand increases, yields rise, and so do current capital values (the marginal efficiency calculation (MEC)); as current profitability rises, so do projected yields (the conventional projections (CP)). So the 45-degree line tells us that prospective yields translate exactly into the 'correct' amounts of capital, and that current profitability is projected into the 'correct' prospective yields.[17] But this would only happen if the market were perfectly 'efficient', in an impossibly idealized sense. In practice, the movements of the market will reflect many other influences, and the average opinion will be very imperfectly reflected.

Most importantly, everyone does not have the same opinion, nor do they evaluate information the same way. Diversity of opinion and divergent expectations are part of what keeps markets stable: if everyone thought the same, everyone would sell at the same time or buy at the same time, leading to large market swings. However, two groups stand out, each of which can be identified with one of the equations above. First, there are the corporate managers, who are responsible for planning current production and capacity utilization. They make the projections of future earnings, the conventional projections (CP). This means projecting present conditions into the future; but what we know of the present comes from the various measures that market agents and observers have made and are making – the present is a process still under way and estimates taken at different points and from different perspectives will differ. These differences must be reconciled, so that an agreed point of view can be established, a central position with a distribution of varying estimates. The CP then is such a projection of future returns on the basis of the present conditions; this will be a simple matter only in special cases, where estimates can be readily summed and averaged. But if the variance is wide, and especially if it changes as the present changes, the projection will lose its coherence.

On the other hand, we have fund and portfolio managers who have to decide whether to buy, sell or hold current (claims to) capital assets. The CP must be matched by a symmetrical calculation running backward from estimates of expected future earnings to the value of present assets, a marginal efficiency calculation (MEC).[18] (In both cases, the earnings of financial assets will reflect the underlying real basis, and this must be taken into account. Corporate managers, along with corporate boards, make decisions about investing in new technology, new products, building new capacity, and so on, but the value of this investment will be judged by financial managers.) The claims of expected earnings, as projected by corporate managers, must be evaluated for plausibility and coherence. Then it must be asked: what are the assets that generate those earnings worth? This is the MEC made by the financial community.

Will the fund managers believe the corporate managers, and vice versa? And how much diversity will there be within each group? Evidently we should think in probability terms as we consider the CP and the MEC. Here, however, for the moment we will assume that the distribution of estimates is normal, and the variance relatively small; so we can take the mean values as representative of the respective communities' opinions.

So the managers of the real economy make the CP, the expected future as a function of the present, and the financial managers make the MEC, the present as a function of the expected future. When these are compatible, capital values will be determinate. But, though determinate, they will not be stable if the intersection of the two functions is off the 45-degree line! (See Figure 11.2.) Nevertheless, the intersection together with the 45-degree line will simultaneously determine the value of assets in the present and the expectations of returns – or interest rates – in the future. Long rates and the value of long-term assets, financial as well as real, are thus determined separately from short and money-market rates. Long rates cannot generally be understood as a simple projection of short rates (money-market rates) into the future, adding appropriate premia for risk.

Let's see how this works.

The CP is the projection of the present into the future by corporate managers, adjusted for special information. They consider today's capital value, based on current earnings, and project the likely future. Along the 45-degree line, the projected earnings would exactly justify a discounted value of earnings equal to the current value of capital, according to current earnings.[19] Suppose the corporate managers are optimistic. Then from any value of current capital, a position on the horizontal axis (calculated on the basis of current profits), they choose a level of expected earnings above the 45-degree line. But positions further out along the axis, representing higher valuations of current productive capacity, approach the desired full capacity level of operation. This should be considered a special point of attraction; it is certainly a preferred point, and it will often or normally be more frequently experienced. Hence 'mean reversion' would suggest that it is at least in some circumstances more likely. Thus, near that level, the estimates will tend to be closer to the 45-degree line. The resulting curve will start above the 45-degree line and gradually bend down to cross it. (Beyond

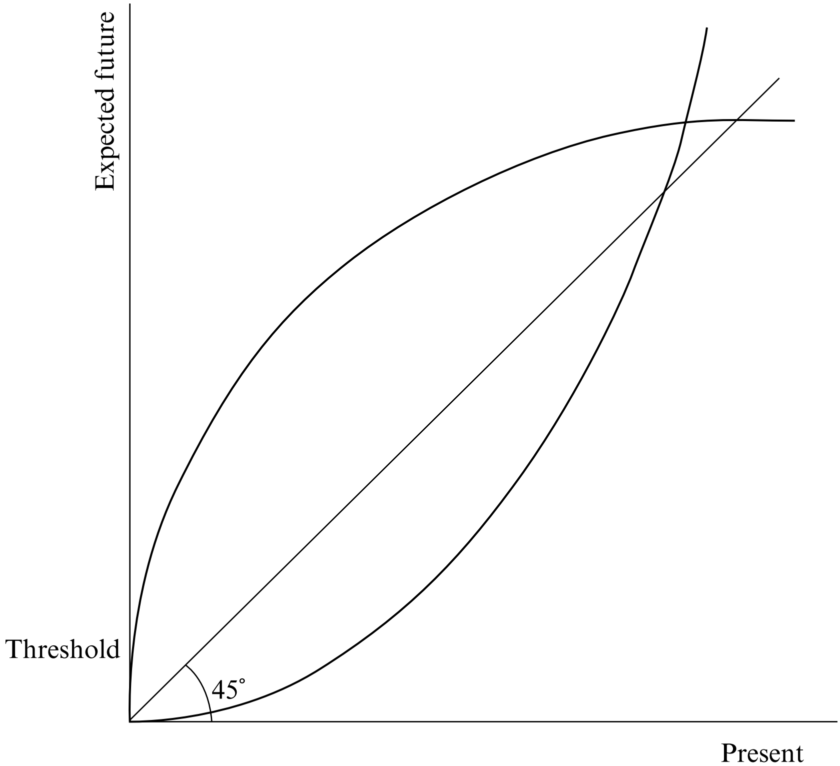


Figure 11.2 The CP and the MEC connect the present to the future, off the 45-degree line

the full capacity point, the value of capital will lie below the 45-degree line, because it will be thought that the high earnings cannot be sustained.)

Now consider the fund managers. Suppose they, too, are optimistic.[20] They will consider the level of earnings, and calculate a corresponding value of capital – the price they would pay to own it. Being optimistic, they will choose a value of capital higher than that indicated by the 45-degree line; thus, reading horizontally from the earnings axis, they will choose a point below the 45-degree line, and as we consider higher earnings points, the curve will bend upwards as we approach the full capacity level. Consider, next, a level of capital below the capacity point, as defined by current profits; the earnings projected by corporate managers are greater than the earnings required by fund managers to hold this level of capital – there is room for expansion. (This is the message behind Tobin's Q.)

But Keynesian thinking suggests that this tells us that we have a very fragile situation. The 'full capacity' or full employment level of output and capital is not very precisely defined. 'Mean reversion' is itself, as a pattern of behaviour, subject to shifts and reversals. The argument depends on whether there is a mood of optimism or pessimism – and, notoriously, these can change unexpectedly. And what if corporate managers were optimistic, but fund managers pessimistic, or the reverse? The curves would then both lie on the same side of the 45-degree line, and so would overlap, possibly leading to market paralysis – or to sudden shifts.

Finally, the state of confidence depends on a comparison between the present and the past; for example, if the present is very different from the past, in the absence of agreed-upon and special reasons, then confidence will be low. This will cause the functions relating expected future returns to present capital values to change curvature so that the intersection will shift inwards. Similarly, strong support from the past for present conditions will raise the state of confidence and cause the curvature to change so that the intersection will shift outwards. Moreover, present information may well not be enough to make the best prediction; present information compared to the past – to see the direction and magnitude of changes – may be what is called for. The past matters, in Keynes's view.

What do we learn from this? First, that floating expectations can be and are formed in a systematic way, such that market pressures tend to pull them together. Second, that this process is fragile and liable to sudden shifts, so that instabilities and multiple equilibria are possible. Even to model it, we had to assume that the distribution of opinion was normal, an assumption that could break down easily. Sudden shifts of opinion cannot be ignored or assumed away; nor can they be foreseen. (And other assumptions – mean reversion, normal capacity as an attractor – are also based on weak foundations.) Third, that interaction between the judgements of the managers of the real economy and the managers of finance is a crucial aspect of expectation formation, and when these differ financial markets will be particularly unstable. Finally, in general, the present influences the future, and the future influences the present; the relationships can be modeled, but in each case there is underdetermination; both are 'open'. We have to expect volatility in such markets!

11.2.1.4 From Klein to Nell

Nell's (1998a) general theory of transformational growth (TG) rests on methodological institutionalism, which could be considered a kind of structuralism in that the institutions, which are defined by formal relationships, do in a sense generate the observed phenomena.[21] Institutions may be related to one another in formal and legal ways, and when assembled together, these relationships –internal relationships – may also be considered structures. Klein's (1950; 1982; 1985) approach to econometric model building can be described as a kind of methodological structuralism (identifying the underlying structures is the basis of his approach)[22] and it overlaps here with Nell's methodological institutionalism.

Both approach the explanation of economic events in terms of a social world made up of institutions, roles, responsibilities, powers and so on. Critical realism considers the socio-economic system to be made up of 'structured objects' whose powers exist independently of our knowledge or perception. The wording is a little abstract, but it is easy to understand; the policeman has the power to arrest us, and the President has the power to call up the National Guard, whether we know it or not. These objects, relationships, powers and duties constitute the basis of the causal relationships that economic science describes. Employers can hire and fire workers and can order them around; firms can move capital from place to place, opening and closing plants. The TG approach (like critical realism; see Nell 2004) contends that social structures are based on obligations, contracts and promises, and both note that structures maintain their existence through interdependent production and reproduction. (Earlier we argued that these powers in turn rest on what we called *O* and *V* relationships.)

An implication of Nell's TG, relevant to econometric work and overlapping with Klein's methodological structuralism, is that the social domain appears to be open, so it must be described by theories that reflect and acknowledge that openness. Saying that the social order is open means that it cannot be circumscribed and summed up in a deterministic model. Nor can openness be accounted for in terms of stochastic regularities of the sort presupposed by modern econometricians. 'Openness' means that some of the key probability distributions could shift unexpectedly, for reasons that cannot be foreseen. The reasons should be clear from our earlier discussion, and from a half century of econometric failures to find event regularities (Lawson, 1999a, p. 221). Finding 'event regularities' is not the point of structural econometrics; on the contrary, the aim is first to show how the system works, and then to express its working quantitatively – to answer, not only 'how', but also 'how much'? This is perfectly compatible with openness.

A further step, however, leads directly to TG, which seeks to provide an account of the way these structures evolve and develop over time. Klein implicitly treats all relationships the same, letting the evidence tell us which are more reliable than others. Klein (1979, p. 317) argued that 'great ratios' are constants over long periods, as a simple matter of fact. Other things are more changeable and some features of the economy in his view are likely to be quite volatile. But he presents no systematic account of this distinction. (We base the very imperfect 'constancy' of the 'great ratios' on the stable structure of the socio-economic system.) But the distinction between reliable and volatile relationships offers a way of dividing the study of the economy: reliable relationships are grounded in current contracts and obligations, so will only change as these change. Such changes can be noted, even foreseen, by fieldwork. Volatile relationships depend on expectations of the future that cannot be based on any certainty – hence they are subject to unforeseeable shifts and fluctuations. The time series of observations for reliable relationships may well have ergodic properties, as David Hendry suggests, but those for volatile ones will not.

In sum, we propose that relationships based on or calling for activities in response to expected future events or activities will be systematically less reliable than relationships grounded in current obligations and contracts, to be carried out in the present on the basis of, or in response to, current economic activities. Future-oriented activities will be volatile, meaning that they are liable to shift or change unexpectedly, and to unexpected degrees. It may be possible to draw the broad outline and establish the limits of the volatile sectors, and it may be possible for a short time to establish precise relationships. But shifts and changes, even radical ones, can happen at virtually any time, due to changing views about the future. (An implication is that macroeconomic processes, as a whole, cannot be considered ergodic, even if some parts may be; conversely, if the macroeconomy is not ergodic it must have some volatility; see Davidson, 1982).

11.3 ECONOMIC GROWTH AND HISTORICAL CHANGE

We suggest that econometrics and macroeconometric model building should be more sensitive to large-scale changes in the characteristic patterns of data. For example, it is well known that in the nineteenth century cyclical fluctuations show up more prominently in price data than in employment or output series, whereas in the twentieth century the reverse is true, particularly after World War II (Sylos-Labini, 1989; 1993; Nell 1988). Accordingly, transformational growth contrasts two general 'models of adjustment' of capitalist societies. These are both macro models, and are both based on reliable relationships – firms selling in competitive conditions, households spending to support themselves. Each model is abstract and quite general, but nothing has been 'idealized'. Each is presented 'mathematically', although the functions are abstract and aggregate, so that fitting them to data would require careful attention to the definitions of the variables. But it is argued that the functions correctly represent directions of variation, and rough relative orders of magnitude, and that each represents the working of the system during a particular historical period.

The first claims that the early capitalist societies, running up to World War I, were characterized by a stabilizing 'price mechanism', so that when investment or exports fell, reflecting uncertainties and shifts in expectations, prices would fall relative to money wages, leading to a rise in real wages and thus consumption spending (Nell, 1998a; 1998b; 2004). This was accompanied by a stabilizing but unreliable monetary and financial system: when prices fell or rose, interest rates would also fall or rise, tending to encourage or discourage investment. Unfortunately this financial system periodically broke down and the price mechanism was not strong enough to cope with large shocks. But the general idea was that the price mechanism worked 'counter-cyclically' to enable these economies to adjust to various kinds of external shocks; and the banking system complemented this, as the interest rate also generated counter-cyclical pressures.

Empirical support for this model comes from studies of 'marginal productivity' – Dunlop and Tarshis, and many others (confirmed by Nell, 1998b), found that an inverse relationship existed between real wages and levels of employment for pre-World War I data on advanced economies. Further support for the existence of a weakly stabilizing price mechanism comes from the re-examination of Gibson's Paradox; see Nell (1998a; 1998b). By contrast, after World War II, there is little trace of such a mechanism; on the contrary, real wages now appear to be positively correlated with levels of employment (Nell, 1998b; Blanchard and Fisher, 1989) and the Gibson relationship has disappeared (Klein, 1979). In place of the stabilizing system there is an unstable or volatile mechanism, the 'multiplier–accelerator', which actually amplifies external shocks (see Fair, 1994; 2004). Stabilization has to be provided by the government, partly through its sheer size (a new feature), partly through the fact that its budget is weakly counter-cyclical, and partly through discretionary policy.

Contrasting these models creates a framework in which many different features of early and late capitalism (underdeveloped and developed economies) can be classified and analysed (Nell, 1998b).[23] Moreover, the counter-cyclical adjustments in the markets of early capitalism created important pressures on business. Profits were squeezed, bringing risks of bankruptcy; to counteract that, businesses innovated in ways that made costs more flexible. The combined effect of these innovations was to change the system, moving it towards the second pattern of adjustment. Let's examine the transformation from a craft economy to mass production.

11.3.1 The Price Mechanism and Marshallian Technology

The principles underlying the craft economy centre on the short-run employment–output relationship.[24] In the craft economy (Nell, 1998a;

1998b), we can reasonably assume diminishing returns to the employment of labour, in relation to a normal position. Adding extra workers to work teams operating given equipment brings progressively lower additional rewards, while removing workers leads to progressively larger losses of marginal output. There will be a point in between where the given equipment is being operated most efficiently. In general, it will take time and effort to adjust levels of employment; it will not be done lightly (Marshall, 1961). Workers cooperate in teams that cannot be lightly broken apart or added to; all workers have to be present and working for a process to be operated at all; processes cannot easily be started up and shut down. So the craft economy not only has diminishing returns; it also has inflexible employment (Nell, 1998a, ch. 9).[25]

Our model is based on such an aggregate function, where we have assumed a conventional shape and properties. (Aggregation will be based on long-run normal prices, those ruling at the optimal points. It might reasonably be assumed that there is a normal distribution of efficiency; then the aggregate function would be the representative average function multiplied by total capacity.) In a craft economy,[26] it is reasonable to suppose that output increases with labour according to a curved line that rises from the origin with a diminishing slope (by contrast, mass production will be characterized by a straight line rising from the origin[27]). As a first approximation, consumption can be identified with wages and salaries;[28] while for the purpose of drawing the diagram, investment can be taken as exogenous. As employment rises, the wage bill – and so consumption spending – will rise at a constant rate, namely the normal wage rate. Total expenditure will then be shown by adding investment to the wage–consumption line.

The diagram presents the aggregate utilization function, with output on the vertical axis and labour employed on the horizontal. (We will call this the 'production function', though it is *not* the 'true' neoclassical concept, where each point shows the optimal adjustment of equipment.) The output function of the craft economy is curved, its slope falling as N increases (the mass production line would rise to the right with a constant slope). The wage bill (including salaries) will be assumed to be equal to consumption spending (transfer payments could be included also). No household saving and no consumption out of profits – but both assumptions are easily modified.[29] So the wage bill, also representing consumption spending, is shown by a straight line rising to the right from the origin; its angle is the wage rate. Investment spending will be treated as exogenous in the short run, so will be marked off on the vertical axis. Aggregate demand will then be the line $C+I$, rising to the right from the I point on the vertical axis; its slope is the wage rate.

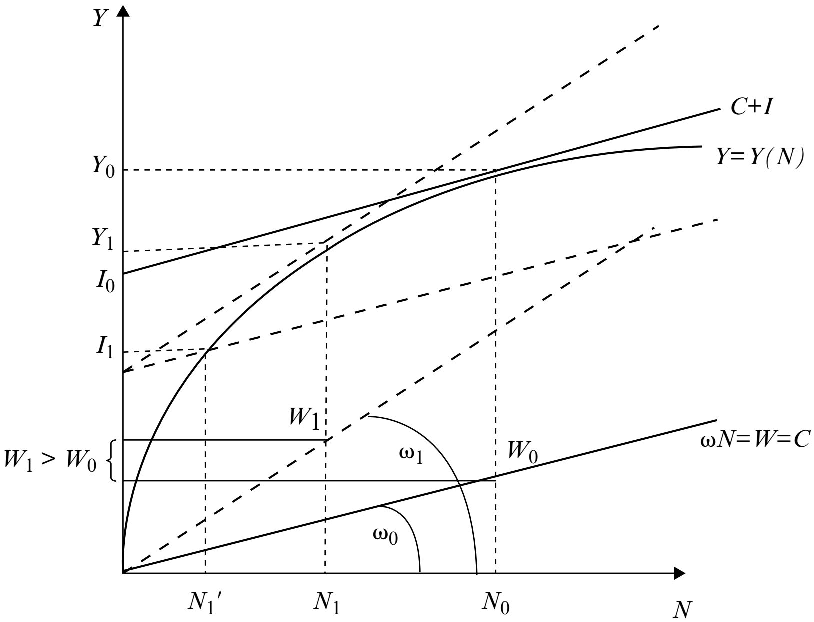


Figure 11.3 Adjustment in the craft economy

11.3.2 Adjustment to Demand Fluctuations in the Craft Economy

Suppose investment is unusually low – below normal – so that this line cuts the utilization function at a point below the normal level of output and employment, N_1'. Since it is difficult to adjust employment and output, there will tend to be overproduction, and prices will fall. Since it is even harder to adjust employment than output, prices will fall more readily than money wages. Hence the real wage will rise, from w_0 to w_1. As a result, the $C+I$ line will swing upwards, until it is tangent to the utilization function; employment thus settles not at N_1' but at N_1. Notice that this point of tangency will tend to be close to the normal level of employment and output, and will be closer the more concave the function. In short, when Investment is abnormally low, the real wage will rise; if the rise in real wages is proportionally greater than the decline in employment, consumption will increase. This is the case illustrated in Figure 11.3; investment falls from I_0 to I_1, prices fall and the real wage rises. Clearly the wage bill, and so consumption, is higher at N_1 than at N_0.

Conversely, suppose investment were exceptionally high, or that the $C+I$ line had too steep a slope, indicating too high a real wage. In either

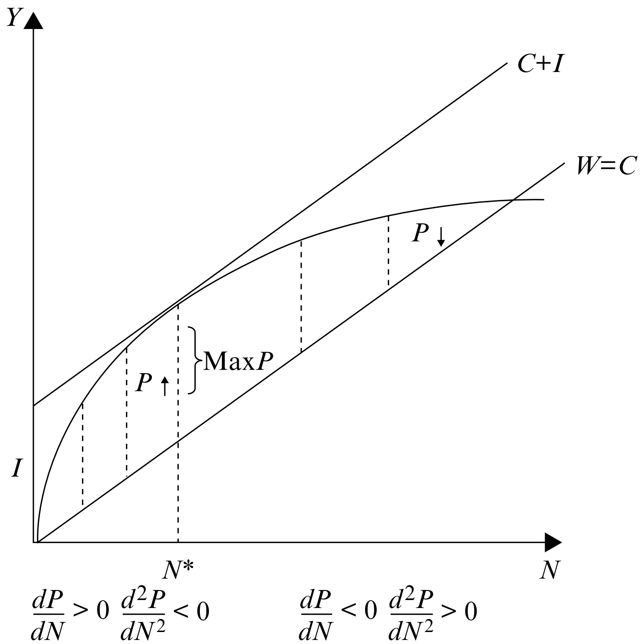


Figure 11.4 Behaviour of profits

w, expenditure would lie above output at any feasible level of employment. Under these conditions, prices would be bid up relative to money wages, and the $C+I$ line would swing down, until it came to rest on the utilization function in a point of tangency (Nell, 1998a, pp. 455–7). Again, this point would tend to lie close to the normal level, being closer the more concave the function. When investment is unusually high, consumption will tend to adjust downwards.

Notice that adjusting the real wage to equal the marginal product of labour both assures a unique equilibrium and maximizes profit.[30] When the $C+I$ line is tangent to the utilization curve, the distance to the wage line is at a maximum; if $C+I$ cuts the utilization curve, there will be two equilibria and the distance between the intersection points and the wage line will be less than that at the tangency. (Given the real wage, profit rises with employment at a diminishing rate from the origin to the tangency point; it then falls at an increasing rate until it reaches zero at the point where the production function intersects the wage line – see Figure 11.4.)

We need to define the point of full employment – at which the entire labour force has jobs. An appropriate concept of full employment would be 'no vacancies' or, rather, 'no vacancies except turnover vacancies'.

Employment is full when all farms, factories, offices and shops have hired the employees they need to operate at their optimal level. Output at the point of full employment will be associated with a marginal product; that marginal product will become a real wage, which, multiplied by the level of full employment defines the wage bill, equal, ex hypothesis to consumption. The difference between full employment output and consumption must be filled by investment. Now let investment fall below this full employment level. As it does, it will trace out the marginal product curve; at each lower level of investment, prices will fall, and the real wage will rise while employment falls; the overall effect on consumption will depend on the elasticity of the marginal product curve. But each point on the curve will be an equilibrium, in the sense that money wages and prices have adjusted to produce the profit maximizing position.

That this pattern of price flexibility dampens fluctuations by partially offsetting them, in conditions of strongly diminishing returns, can be shown very simply. Recalling our equations: Y is real output, N employment, w/π the real wage, and I investment. All wages are consumed. As above,

$$Y = Y(N),\ Y' > 0,\ Y'' < 0$$

$$Y = C + I$$

$$w/\pi = Y'(N)$$

$$C = (w/\pi)N.$$

Clearly

$$Y = I + (w/\pi)N,$$

so

$$dY/dI = \delta I/\delta I + N[\delta(w/\pi)/\delta I] + (w/\pi)[\delta N/\delta I] = 1 + N\,[\delta(w/\pi)/\delta I] + (w/\pi)\,[\delta N/\delta I]$$

where

$$N[\delta(w/\pi)/\delta I] < 0 \text{ and } (w/\pi)[\delta N/\delta I] > 0.$$

So

$dY/dI >$ or < 1 according to whether $N[\delta(w/\pi)/\delta I] >$ or $< (w/\pi)[\delta N/\delta I]$.[31]

So long as returns diminish sufficiently, $dY/dI < 1$; price changes due to variations in investment demand will lead to a partial offset.[32]

In short, so long as diminishing returns are significant, the price mechanism will lead consumption to adjust so that it will tend to make up for a shortfall or offset an excess of investment. It thus tends to stabilize demand around the normal level of output and employment.[33]

11.3.3 Growth and the Price Mechanism: Flexible Prices and the Golden Rule

The craft economy can be assumed to consist of a large number of small firms and farms, each normally operating at an optimal – minimum cost – level, paying wages to its workers, and what Mill called 'wages of superintendence' to its managing owners (Robinson, 1931). Profits will be distributed as interest and dividends to banks and owners, respectively. (Taxes will support schools, police and infrastructure, although the public sector will not be considered here.) Firms will be divided between established and new; established firms have a constant age structure of their workforce – new entrants are hired regularly as ageing workers retire. Retired workers live off and consume their pensions. However, as a first approximation, apart from pensions and 'saving up' for consumer durables, worker households do not save. In permanent saving, capital accumulation comes out of capital income, not from household wages – neither worker wages nor wages of superintendence.

As a first approximation, we can assume that all profits are saved and invested in setting up new firms, which hire new entrants to the labour force. New entrants are cheaper, but also inexperienced; the new firms will have to go through an internal organizing and learning process. (Saving always equals investment because investment, the active force, affects prices, which affect profits, so that savings adjusts to investment.)

Owners either invest profit income or they bank it and receive interest, and the banks loan the funds to entrepreneurs who wish to start new businesses. Retired owners do not consume their capital (as retired workers do); they pass along the management of the firms they own, and live off the interest of their holdings. (If they saved for retirement out of their wages of superintendence, they would consume those savings.) When they die, they leave their capital to their children. If the rate of growth of the population of capital-owning families is equal to the rate of accumulation of capital (and family size remains the same, etc.) then wealth per head will tend to be constant.[34]

In Solow's approach, the growth of the labour force sets the growth rate of the economy. However, he does not offer an account of a market mechanism by which this will be brought about. He simply shows that there is always a capital accumulation path consistent with any rate of growth of the labour force – and then assumes full employment. The capital–labour ratio then adjusts appropriately.

This result – that the growth of labour sets the pace for the economy – is surely correct for a craft economy; but it is important to show that it can be brought about by a market mechanism. The argument can only be suggested here: starting from a balanced path, in which the growth of the labour force equals that of capital, if the growth of labour speeds up (slows down), entry-level wages will fall (rise), encouraging (discouraging) the formation of new firms. When entry-level wages fall, for example, expected profits will increase, so the MEC will rise, encouraging investment.[35] Entry-level wages are not currently paid, so a decline will not affect the current level of consumption spending.

The price mechanism explored above can be adapted so as to show the key elements in the process of growth in a craft economy. Measure Y/N on the vertical axis, K/N on the horizontal. Then a line rising from the origin left to right will measure I/K, the rate of growth. Add on to this the wage bill per capita, the wage rate; the two added together will be aggregate expenditure per capita, and this will adjust until it is tangent to the production function. If it lies above, this will drive up prices, swinging the line down; if it is below the production function, prices will fall and the line will swing up (see Figure 11.5).

The similarity of this to the Solow growth model is unmistakable; the diagram has fundamental similarities. But Solow added an assumption that is usually overlooked: although he introduces the marginal productivity relationships for both the real wage and 'quasi-rents', he assumes that prices will be constant (Solow, 1956, p. 79).[36] As a result, there is no price mechanism in his model: savings is assumed to drive investment and the equilibrium is determined by the changes in the capital/labour ratio brought about by saving. There is no justification for Solow's assumption of a constant given price level, nor does he pretend to offer one – but it completely changes the character of the model.

Figure 11.5, although the diagrams look just like Solow's, shows the working of a price mechanism in which changes in investment affect prices so as to change the level of real wages. This changes profits, and profits are savings, which here adjust until they are equal to investment. The intercept of the aggregate expenditure line is $(w/\pi)N/N = w/\pi$. The slope of the line is the growth rate I/K. The equilibrium will be given by the point of tangency; if the expenditure line is not tangent, then the price level will rise or fall, adjusting the intercept until the line just touches the production function. This equilibrium maximizes profits; it has optimality properties, which might be expected in a neoclassical approach, although they are not present in Solow's. Starting from an equilibrium, if I increases, raising the growth rate, then the expenditure line will swing up above the production function. But this will drive up the price level, reducing the real wage, so lowering the intercept until the line is tangent again. Similarly, if the real wage were too low, given a level of g, then the price level will fall.

The tangency between the expenditure line, with a slope of g, and the production function implies that the rate of growth will equal the marginal product of capital, which is the slope of the function. For example, a rise in g will raise the expenditure line above the production function, indicating demand pressure that will bid up π, lowering the real wage, swinging the line back down to tangency at a higher level of profits, thus leading to a new equilibrium. This tangency point instantiates the golden rule, and contrary to Solow, it is the true, market-driven equilibrium, based on profit maximization.

Note further that it also shows the importance of prices in the adjustment, at all levels of employment or utilization, contrary to Kaldor, for whom prices vary only at or near full employment. Solow has assumed fixed prices in an economy with diminishing returns and marginal products; this cannot be. Kaldor, on the other hand, has (partly) flexible prices in an economy with constant returns and no marginal products. But if costs are constant, and demand varies at levels below full employment/full capacity, firms will tend to keep prices steady. If there is demand pressure at full employment, prices will undoubtedly tend to rise, but fully employed labour will be in a strong position to push for higher wages in response to the rising prices. Rather than there being a price adjustment that would tend to reduce the pressure, a wage–price spiral is likely to emerge. This could lead to an adjustment if prices rose faster than money wages, but if money wages outpace prices (as in the late 1960s in the USA) then the demand pressure would most likely be intensified. Both Solow and Kaldor have failed to provide a proper role for the price mechanism in their approaches.

In this system, technical progress – increasing productivity – will be shown by a shift upwards in the production function; if initially the expenditure line had been tangent, the upward shift would leave it now below the production curve, implying output in excess of demand, leading to a tendency for the price level to fall. The benefits of technical progress will be distributed by falling prices, with money wages constant (as happened all during the nineteenth century).

Figure 11.5 is represented by three diagrams. The first diagram shows the tangency growth equilibrium; the second shows the adjustment to a rise in the growth rate – the real wage falls; the third shows the adjustment when aggregate demand is too low – the real wage rises.

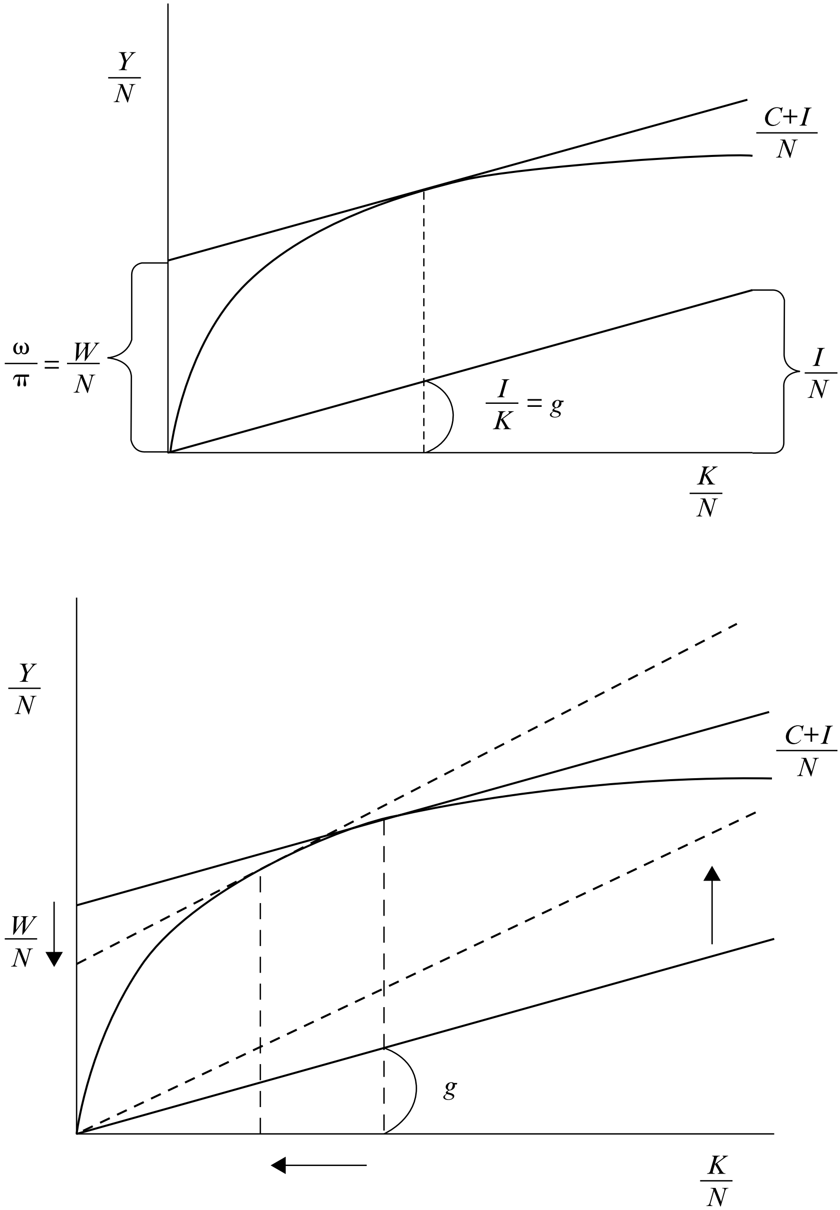


Figure 11.5 Growth adjustment in the craft economy

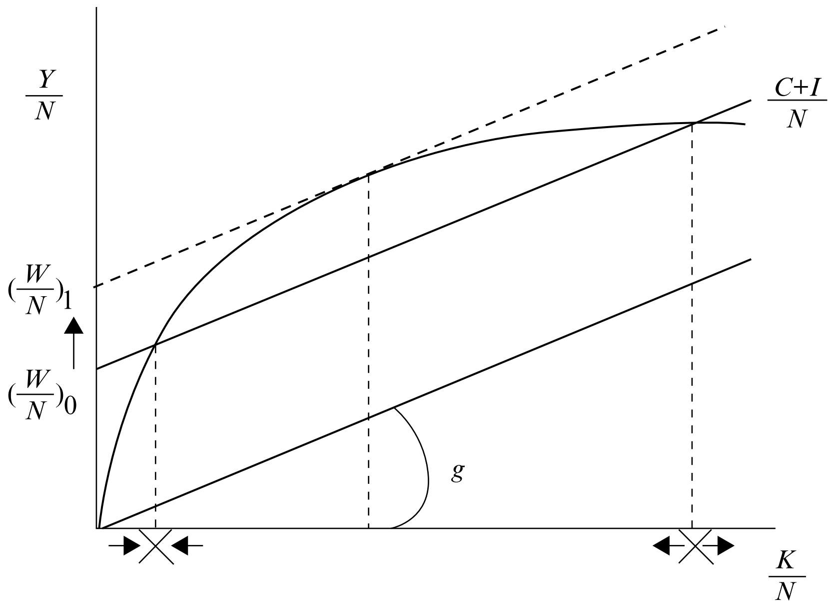


Figure 11.5 (continued)

The replicative growth process will exhibit a definite pattern, in which the growth of capital per worker will tend to equal the growth of output per worker, which in turn will equal the growth of the real wage. As indicated above, when the production function shifts up, the price level will fall, and real wages will rise. When the production function shifts up – disembodied technical progress – it means that production processes have been reorganized, so that work will be done faster (Nell, 1998a, ch. 7). Hence more energy will be used, more materials will be processed, more wear and tear will take place; in other words, working capital will be increased in proportion to the speed-up. Hence K/N will increase in proportion to the rise in productivity.

$$d(K/N)/(K/N) = d(Y/N)/(Y/N) = d(w/\pi)/(w/\pi)$$

This will also tend to be true for 'mechanization', where a proportional increase in K/N is just matched by a proportional decrease in N per unit of output. This will maintain the equality on the left, and that on the right will follow as before, from a rise in productivity leading to a proportional fall in prices.

As a result, the capital–output ratio and the rate of profit will tend to stay steady. With both of these holding steady, the shares of profit and wages will also be unchanged. This is the 'Victorian equilibrium'.

As the economy grows, the banking system must grow pari passu, which means bank capital and bank reserves must be augmented along with other investment. The level of bank capital – the capital of the banking system – will support a certain level of bank loans,[37] while the difference between deposit and lending rates will provide the profits of banks. The sustainable ratio of bank loans to bank capital can be indicated by λ; then

$$\lambda(i_l - i_d) = r_b,$$

the profit rate of banks.

When this profit is reinvested, bank capital, and therefore sustainable bank lending, will grow at this rate. If the profit rate in banking is the same as in the rest of the economy, and the rest of the economy likewise reinvests its profits, and grows at the golden rule rate, then the credit money required will always be available.

What we have presented is an abstract, but not idealized, picture of the working of capitalism in an era of craft industry and traditional agriculture, portraying a system that is self-adjusting in a weakly stabilizing manner, while tending to establish an equilibrium that can claim some optimality properties. It has some affinity with the traditional ideas of neoclassicism, but it does not rest on rational choice foundations.

11.4 FROM CRAFT TO MASS PRODUCTION

Keynes accepted the idea that the price mechanism adjusted to ensure that the real wage equalled the marginal productivity of labour. He did not, however, explain how this equality was brought about in a labour market in which behaviour responds to money wages. In his view, the equality of the real wage and the marginal product justified calling the position an equilibrium; but, as reconstructed here, the argument shows that there will be a large number (on plausible assumptions, an infinite number) of such positions, besides the full employment level. The way this works has been shown earlier on a diagram in which it is clear that price changes tend to move the system to a profit-maximizing position, for any given level of investment.

This certainly appears to be a stabilizing pattern of adjustment. Each position of the economy will be a combination of a level of investment and a level of consumption (equal to the level of the real wage bill), such that higher investment (driving up prices, lowering real wages) would appear to be associated with lower consumption spending. This is stabilizing.

When investment falls, for example, prices will fall; and consequently real wages and therefore consumption spending will rise, offsetting the decline in investment.

But such a pattern of adjustment puts the burden on profits; prices would fall in a slump, and firms would have to draw down their reserves. Accordingly, firms should seek to develop greater flexibility to allow them to adjust the level of employment to market conditions, laying off and rehiring workers as demand changes. This provides an important incentive to innovate (Nell, 1998a).

Keynes did not examine this. But what he saw is that price adjustment was *not* working to stabilize the system. On the contrary, fluctuations in investment appeared to set off destabilizing movements. A key point of his lectures was to explain this, showing that investment and consumption moved together, not inversely; thereby increasing volatility. This is a consequence of reducing the rate of diminishing returns, 'flattening' the production function. Furthermore, he argued that investment was the active variable, the causative force, while consumption (and saving) simply reacted passively. So prices and employment could adjust in such a way that the real wage and the marginal product of labour were brought into equality, thereby maximizing profits, while investment and consumption moved together, rather than inversely, creating 'multiplier'-based volatility in the system. There is no pressure in this system to move to full employment, but each position can reasonably be considered an 'equilibrium'.

11.4.1 Changes in the Production Function: The Multiplier Replaces the Price Mechanism

When the curvature of the production function is considerable, the elasticity of the marginal product curve will be greater than –1, so a fall in investment will lead to a rise in the wage bill and therefore in consumption spending. But when the production function is rather flat, the elasticity of the marginal product curve will be less than –1, so that a fall in investment will lead to a decrease in the wage bill and consumption spending, as indicated. In this case, there is not only no offset to the drop in investment; the effects are actually made worse. And that is the conclusion Keynes reached and tried to explain in the lectures he gave in Cambridge.

The variability of profits provides an incentive to change the technology so as to control current costs; the innovations must change current costs from fixed to variable, and this can be done by taking on additional capital costs. This will be particularly advantageous when there are pressures for the real wage to increase; at the higher wage, it will be worthwhile to mechanize, so in current prices capital per worker rises, and the scale

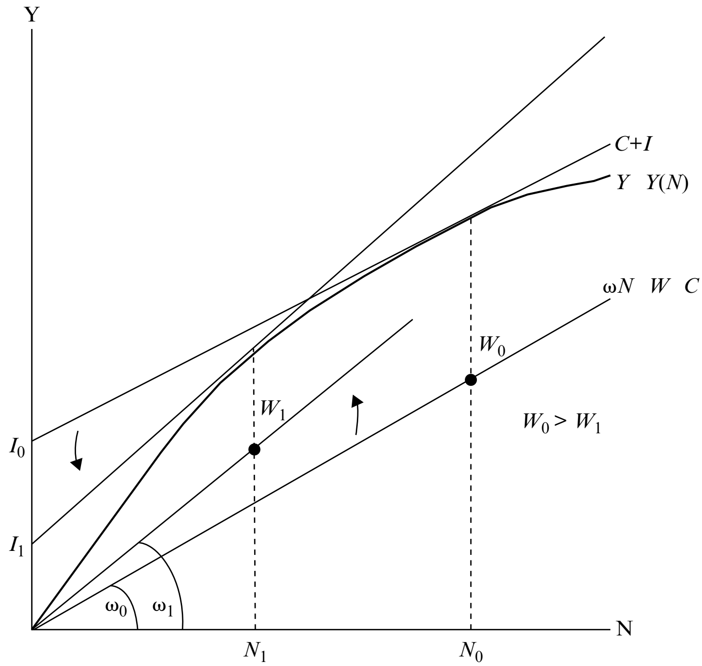


Figure 11.6 Consumption moves with investment

effects allow for greater flexibility in adjusting employment to changes in the level of demand.

Fluctuations in I will normally have some impact on N even in a craft economy (see Figure 11.6). But there will be an offsetting movement in C so long as the curvature of the employment function is large. The price mechanism is stabilizing for the system as whole, but the effect is that profits fluctuate sharply for individual businesses. So firms will be motivated to redesign their production systems to allow greater flexibility in adapting to demand fluctuations. This means being able to add on or lay off workers, without greatly disturbing unit costs. As such redesigning takes place, it will reduce the curvature of the employment function; that is, diminishing returns will be lessened. We can think of this as a progressive 'flattening' of the employment function. When this has reached the point where the marginal product curve has unitary elasticity, so that the proportional change in the real wage is just matched by that in employment, then the total wage bill is unaffected by the price changes following the change in I. If the total wage bill is unaffected, then, on the assumptions made earlier, total C will be unchanged.

This will be the case, for example, when the employment function takes the form: $Y = A(ln\ N)$. Hence I may fall, for example, but C will not change. There will be no offset. So $dY/dI = 1$. Any further reduction in the rate at which returns diminish will mean that the change in employment will outweigh the change in the wage bill, so that C will move in the same direction as I. In this event, $dY/dI > 1$ will always hold (Nell 1998a; 1992a; 1992b).

It can be argued that this was the conclusion that Keynes seems to have been seeking. In his Second Lecture in the Easter Term, 1932, Keynes reached 'the remarkable generalization that, in all ordinary circumstances, the volume of employment depends on the volume of investment, and that anything which increases or decreases the latter will increase or decrease the former' (Keynes, 1936, vol. XXIX, p. 40).[38] (See also T.K. Rymes, 1989, *Keynes's Lectures, 1932–35*, pp. 30–44.)

11.4.2 Adjustment to Demand Fluctuations in the Mass Production Economy

Modern economies appear to be subject to strong fluctuations in demand. Indeed, examples of market instability can be found everywhere, although the instability is usually bounded in some way. But in the modern world there do not appear to be strong and reliable market-based forces ensuring stability. Investment spending appears to be a major source of demand variation. Yet if the purpose of investment were simply a corrective, moving the actual capital–labour ratio to its optimal level, then stabilization would hardly be needed. Such a long-run position would be stationary, or, if the labour force were growing, the economy would expand uniformly. This is the picture presented by neoclassical theory, articulated, for example, by Hayek (1941).

But both Keynes and the older classicals, especially Ricardo and Marx, offer a different view: investment is the accumulation of capital, a process by which productive power is created, organized and managed. It is driven by the desire for power and wealth, and there is no definable 'optimum'. Investment expands productive power, but does not move the economy towards any definite destination. Given such motivation and the important role of technological innovation, the urge to invest will sometimes be strong and widespread, but at other times weak and uncertain. This may help to explain the need for stabilizing policies, arising from the demand side.

In post-war mass production economies (Nell, 1998a), prices do not play an important role in adjustment to changing demand. In Hicks's (1965) terms, this is a 'fix price' economy. Employment is much more

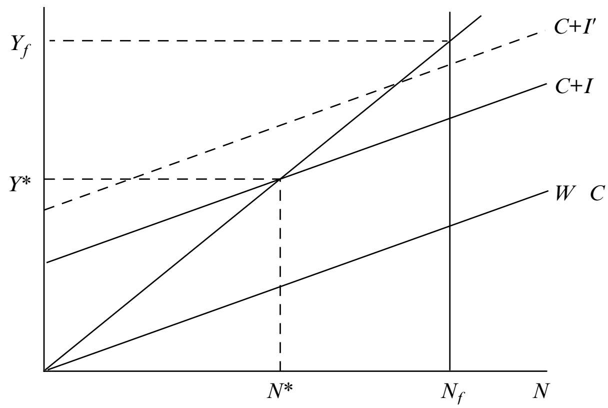


Figure 11.7 Adjustment in the mass production economy

flexible, and constant returns appear to prevail in the short run; to put it differently, unit costs are broadly constant as employment and output vary over a wide but normal range. Prices can therefore be maintained at their long-term levels, while permitting only small temporary variations around that level. Workers need only be semi-skilled and teams can easily be broken up and re-formed; processes can be operated at varying levels of intensity in response to variations in demand, and they can easily be shut down and started up. It is likewise easy to lay off and recall workers.

As before, we have an aggregate utilization function: here the mass production economy (see Figure 11.7) will be characterized by a straight line rising from the origin, showing constant marginal returns in output to additional employment – that is, to more intensive utilization.[39] As a first approximation, consumption can be identified with wages and salaries, while investment can be taken as exogenous. As employment rises, the wage bill – and so consumption spending – will rise at a constant rate, namely the normal wage rate. The wage bill – assumed equal to consumption spending – is represented by a straight line rising to the right from the origin; its angle is the wage rate. Investment spending will be treated as exogenous in the short run, so will be marked off on the vertical axis. Aggregate demand will then be the line $C+I$, rising to the right from the I point on the vertical axis; its slope is the wage rate.

The origin, here and in later figures, is the point at which labour cost absorbs all output. Employment in such an economy will depend only on effective demand; there is no marginal productivity adjustment.[40] Output will increase with the amount of labour employed (capacity utilized), with a constant average productivity of labour; all and only wages will be spent on consumption, and all profits will be saved as retained earnings.[41] Investment can be taken as exogenous as a first approximation.[42] Expenditure is given by the $C+I$ line. (This ignores G, government spending, for the moment, although in the modern world it will be much greater than in the earlier forms of the capitalist economy.) But the output function will be a straight line rising from the origin, with a slope equal to the average productivity of labour. Suppose Investment is exceptionally high; then employment will be increased, and consumption will also be exceptionally high. Conversely, if Investment is low, employment will be low, and thus so will consumption. Consumption adjusts in the same direction that Investment moves.[43] When investment rises, consumption, output and employment also increase in a definite proportion.[44]

Simple as this is, it provides us with a number of powerful insights. Admittedly, they are derived on the basis of very great abstraction, so they cannot be expected to prove literally true – but they may nevertheless give us genuine guidance in investigating the way the world works. For example:

- Investment and profits are equal here; this suggests that we should expect to find them closely correlated in practice – as we do (Nell, 1998a, ch. 7; Asimakopulos, 1992).
- Investment determines profits here; investment is the driving force. We should expect to find something like this in reality – which many studies suggest we do.
- The multiplier here will equal $1/(1 - w/a)$, where w is the real wage, and a the average productivity of labour. That is, the multiplier will reflect the distribution of income, and will not be very large. Again this seems plausible.
- Real wages and the level of employment and output are positively related. This can be seen by drawing in a steeper wage line, with the same level of investment. The $C+I$ line will then also be steeper; so it will intersect the output line at a higher level of output and employment. In fact, most empirical studies of the post-war era do find real wages and employment to be positively related (Nell, 1998b; Blanchard and Fisher, 1989).
- Household savings reduce output, employment and realized profits! (Obviously, qualifications are needed, and it must be remembered that this is a short-run analysis – but the long-run may never come! If this proposition seems hard to accept, think about Japan in the 1990s – and even recently.)
- Unemployment is indicated by marking off the level of full employment on the horizontal axis. It clearly results from deficiency in

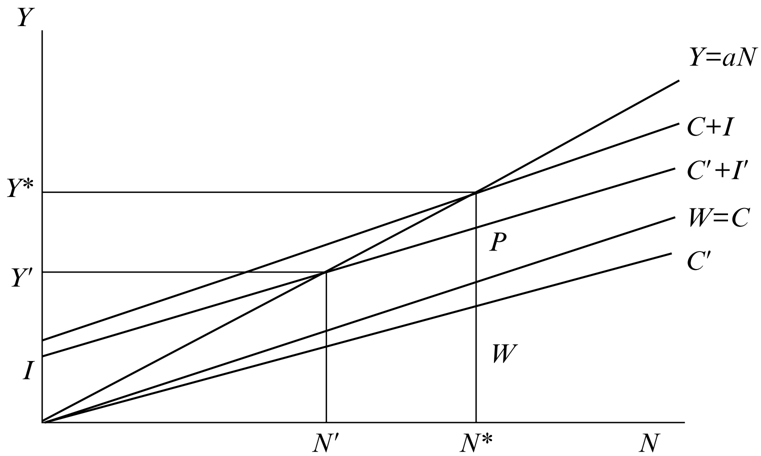


Figure 11.8 Effects of interest on saving and investment

demand. That is, either investment is too low or wages are too low; which implies that unemployment can be reduced by increasing either.

Finally money: let household saving increase with the rate of interest (as consumer durable spending declines), while business investment declines as the rate of interest rises. (Neither influence is likely to be very great.) More precisely, when interest is relatively high, businesses are likely to curtail or postpone investment projects, and households may cut back on consumer durables. Thus, when interest is high, the investment line must shift down to a lower intercept, while the household consumption line will swing down, reducing its angle. When interest rates are relatively low, investment and household spending will be correspondingly higher. Thus we can construct a downward-sloping function (see Figure 11.8 (an analogue to the traditional *IS*)) relating the rate of interest, *i*, to employment, *N*.

This function will intersect a horizontal line representing the level of the rate of interest as pegged by the central bank; this will determine the level of employment (see Figure 11.9)

There is no classical dichotomy here; monetary and real factors interact. Yet – not so fast! In the craft economy, the interest rate tended to rise and fall with the profit rate, moving procyclically. What if we imposed that condition here? Then the structure of asset prices would have to adapt to the real conditions of profitability – this could well imply that the long rate would tend at times to move independently of the short. A form of the dichotomy might re-emerge (Nell, 1998a). But this is another story.

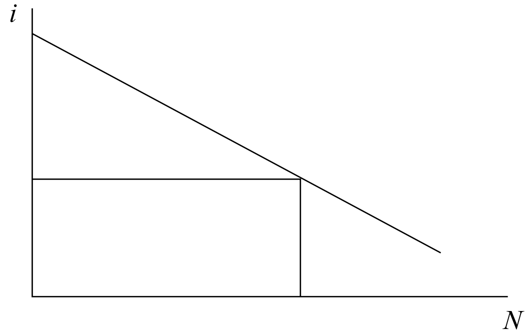


Figure 11.9 The central bank's interest rate determines employment

11.4.3 Growth and Prices in Mass Production

Once again, here is a model of short-term demand adjustment, based on investment spending, ready to be extended to growth. Of course, very early on, Harrod and Domar did just that. The short term adjustment pattern depends on investment spending in the context of a given capital stock, but the Keynesian analysis considers only the expenditure implications of investment. So to extend it to the long run it seemed only necessary to take into account the capacity creation brought about by the investment spending.

The balance between investment spending and capacity creation could be taken for granted in 'replicative growth', because the growth simply reproduced the prevailing relationships. The level of I would generate the income necessary to employ the presently existing capacity of the economy; and it would create *new* capacity of an amount that would just be employed by *new* or increased I, dI (a point very nicely developed by Domar, 1957). Balance between saving and investment and between new capital and expansion of the labour force will be brought about by the price mechanism. But that will not be possible in a 'fix price' economy.

In fact, in the mass production economy, much of the growth will be the firm ploughing back its profits in conditions where economies of scale exist, or the firm will be innovating – that is, developing altogether new ways of producing its list of goods and services. But either of these will imply changes in productivity, and also very likely in the skill requirements for the jobs the firm offers. This will not be a steady state, even if aggregate demand and capacity growth are in balance. To keep the economy on a growth path may well require active policy; no system of price adjustment will do this.

The Harrod and Domar models do not adequately explain growth.

Indeed, what they show is that there is a level of I, the spending of which will generate a multiplier process that will employ the full capacity of the existing capital stock. We are familiar with this in modern conditions, with inflexible prices; but it also holds for the craft economy. With wages spent on consumption and profits invested, this can be expressed as

$$\text{Aggregate demand} = \text{capacity output},$$

which is to say

$$I \times \text{multiplier} = K \times \text{productivity of } K\text{: } I/z = Y/v.$$

But with all firms operating at the optimal point, the multiplier,

$$1/z = Y/P, \text{ and } v = Y/K, \text{ so}$$

$$I(Y/P) = K(Y/K) \Rightarrow I/P = 1, \text{ that is, } I = P.$$

Next, consider what happens when

$$I/z > K/v.$$

This will tend, in craft conditions, to drive up prices relative to money wages, so P will tend to rise; the multiplier will thus fall. When $I/z < K/v$, prices will fall and the multiplier will rise. The position is stable.

Of course, the above also implies that $I/K = z/v$, where z and v are defined for the optimal level of operation. When $I = K(z/v)$, the level of demand will fully employ the available capacity; moreover, z/v will define the growth in I at which the demand generated by the multiplier (applied to the increase in I) will just balance the new capacity the investment creates, thus maintaining full utilization as the economy expands (Domar, 1957).

But when the shift to mass production takes place, these relationships change in important ways. First, z and v are no longer defined for the unique optimal level of operation. They hold for the full range of constant variable costs. And since there are no longer price adjustments, the position is no longer stable. That is, both the level of I and the growth in I tend to be unstable in the sense that small variations in I will send a signal calling for a quantity adjustment in the wrong direction.

In any case, this still leaves open the question of what determined the level of I in the first place: it does not explain why firms should wish to expand their capacity; nor does it help to account for the prices they will propose to charge for their new output. Nor does the Harrod–Domar approach provide insight into the real wage, or the productivity of the new investments.

Insight into why firms expand requires going beyond the Harrod–Domar framework, which deals only with investment spending, to look into planning for growth. There is a considerable literature on this; investment plans and long-run prices tend to be determined together, along with the choice of technology, including product design. Very broadly, firms will want to expand if they see their markets are expanding; they build new capacity in response to the expectation of growth in demand, which may be stimulated by better products, or by lower costs and prices. The growth of markets in general will be greater or faster, the lower are prices. This is similar to the accelerator, but here it is combined with considerations of price (Nell, 1998a).

To determine the plans for growth, as opposed to current spending on growth, *two* equations are needed because, in general, growth and prices will be determined together: one equation will show the growth of demand as a functions of prices; the other the growth of supply, also depending on prices. According to the first equation, higher prices will mean a lower rate of growth of demand, and lower prices a higher rate. High prices will make it harder to break into or develop new markets, low prices will make it easier. According to the second, higher prices will provide the funds that will finance investment for a higher rate of growth (Nell, 1998a, chs 10 and 11). These can be solved for planned prices and growth. (Being forward-looking, of course, these equations are subject to a great deal of uncertainty, and are thus liable to frequent revisions.)

Here is how such a model might look[45] (Nell, 1998a, pp. 477–8; Nell, 2002, pp. 261–3):

$$g = g(w/\pi, x), g'(w/\pi) < 0;\ g'(x) > 0$$

$$w/\pi = w(g, x), w'(g) > 0; w'(x) > 0$$

$$x = x(g, w/\pi), x'(g) > 0, x''(g) < 0; x'(w/\pi) > 0 \text{ up to a point, then } < 0.$$

The first equation is the wage-accumulation curve, assumed linear here; it is negatively sloped and will shift with changes in productivity. The second is the wage-rate growth of demand relationship, and is assumed sigmoid in shape. A rise in the level of the real wage will increase the rate of growth of demand – because higher incomes will raise new households into the middle class, and set them on their way to establishing a new lifestyle, which will mean new basic expenditures and additional investment in human capital (Nell, 2002, pp. 257–60). At low levels of the wage, a rise will bring only a little increase in the rate of growth of demand, but as the wage rises further this will speed up, and then finally slacken off. The shape of the curve will therefore be sigmoid. The third equation is a Verdoorn–Kaldor relationship between productivity growth, output growth and the real wage.

To spell this out adequately would take us far afield. Given reasonable assumptions, these equations can be shown to have a unique positive solution, stable by normal criteria (Nell, 1998a). But this will not be a 'long-term equilibrium', nor can it really be stable; demand is growing because families are trying to improve themselves. They are changing their patterns of spending, and they are innovating, and so are businesses. This is not steady growth; the economy is transforming itself, and many interesting dynamic patterns can be explored. (An obvious one is the tracing out of the sigmoid curve as investment shifts the wage-accumulation curve; this will provide a picture of the business cycle.)

So it is clear that growth in these conditions will no longer necessarily exhibit the golden rule in equilibrium, nor will it tend to be stable. Indeed, equilibrium may be very difficult to define in these conditions, given that innovation and productivity increases are ubiquitous. Instead, economic analysis should perhaps look to determine the direction, speed and extent of economic change.

11.4.4 Summing up and Moving on

Growth should be considered a market-driven process, especially by those in the neoclassical tradition; but the Solow–Swan model – more or less the neoclassical standard – does not contain a price mechanism, in spite of assuming diminishing returns and requiring that the marginal conditions will be met. However, a model of 'replicative growth', based on historical conditions, can be developed that shows growth being driven by a stabilizing price mechanism that tends towards optimal positions. This has a neoclassical flavour, but the system puts excessive pressure on profits, which will lead firms to innovate to gain greater control over their costs. Such innovation will lead to a new, flatter production function, with a different structure of costs, which, in turn, will bring about a different pattern of adjustment. This is the process of transformational growth.[46] The new system has constant costs and no marginal productivity conditions; in the short run, prices tend to be fixed, reflecting the constancy of costs and the flexibility of employment, so that adjustment takes place through the multiplier–accelerator. Growth comes about through investment that may be 'extensive' – new plant and equipment, new facilities and new projects – or 'intensive'; that is, ploughing back profits into the reorganization and reconstruction of present facilities. But whichever it is, the plans for growth must be developed in conjunction with plans for prices, since the expected growth of the market will depend in part on the anticipated prices.[47]

But the macro models discussed so far have a serious shortcoming. They sum up the entire financial system in a single variable, *the* rate of interest, *i*, which is then implausibly introduced as a major determinant of investment and partial determinant of consumption. This, in turn, is seen as the major connection between the financial system and the real economy. In fact, summarizing much of Keynesian and post-Keynesian research, there are many rates of interest, they do not always move together, none are major determinants of investment (though they are sometimes an important influence, and on durable consumption as well), and the connections between the real and financial sides of the economy are more subtle and more complex (Nell and Semmler, 2009). We can do much better than that.

11.5 REAL AND FINANCIAL INTERACTION

11.5.1 A Minsky–Nell Approach

Our macro approach so far, on which we built our growth models, could be called Klein–Nell; we can now introduce a better account of the financial sector and move to 'Minsky–Nell'. There is no reason to restrict the model to linear relationships, and good reason to introduce non-linear ones; they extend the analysis to multiple equilibria, and make it easier to study instability, important when we come to analyse real-financial interactions.

We can redesign the model by appealing to the analysis in Nell (1998a, ch. 13). We retain:

- the output–employment function – this will be reliable;
- the consumption income function, adjusted for wealth/profit income; and
- interest rate – this will be reliable except for the interest rate component.

But now we replace the investment function and introduce an equation system to determine investment. We define two functions, one which shows the returns generated by investment spending, the other showing the level of returns required to sustain a given level of current spending on investment projects. These functions are written on the assumption of a given long-run position determining target prices and the 'normal' rate of growth. When the long-run position changes, the two functions below will be affected:

1. *rE* function: This function will show the profits generated by the various possible levels of investment spending. It will be reliable, since it is based on the multiplier. It can be written to show the level of profits generated by a given level of investment spending, or it can show the rate of profits realized at each level of the rate of growth.
2. *IF* (investment finance) function: This will capture most of the volatility. It is based on the financing of investment spending, and shows the level of profits needed to support a given level of investment spending, in the light of the way that spending is financed, and in the light of the risk entailed by that level of investment activity.

That is, the *IF* shows the level of current profits required to continue the current level of investment spending. Its intercept depends on autonomous or fixed business costs, and its initial slope (at low levels of investment spending) depends on the leverage ratio; that is, on how much firms are willing to borrow, given their current profits. (This borrowing enables them to finance investment.) But higher levels of leverage bring higher risks. And over time, higher borrowing means higher fixed costs; and this, in turn further increases risk. Since risk will rise as the level of current investment spending increases, the *IF* curve will turn upwards.

No concept of the (representative) interest rate is needed here, yet interest costs play an important role. When interest rates fall, a higher degree of leverage can be tolerated, and also the degree of risk will be lower – so the slope of the *IF* declines, and its curvature flattens. When interest rates rise, acceptable leverage falls, and the degree of risk rises, so the curvature will increase. Interest costs are a major factor in the intercept of the *IF*, also.

11.5.2 Investment Decisions and Investment Spending

The business firm is the central player in this story of the cycle. Business decisions as to prices and investment set the stage; business spending, implementing those plans, determines the *I* component of aggregate demand, while the firm's current employment and output decisions establish the wage bill, and thus largely fix household consumption, the *C* component of aggregate demand. By contrast to business, households and banks are comparatively passive.

The spending on investment, to implement the decisions, will be determined by balancing the earnings firms can reasonably expect, given the expected level of activity, against the earnings required to safely carry out a level of investment. This provides the rationale for the two economy- wide or aggregate functions just outlined, relating the current (realized) rate of return on present capital to investment spending on the acquisition of new capital, in relation to existing capital. (The axes are therefore *r* and *g*.) Capital is valued at the supply price, but we assume that the investment being carried out reflects implementation of investment decisions made by balancing capital asset demand prices – the expected streams of quasi rents – with the corresponding supply prices. Hence the value of the capital stock is well defined.

The first aggregate function will show the rate of return on the current capital stock generated by investment spending, through the multiplier; the second, the rate of return required on current capital in order to support a level of investment, consistently with firms' other obligations. These two are differently constructed. The first shows the results of an aggregate process – sales resulting from the multiplier – distributed over the population of firms. The second, however, is the aggregation of the individual calculations of firms: each firm considers what rate of return it would currently require in order to feel safe investing a certain amount. (Conversely, given a current rate of return, how much investment would it feel comfortable undertaking?)

More specifically, in the first of these relationships, the *rE* function, the rate of return generated through the multiplier by different levels of investment expenditure, will rise from left to right. It will begin from a positive intercept expressing fixed income consumption and autonomous investment, including government spending on infrastructure. Note, however, that autonomous investment and government spending will tend to generate growth, especially productivity growth The second function, *IF*, shows the rate of return that must be currently earned for firms to feel justified and secure when carrying out such investment spending, while meeting their various fixed-cost obligations. Any spending commitments carry risk, and have opportunity costs. To justify such commitments, firms must feel that their current cash flow will be sufficient to cover the risks they imply, given their importance. That is, firms can be expected to attach a degree of importance to the various stages in implementing their investment plans. Each level of investment spending represents a different level of implementation of investment plans. The different stages may all be considered equally important, or they may carry very different weights, some urgent, some less so. For example, all components of investment might be considered equally important; higher levels of investment would then carry proportionally higher weight. On the other hand, low levels of investment might cover spending that the firm felt absolutely must be done, while higher levels might include projects whose urgency is less immediate. That is, there would be a large risk or anticipated loss from not carrying out certain spending, but delays in other projects would be less costly, or not costly at all. Thus, as the level of investment spending rose, the urgency at the margin would decline. For low levels of investment, with high urgency, only the minimal current cash flow would be required to cover them. But as marginal urgency declines, higher levels of investment spending would require greater marginal coverage – the extra risks would be less worth taking. In the first case, the cash flow required, represented by *r*, would rise at a constant rate with the level of investment, whereas in the second case it would rise at an increasing rate.

Let us take the case of constant importance first. The function will begin from a positive intercept, reflecting fixed costs, and then rise with a constant slope, showing the rate of return minimally required to support each level of investment spending. This rate of return will not have to cover the full costs of the investment – which would imply a slope equal to that of the *rE*. The possibility of external finance permits it (initially) to have a shallower slope than the *rE*. A rise in investment spending requires an increase in current earnings to underwrite it, but the increase is less than the rise in spending. Only a fraction (usually a majority) of current investment spending comes from retained earnings; the rest can be borrowed, usually with a minimal outlay in expenses. Given a debt–equity ratio, the slope will be constant, and if the ratio is maintained (and the interest rate is unchanged), the intercept will remain the same from period to period, as fixed costs will rise at the same rate as capital.

These two together form an unstable system, as can be seen in Figure 11.10. (If *IF* lies wholly below *rE*, the system will be unstable

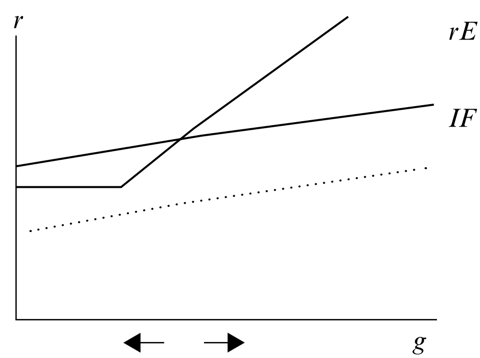


Figure 11.10 Interactions in a growth cycle

upwards; if they cross, the intersection will be unstable in both directions.) However, the effects of fixed-cost obligations must be brought into the picture; as current spending on investment projects rises, after a point the possibility must be considered that current sales might falter. Risks would be greater. Moreover, as noted earlier, the urgency attached to some of the components of higher levels of investment will be lower. Hence the *IF* curve will turn upwards and rise with increasing steepness, indicating the extent to which the rate of return must rise to offset the increasing risk attached to progressively higher levels of investment spending.

The intercept of *IF* will change as fixed costs – financial obligations and contractual managerial costs – are adjusted to reflect current market conditions. As productivity changes, affecting inflation (as outlined in Nell, 1998a, ch. 11), the position of the *rE* function will change. Productivity changes may also affect the *IF*. These shifts in the two curves determine the movements of output and employment, and the interaction traces out a simple cycle. But first we need to explore the curves in more detail.

11.5.3 Increasing Risk

It is common to define two kinds of increasing risks: borrower's and lender's. Borrower's risk is a subjective judgement that reduces the expected value of a stream of quasi rents and rises with investment. Lender's risk manifests itself in bankers' demands for higher rates or shorter maturity dates; it also rises with investment and will shift with changes in debt–equity ratios. There are two problems with this approach. First, it is usual to apply both to the calculations involved in the plan – the investment decision. Lender's risk, certainly, and a practical version of borrower's risk, should be considered at the stage of implementation, as part of the cost of investment spending – since the risk is the risk of failing.

But it does not follow that errors will be random, for the level of sales will not be the result of a deterministic process subject to random shocks. On the contrary, the model here is subject to sudden and partially – but not fully – predictable switches in the direction of movement of major variables. Many changes will not be random, but they will not be altogether predictable, either. Expectations will therefore be subject to uncertainty, and different agents can reasonably come to different conclusions.

In these circumstances, the state of confidence becomes important. All available information may indicate that sales should continue strong in the immediate future; but perhaps the bloom is fading from the boom. There is no hard evidence of troubles ahead, but the time is ripening, and the downswing could begin any moment. So the expectation of strong sales will be held, but with weakened confidence. Conversely, when the slump is over and the upswing begins, confidence will improve. The effect of changes in the state of confidence is to increase or decrease the curvature of the *IF*. When confidence falls, a higher subjective risk premium must be added, increasing the upward curvature; when it rises, a subjective factor can be subtracted from risk, and the curvature will be flattened. (Any weakening in the normal rate of growth of demand, any weakening of confidence in it, any widening of the variance in expectations, will be reflected both in an increase in the curvature of the *IF* and in its propensity to shift with changes in the rate of interest.)

The other factor is the cost of failing to meet obligations. If revenues fall short, either fixed obligations or contracted payments for the investment project will have to be postponed or renegotiated. In either case, penalties, legal fees and/or emergency borrowing will be required. These costs will be larger, the larger the sums involved; hence they can be expected to rise with the planned level of investment spending.

The sum measuring risk, then, is the product of the probability of failure multiplied by the anticipated cost of failure, and the rate of return that would compensate for risk is that sum divided by the capital presently in the market. Notice that even if both the probability and the cost of failure rose at a constant rate with planned investment spending, the sum at risk – their product – would rise at an increasing rate. An example: let the probability of failure rise linearly with *I*; as *I* rises 1, 2, 3, 4, the probability rises: 10%, 20%, 30%, 40%. Let cost rise linearly with *I*; as *I* rises 1, 2, 3, 4, the cost rises 100, 200, 300, 400. Then 'risk', meaning the product of probability of failure multiplied by the anticipated cost of failure, rises with *I* (1, 2, 3, 4): 10, 40, 90, 160 (see Nell, 1992c).

Risk can now be added to the *IF* curve, causing it to turn upwards and rise at an increasing rate with investment spending (Figure 11.11). Moreover, as noted above, the marginal urgency of higher levels of investment will be lower, so that the cash flow coverage required will be proportionally higher. Putting the curves together – taking the origin as (r_0, g_0) to simplify the figure – there are three possibilities, depending on the position of the *rE* with respect to the *IF*. Taking the top *rE* line first: the *IF* could cut *rE* from below; the second *rE* line illustrates that *IF* could intersect *rE* twice; while the third shows that it could be just tangent from above. When the required rate is above the market-generated rate, spending will be cut back; in the reverse case, spending will be expanded. Hence, when the *IF* cuts the *rE* from below, investment spending is stable. In this case, it is clear that financial markets constrain an otherwise unstable system. When it intersects from above, however, investment will be unstable, and the tangency point is unstable downwards. Notice that when the *IF* cuts the *rE* from below, the implication is that if growth is low, the system will expand

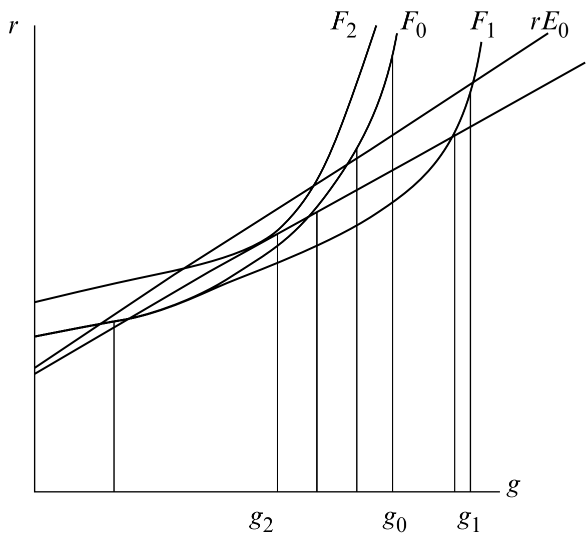


*Figure 11.11 Investment finance (*IF*) curve and risk*

in a boom, whereas if *g* is high and the *IF* is tangent, there will be a downswing. These movements have implications for inventory policy. If there is a possibility of an upswing, firms must have inventory at the ready. For, as the system swings up, shortages will develop. If a given firm, waiting to be sure, has failed to stock up, and so, as the boom takes off, lacks inputs where its competitors have them, it will lose markets. Similarly, when a downswing looms, firms must be careful about cutting inventory. If, anticipating a slump, a firm tried to run lean and turned out to be wrong, they would lose markets to those who didn't. On the other hand, if the slump did come, those who cut back early may weather it better, but they wouldn't gain any ground on competitors. Inventory mistakes are likely to be more costly, or costly in a more permanent way, than is justified by the gain from being right. On the whole, firms are better off carrying inventory that is likely to prove excessive. So far, we have considered investment on the basis of expectations of the revenue generated by the anticipated level of current spending. Clearly the higher this is, the lower the probability of failure, and so the lower the risk; hence the higher will be investment spending. But there is another element to consider: the rate of interest, for this will affect both the level of fixed costs – the intercept of the *IF* – and the costs of failure, a determinant of its slope.

The Minsky–Nell approach is demand-determined, allows for cost and supply variations, deals with financial-real issues, models increasing risk, and not only does not depend on an overly simple and unrealistic rate of interest based investment function, it puts investment at the centre of the model, balancing the gains from investment against the costs and risks. The *rE* and the multiplier processes are reliable; as are some aspects of the *IF*. But in general the *IF* will be volatile. So we can see clearly the interaction between reliable and volatile sides of the economy.

In terms of the MTC, it is conceptually coherent – it draws on well-established Keynesian precepts – and it is applicable/relevant. The distinction between investment decisions or plans and the spending to implement those plans, current investment spending, is well-grounded in business practice. The concepts of profits, debt, risk and aggregate business spending are all obviously measurable. Moreover, improvements or clarifications in theory will enhance applicability and measurement, and vice versa – a mutually beneficial interaction can be set up.

Yet, for all that, there are some shortcomings. Although the *IF* is based on financial activities, it does not present any detail about the financial side of the economy, and it gives a very slim account of the factors underlying increasing risk. Indeed, it rests on a very simple, unitary idea of risk, whereas even a little reflection will tell us that there are at least two clearly distinguishable concepts of risk.

11.5.4 Some Observations on Risk

There are long and short markets, both public and private, and they interact both with each other and with equity markets. We can represent them by drawing up a financial quadrangle, with the degree of default risk on one axis, and the length of time on the other (see Figure 11.12). (Later we shall redefine this as market or liquidity risk.) Just looking at this shows that the rate of interest on any security can be positioned on this map;

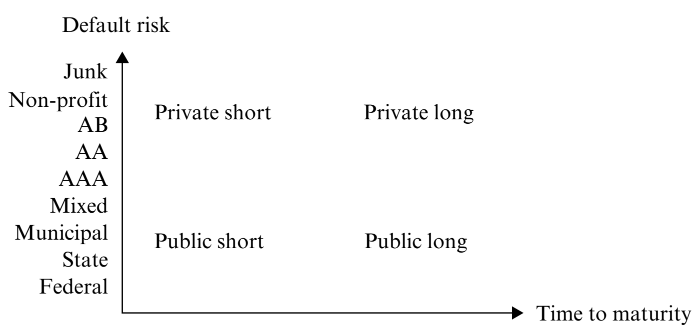


Figure 11.12 A financial quadrangle

any rate of interest will have a position in the ratings, and it will have a time to maturity. In other words, any rate of interest may be said to have an angle or direction. The quadrangle neatly displays the four important sub-markets or natural 'habitats' in the financial system: public short and public long, private short and private long.

11.5.5 Default Risk and Market Risk

Let's replace the time to maturity by the degree of market risk – the longer the time, the greater the risk. We now have two kinds of risk, both of which relate to true uncertainty. Default risk arises because we cannot know that the inventions will work, that the innovations will be welcome, that the market will expand as much as we hope, that productivity will rise – we are uncertain about the future of the real economy. *Market risk* is inherent in the working of financial markets; we do not and cannot know how views of the future will develop and affect the way supply and demand will interact in the market, or what effects external shocks will have. In both cases, the best we can do is make bets; we add a premium to try to cover the risk, but even though we make precise calculations, the further ahead we look, the less reasonable are the grounds for the precision. We do not know; but we do the best we can.

Default risk as used here is essentially the same a 'credit risk' in many finance texts; it is the chance that the loan will not be repaid, or that the security will become worthless because the underlying economic project has failed. It is a risk that arises ultimately from the real economy. It depends on the relation between the structure of costs in relation to the volatility of demand in the relevant markets. If the margin between costs and revenue is large and if costs can be readily varied when sales vary, default risk will be low. But if the margin is low, and the costs are sluggish and hard to vary, while sales volatility is high, default risk will be high – and even higher if input, energy or labour costs are also volatile.

We might consider calculating risk (see Nell, 1998a) by establishing the distribution of returns for the economy as a whole, or for a group of relevantly similar firms, and observing its shape. (This will entail assuming that present and past observations are a good or workable sample, and presage those to come.) Set out the distribution, and then draw a 'bankruptcy' line; when returns fall below this line, firms cannot meet their current obligations. Then establish the expected cost of renegotiating obligations and/or reorganizing the company. The area under the distribution curve at or below the bankruptcy line taken as a ratio to the total area under the curve will give the probability of bankruptcy; that probability, multiplied by the expected cost of bankruptcy, will be the measure of default risk. If the economy swings up, the distribution will tend to shift to the right, and the probability of default will fall; if it slumps, the distribution will likely shift to the left and the probability will rise.

Here risk will be constructed as a frequency; see below. But this depends on there being a class of similar securities or firms that fluctuate in value; default risk for a representative security or firm is then defined in terms of the percentage of fluctuations that fall below a critical value and the cost that arises as a result of falling below that value. A large literature takes a different perspective and seeks to derive default risk from the value of the firm, taking into account its financial structure, sometimes paying particular attention to credit spreads. (Moody's KMV model is a well-known example of this.) There are reasons to take this approach – for example, when a firm or a security is unique or uncommon, so that there is no class of comparable items – but there are two serious problems. First, the value of the firm – and its financial structure, including credit spreads – depends in various circumstances on some or many factors that have nothing to do with default risks. Second, it is not clear what the participants in the market know that external observing economists do not; why should we think that the market's weighting of the opinions of its participants is a better judge of default risk than that of outside observers? (For example, under many circumstances markets are likely to exhibit herd-like behaviour, especially if they experience waves of optimism or pessimism.)

Market risk is the risk, estimated on the basis of volatility, of capital loss on a security or set of securities, due to a rise in interest rates.[48] A fall in the relevant interest rate would produce a capital gain, but the anticipated gain from holding the securities will have included such gains as well as the normal yield. So the risk associated with the proposed gain from the securities will be the downside risk, based on the demonstrated (and expected) volatility. Note that a certain percentage fall has a negative impact larger in absolute terms than the positive impact of the same percentage gain; in conventional terms this would be attributed to diminishing marginal utility. (Here, however, we might attribute it to the increased danger of bankruptcy that a loss brings.) The independent basis of market risk comes from the ability of financial markets to develop self-sustaining, or self-reinforcing, patterns of movement, bubbles or spirals moving upwards or downwards.[49] Stocks begin to rise, for example, generating optimism and demand for credit for speculation; because stocks have risen, the capital of banks and financial institutions generally increases, permitting the supply of credit to expand, leading to further stock price appreciation – and so on. Agents believe or come to believe that the rise in security prices is due to a correct understanding of future prospects. Each time they borrow and invest, more prices go up, confirming the wisdom of their decisions. This can take place independently of developments on the real side of the economy.

What are the implications of this for Minsky–Nell? First, it will allow us to develop a better account of the reasons for increasing risk, leading to better ways to model it. Second, it calls for a study of the interaction between market risk and default risk, adding a whole new dimension, since these two relate differently to the real side of the economy. Third, and perhaps most important, we can connect this with our earlier account of the present and the future in relation to capital values, and develop this into a full-scale model of capital markets.

Again, the object here is not to develop a macroeconometric model; it is to explore possibilities and suggest the direction in which development should take place, providing examples that indicate what we mean by conceptual analysis and fieldwork.

11.6 CONCLUDING OBSERVATIONS ON EMPIRICAL ISSUES

11.6.1 Keynes and Friedman on the Multiplier

The old system depended on a price mechanism; the new rests on the multiplier. These are relationships that don't depend on the uncertain future. Keynes thought that reliable relationships should be examined empirically: 'I think it most important [. . .] to investigate statistically the order of magnitude of the multiplier, and to discover the relative importance of the various facts which are theoretically possible' (Keynes, 1973, letter to Harrod, 16 July, 1939, p. 299).

By contrast, Milton Friedman, quoted earlier (see p. 199),[50] argued that the uncertainties implicit in the chain of events traced out by multiplier effects rendered the idea of a stable multiplier virtually useless. But, surprisingly, a close examination of his claims actually provides solid ground for supposing that we should be able to give a good empirical account of the multiplier, provided we carefully distinguish between reliable relationships and volatile ones, tethered expectations and floating ones. Friedman is clear enough that we can see exactly where he goes wrong. Taking the points in the quote on p. 199 one by one and examining them in the paragraphs below, we see that some of his objections just evaporate, while others contain valid points but don't apply to the multiplier re-spending process: 'a complex of interrelated factors, many of which cannot be observed before the event'. But reliable relationships are grounded in contracts and obligations, and social pressures, so they can be known beforehand.

'who receives the increased outlays, how much of it they decide to save' In a practical sense, expenditure necessarily traces out the vertically integrated production and distribution relations – firms *have* to buy inputs, and *have* to pay wages and profits and interest and so on. Saving reflects business and social norms and is normally a stable function.

'the reactions of consumers to price changes' In a mass production economy prices will tend not to vary with demand; there may be inflationary pressures – that is a different story – but they can be neglected in a short-run multiplier analysis.

'the anticipations of consumers about future price movements and availability of supplies, the extent to which entrepreneurs try to expand their capital equipment' Consumer spending reliably depends largely on current income and current borrowing capacity; anticipations turn out not to be so important (cf. the well-known 'overdependence' of current household spending on current household income). Expectations of entrepreneurs – animal spirits – will either be optimistic or pessimistic, or mixed. They are volatile and will be important, but not for the multiplier, which depends only on the *responding* reliably generated by an initial outlay. The effects on investment are not relevant to the multiplier, strictly understood. Friedman mixes up long run and short run here.

'the costs that entrepreneurs must incur to expand output' These are largely wage costs; unit wage costs will be fixed in the short run; in the aggregate, the spending of wages will constitute a large part of consumer demand. This will be an important and reliable part of the multiplier process.

'their anticipations about future price movements and hence their inventor policy' It is anticipations of the volume of sales at current prices that count; relative prices do not vary much under mass production (unless costs change). (Expectations with respect to inflation might, however, influence inventory holding.)

'the flexibility of wage rates and prices of other factors of production' In the short run, under the cost and technical conditions of mass production, wage rates and most other prices will be reliably fixed in relative terms, and inflexible downwards in money terms.

'the demand for credit, the policies adopted by the banking community' The demand for credit will depend on the demand for goods and services; the banking community under the conditions of mass production will offer

lines of credit to businesses, allowing them to draw automatically when they wish to undertake production. A large class of household consumers will have credit cards or overdraft accounts. In other words, credit money will be available endogenously at the going rate of interest.

'The expansion in output depends on the quantity and kind of unused resources, the mobility and transferability of these resources, the rapidity with which output can be increased, (and) the degree of competition.' Probably the most important qualification concerning the multiplier is not even mentioned by Friedman; it is asymmetrical in that, while it always works downwards, it does not work upwards, if the economy is near full capacity. Some VAR studies that sought to discredit the stimulus package estimated the effects of upward stimuli during the 1990s, when there was little room to expand in the short run, and then announced that the value of the multiplier was too low for the policy to work. Similarly defective estimates were advanced to support the case for austerity in the Irish economy (Kinsella, 2010). Needless to say, we consider VAR a possibly reasonable way to explore data – not as good as plotting and looking at it, but useful for masses of data – though a terrible basis for any sort of conclusion.

A mass production economy always has underutilized capacity, and these resources are always readily available (except under very high demand pressure, such as during World War II). Output can normally be expanded substantially at constant or even falling marginal cost. The degree of competition can be taken as given in the short run.

The Friedman example shows that while the two cases of reliable and volatile relationships, and tethered and floating expectations, are quite different, much, perhaps most, empirical work just runs them together. An example (Nell, 1998a, p. 58) can be drawn from the generally admirable work of Ray Fair, who states that

> the word 'multiplier' should be interpreted in a very general way . . . (as showing) how the predicted values of the endogenous variables change when one or more exogenous variables are changed. (Fair, 1984, p. 301)

In Nell (1998a), this point is explored further:

> First the model is estimated, then the initial values of the exogenous parameters are set, and the values of the endogenous variables are calculated. The exogenous parameters are then changed, and the new values of the endogenous variables are found. The difference between the two sets of values shows the impact of the change; if only a single parameter is changed, then the value of a single endogenous variable can be divided by that change to calculate a 'multiplier'. The advantage of this approach is its generality; the disadvantage is that

> it incorporates into the same calculation of the impact of a change, processes that rest on foundations that differ greatly in reliability. The 'passing along' of expenditures and costs is measurable and reliable, but the response of financial variables to other changes is less so, and the response of real variables to financial variables is notoriously unreliable.

Multipliers estimated following Fair's method are unlikely to be worth much, in contrast to traditional, well-grounded expenditure multipliers. The soundness possible with purely reliable functions is mixed with the unreliable estimates of volatile relationships and while the result apparently has some value, it is evidently not up to the expected standards. Many of the problems faced by traditional econometric models can be thought of this way: the models are a mix of reliable relationships that can be reasonably estimated, and volatile ones that simply cannot. The result is an unstable mix of apparently reasonable calculations that mostly seem on target, leading to carefully estimated functions that can turn out to be wildly wrong. What is needed is to separate the two.

11.6.2 Empirical Approaches to 'Macro' Functions

So let's examine how macro models should be developed. In the conventional view, individual agents make choices, based on their preferences, when faced with varying economic data; and then these choices are aggregated. This approach builds from the bottom up: atoms are combined into molecules – but such an aggregate is, in a sense, arbitrary, even accidental. The preferences are individual and unexplained. Surely some or many of the individual choices could have been quite different – surely they could change any time, at a moment's notice. We all know that as people's lives change, their preferences change. It is unjustifiable to presume them to be constant.

By contrast, here we assume that there are contracts, obligations and social norms, combined with the technologies in place, which govern the ways households and businesses can and normally do act. Household spending, for example, is governed by the norms of the marriage contract, the normal obligations of parents to children, the expectations of employers and society at large (and the laws) regarding normal dress, the technology of transportation, and the so on. Different categories of households will be governed by somewhat different norms – working class, middle class, professionals, and the like. And the various households within a category can be expected to follow the norms somewhat differently. If we have a good understanding of the grounds for the target, and the reasons for the deviations, we can collect information about the distribution, and set up a stochastic model of the relationship.

As mentioned, the objective of the study is to represent the way the rules and norms translate into behaviour. The aggregate relationship is not accidental; it is the outcome of everyone living up to their obligations and acting according to the norms of the system. Everyone follows the norms, or lives up to their obligations, and this leads to an outcome – not necessarily one the agents had in mind. It could be a stable market outcome, or it could be a 'paradox of thrift' type of outcome.

Let's focus on the macro relations, and see how to work this out in practice:[51] first, the time series for the variables must be suitably adjusted to reflect the correct theoretical meanings of the variables. Also, either government must be 'removed' from the data, which is likely to be an arbitrary and ad hoc process, or government must be taken into account in the equations. Once this is done, we can set about estimating the reliable parts of the reliable relationships. This can only be done by removing the influence of the volatile variables. To do this, we have to find the points where those influences are at a minimum, and then compare to points where those influences are at a maximum, and use the differences between these points as the basis for constructing the reliable relations. If a function is reliable, it won't take many observations to get a picture of it. This will be especially important if we relax our ambitions, and, following Keynes, aim only to get a good estimate of the order of magnitude, rather than a precise figure, for the coefficients. We want to know whether the coefficients are positive or negative, and whether they are large or small. The assumption of linearity may serve as an approximation. Of course, the more precise the formula and the more exact the numbers, the better it will be.

We should not expect the functional relationships to hold over large ranges of variation; typically the variance will be limited. For example, the variance of unemployment over the cycle is of the order of 10–15 per cent; this should be the range of all the reliable variables. Investment, however, may fluctuate more than that; so may stock market indices, bond prices, and other financial instruments, not to mention the price level in inflationary times.

To make our estimates, we look for periods in which the volatile variables are constant, while the reliable ones are fluctuating. We can also use data from time periods where the volatile influences are weak or nonexistent – which must be determined from independent data.

Reliable functions represent real processes, in the same sense as in applications of statistics in manufacturing and industrial processes. 'Real processes' here can be taken to mean processes such that, if operated differently, would cause the agent to incur costs. Alternatively, if the processes do not achieve the desired results to a specified degree of accuracy, costs will be incurred. If employment yields more or less output than it 'should', given the design of industrial and commercial processes, work norms, standard hours and labour discipline, then companies will incur costs. If 'too much' has been produced, they will incur inventory costs, and will have to adjust output to sales; if 'too little' has been produced, they will find it difficult to meet delivery schedules and will have to run down inventory and adjust output again. Similarly, households must spend the right amount to maintain themselves and keep up appearances; if they do not, they will suffer practically and/or socially.

Probabilistic methods can be used in the case of reliable functions. We argued in Chapter 9 that Allais's (1997, p. 7) contention in his T Theorem is that a large number of interacting cyclical fluctuations, from divergent sources and with incommensurable periods, will generate an 'almost periodic function' whose successive values over time will be normally distributed. This is a 'time series' generator of a normal distribution of errors. There is likewise a possible 'cross-sectional' generator of normally distributed errors. We argued in Chapter 8 that Calot (1967a) suggested a perhaps related set of conditions for a normally distributed random variable. He argued that a normal distribution of random errors – deviations from the true value – would arise from a large number of independent impacts on a manufacturing process, where these arise from exogenous causes, which are additively random, and where each has a small variance relative to the total variance. Both authors are dealing with the interactions of deterministic processes showing that even though these are deterministic, with specific cause generating specific effects, the result of the interaction is a normal distribution of errors. (In each case, presumably some portion of the errors could be reduced, by isolating one or more of the processes, and controlling it. But this might reduce the focus on some other processes, or it might simply not be worthwhile.)

As with many macro models, the functions here relate to flows of revenue; unlike supply and demand models, they do not show, for example, the quantity response calculated when confronted by a price stimulus. Instead, these functions show the way rules govern, say, the outflow of expenditure given an inflow of revenue into a center of economic decision/action – a household, a firm, a bank, the government. (Essentially the same holds for the application of labour, generating output.) These functions will tend to be reliable, as the revenue inflows and outflows will characterize the 'nodes' of the model, which can often be written as a network. They can also very often be observed more or less directly in fieldwork studies – household consumption out of income can be seen in studies of household budgets or in survey data. Productivity studies can relate employment and output. Balance sheets show revenue from sales in relation to the payment of wages and profits, for firms, and for banks, deposits and repayments in relation to new loans. The whole network makes up a system of simultaneous equations, but under the right conditions the individual 'nodes' can each be examined separately, so that multiple regression methods might be applied. And when reliability is well established, the disturbance terms can be plausibly considered to have most of the desired properties.

Now what about the volatile aspects of the system? When portfolios or for that matter real investments change composition, the specifications of the volatile functions will have to be adjusted. When expectations change, the shape and position of the present/future functions will tend to change. Variables may be affected. For example, spreads may increase, and the term structure may widen. In that case, long rates and risky rates will be driven higher compared to the safe short rate. This could induce a larger negative impact on investment spending. Most of this is well-known; our point is that fieldwork and conceptual analysis will help us to figure out the way stable relationships and variables will be affected by the volatile ones, for this will be driven by normal economic motivation and the effects of competition – but when expectations are floating, there is no way to reliably anticipate changes in the volatile aspects of the economy.

11.6.3 Implications of the Reliable/Volatile Distinction

Reliable functions and variables are grounded in the conditions of the present, while volatile functions and relations are determined by expectations of the future. But the distinction goes beyond that: reliable relationships are largely real, while volatile ones are monetary and financial. The short-run output–employment relation is reliable and real; so is the relationship between real income and consumption – but the price level as a function of the exchange rate is monetary and volatile, and the price level as a function of the quantity of money is notoriously volatile. The speculative demand for money (or securities) as a function of the interest rate is volatile and financial. What this distinction does, then, is help to formulate the general claim that, in the short-run, the real side of the economy is persistently 'shocked' by fluctuations emanating from the financial side. But these financial fluctuations are not themselves necessarily 'caused' in any systematic way, by any systematic 'forces'. The functions are inherently unstable, because they depend on our expectations of the future – the correctness of which cannot be determined. Hence an indefinitely large list of factors can influence these expectations in an indefinitely large number of ways.

Consider how 'true values' could be established in the contrasting cases of consumption and investment. For a reliable function such as consumption, the 'true value' of spending will reflect the normal social expectation of the level of consumption that will support a family at the appropriate social level. (If the parents do not properly care for their children, they may be in trouble with the law – or the neighbours!) This true value can be found, at least approximately, by fieldwork, and will be the same for all families at that social level.

Contrast this with the investment function. The 'true value' of investment spending would have to be based (among other things!) on a calculation such as that for the marginal efficiency of capital – that is, on the discounted sum of the expected net earnings over the lifetime of investments. To calculate this requires knowing future prices, wages, the future performance of technical equipment, and so on. Different agents will surely have different views of the likely values of all of these; none will have solid grounds for their expectations – no such grounds exist. It will also be necessary to know how fast markets can be expected to grow, and what the plans of competitors are, and whether new technologies will come on line, making one's own obsolescent. Data can be gathered and estimates, or at least educated guesses, can be made on all these matters – but the inherent residue of uncertainty cannot be eliminated. At some point the case will be strong enough for a company to put up the money – or to pull out! But what we act on is not a true value, or an estimate trying to approximate a true value; it is a good bet! In short, true values can be reasonably said to exist for consumption; but they don't exist, or are very fragile at best, for investment.

11.6.4 The Reliable/Volatile Distinction in a Klein–Nell Model

We will write out a simple Klein–Nell model in order to indicate clearly which parts of the model are reliable and which are volatile. Then, in solving it, we can show the reliable parts as functions of the volatile, where fieldwork, common-sense, vernacular knowledge and conceptual analysis could help us write short-term hypotheses for the volatile relationships. With this we can get reduced forms that can be estimated.

This model is illustrative only. The consumption function is not adequate; wealth and borrowing capacity would have to be added, especially for upper income levels (Klein's model does capture this), and government welfare and unemployment support should be shown for lower income levels (Klein is also good in this regard). The output–employment function is adequate, although it would surely be good to distinguish sectors. But money and banks are needed, and there is no mention of the stock market or the bond market. Income is paid as wages and profits, but there is no connection between the distribution of profits and the payment of interest. Investment should be shown to depend (partly? largely?) on self-finance; it will also reflect the stock market. Earlier, we offered an approach to inflation; this should be added to the model. As we noted, if we reject the neoclassical account of the labour market, two variables are needed to represent inflation, the money wage and the price level, and it is necessary to model the distributional effects as well. Making these changes would create a better model, and fill out the picture of the volatile functions, but would depart significantly from the design of the Klein models (see Nell and Errouaki, 2008a).

First, let's note that a Klein–Nell model would readily fit into the framework of the methodological 'triangle-circle': theoretical–coherence, relevance–applicability, and measurement–quantification. The theory assumes a structure of production, a Leontief–Sraffa background, with a Cambridge approach to effective demand, and a modern monetary system. No agents are assumed to have impossible or unrealistic powers (perfect foresight, costless mobility); no unexplained institutions are postulated (Walrasian auctioneers). So the theory is coherent and the demand side reasonably well worked out, with the supply side implicit, but available from other input–output studies. Applicability is guaranteed by fieldwork and by our knowledge of the vernacular. Measurement is provided by national income statistics.

We saw earlier that Klein's model is based on three behavioural equations – consumption as a function of wage and profit income, investment as a function of profits, and demand for labour as a function of wages. Here is a slightly different version, with essentially the same three behavioural functions, somewhat differently specified but now divided into Reliable and Volatile relationships, with Volatile variables and parameters written in bold italics.

$Y = aN$	Output is a function of employment
$C = C_1 + b\mathbf{w}N - \boldsymbol{ci}$	Consumption depends on wages and interest
$\boldsymbol{I} = \boldsymbol{I}_1 + \boldsymbol{j}C - \boldsymbol{ki}$	Investment depends on consumption and interest
$\boldsymbol{i} = \boldsymbol{i}^*$	Monetary Authority pegs interest
$Y = C + I$	Equilibrium
$Y = \mathbf{w}N + P$	Income

Here $\mathbf{w}$ is given (for the short run); Y, C, I, N, P, i are the six variables.

The output function is reliable; the consumption and investment functions each consist of a reliable part, and a volatile term.

To complete the model for practical purposes we would need to add the Government sector:

$T = T_1 + tY$	Taxes consist of inheritance, etc., plus flat income tax
$G = G_1 - gY$	Government spending falls as employment rises

Both of these will be reliable functions. A tax term must then be introduced into the C function, and G added to the equilibrium.

Aggregate variables are combinations; reliable ones keep their proportions intact, while volatile ones may change unexpectedly. Consumption in the short run is reliably composed of durables, non-durables and services; the income of households is reliably composed of wages, salaries, earnings from self-employment, and for wealth-owning households, dividends, interest and capital gains. But the rate of interest is an average of short and long rates, on private loans and bonds of various grades, Treasuries, etc., and the composition of portfolios, together with the spreads and the term structure, can change unexpectedly.

Reliable parameters will change value only slowly, and in ways that can be understood and foreseen. They are based on existing contracts, technology, obligations and social norms. It is reasonable to try to estimate the parameters here using the Cowles methodology. Volatile coefficients and parameters, however, are based on expectations of future market developments and can change unexpectedly and in unexpected ways. They may indeed be fluid, at any time, and may take a range of values. The first effort should be to determine the range, and then find whether there is a tendency for the various agents to behave in a similar manner, or to diverge. If the pattern of behaviour is to pull together, then the task will be to find the most likely values within the range to which behaviour will converge. If the pattern is divergent, the task will be to find the limits of the explosive behaviour.

Reliable variables and parameters	*Volatile variables and parameters (written in italics)*
Y, N, C, and some part of I in the short run (Committed I spending may be reliable)	I in the long run, i (I_1 is autonomous); I_1, k, c – and j
a, C_1, b, $\mathbf{w}$	$\boldsymbol{k}$, $\boldsymbol{c}$ likely move together
In the long run: j (j is the ratio of I to C, wh ich will tend to a steady level in the long run.)	$\boldsymbol{I_1}$ and $\boldsymbol{j}$ likely move together

Leaving Government to one side, then, substituting and regrouping:

$$N = ((1 + \boldsymbol{j})C_1 + \boldsymbol{I}_1 - \{(1 + \boldsymbol{j})\boldsymbol{c} + \boldsymbol{k}\}\boldsymbol{i}) / \{a - (1 + \boldsymbol{j})b\mathbf{w}\}$$

Further regrouping, we obtain a single reduced form equation:

$$(a - b\mathbf{w} - \boldsymbol{j}b\mathbf{w})N - (\mathbf{1} + \boldsymbol{j})C_1 = \boldsymbol{I_1} - ((\mathbf{1} + \boldsymbol{j})\boldsymbol{c} + \boldsymbol{k})\boldsymbol{I}.$$

This shows N as a function of i, which will be set by the Monetary Authority. This would enable us to solve for N. But no reliable relation between N and i can be formed, nor can a decisive test be conducted. For, here, the left-hand side of the equation consists of *reliable* terms (except for the appearance of j; however, although j is volatile in the short run, it is reliable in the long run, and the long run value can be used, with caution). The right-hand side has the volatile terms. Thus N on the left-hand side will vary as a function of i, the volatile variable, and it will also vary with the fluctuations of the unreliable parameters. In short, N as a function of i will be a mix of volatile and reliable.

Rewriting,

$$N = (\boldsymbol{I}_1 + (1 + \boldsymbol{j})C_1)/(a–(1 + \boldsymbol{j})b\mathbf{w}) – ((1 + \boldsymbol{j})\boldsymbol{c} + \boldsymbol{k})\, \boldsymbol{i}) / (a–(1 + \boldsymbol{j})b\mathbf{w})$$

Now let

$$D = (a–(1 + \boldsymbol{j})b\mathbf{w}).$$

Then we have

$$N = \boldsymbol{I}_1/ D + (1 + \boldsymbol{j})\, C_1/ D – ((1 + \boldsymbol{j})\boldsymbol{c}+\boldsymbol{k})\, \boldsymbol{i})/ D,$$

where $1/D$ can be considered the employment multiplier.

D is reliable throughout; the multiplier is not only reliable – it can be estimated without reference to simultaneous equations. The second term is reliable. The numerator of the first term is not reliable, but it could be estimated directly from fieldwork, interview data, and studies of corporate plans. So the major source of uncertainty will be the numerator of the third term.

If k and c move together this will make the effect of the rate of interest more pronounced when it is stronger and even less important when it is weak. When k and c are both zero, N will depend only on reliable coefficients, autonomous consumption and the (volatile) level of autonomous investment. So it should be possible to distinguish interest-responsive periods from interest-unresponsive ones. When i changes composition, the specifications of the functions will have to be adjusted. Suppose that spreads increase, and the term structure widens. Long rates and risky rates will be higher compared to the safe short rate. This should induce a larger negative impact on investment.

As noted earlier, like the work of Klein, this model is a revenue-flow model; the functions relate flows of revenue; they are not single-variable stimulus-response functions. They concern the inflow of revenue into a centre of economic decision/action – a household, a firm, a bank, the government – and the corresponding outflow, assumed to follow a regular rule. In the case of the employment function, of course, it is the inflow of labour, rather than revenue. Revenue inflows and outflows tend to be the Reliable functions, characterizing 'nodes' of the network of circulation. Thus, in the case of household consumption, income flows in, expenditure flows out. In the case of business firms, employment is the inflow, output the outflow. Revenue from sales is an inflow, payment of wages and profits an outflow. For banks, deposits and repayments of loans are the inflow, loans are the outflow. Under some conditions the relation between outflow (for example, consumption spending) and inflow (income) could be observed directly, without going through the estimation of a simultaneous equation system. The whole network is a system of simultaneous equations, but under the right conditions the individual 'nodes' can each be examined separately. Thus simultaneous equation problems can be evaded, and multiple regression methods can be applied. For the reasons suggested above, the disturbance terms can be plausibly considered to have most of the desired properties.

11.6.5 Evidence?

What is the evidence? Surely a conventional economist will simply deny that the economy can be usefully divided into reliable and volatile, and will argue that all functions are somewhat volatile, and all are reasonably reliable, too. Productivity growth and deviations from the trend of productivity growth, for example, are certainly volatile, by any ordinary standard. Yet productivity is supposed to be one of the reliable relations. And so it is. It is stable, and moving averages of it show a mostly stable pattern of growth. But actual, current productivity growth, and especially deviations from the growth trend, will show volatility – precisely because these are driven by current investment, which is volatile.

Studies of investment, of the stock market, of inflation, of the natural rate of unemployment, and of interest rates, including term structure and spread, all suggest that these are particularly likely to shift or change character as expectations and judgements about the future change.

It is important to distinguish real from nominal volatility. Money and the price level are volatile, and therefore monetary measures of variables will reflect that volatility. But even when such nominal volatility is removed, or when money and the price level are stable, there will be volatility in real investment. Neither the future course of markets, nor of technology, can be known. So investment will fluctuate unexpectedly, regardless of whether or not monetary relationships are stable.

To measure volatility, and draw the line between 'volatile' and 'reliable', we can use standard measures, like *t*-statistics and variance. To examine degrees of real volatility, we should look at good data in real terms over one or two business cycles covering output, employment, wages and income, consumption, investment and interest rates. The data should be seasonally adjusted, but should not be adjusted for anything else. USA quarterly data for the 1980s and 1990s could be a choice. Then construct simple OLS estimates of output–employment, consumption and investment functions, drawing on only part of the data; this could be done in several ways – estimate on the first half of the period, testing on the second half, or estimating from early and later parts of the period, and testing on the middle, or spreading it out, and estimating every other quarter. According to any of these methods, we should be able to see a sharp contrast between $Y = Y(N)$, and $C = C(Y)$, on the one hand, and $I = I(i, Y)$, on the other.

We can choose any ordinary measure of deviation from the 'true value'. The output–employment and consumption functions will normally meet the test. When given N, the expectation of Y will lie, let us say, within two standard deviations 95 per cent of the time; given Y, C will lie in that range. This is not an exacting test, but it is unlikely that Investment can meet it. For a given i, we cannot have any assurance that I will lie within any reasonable range – and it is likely to be different at different times. Deviations of Y, given N, and C, given Y, will always be 'small' and will have the expected sign. But deviations of I, given i – or for that matter, given Y – need not be small, and need not have the correct sign. First derivatives, dY/dN, and dC/dY, will lie within the prescribed ranges, and will always have the correct sign; moving averages will be stable. But first derivatives of Investment can be negative, zero or infinite, and need not have the correct sign, nor will moving averages necessarily show greater reliability.

Volatility is not the same as structural instability (Vercelli, Hagemann) though it implies it. That is, volatile functions are structurally unstable – they can change form – but volatility is more than structural instability. It also suggests frequency of large or extreme changes and, further, that these changes may be unexpected, and even hard to explain. They may seem to have no apparent cause; explanation may be possible only after the fact. Volatility results from the openness of the economic system (Lawson) and leads to the uncertainty stressed by both Knight and Keynes. (Vercelli, 1991, p. 74, defines uncertainty in terms of the unreliability of any quoted insurance risk. He introduces the notion of *k*-uncertainty, and shows that it is quite reasonable to reject the coherence criterion of Kolmogorov probability in the face of *k*-uncertainty – and it is clear that this is related to Keynes's conception of liquidity preference, ibid., pp. 75–6.)

11.7 AND IN THE END . . .

Econometrics is about economics; it proposes using measurement to amplify, develop and test *explanations*, and with it, our understanding of the structure of the economy and how it works; it is not about forecasting, or about reliable statistical relationships that lack economic content. Econometrics provides numerical content to economics, making it possible to test and amplify economic theory by directly applying it empirically. It rests on three pillars: theory–coherence, applicability–relevance, and measurement–quantification.

We have argued that scientific relationships must exist, and this justifies our search for the correct parameters. Finding them will enable us to flesh out and develop our theories, just as the early econometricians hoped. Conceptual analysis and fieldwork will help us define the relationships, and tell us where and how to look for ways to measure them. But estimating the parameters is not easy, as we can see from the widespread presence of uncertainty and volatility, and from the fact that patterns of uncertainty are more likely to change in some areas than in others. It is important not only to distinguish reliable and volatile, but to clearly specify where each is paramount.

Reliable relationships tend to be real, and based on current conditions, while volatile ones, being future-dependent, are generally financial or monetary. So attending to the reliable/volatile distinction means paying attention to real-financial interactions. In which case, why don't we just estimate relationships and let the chips fall where they may?

First, a major advantage is that, in the case of reliable functions, statistical assumptions such as normality and so on can be justified. There is a 'true value' because it is defined by what should be done – given the obligations and contracts, and the like. Deviations from this will result in various kinds of pressures and problems, which can be understood, and which will enable the investigator to decide about the distribution of the error term. In fact, drawing on Spanos and Hendry, we can develop measures of statistical adequacy and get very good estimates indeed. By contrast, for volatile functions and variables, 'true values' generally cannot be defined, and it may even be hard to say what is an 'error'. For these, Vercelli's k-uncertainty holds.

Second, drawing the distinction allows for a more careful tracing of lines of causality. Volatile relationships change; volatile variables may change unexpectedly. The reliable relationships act as transmitters of influence – and can be combined into the multiplier, for example. So we can see more clearly what starts the processes of change, and how the changes work themselves out.

Third, by isolating volatility, and allowing for non-linearity, so that we can trace the complexities of its impact, we can see how it can generate a cycle. Klein's model is, as noted, essentially based on the same behavioural grounds, but he has no cyclical mechanism. Nor, of course, does the first Nell model. But the second does.

Fourth, the reliable functions are based on current revenue inflows and outflows; funds or resources flow into a 'decision nexus' – a household, a business firm, a bank, or a government – and a stream of payments or products or activity flows out. It is the household's income and the household's spending; the business firm's revenue, and its wage costs and profit stream; the bank's deposits and loans; and so on. The importance of this is that it means that the identification problem does not arise.

Finally, understanding volatility, understanding the sources of its unpredictability, learning to explain it *post facto*, and getting a feel for the impending shifts in optimism and pessimism, through fieldwork, provides a foundation for developing policies to establish control over volatility. It is not necessary to lament volatility – some volatility certainly cannot be avoided; it is the result of openness in the economy, and much of it results from innovation, both social and technological. But if volatility cannot be avoided, it can be channelled, controlled and limited. It can be circumscribed so that it will not do so much damage.

The ambitions of the early econometricians were not wrong. They thought that conventional economic theory was largely sound, and that their job was to fill in the numbers in the relationships that theory proposed. They were wrong; much theory has been misleading, and much has simply been irrelevant – having no application. But the economy is a system, a social system, and it keeps itself in existence by engaging in a pattern of production, distribution, consumption and exchange, regulated by ownership rights and value transactions. Once we understand *this*, we can see our way to uncovering and defining the relationships involved, and this will give us a foundation on which to build. We can do econometrics – not exactly the way the founders wanted, but well enough to provide a testing ground for our theories.

NOTES

1. Ilya Prigogine received the Nobel Prize in Chemistry 1977 'for his contributions to non-equilibrium thermodynamics, particularly the theory of dissipative structures'. See Prigogine's foreword in Federico Mayor Zaragoza (1995). Federico Mayor Zaragoza, in correspondence and in personal discussion with Errouaki, explains what Prigogine saw in physics' grudging century-long attempt to come to grips with irreversibility (and history) in thermodynamics. In economics, Georgescu-Roegen (1971), Hodgson

(2001), Lawson (2003), Mirowski (1989b) and Nell (1998a), among others, have argued that economics has been formulated as a theoretical science when it really should have been formulated as a historical–practical science, so that as a result the 'basic theory' is misleading. They argued that economics should move from the realm of timeless theoretical knowledge (on the model of math and fundamental physics) into the practical–historical realm where it belongs.

2. But with such weakness in demand, won't prices and money wages fall, tending to increase the real value of cash balances? And won't this increase in wealth then lead to a rise in spending, leading to recovery? The widespread popularity of this idea at one time surely represented a triumph of ideology over common sense; as Kalecki pointed out to Pigou in the 1940s (and as Tobin and Nell explained in excruciating detail), such a fall in money wages and prices implies a corresponding rise in the burden of debt for both households and business. General deflation leads to bankruptcies and hard times, as everyone knows (it's vernacular knowledge), not to a boom.
3. A fall in productivity would shift the demand for labour inwards and downwards, so there is a new equilibrium at a lower wage and level of employment. If the wage is sticky, employment will fall considerably below the new equilibrium level.
4. Hendry observes that 'historically growth rates of real variables have not varied wildly, and a number of ratios (such as consumption/income, capital/output, etc.) also have fluctuated within relatively narrow bands' (Hendry, 1995a, p. 100). The remark occurs in a discussion of whether and under what conditions the assumption of 'ergodicity' might be allowable in the analysis of time series.
5. In largely agricultural and craft economies, diminishing marginal returns to productive activities may be plausible. But the technology of mass production was designed precisely to eliminate such phenomena. For further details, see Nell (1988; 1998a).
6. The theoretical foundations for 'well-behaved' aggregate production functions, whether economy-wide or sectoral, have been seriously undermined by the results established in the famous (or infamous) 'Capital Controversy' (Harcourt, 1972; Nell, 1980; 1992c; Garegnani, 1966; 1970; Pasinetti, 1977; and Schefold, 1997). Essentially the debate has established that the possibility of 'reswitching' and 'capital-reversing' cannot be excluded, with the consequence that a unique correspondence cannot be established between technologies and the rate of return, nor can there be a monotonic inverse relationship between capital-intensity and the rate of return. A function made up of 'switching points' will exhibit discontinuities. As a result, the usual 'marginal productivity' conditions may not hold, and may even not be well-defined.
7. For example, see Acemoglu (2009, ch. 3), where estimation of production functions is presented uncritically. For a thoughtful discussion, see Temple (2006).
8. As in the case of Solow's reply to the original Humbug paper; he used a linear time trend that would have invalidated his own original 1957 estimates (Felipe and McCombie, 2005, p. 482; Shaikh, 1980).
9. Felipe and McCombie (2005) don't discuss this, but they do consider the effects of changes in the markup, p. 477.
10. To be sure, from 2008 to early 2010 the Fed increased the base money supply from about \$850 billion to \$2.1 trillion by buying a trillion dollars' worth of toxic assets (subprime mortgages), about \$300 billion of Treasuries (to support their price) and over \$100 billion of other government bonds. This certainly increased the money supply, but, notoriously and regrettably, it did not increase the monetary circulation. You can't push on a string.
11. It is here that Lautzenheimer and Yasar (2004) correct the slip in Nell's theory of circulation and go on to develop the argument nicely. Assuming equal capital–labour ratios for convenience, the expression for velocity simplifies to $Y = \{1/(1-wn)wn\}\, W_k$. But $I = W_k/wn$; so $Y = [1/(1-wn)]I$, which is the multiplier formula.
12. It has been found empirically that when the markup has changed, as with the oil shocks of the 1970s, the 'transactions demand for money' has shifted accordingly. See Nell (1998a).

13. And it shows the impossibility of such perennial gems of the introductory course as: a single dollar could exchange against all the goods and services produced if it could just circulate fast enough, a claim that one of the authors remembers from his course at Princeton long ago, and is apparently still taught there, to judge from Caldwell and Thomason's recent novel, *The Rule of Four*, published in 2004.
14. An example is the housing crash: the housing market was going up and, it was believed, would continue to do so indefinitely. Buyers wanted to get in early; lenders could offer easier terms because the collateral asset would rise in value. This facilitated securitization, allowing assets of varying quality to be combined, making it possible to market the riskier mortgages by packaging them together with better quality ones. These packages were then priced by evaluating the risks, relying on the assumption of normal distribution of non-performance – inability of borrowers to meet their obligations – without paying any attention to systemic risk, the possibility of general collapse of not only the housing market but also employment. Not only was normality unwarranted, but as the system moved towards crisis, even the assumption that the distribution of risk had any definite form (so that a sample now would be representative of the distribution next period) would have been unwarranted. And, of course, the same is true of the assumptions about risk underlying the pricing formulas for derivatives. For further details, see Nell and Semmler (2009).
15. Along with the MEC and the conventional projection (CP), Keynes (1973) also introduces the 'state of confidence', which affects the MEC (p. 148) and which by implication also affects the way we project the present into the future.
16. Rumsfeld in 2002: 'There are known knowns. These are things that we know that we know. There are known unknowns. That is to say, these are things that we now know that we don't know. But there are also unknown unknowns. These are things that we do not know that we don't know.' Quoted by Tim Taylor and Chris Calveley Cove Hole (2009): 'The Rumsfeld approach'. See www.uplandcavesnetwork.org.
17. The discount rate would be the long-term golden rule rate, $i = r = g$, where g is the rate of growth corresponding to the normal rate of utilization of the capital stock. There will be times when there is disagreement on what the discount rate should be.
18. The CP is the estimate of the future projected for sales and production by corporate managers; the MEC is the valuation of securities representing present capital made by fund managers. These estimates are summed up in the *variance* of the values of the present. So for the future to have a definite value, it will have to be the square root of the present. For the CP, the future is the square root of the present multiplied by the growth rate appropriately compounded.
19. We should take note of martingales and markov processes. A martingale sings that present information is the best predictor of future positions. A markov process is one in which the current position is the only determinant of the next position, regardless of the path by which the current position is reached. Along the 45-degree line, with the MEC and CP coinciding, the CP would be a martingale, and the interaction between the CP and the MEC would be a Markov chain.
20. The pessimistic story will give rise to a diagram just the reverse, with the upper curve being the MEC instead of the CP, and the lower the CP rather than the MEC.
21. Klein's methodological structuralism is very close to Nell's methodological institutionalism; in each case, the point is to build on the actual rules governing the way the system works. And these rules have to be discovered by active investigation of the way the world works – not by sitting in a library or computer center mechanically crunching numbers. For an account of Klein's vision, see Nell and Errouaki (2008a).
22. Klein (1950, p. 63) argued that many economists will recognize the resemblance between Klein's (1950) three equations model I (discussed in Chapter 6), Kalecki's (1935) models of the business cycle, and some of ideas of Marx. Klein's (1950) model I could actually be called a Marxian theory of effective demand. Klein (1947) has shown that is possible to develop this model from the un-Marxian principles of utility and profit maximization, but it is also possible to develop this model from purely Marxian principles. The

same model can be consistent with a multiplicity of hypothesis. Furthermore, Klein (1950) pointed out that the problem of developing models from Marxian principles is of great interest from the point of view of the history of economic thought, but is not an essential problem of his book, which is concerned mainly with quantifying a true description of the structure of the US economy. For a comprehensive understanding of the methodological foundations of Klein's structural approach, see the early work of Klein (1943; 1947; 1950; 1953; 1957; 1960; 1966; 1969; 1979) and Klein and Kosobud (1962), as well as the early models he has built with other scholars (for example, Klein and Barger, 1954; Klein and Goldberger, 1955; Klein et al., 1961; Klein and Fromm, 1972).

23. Early capitalism, through the nineteenth century, appears to have had a weak built-in automatic stabilizer in a price mechanism, which depended on technological inflexibility, and moved countercyclically, in tandem with the monetary system. This was swept away with the advent of mass production, and replaced by a volatile pattern of adjustment, in the multiplier augmented by the accelerator (or capital–stock adjustment process), so that the system came to rely on the government for stabilization. This has been explored for six countries: the USA, the UK, Canada, Germany, Japan and Argentina, in which adjustment during the period 1870–1914 is contrasted with that of 1950–90. Evidence of a weakly stabilizing price mechanism is found in all six in the early period; the transition to a multiplier-based adjustment is apparent in all but Argentina, which did not seem to fully accomplish the transition to a modern economy during the period studied. For further details, see Nell (1998b), in which Nell brings together 13 theoretical and empirical papers that attempt to outline the general theory of transformational growth and its applicability.
24. This is a short-run relationship in which given plant and equipment is operated with more or less labour. Marshall and Pigou arguably operated with such a conception (Hicks, 1989). A 'true' production function (Hicks, 1963) would require changing the technique when the amount of labour per unit capital varied. This is not a viable conception, as the 'capital controversies' showed (Kurz and Salvadori, 1995; Laibman and Nell, 1977).
25. In post-war mass production (Nell, 1988; 1998a), by contrast, constant returns prevail in the short run; to put it differently, unit costs are broadly constant. Workers need only be semi-skilled and teams can easily be broken up and re-formed; processes can be operated at varying levels of intensity in response to variations in demand, and they can easily be shut down and started up. It is likewise easy to lay off and recall workers. The widespread existence of constant unit costs came to light beginning with the debate on prices and pricing in the 1930s and 1940s (see Hall and Hitch, 1939; Andrews, 1949). The suggestion here is that constant costs were the result of technological developments in manufacturing processes (Hunter, 1985). The evidence for constant costs is summarized and discussed in Lavoie (1995, ch. 3). Under constant costs, of course, the real wage will not be governed by marginal productivity.
26. To move from individual firms to the aggregate, it is not necessary to hold the composition of output constant, so long as the movements are small. In both craft and mass production, the adjustment is better shown in two sectors. The aggregate function oversimplifies. When proportions of capital to consumer goods change in the craft world, prices change; when they change in mass production the degree of utilization changes, but unit costs and prices are not affected.
27. The Penn World Tables provide data making it possible to plot output per head against capital per head with a large number of observations. When this is done for the advanced OECD economies, the scatter diagram shows no evidence of curvature. The same plot for the backward economies exhibits pronounced curvature, for middle-range economies moderate curvature. Of course, this can be considered no more than suggestive.
28. Wages and salaries in the aggregate are closely correlated with consumption spending, but do not fully explain it. Some obvious adjustments are easily made. Consumer

spending also depends on the terms and availability of consumer credit. In addition, it reflects transfer payments. Wealth and profitability are significant variables. But for the present purposes, which are purely illustrative, a simple 'absolute income' theory will suffice.

29. This, of course, directly contradicts one of Modigliani's most celebrated contributions, the life cycle hypothesis. But half a century of empirical evidence has shown that, in the US (and other advanced countries), household consumption spending tracks wage and salary income 'too closely' for any simple version of the life cycle hypothesis to be correct (Deaton, 1992).
30. Nothing is implied in this discussion about the marginal product of capital. Here, capital is given in amount and fixed in form; when we come to growth we shall consider the capacity creating aspect of investment.
31. It is tempting to set out the model in the form $Y = AN$(exp a) so that $w/\pi = aAN$(exp a–1). Then a becomes the parameter governing the rate at which returns diminish. However, the power function is only one of several forms that the relationship between Y and N might take. In particular, the log form will be important.
32. Rymes (1989, pp. 37–8) suggests that the real argument of the 'Manifesto' by Robinson and Kahn concerned this effect. Rymes argues: 'If the increase in investment [. . .] results in a sufficient increase in demand, not only a higher price but also an increase in the costs of production facing the entrepreneur in the consumption goods sector, such that the new equilibrium [. . .] entails a higher outlay on consumption goods, then it is possible the decline in the *output* of consumption goods could, in terms of effects on the volume of employment, more than offset the increase in the output of capital goods' (italics added). Investment increases and consumption declines.
33. This form of adjustment brings to mind the doctrine of 'forced saving' of authors like Thornton, Hayek and Robertson. Here, however, the price changes are assumed to reflect changes in demand pressure – not necessarily connected to changes in the quantity of money – and are shown to result in a Marshallian 'marginal productivity' equilibrium. The traditional 'forced saving' discussion usually started from an assumed increase in the money issue or in an exceptional extension of credit, and, indeed, a rise in demand of the kind considered here would require just such additional finance – which the resulting rise in prices relative to money wages would tend to support. (The higher profits will allow banks to charge higher interest rates, enabling them to attract additional reserves. The higher interest rates, however, should tend to dampen further expansion.)
34. This has features in common with 'overlapping generations' models, but it should be clear that a number of fundamental assumptions are different. For one thing, Keynesian uncertainty is assumed to be present here; so neither firms nor households can have anything like 'perfect foresight'. Saving does not depend here on a 'utility-maximizing' calculation, comparing consumption today with consumption tomorrow. Nor are there any general assumptions about time-preference, assumptions that are notoriously difficult to justify. Saving here is assumed to follow simple rules that can be expected to yield desirable results even in the face of great uncertainty. Moreover, patterns of saving and spending differ not by age, but by class or function. Workers and managers will tend heavily to consume, and capitalists will want to maintain their holdings – in extreme form these become: no saving out of worker/manager income over their lifetime, no consumption out of capital income, and capital will be passed along intact. By contrast, in the overlapping generations models, all old workers tend to consume everything, while only the young save. But, of course, the main difference is that in the standard overlapping generations' model saving determines investment.
35. In the less likely case that established wages were also to be lowered by a rise in the growth of labour, it might be thought that this would increase the profits of existing firms, raising savings, which would tend to lower interest rates, also encouraging investment. But if established wages were lowered, consumption would fall, reducing revenues; so realized profit would *not* increase.

36. 'There are four prices involved in the system: (1) the selling price of a unit of real output (and since real output also serves as capital this is the transfer price of a unit of capital stock) $p(t)$ [. . .] [he goes on to define (2) the money wage rate, (3) the money rental per unit time of a unit of capital, and (4) the rate of interest] [. . .] we can eliminate [the price of output] immediately. In the real system we are working with there is nothing to determine the absolute price level. Hence we can take $p(t)$, the price of real output, as given. Sometimes it will be convenient to imagine p as constant' (Solow, 1956, p. 79). He then introduces the marginal productivity conditions (his equations (10) and (11)); when the price of output is taken as given, then when the capital–labour ratio is determined, the marginal productivity equations will determine the nominal wage and the rate of return to capital. But this is completely passive; there is no adjustment mechanism here.
37. We assume that an appropriate portion of bank capital is invested in reserves of the sort required to back the issuing of notes or loans. Thus expansion of bank capital at the long-run equilibrium rate will automatically provide expanded reserves.
38. The 'Manifesto' written by Joan Robinson and Richard Kahn, with the concurrence of Austin Robinson, challenged not the result, but aspects of the reasoning. As noted above, part of their discussion concerned the effects of price changes on demand. Rymes (1989) observes, 'The '[M]anifesto' claimed that the case of no increase in the demand for consumption goods [following an increase in investment spending] was the one exceptional case Keynes had dealt with [. . .] It is [. . .] an obviously special case'. On the assumptions here, it is the case where the elasticity of the marginal product curve is unitary. Both Keynes and the 'Manifesto' authors considered the 'elasticity of supply' to be a determining factor, but neither presented a general analysis of the way changes in I led to corresponding changes in C.
39. Neoclassical production functions have frequently been 'fitted' to data from modern mass production economies, often in connection with the Solow growth model, in spite of the evident presence of constant costs. This usually involves a sophisticated but disastrous mistake; what is actually being captured is the income distribution identity.
40. That is, employment is *not* determined in the labour market. It follows directly from the demand for output, given the output–employment function – as in Kalecki. Hicks, following Keynes, initially modelled effective demand by setting up the IS-LM system together with a labour market and a conventional production function. Later he came to feel that this was a mistake (Hicks, 1977; 1989). But if returns are constant and there is no marginal productivity adjustment, the markup must be explained (Rima, 2003).
41. Even in the USA, changes in employment don't follow changes in output strictly according to the labour actually needed; there is labour hoarding, as studies of Okun's law show.
42. On these assumptions, investment determines – and equals – realized profits. When households save a certain percentage out of wages and salaries, the consumption line will swing below the wages line – profits will be reduced. When wealth-owning households (or businesses subsidizing top managers) add to their consumption spending in proportion to the level of activity, this swings the $C+I$ line upwards, increasing profits.
43. The output multiplier in this simple example will be $1/(1 - (w/p)n)$, where w/p is the real wage and n is labour per unit of output.
44. This is the point that Keynes wrestled with; it shows up in a very simple form here.
45. This is written as a macro model, but it could easily be adapted to a neo-Ricardian format, with an inverse relationship between growth and consumption (and relative sizes), and another between wages and the profit rate (and relative prices). The real wage would be connected to growth through prices and profits. The rate of profit would underwrite the rate of growth, and realized profits would reflect investment spending. The real wage-growth relationship would be a vector equation showing the expansion of demand in the various sectors; likewise the productivity relationship.
46. Empirically, we might examine the vectors of sectoral outputs, class income payments; if there is equilibrium steady growth, then first differencing will eliminate the growth rate, g, and the result will be a random walk, $Y_t(0) = Y_{t-1}(0) + e_t$ where $Y_t(0)$ is the

vector of detrended sectoral outputs. But if growth is not steady, outputs and incomes will not be a random walk; if the bias is pronounced and persistent, it will be an indication of transformational growth.

47. At low levels of investment, the disruption to a firm of having to halt spending on an investment project may not be very great. But larger projects will involve more of a firm's management and affect more of its current operations; a break will therefore disrupt a larger proportion of the firm's activity. The example below in the text can be reinterpreted: the impact on the firm rises linearly with I (100, 200, 300, 400), but the proportion of the firm's activity affected also rises linearly (10%, 20%, 30%, 40%). Then at higher levels of I the cost of disruption will increase more than proportionally: 10, 40, 90, 160.
48. But if a bond's maturity date is properly matched to the date that funds are needed by a particular borrower, there is no risk for that borrower. Even if market rates change, the funds will come due in the right amount at the right time. The fact that the borrower might have done better or might have suffered a loss is irrelevant.
49. This idea is common in heterodox economic thinking, but it has a near analogue in mainstream analysis. A *self-confirming equilibrium* is one based on beliefs that differ from the 'true' model, but which generate a pattern of activities, given the policy environment, that produce actual data to match what the true model would produce. The model is false but it cannot be distinguished from the true model – in that policy environment. A self-sustaining spiral also confirms the agents' beliefs, and it is hard to show, based on the data generated, that it is false. A 'true model' could generate such data, and indeed, the agents believe that is just what is happening.
50. See Friedman (1943) quoted in Epstein (1987, p. 109).
51. See Semmler and Franke (1991) and Semmler and Gong (1997).

Conclusion

This book began with a complaint and a vision.

We complained that mainstream economics has offered the 'rational individual' as the basic building block in model construction, with 'methodological individualism' as the accompanying scientific program, facing a virtually insuperable obstacle in the form of the problem of induction. In fact, the problem of induction does not obstruct research at all, as we have demonstrated, but that is because actual research does not adhere to the methodology.

We developed our proposal to overcome the problem of induction and establish the existence of lawlike regularities in economics, justifying the assumption of a 'data generating mechanism'; this led to our MTC diagram, which summarized our methodology. The MTC diagram helped us to answer the following question: to what degree may we expect our model to fulfil its objective; that is, to *work*.

The vision we proposed argued for a different starting point, from a self-replacing *system* – a socio-economic system – which acts as a data generating mechanism. On this basis we proposed 'methodological institutionalism' as an alternative to methodological individualism, contending that there *are* lawlike relations to be discovered in the economy, though they are not quite the same as the lawlike relations in the physical sciences. In particular, we argued that Keynesian uncertainty has to be given its proper due; there are some variables and some relationships that exist and are important, but which are inherently liable to shift unpredictably. We can identify and estimate them temporarily – and unreliably – but we cannot capture them once and for all. Their nature reflects our abilities to innovate and to change our minds, and this cannot be fully tied down in a model.

We argued that the Cowles approach was certainly on the right track, contrary to much recent opinion. The Cowles group was large and varied and unusually talented, a mix of European and American economists, who found it possible to work together in an astonishingly productive way. They agreed with Haavelmo that the probability approach was the most promising, that if they were to use statistical inference systematically, they had to adopt the framework of probability theory, which underlay it.

The Fisher paradigm had been worked out by this time; it was Koopmans who introduced it, using it to recast Frisch's errors-in-variables formulation, and relating least squares to maximum likelihood. They approved of Tinbergen's work and wanted to carry it further, but felt it needed to be reworked to avoid simultaneity bias and the identification problem. If they did this they felt they would be able not only to guide Keynesian policies but also to help solve the fundamental economic and social issues that had come to light in the depression and in the aftermath of World War II.

A lot of researchers in the academic community moved away from the big macroeconometric models, partly because they lost confidence in them, but partly because they preferred to work individually on projects they could put their own names on, rather than being part of a team effort. Klein (in Mariano, 1987) argued that this is understandable in terms of academic promotion, but it has not been good for econometric research. Many of these researchers, wanting to work with small systems, adopted programs of 'measurement without theory', using vector autoregressions and similar techniques. As Klein (ibid.) put it, the proper use of these is in checking results, not in guiding research or developing explanations, let alone policy. This is not progress; it is a step backwards.

We argued that a great deal of work in macroeconomics and macroeconometrics has gone down an unpromising road. Klein, Malinvaud and Nell have always insisted on a good theoretical basis for equation specification, a basis that has to be in close correspondence with reality. Theoretical ideas cannot be based on implausible or impossible assumptions. Many recent and current researchers are overly impressed with pure theory-spinning and are not careful of reality and don't subject their work to forecasting tests.

Like Klein, we always thought expectations were important. But, as Klein argued, the approach of 'own-model generated expectations' – so-called rational expectations – is not a step forward. It asks too much of the data, requiring it both to generate the expectations, and to provide the model simulated estimations. People who want to use the sample both to generate expectations and then to estimate the model are 'eating their own tails', in the phrase Cowles researchers used to use. Making very strong and unrealistic assumptions about the way expectations are formed, simply for the sake of getting definite analytical results, is just deplorable.

Like Klein, we think that expectations should be based on what agents are actually thinking and doing, in the light of the latest information available to them. Following Klein, we should have sample studies of what agents state their expectations to be, together with knowledge of the state of the stock market, the bond market, the movement of inflation rates, and the movement of monetary instruments. European business test surveys, surveys of consumer sentiment, surveys of inflation, statistics on orders, housing starts, and other 'anticipations' variables should all be at hand, and should be integrated into our models. We call these and related studies 'fieldwork' and we want to take it very seriously. Like Klein, we don't want to see this information just packed into the format of a pre-existing model; we want it examined for insights into how agents actually interact and make decisions, and these further insights should be used in the formulation of the models. We call this 'conceptual analysis'. And we propose that these two activities can help us with model selection and identification. (We pointed out that Adam Smith visited a pin factory, and this changed the world of economics forever.)

Like Klein, we think that there are precise scientific regularities in economic relationships, regularities that carry causal force, and that these can be discovered by econometric methods. These regularities are analogous to the 'laws' in other scientific fields. We tried to explain why this *has* to be the case. First, these regularities and relationships develop historically, so that the 'laws' governing some kinds of economic phenomena may change from one historical period to the next. Second, we also argue that there are important, measurable, economic relationships that *do not* have the characteristics of scientific laws. They may look like solid relationships, they may even have some kind of causal force, but they are *inherently* unreliable. They can change unexpectedly in wholly unpredictable ways. Again, we tried to explain why this must be the case, and we suggested criteria for picking out such relationships. The task of structural econometrics, then, in this view, is to develop sound estimates of reliable relationships, and establish how they are connected to the unreliable ones. The unreliable relationships have to be studied not only by econometric methods, but by drawing on the other social sciences. For these relationships are not wholly unknowable; we can learn a lot about them from fieldwork and the study of other aspects of society. And when they change, they will have an impact on the reliable part of the system.

To sum up: we argued that a transformed structural econometrics must reflect the real world, not abstract deductive models based on rational individual agents. We began by rethinking the scientific foundations, offering a way around the problem of induction that also justifies the assumption of a 'data generating mechanism', and provided ways to model this. We went on to explain how current critiques of the methodological foundations of structural econometrics are direct consequences of implicitly accepted but seriously flawed elements in neoclassical thinking. In the final part, we presented our methodological contribution: a blend of fieldwork and conceptual analysis designed to ensure that their models are well grounded in reality and, at the same time, conceptually coherent as well as statistically adequate. In so doing, we also outlined a number of elements that will be needed to develop a 'good' macroeconometric model of an advanced economy. We examined some specific modelling, for example with regard to wage–price spirals, the analysis of money supply and demand, Keynesian uncertainty, and Minskyian financial instability. As Klein pointed out in the Foreword, '[t]hese ideas may seem unorthodox in today's context; but they would not have seemed out of place to many of the early econometricians, for example at the Oxford Institute of Statistics'.

In short, we argued that econometrics must rest on three pillars: theory or conceptual coherence, applicability or relevance, and measurement or quantification. All three are necessary to make an adequate model, and they are interdependent. Many of the most important recent writings on econometrics do not have the right balance between these three. The present book is an attempt at such reconstruction.

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