

Macroeconomic Forecasting

A sociological appraisal

Robert Evans

Routledge Studies in the Modern World Economy



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MACROECONOMIC FORECASTING

Macroeconomic Forecasting explores the gap between economic models and the economic world. Rather than removing the politics from economics, Robert Evans argues that economic models are precisely the place where politics are put in. The book provides a fresh and timely new perspective on the emerging debates about the roles and contributions of empirical models to policy making.

Robert Evans applies the theories and methods of the sociology of scientific knowledge to map out the intellectual world of macroeconomic modellers. In this way, the book illustrates not only the key issues in science studies but also several major debates within economics. From the normative commitments which shape decisions about the variables to include, to the interpretative flexibility of data and the eventual normalization of some accounts, the book shows how economic forecasts are the product of both econometric evidence and socially grounded judgements. Taking macroeconomic modelling and forecasting - a science which affects practically every citizen - as a case study, *Macroeconomic Forecasting* provides a coherent, sociologically informed view on the way in which computer models can be used to inform policy.

At a time when central banks are being given increased powers, the book provides a timely and critical perspective on the models used to take decisions about interest rates and taxes. *Macroeconomic Forecasting* will therefore be suitable for a wide range of readers including students in economics, sociology and management, and policy makers in fields where economic or other computer models play an important role.

Robert Evans is a lecturer in Sociology at Cardiff University. His research interests focus on the science and practice of computer modelling in policy making.

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CONTENTS

<i>List of figures</i>	ix
<i>List of tables</i>	xi
<i>List of equations</i>	xiii
<i>List of models</i>	xv
<i>Acknowledgements</i>	xvii
1 Who knows what tomorrow brings? Brief introductions to the sociology of scientific knowledge and to economic forecasting	1
2 Cherished beliefs and t-statistics: how economic models are estimated	24
3 Extrapolations and expertise: how economic forecasts are made	50
4 Soothsaying or science? Choosing between economic models	77
5 Different models: economic forecasts in February 1993	101
6 Different futures: controversies and disputes in July 1993	128
7 Difficult choices: policy recommendations in October 1993	153
8 Economic models, economic policy and science studies	172
Appendix A: equations used in the econometric model	191
<i>Notes</i>	201
<i>Bibliography</i>	214
<i>Interviews</i>	224
<i>Index</i>	225

FIGURES

2.1	Flows of income and expenditure in a simple economy with a tax-raising government	26
2.2	Simple IS-LM model	29
2.3	A typology of forecasts	45
2.4	Fitted and forecast values for consumer expenditure	47
2.5	Forecast errors for equations based on theory and data fitting	47
2.6	Root mean square errors for equations based on theory and data fitting	48
3.1	Flow chart of macro-econometric model	56
3.2	Model-only forecasts for GDP and its components	62
3.3	Actual and fitted values for the adjustment to factor cost	63
8.1	Forecasts and outturns for GDP growth and inflation in 1993	173
8.2	The traditional policy-making process compared with the Panel of Independent Forecasters' process	187
8.3	An alternative policy making framework	188

TABLES

2.1	Unrestricted regression coefficients	33
2.2	Unrestricted regression coefficients using full data set and data from 1956Q4 to 1980Q4	34
2.3	Coefficient on price level in first restricted regression	36
2.4	Rearranged regression equation (Evans's estimates)	37
2.5	Regression with further rearrangement	38
2.6	Long-run form of equation	38
2.7	Regression with steady state growth	40
2.8	Regression with interest rate difference	42
2.9	Equation derived by application of statistical rule	45
3.1	A selection of forecasts for 1993, together with outturn data	60
3.2	Residual adjustments made to the price equation	66
3.3	Residual adjustments made to the consumer expenditure equation	69
3.4	Residual adjustments made to the wage equation	70
3.5	Residual adjustments made to the employment equation	71
3.6	Residual adjustments made to the stockbuilding equation	71
3.7	Residual adjustments made to the fixed investment equation	73
3.8	Residual adjustments made to the adjustment to factor cost	73
3.9	Summary of forecasts for 1993 by R. Evans and the Panel of Independent Forecasters	74
6.1	Views on fiscal policy	151
7.1	Arguments for and against tightening fiscal policy	169
7.2	Arguments for and against intervention to 'solve' the twin-deficit problem	170

EQUATIONS

2.1	General consumption function	32
2.2	Long-run consumption function	38
2.3	Average propensity to consume, derived from Evans's estimates	39
2.4	Average propensity to consume, derived from Keating's estimates	39
3.1	Identity for GDP	61

MODELS

3.1	A simple model of a closed economy	53
3.2	A simple linear-in-logs model of an open economy	54

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Robert Evans
November 1998

WHO KNOWS WHAT TOMORROW BRINGS?

Brief introductions to
the sociology of scientific knowledge
and to economic forecasting

Tuesday 3 November 1998.

The Chancellor, Gordon Brown, makes his Pre-Budget Speech to the House of Commons. He announces that because of the global economic downturn, which has seen world trade growth set to fall by two-thirds, world growth forecasts virtually halved and put one quarter of the world into recession, the UK government is reducing its economic growth forecast for 1999 by 3/4 per cent. The new forecast, of between 1 and 1½ per cent is above the average of recently published forecasts, but the Chancellor insists that it is based on 'cautious assumptions' and that spending targets can still be achieved.

Chancellor's Pre-Budget Statement
as reported on HM Treasury web site
<<http://www.hm.treasury.gov.uk/>>

Introduction

Economic forecasting is a big business. Governments, banks, financial institutions and commercial organisations and corporations of every shape and size are involved in it. Every single minute of every single day the fortunes of individuals, companies, and sometimes it seems whole nations, are gambled on forecasts of the price of anything from coffee beans to money itself.¹ As the recent events in the world economy show, economic forecasting and its consequences have the potential to affect just about anyone and everyone.

It could be assumed, therefore, that the way in which these forecasts are produced would have been a topic of great interest for academic research. Strangely this is not the case, at least as far as sociology is concerned.² This

book begins to fill this gap by providing a sociological account of the models, methods and forecasts produced by one of the most important groups of economic forecasters: the forecasters who advise governments on matters of policy.

These forecasters are particularly interesting. First, the methods they use to produce their forecasts are quite distinctive, and because of their connections with formal economic theories they are particularly interesting to the sociology of scientific knowledge (SSK), my own academic discipline. Indeed one of the most obvious reasons for choosing to study these forecasters is that they actually use the techniques, theories and methods of economic science. In other words, these forecasters use data not just to make predictions but to test theories and to explain why the economy develops in the way it does. There is therefore a clear sense in which the economic analyses and forecasts produced in this way can be said to be 'scientific' and not just instrumental.

I would suggest that it is precisely the idea that the forecasts and models are scientific that underpins the belief that in conducting economic policy there are few, if any, alternatives. Other sorts of forecast – for example, those based on the reading of charts – are so obviously dependent on interpretation and judgement that an analysis of the ways in which their meaning is negotiated within a professional group would probably seem trivial. Some economists might even dispute whether this activity could be classed as economic forecasting.

In contrast, forecasts and policy recommendations based on theoretically informed and empirically validated understandings of the economy provide a much firmer basis for studying the role of scientific knowledge in policy making and the ways in which legitimacy is created. This is a second reason for writing this book. The role of these forecasts in providing information for policy makers makes them the legitimate concern of every citizen, but in my experience most publications by economists about economic forecasting are written mainly for other economists and are inappropriate for a general readership.³ They tend to be formal, technical, and to emphasize the mathematics of the models rather than the practicalities of using them. They thus tend to portray economic forecasting as a technical activity and do not consider the social context that makes economic forecasting so important and interesting.

Although understanding the theoretical and mathematical rigour of the models is important, particularly for economic practitioners, economic forecasting also affects many areas of social life. As a result a more general and less technical account of how the models are used is also important. To address these issues, the meetings and recommendations of the Panel of Independent Forecasters, will be examined.

The Panel was a diverse group of seven economic forecasters appointed to advise the UK Government following the UK's exit from the Exchange Rate Mechanism (ERM) in September 1992. It met for the first time in February 1993

and was required to produce three reports a year. These contained policy recommendations for the Chancellor and were also made available to the public, a significant change from the previous practice in which the official sources of economic advice were restricted largely to economists in the Treasury, and recommendations were not published.

The seven members of the Panel reflected the broad spectrum of views, backgrounds and analyses that characterize economics in the UK. Theoretically they ranged from traditional Keynesianism, through (mainstream) neo-Keynesianism to New Classical economics and Friedman-influenced monetarism. They worked in organisations ranging from universities to merchant banks. Most had previously been involved in giving policy advice.⁴ In practice, after the novelty of their appointment had worn off the Panel had a fairly low public profile. Its members devoted little time to it – each report was the product of two one-day meetings – and were free also to publish their own forecasts and analyses.⁵

The activities of the Panel raised several interesting issues for sociologists of science. First, they highlighted the interpretative flexibility of economic data. Given the theoretical differences that existed between the Panel members it was obvious that they would interpret the data differently. This should not necessarily be interpreted as a bad thing: one criticism made of the Treasury economists was that they did not consider all the alternatives. Nevertheless it posed an intriguing question: would the Panel agree on anything?

Second, the Panel of Independent Forecasters raised questions for sociologists regarding the use of science in policy making. Much literature on the sociology of scientific knowledge has focused on debates *between* scientists, but it is important to remember that claims of scientific knowledge often form the basis of policy interventions. Policy arenas and debates are frequently the place where scientific controversies spill out into the wider society. The interaction between scientists and policy makers is another longstanding concern in the sociology of science, and the Panel provided a new and interesting window on to this process.⁶

Finally there was the possibility of evaluating the Panel as an institutional mechanism for shaping economic policy and enhancing its legitimacy. Sociological theories of science and technology suggest that the successful translation of science into policy and vice versa depends critically on social agents accepting the roles specified for them. In economics, the roles are those of the economic agents specified in macroeconomic models and theories. If key groups, who may be policy makers, employers, workers, shoppers or the unemployed, do not behave as the ‘intended agents’ postulated by the modellers, then the analysis and recommendations will be difficult (if not impossible) to apply in practice.⁷ In other words, for economic analysis to be effective as policy advice it will need to be credible not only as economics but also as a social and political strategy.

This book examines the range of theories and models that lay behind forecasts and recommendations made by the Panel of Independent Forecasters in 1993, and the range of socio-economic futures that the models claimed to legitimize. It is argued that a diversity of models is a good thing, although this is not because policies produced in this way are more 'robust' in the economist's sense. Rather, using a plurality of models enables a decision-making process that is more reflexive about human subjects and social institutions it seeks to enrol. It brings out into the open, and therefore into the discussion, the ways in which different economic models draw the boundaries around policy issues, for example by defining responsibilities, channels of influence and transmission mechanisms. This book argues that the expert deconstruction made possible by the Panel's intellectual diversity - and their resulting disagreements - was one of its main strengths. In particular, it allowed the interests, values and assumptions that underpin the scientific representations of economic models to be articulated and discussed. As a result, this expert debate had the potential actively to promote an understanding of the interpretative flexibility of economic data, an understanding that I believe is as important for policy making as 'getting the facts right' (Jasanoff 1995).

Thus, it is not suggested that the Panel were an alternative to elected and accountable policy makers. Rather, its role was to provide information and recommendations. However, this still leaves the question, information about what? The preceding paragraphs have suggested that one thing the Panel could have provided information about was the uncertainty involved in their own science and the assumptions and values on which it depended. In practice, however, the Panel tended not to focus on such reflexive concerns but instead to emphasise where it agreed and generally to try to minimize the appearance of controversy by presenting consensus recommendations. While this had the advantage of providing a single piece of policy advice, the cost was that the diversity and excitement of the Panel's economic (and econometric) models faded into the background.

In fact the Panel's models suggested that a range of policies had the potential to make sense as economics. The Panel thus had the potential, and the expertise, to legitimize a shift in debates about economic policy towards more inclusive agendas of social, political and moral responsibilities. In their individual submissions the Panel members often proposed quite radical and sometimes interventionist policies. By discussing these differences in private, however, and minimizing them in public, the Panel did little to encourage political and public debate about the wider social dimensions of economic policies. Indeed, somewhat ironically the effect of the Panel was almost exactly the opposite, and three years after it was set up the Panel was replaced by the Monetary Policy Committee of the newly independent Bank of England. Far from re-politicizing economic policy, the Panel proved to be the prelude to an attempt to remove anti-inflation policies from political debate completely.

This book is motivated by the belief that understanding the process of economic forecasting should lead to an awareness of the variety of economic strategies that have expert backing, and of the possible futures to which adopting them might lead. The analysis clearly demonstrates that there are significant disagreements between economic advisers on a range of important social issues. However, we should not be confused into thinking that this disagreement is the problem. It is not. The problem is that, particularly in the UK, there are very few institutional mechanisms for mediating economic and other professional disputes in order to provide reliable information and advice to policy makers.

Invariably, expert committees are set up to provide advice, which is then implemented. The Monetary Policy Committee is a good example. The expert committees are asked to advise on issues which are often at the very limits of their professional expertise. As a result, their members disagree – as did the Panel – not because they are incompetent or because some of them are mistaken, but because there is no obviously ‘right’ answer. In these circumstances competing recommendations may be evaluated using other criteria, such as the apparent trustworthiness, competence or motives of their proponents.⁸

It is argued here that the solution – the way to build social legitimacy out of professional controversy – is not to deny the controversy but to acknowledge it. The decision between competing recommendations then becomes an unambiguously political decision. The central message of this book, therefore, is that delegating difficult decisions to professionals does not guarantee the legitimacy of policy decisions. The choice between the proposals of experts who disagree is a social and political choice and needs to be justified as such.

This is clearly a big claim to make, and its justification comes in several parts. The remainder of this chapter outlines the sociological theory and research that inform the empirical chapters which follow. It also provides a brief introduction to the development of macro-econometric modelling in the UK and refers to a selection of economic literature, some of which is critical of economic modelling. The book is then structured so as to mirror the learning and research process which underlies it.

Chapters Two and Three explain and analyse the methods and techniques used by economic modellers. The aim is to convey something of the economic forecaster’s world to readers unfamiliar with the area. Economists will no doubt be familiar with much of the material covered, but sociologists and others may find it new. However, both chapters are more than a recapitulation of standard econometric texts and the material is always analysed through the lens of sociology. As such the argument blends econometrics with a sociological commentary that will, I hope, make it interesting to economists. To avoid any misunderstanding, these chapters do not claim to explain the rules of economic forecasting. That would clearly refute the sociological theory on which the argument is based. Instead the analysis conveys something of the spirit of the

activity, while identifying the points at which the judgement of the lay person will be found wanting.⁹

Chapter Two begins by discussing the building blocks of econometric modelling and introducing some of the concepts and techniques used by econometric modellers. The focus is on national income accounts, the estimation of regression equations and different types of forecast test. One theme highlighted in this chapter is interpretative flexibility of economic data and the effect of this on econometric tests. In particular it is suggested that because of the ambiguity of econometrics, economic modellers need to have strong prior beliefs if what sociologists of science call the experimenters' regress (Collins 1992) is to be avoided. In other words, econometric testing only seems convincing in those cases where the economics community has already agreed on the correct answer. Of course, to make these judgements correctly (i.e. in a way that economists would find acceptable) requires a high degree of socialization or professional training in economics.

Chapter Three develops these themes by illustrating how an economic forecast is produced and examining the relative importance of econometrics and judgement in this process. The chapter shows how residual adjustments - alterations made to the error terms in the individual equations that make up an economic model - can be used to fine-tune a forecast. The chapter uses as its example the average of the forecasts published by the Panel of Independent Forecasters in February 1993. As with the previous chapter, the aim is not to criticize economic modellers, but to show how the scientific process of economic forecasting is invested with the skill and judgement of the forecasters. It is most certainly not critical of economic *modellers*, although it is perhaps rather sceptical of econometric *models*. The basic argument is that economic models cannot produce the forecasts unless economists add a lot of expertise, experience and judgement. Rather than showing that making economic models and forecasts is easy, the chapters offer an account of why they are hard.

After this outline of the foundations of economic modelling and forecasting considered in isolation, the next four chapters shift the focus to their discussion, evaluation and use in wider social settings. Chapter Four discusses their evaluation amongst economists; Chapters Five, Six and Seven include other policy actors. The argument in Chapter Four concerns the way in which economists evaluate each other's models and forecasts.¹⁰ It includes interviews with members of the Panel of Forecasters as well as with Ken Wallis and Paul Ormerod. Professor Wallis is the Director of the ESRC Macromodelling Bureau at the University of Warwick and, with a variety of collaborators, is the author of some of the most detailed and authoritative studies of UK macroeconomic models. Paul Ormerod has been an economic forecaster for both the NIESR and the Henley Centre for Forecasting. At the time of the interview in 1993 he was a Professor of Economics at the University of Manchester.

Although the chapter shows that neither forecast mistakes nor econometrics seem to falsify economic models, the overall argument is not a negative one. Rather the point is that, even though there is considerable disagreement about how economic data should be interpreted and explained, the process of building the models which try to do this is an important one. It is through estimating, using and updating macro-econometric models that economic forecasters acquire the expertise that transforms an extrapolation into a forecast.

Chapters Five, Six and Seven develop these themes in more detail by examining the forecasts, meetings and policy recommendations produced by the Panel of Independent Forecasters during 1993. Taken together, these three chapters set out the economic choices that existed at that time and examine the ways in which controversies were both sustained and (temporarily) resolved. In contrast to Chapter Three, which showed how judgements can be used to produce agreement by bringing an individual forecast closer to the average, Chapter Five is about the controversy and disagreement that characterize economics in real life, at least in the UK. In particular, it is about the different theoretical and interpretative models that the Panel members used to analyse and understand the economy. The focus is on the 'big picture' and the chapter identifies two main sets of theories and models. The aim of the chapter is thus to begin undermining the idea that there is no choice when it comes to economic policy by showing, as clearly as possible, that there are alternatives. What is more, these alternatives are not just choices between economic theories; they are also choices between different types of social and economic future. As such, the disagreements between the Panel members are not just matters of academic economics, but social and political issues that ought to concern everyone.

Chapter Six examines a selection of these controversies in some detail and focuses on debates about unemployment, the public sector borrowing requirement (PSBR) and the deficit on the balance of payments of the current account (the trade deficit). These disputes are typical of the controversies that characterize economics in the popular imagination, but they also reach right to the heart of the rationale for economic policy. Thus, the chapter is not just about interpretative flexibility; it is also about the links between ideas and social actions. Just as germs, once recognized, call for certain actions by doctors and nurses, so the existence (or not) of labour market 'outsiders' who cannot compete effectively for jobs, once it is recognized, calls for (or renders obsolete) a certain type of policy response from the government.

It is these larger issues which make controversy in economics important. Decisions about how to understand the economy have ramifications that reach way beyond the world of academic economics. What is more, the outside world can rarely wait for economists to resolve their disputes and establish a collectively endorsed theory. How then are policy makers to decide, when the experts themselves do not know? It may be that what is needed at this point is

not more economists, but some way of choosing between economists. The implication of the sociology of science is that the range of people equipped with the skill to make this latter sort of judgement is much wider than is often thought.

To round off the analysis of the Panel of Forecasters' activities in 1993, Chapter Seven looks at a particular application of these models and theories: producing policy advice in October 1993. In particular, it examines how the general (conceptual, but also empirical) models of the Panel were applied to the UK economy in late 1993. The analysis once again highlights the wide range of views that can underpin economic policy choices, but also shows how consensus and convergence were achieved. However, as noted above, there are costs to this consensus. The recommendations made by the Panel are compared to the policies announced by the Chancellor, and it is argued that issues which the Panel had identified as important failed to make the policy agenda, perhaps as a result of their decisions how to present their recommendations.

Finally, Chapter Eight looks back on the Panel of Forecasters' first year and considers what lessons were learned from it. As well as interviews with the Panel of Forecasters it includes an interview with Sir Alan Budd who was at the time the Chief Secretary to the Treasury. He is now a member of the Monetary Policy Committee of the Bank of England.

The first part looks at the degree to which Panel members made changes as a result of the economic events of 1993. Did they alter their models or could any forecast mistakes be explained by reasons other than model short-comings? Perhaps unsurprisingly, none felt that their models needed to be altered significantly and as a result the controversies that existed at the beginning of the year rolled over into the next still largely unresolved. The last part of the chapter, and the book, returns to the questions raised at the very beginning, how economic science can be used in policy-making processes. Drawing on the analysis of economic modelling as skilful practice, and the Panel as an institution, it is argued that the value of economic models (and modellers) lies in their potential to provide a level epistemological basis for the range of social, political and moral theories that can be used to frame economic policy. The recommendation is that citizens and policy makers should rethink their roles and actively shape the economic future.

The sociology of scientific knowledge

Although this is a book about economic modelling and forecasting, it is primarily a contribution to the sociology of scientific knowledge (SSK), a diverse field that takes as its subject matter the making of scientific knowledge. It is sometimes treated as part of the more general field of science and technology studies (STS) or science, technology and society (ST&S), but this tends to obscure its distinctive commitment to the sociological investigation of the ways

in which scientific knowledge claims are made, supported and assessed. Although there are several distinct schools and methods within SSK, they are all concerned with the empirical working through of the later philosophy of Wittgenstein, especially the *Philosophical Investigations*.¹¹ In this work, an influential summary of which is to be found in Winch (1958), Wittgenstein develops the idea that it is the use of a concept within a form of life that gives it its meaning. Knowledge, and scientific as knowledge in particular, is thus understood as the counterpart of the practices and ideas that make up a social group or community. (This is not to deny the objective validity of scientific knowledge.) The sociology of scientific knowledge uses this idea as the basis for analysing knowledge claims as social phenomena.

The four cardinal tenets of the sociology of science were given by David Bloor:

- It is causal, that is, concerned with the conditions which bring about beliefs or states of knowledge.
- It is impartial with respect to truth or falsity, rationality or irrationality, success or failure.
- It is symmetrical in its style of explanation, using the same types of explanation for both true and false beliefs.
- It is reflexive. In principle its patterns of explanation must be applicable to sociology itself.

(Bloor 1976: 4-5)

These requirements of causality, impartiality, symmetry and reflexivity form the basis of what has come to be known as the 'strong programme' in SSK.¹² The aim of the strong programme is to demonstrate that the perceived objectivity of scientific knowledge is, at least in part, a function of the social processes which make up and define scientific activity. More specifically, it seeks to relate the closure of scientific controversies around particular theories to social interests congruent with them. Other, equally influential, sociologists of science from the same period (for example, Collins 1985) were less concerned with identifying social interests and focused rather more on the interpretative flexibility of scientific data, the way in which the same data could be used to support different, competing hypotheses. In either case, the research sites were hard sciences such as mathematics (Bloor 1973, MacKenzie 1981) and physics (Collins 1974, 1975).

For example, in one of the classic case studies in the field, Harry Collins (1985) showed that apparatus to detect high-fluxes of gravity waves could only be constructed if the experimenters knew what they were looking for. In order to use these detectors it was necessary to know quite a lot about the gravity waves. If the detector did not detect anything, did that mean there were no gravity waves to be seen, or that the detector was a bad detector? Conversely, if the detector did appear to detect something, did that mean that there were gravity waves, or were the signals a product of the detector's design?¹³ The

sociological interest arises because the resolution of this problem depends on establishing the correct way to interpret the available evidence. In other words, how is it that one particular interpretation comes to be dominant when others, put forward by equally well-qualified physicists, are also compatible with the data?

SSK theorists felt it important to begin with the most prestigious sciences, such as physics, because it was generally believed that although sociology could be used to explain scientific errors, it could have nothing to say about scientific knowledge. This was, by virtue of the scientific method, free from all social influence. Thus, sociologists of science such as Robert Merton (1976) had not thought it appropriate to analyse scientific knowledge claims as social phenomena. (Mistakes on the other hand were clearly the result of social 'contaminants'.) Breaking with the belief that scientific knowledge was asocial was a vital first step in developing a full-blown sociology of science, and this is why Bloor's tenets of symmetry and impartiality were so important. Beliefs that came to be seen as true were treated in exactly the same way as those that came to be seen as false. This is not as controversial as it may sound. In the course of a scientific controversy no one knows which beliefs are (going to be) the true ones and which are (going to be) the false ones. That is only apparent once the controversy is resolved.

It is worth pointing out, as it is not always understood, that although the proponents of the new sociology of science wanted to change the way sociologists, and quite possibly society, thought about science, they did not necessarily want to change science itself. Indeed many were, and still are, strong supporters of science (for example, Collins and Pinch 1993). SSK's dispute was really with the philosophers of science, and with the way these philosophers positioned scientific knowledge in relation to other forms of knowledge. In other words, the aim was to reduce the epistemic supremacy granted to science by others in society, through demonstrating that scientific expertise was grounded in everyday practices and procedures, and was not the result of a privileged method of discovering the truth. In this new world, scientists would behave as they had in the old one – it was the philosophers who were wrong – and their expertise would still be respected. The change would be in the relationships between these experts and other social agents. The expertise of scientists would be placed on a par with other sorts of expertise and not reified as a transcendent truth. The challenge, as I interpret it, was to create a new set of ideas with which to think about science, ideas which are informed by an understanding of what scientists do, but which also allow for more sophisticated judgements than simple dichotomies such as true/false or right/wrong (see Collins 1987). I think this is still an important task. In the case of economics, the idea that there is no alternative seems to be one example of the effects of misunderstanding science. Indeed, if the Panel of Forecasters showed anything, they showed that there were important choices to be made.

*Symmetry, neutrality and the analytic
critique of science*

Unfortunately, because of the commitment to explaining both accepted and rejected knowledge in same way, the notion of making choices can be a difficult one for SSK. Traditionally, the virtue of a symmetrical analysis (i.e. one that conforms to Bloor's third tenet) has been that it ensures social researchers remain even-handed in their analysis of scientific controversies. The alternative is a return to the sociology of scientific error, in which social explanations are reserved for one side (inevitably the 'losers') and natural-empirical ones for the other (inevitably the 'winners'). The symmetry principle ensures that sociologists of science avoid this by promoting a professional agnosticism with regard to the outcome of the science being studied.¹⁴ While this certainly helps to explain how science develops, upholding the tenets of symmetry and neutrality also has a downside: sociologists of science tend to be more comfortable with enumerating the different positions within a particular controversy than with supporting or evaluating them. This can raise problems when the scientific controversy being studied is directly implicated in everyday life, for example, the medical testing of cancer or AIDS therapies. In these cases, the perception that SSK should remain on the sidelines and say nothing about the social processes that mediate the controversy seems to render the work academic in the worst sense of the word. As a result there has been a long-running debate within SSK about the ways in which it can become more 'committed' without compromising the principles of symmetry and neutrality that define it.¹⁵

In its early stages, this debate about symmetry and commitment tended to focus around the use of the sociological account by one side in an ongoing controversy. The belief was that sociologists would always be captured by the underdog, who would then benefit from SSK's deconstruction of the orthodox science. The issue was what sociologists should do about this. The positions adopted ranged from doing only research on controversies in which one's personal sympathies lay with the underdog, to arguments that sociologists *qua* sociologists were committed to neutrality in their professional life, no matter what their beliefs or actions as individual citizens. As is often the case in SSK, however, the more the categories 'symmetrical', 'neutral' and 'underdog' were analysed and debated, the more complicated things became.

When the dust finally settled, it seemed that the 'symmetry' tenet did not necessarily imply neutrality. Thus, in cases where the underdog was of a particularly low status, the sociologist would have to work extra hard to deconstruct the orthodoxy and make the unorthodox credible. Such an endeavour would not be neutral, but it would be symmetrical.¹⁶ More importantly there are instances where science and policy meet, and where sociologists of science will wish to draw upon sociological insights and research to intervene. In these circumstances, SSK informs an analytic critique of science

which is not neutral, as it is clearly intended to influence practice, but which remains symmetrical, in as much as no particular knowledge claim is supported.¹⁷ In other words, the focus of any SSK intervention is the processes or procedures through which scientific knowledge is mediated, not the knowledge itself.¹⁸

Although these ideas are extremely important, the sociological study of economic modelling poses one additional problem. The idea of an analytic critique of science is clearly helpful because it legitimizes the sociological critique of econometric modelling, seen as a science. However, within the analytic critique of science there is an implicit separation of the sociologist's expertise from that of the scientists. Thus, the analytic critique of science might suggest reforms to the process that would change the way in which science was represented, but it would not directly challenge or question the science itself.

Experience has shown that this approach will work well in many cases. Even when the topic of sociological study is an applied science, the distinction between being an analyst and being an activist can be maintained, although it may be harder to do so. Thus, for example, science policy analysts such as Bryan Wynne (1989, 1992) and Sheila Jasanoff (1992a, 1992b) use sociological analyses of the ways in which various institutions deal with science to argue for alternative systems which would, they believe, do a better job. This book is similar in spirit. For example, where the analysis is concerned with theory choice in economics, and the ways in which the ambiguities of econometrics are discussed and resolved, it is both symmetrical and neutral in the classic traditions of SSK. To the extent that skill and judgement are important in the natural sciences, the book can be read as supporting the scientific credentials of macroeconomic modelling. However, there is also a more critical element. The outputs of macroeconomic modelling remain influential in a number of important areas, including national and international economic policy making, despite criticism from within and outside economics. It would therefore seem incumbent upon the would-be critic to offer some solutions that would improve the way economic models, as decision-making tools, and economic forecasters, as expert advisers, are used in the policy-making process.

Economic modelling

This section provides a brief outline of some of the relevant literature on economic modelling, focusing mainly on the UK, but also locating it within the wider corpus of economic literature. It thus provides an overview of the main developments in macroeconomics which form the background to the rest of the book. The discussion covers three main areas. First it describes how economic modelling has developed in the UK, focusing principally on academic and publicly sponsored research teams, but also mentioning the key developments elsewhere. Next it provides some historical background on the

modelling teams that feature in the later chapters and gives some idea of the differences between the various UK macroeconomic modellers. Finally, there is a brief survey of some of the comparative research conducted by economists in order to evaluate the forecasters' performance.

A brief overview of macroeconomic modelling

Macroeconomic modelling, at least in its conventional form, traces its roots back to the Dutch economist Jan Tinbergen who estimated the first macroeconomic model in 1936.¹⁹ The model, which was presented to the Dutch Economic Association in The Hague, not only analysed the Dutch economy but also simulated the likely impact of various policy responses to the Depression. Although it was reviewed critically by Keynes in *The Economic Journal* (Keynes 1939), Tinbergen's method was none the less influential, and this Dutch model was quickly followed by a commission from the League of Nations and a two volume study on the *Statistical Testing of Business Cycle Theories* (Tinbergen 1939).²⁰ The second of these volumes, which was subtitled *Business Cycles in the United States of America*, detailed an economic model comprising 71 variables and 48 equations.²¹ Also around this time, although not published until 1951, Tinbergen estimated a model of the UK economy (Tinbergen 1951).

The next conventional milestone in the history of macro-econometric modelling is the post-war work of Lawrence Klein at the Cowles Commission in the US. Building on Tinbergen's pioneering work, Klein formulated a new model of the US economy (Klein 1950) and, with colleagues at Oxford in the 1950s, developed the second model of the UK economy (Klein, Ball, Hazlewood and Vendome 1961). This UK model has the distinction of being the first model of any national economy to be estimated using quarterly data.

The post-war period saw several other significant developments in UK macroeconomic modelling and forecasting. Her Majesty's Treasury (HMT) began preparing qualitative assessments of economic prospects. Initially these forecasts were not derived from a formal econometric model but were based on the judgement and intuition of Treasury economists. As this practice became established, the National Institute of Economic and Social Research (NIESR) was charged with providing an independent set of forecasts with which the Treasury's assessments could be compared. The first NIESR forecasts were produced in 1959, and like those of the Treasury, relied mainly on the judgement of the economists who prepared them. By 1961, as the judgmental relationships that underpinned the original forecasts were gradually replaced with estimated equations, so the qualitative assessments of the Treasury and the NIESR became quantitative projections.

In 1965 the Social Science Research Council (SSRC) was directed to allocate public funds to macro-econometric research. The first grants were awarded to the NIESR, the London Business School (LBS), the Cambridge Economic Policy

Group (CEPG), the Cambridge Growth Project (CGP) and Southampton University. The LBS produced its first forecasts in 1966 using a fully computerized economic model made up of 16 equations. Other forecasters soon followed this lead and developed computerized models of their own. Officially, the NIESR adopted a computer model in 1969 and the Treasury from 1971, although both had produced experimental computer-based forecasts before this. Other organisations also began producing their own economic forecasts at around this time. For example, the Bank of England began producing economic forecasts in 1973. More generally the 1970s saw a marked growth in the number of organisations producing macroeconomic forecasts, and by the end of the decade it was estimated that there were 99 separate organisations producing economic forecasts for the UK (Cyriax 1981).

However, the 1970s were also a difficult time for economic forecasters as events in the world challenged both their theories and their models. The classic example is the 'stagflation' of the 1974-5 recession where high unemployment coexisted with high rates of inflation. The responses of the modellers and their critics were complex and led to both methodological innovation and theoretical critique, as well as efforts to increase the formalism of the models, to improve the methods by which economic models were estimated and to reduce the use of judgemental adjustments.

These methodological criticisms focused on several different aspects of the 'Cowles Commission' style of modelling and three major alternatives developed as a result (Pagan 1997). First, there is the 'general to specific' style of modelling associated with David Hendry (Hendry 1993; Hendry and Richard 1982). Here the emphasis is on starting with the most general form of the equation that the theory suggests and then letting the data define its final form. The second alternative, generally associated with Ed Leamer (1978, 1983), is to consider explicitly a range of plausible alternative specifications of the equations. The criticism of standard models is that they reify essentially whimsical decisions about which variables to keep and which to discard. Finally there is the vector autoregressive (VAR) style of modelling associated with Christopher Sims (1980). Here the criticism of the Cowles Commission models relates to the 'identification' problem, essentially the way in which the models implicitly segment the economy into a number of discrete and separate sectors. Sims argued that the economic variables were much more interdependent, and that other modelling methodologies were needed to capture these effects.

Of course, these critics still accepted the idea of modelling as a worthwhile way of evaluating economic theories and policies. Other economists were more sceptical. The most well known of the theoretical arguments against macroeconomic models was put forward by Robert Lucas (1976) in what has become known as the 'Lucas critique'. The Austrian school of economics associated with Hayek (1949), is also deeply critical of macroeconomic modelling, although for different reasons. More generally it should be noted that within the economics profession as a whole, the high profile and high

prestige positions go not to macroeconomic modellers but to theorists (Kalmer and Collander 1990). However, outside the world of academic economics the need for, and reliance on, economic forecasters and modellers is as great as it has ever been. Economists may not listen to economic modellers very much, but plenty of other people do.

This process of technical development and sophistication was no doubt facilitated by developments in computer technology which made large-scale estimation much easier, but the public and political sense of disquiet at the failures of the existing models must surely have helped to sustain the drive for more scientific foundations. Perhaps unsurprisingly, the 1970s and 1980s also saw institutional innovation as well as methodological innovation. For example the Bray Amendment, which required the Treasury to publish details of its model, can be seen as an attempt to increase accountability and openness. Other changes saw funding for macroeconomic modelling moved out of the mainstream funding competitions and administered separately through a specially created consortium made up of the Research Council, the Treasury and the Bank of England. The 1980s also saw the creation of the Macroeconomic Modelling Bureau at the University of Warwick, which was charged with comparing models, disseminating information and promoting understanding.

More routinely the funding council, which was renamed the Economic and Social Research Council (ESRC) in 1984, continued to review its funding at regular intervals and as a result there have been several changes in the organisations that receive public support. For example Southampton University, the Cambridge Growth Project and the Cambridge Economic Policy Group all lost their funding and grants to both the NIESR and LBS were cut. New grants were awarded to Liverpool University and City University Business School, although both lost out in later reviews. In the latest (1995) funding round, in addition to maintaining the Macromodelling Bureau at the University of Warwick and supporting ongoing research at the NIESR, the LBS and the University of Exeter, the ESRC made new awards to researchers at the LBS and the Universities of Cambridge, Liverpool, Oxford, Sheffield and Warwick.²²

Apart from the changes to the way public money is used to support economic modelling research, there have been several other interesting changes to the institutional arrangements related to macro-modelling modelling in the UK. The most obvious has been the sustained growth in privately funded forecasting.²³ From a sociological perspective the most significant was the creation of the Panel of Independent Forecasters in November 1992 following the UK's exit from the European Exchange Rate Mechanism (ERM).

The founding members of the Panel were Andrew Britton (NIESR), Tim Congdon (Lombard Street Research Ltd), David Currie (LBS), Gavyn Davies (Goldman Sachs International Ltd); Wynne Godley (Kings College, Cambridge and Jerome Levy Institute), Patrick Minford (Liverpool University and Cardiff

Business School) and Andrew Sentance (Confederation of British Industry (CBI)). In December 1993 Andrew Sentance resigned following his appointment to the LBS (already represented by David Currie, reducing the membership to six).

In November 1994, nearly two years after the Panel was first created, the Chancellor Kenneth Clarke reviewed the Panel's membership and terms of reference. As a result of this review the number of meetings was reduced from three per year to two and it was announced that the Panel members would be replaced after serving three years. However, to ensure continuity it was decided that David Currie and Wynne Godley would leave the Panel at the end of 1995. The others would remain until the end of 1996, when further changes would be made.

The new members appointed to the Panel at the end of 1995 were Kate Barker (CBI) and Bridget Rosewell (Business Strategies Ltd). Martin Weale took over from Andrew Britton as the Director of the NIESR and thus also joined the Panel. On 8 November 1996 the Chancellor announced that Patrick Minford would be leaving the Panel and would be replaced by Roger Bootle (HSBC Bank). The appointments of Tim Congdon and Gavyn Davies, the only remaining members of the original 'Wise Men', were extended for another year. However, the changes announced turned out to be more radical than many people had anticipated, and the report of November 1996 was the last one to be published. This seems to have happened more by default than design: Roger Bootle never took up his appointment and the meeting scheduled for early 1997 never happened.

Following the election of the Labour government in May 1997 the Panel was officially discontinued as the new Labour Chancellor, Gordon Brown, gave operational independence to the Bank of England and created a new body - the Monetary Policy Committee (MPC) - to set interest rates.²⁴ Interestingly this decision, which seemed to be based on the need to separate the economics of economic policy from its politics, suggests a re-entrenchment of economic analysis as distinct from other considerations. It therefore created the potential for exactly the sort of 'closed' institutional space, in which a particular economic analysis may be uncritically accepted, that the Panel was supposed to replace. Of course, there is no reason why the MPC should repeat the mistakes of the past, but a lot depends on the extent to which the Treasury Select Committee is able to keep different economic models visible and hold the MPC accountable for the decisions and choices it makes.²⁵ However, before saying any more about the importance of different economic models, it is necessary to set out what these models are like.

Models of the UK economy

As noted at the beginning of the previous section, the first economic forecasts were largely judgemental affairs. Although some econometric work was available, the forecasts themselves were prepared on the basis of a fairly informal model

of the economy. Calculations were carried out by hand using equations that had not (usually) been formally estimated. Instead, the forecasters imposed the majority of coefficients on these equations, relying on judgement and experience.

The criticisms made of these forecasts were that the policy recommendations produced were too late and of the wrong magnitude. The response of the forecasters was to move towards more formal modelling techniques. They did not, however, change the basic structure of the models, which persists to this day.²⁶ The following discussion introduces the models used by the members of the Panel of Independent Forecasters and gives some idea of the state of the art in the early 1990s. The modelling groups discussed are, in alphabetical order, the Cambridge Economic Policy Group (CEPG), Lombard Street Research (LSR) London Business School (LBS), Liverpool Macroeconomic Research Group (LPOOL) and the National Institute for Economic and Social Research (NIESR).

Cambridge Economic Policy Group

The CEPG, which is closely associated with Professor Wynne Godley, has always had a distinctive forecasting style that emphasizes the importance of shifts in the deficits and surpluses held by the different sectors of the economy for understanding macroeconomic events. In the 1970s the CEPG was able to use key accounting relationships to provide a powerful economic analysis while simultaneously questioning the need for large economic models, such as the one maintained by the Treasury, the NIESR and the LBS.²⁷ Later, however, as the deficits and surpluses in the private sector became more volatile, the Cambridge ideas became harder to apply (Holly 1991: 32–8).

Theoretically the CEPG models took a strong Keynesian approach, also known as the ‘new Cambridge economics’. Recent information concerning the specification of the CEPG model is sparse, partly as a result of the ESRC’s decision to withdraw its grant in 1983. In the early 1980s, however, the model was estimated on annual data and contained 38 behavioural equations. By way of comparison, the LBS model of 1985, a ‘large’ macroeconomic model, had 770 variables and over 100 behavioural equations (Wallis (ed.) *et al.* 1987: 5–7). More recently, in interview Professor Godley has suggested that early versions of the model underestimated the extent to which supply-side factors can constrain output.²⁸ It seems reasonable to suppose that some alterations have been made to the specification of the model in the light of this. The model has also been re-estimated since the UK left the ERM in September 1992.

Lombard Street Research Ltd

LSR is an independent economic research and consultancy organisation based in the City of London. It was formed in 1989 by Professor Tim Congdon and its work is mostly orientated towards a client base that comprises industrial,

commercial and financial organisations making strategic investment decisions. LSR is interesting not so much for what it does, but for how it does it. In particular, it is based on Congdon's distinctive economic analysis.

Theoretically, the ideas that inform Congdon's analysis can be grouped into two main categories. First, there is the link between the 'output gap' (the difference between actual output and potential output) and the price level. Put simply, if the economy is not producing as much as it could, then output can increase without prices rising. Second, there is the demand for money, which is understood through an analysis of the balance sheet positions of different economic sectors and of the growth of credit. The output gap is a well-established Friedmanite concept, but his analysis of the demand for money makes Congdon's position distinctive.²⁹ Congdon's views on modelling methodology are also different from what might be thought of as the mainstream. Like Godley, he is sceptical about the utility of large-scale macroeconomic models. As a result, forecasting and analysis at LSR are carried out through a combination of econometric equations and spreadsheet work.³⁰

The London Business School

The LBS has a long history of involvement in economic forecasting. At the time of the Panel of Independent Forecasters' meetings in 1993 it was under the Directorship of David Currie. The LBS was one of the first institutions to receive support from the SSRC and, as noted above, was the first to develop a computerized economic model. During its many years as a forecasting organisation it has continually updated and changed its model.³¹ One particularly important period occurred between 1975 and 1977, when extensive alterations were made to both the structure and the specification of the model. The theoretical underpinning of this revision was 'international monetarism', and its main consequence was that, according to the new version of the model, a devaluation had no long-run effect on output.³²

In addition to this theoretical realignment other changes were also made during the 1970s. For example, like several other modelling teams, the LBS augmented their equation for explaining and predicting consumption by including inflation as an explanatory variable. (Empirical support for this can be found in the influential paper by Davidson *et al.* (1978).) The inverse relationship between consumption and inflation was supposed to model the way in which, as inflation rises, people save more in order to preserve the value of their financial assets.³³ This change in specification is now common to almost all UK models and has the effect that, depending on the influence of inflation, the expansionary effects of higher government spending can be cancelled out by increased consumer saving. During the 1980s the LBS model was revised again, with the addition of a detailed financial sector, in which the exchange rate was modelled as a market-clearing price determined under the assumption of rational expectations. More recently the LBS model has been revised again.

In particular the assumption of rational expectations has now been dropped and replaced by a model of learning based on a 'reduced, reduced form' model of the economy, which economic agents are assumed to know, use and modify.³⁴

Liverpool University Research Group in Macroeconomics

The Liverpool model was developed by Patrick Minford and colleagues at Liverpool University and was first used for forecasting in 1980. Although in some ways continuous with the mainstream macroeconomic models, in the sense that the structure of the model was quite similar, the Liverpool model was in other ways quite radical. In particular it incorporated several recent developments in economic theory, the most significant of which was that it was a 'new classical' model solved using a rational expectations algorithm.³⁵ Other features that distinguished the Liverpool model from its peers were as follows. First, factors that affected the supply of labour, in particular the level of unemployment benefit, were treated in more detail than in the more demand-oriented mainstream models.³⁶ Second, an explicit allowance was made for the impact of changes in wealth on consumption, so that private expenditure decisions were not related to income in the same way as in the other models.³⁷ Third, government spending was determined endogenously (that is, by the model) on the basis of a constant PSBR/GDP ratio and an assumption about the average tax rate. This contrasted with the majority of other models which took government spending as an exogenous policy variable. Fourth, the exchange rate was modelled using the theory of 'uncovered interest parity' according to which any expected appreciation in the exchange rate must be matched by UK interest rates being lower than foreign ones by exactly the size of the expected rise. Fifth, exports and imports were modelled jointly as a function of world trade, domestic income and the actual and expected exchange rates. This contrasted with the majority of other models, which had separate import and export equations. Finally, the Liverpool model was smaller than its mainstream competitors and had generally simpler dynamic specifications.

The National Institute for Economic and Social Research

The NIESR, like the LBS, has a long historical involvement in economic forecasting. Again like the LBS, it modified its model throughout the 1970s and 1980s. However, unlike the LBS it did not undergo any major theoretical realignment. It thus remains in the Keynesian income-expenditure tradition and can be viewed as a quantity adjustment model, driven more by expenditures than by prices.³⁸

In 1977 the NIESR introduced a simple financial system to the model and, in conjunction with this, modelled the exchange rate according to the theory of long-run purchasing power parity. In the NIESR model the exchange rate was determined by UK prices relative to overseas prices, the visible trade balance and the covered interest rate differential between UK and US interest rates.³⁹

Like the LBS, the NIESR also introduced the rational expectations hypothesis into its model during the 1980s, and by 1985 had forward-looking behaviour in the equations for employment, stockbuilding, wages, exchange rate, and the demand for narrow money (M1). However, the exchange rate equation remained problematic for some considerable time, with the forecast rate being highly dependent on the terminal conditions set for the expected exchange rate. More recently the model has been revised again. In 1989 revisions included changes to key wage and price equations, additions of equations to improve the modelling investment flows and the capital account, and a switch away from using unemployment as the measure of excess supply in the labour market.⁴⁰

Summary

Of the five UK economic models that feature in this book, the LBS and NIESR models are probably closest to what can be thought of as the mainstream in economic modelling. The other three offer quite different perspectives on both what a macroeconomic model should look like and how the economy works. In other words, these are not just differences of modelling methodology, they are also differences of economic theory and, as we shall see, of economic policy. The problem for policy makers is that all these models pass the appropriate sets of tests for statistical significance, and all appear to be equally 'good' explanations of economic events. One possible solution to this conundrum might be to ask if the models can be ranked in some other way, perhaps according to their ability to generate accurate forecasts of economic events. (Forecasting is therefore an additional test that the models must pass.) The rest of this introduction considers the extent to which forecasting accuracy can assist in choosing a macro-econometric model. It is argued that, on the basis of *ante* forecasts, no model is unambiguously superior to the rest. The remainder of the book will explore why this diversity of theory and practice continues to exist and what, if anything, should be done as a result of it.

Comparing economic forecasts

The following discussion focuses on the forecasts produced during the 1980s by the main UK economic forecasting organisations funded by the SSRC and its successor, the ESRC. It is intended to illustrate some of the complexities that arise when one tries to distinguish between economic forecasters on the basis of their forecasting record, and does not claim to represent a full survey of the literature.⁴¹ The forecasts considered are those made in 1979-80 and those made in 1983.

The Conservative budget of 1979

The period 1979-80 was a momentous time for economic policy-making in the UK. The election of the Conservative government in April 1979 heralded a

radical change in economic policy and posed some interesting challenges for forecasters.⁴² Specifically, what effects would the policies of the new government have on the economy? The following discussion is based on Ken Holden's summary (Holden 1989) of a comparative study originally published by Barker (1985). Barker compared the forecasts produced by Cambridge Econometrics, CEPG, Liverpool University Research Group, the LBS and the NIESR in 1979 (1980 in the case of Liverpool) for the following three years. The study is of particular interest because it compares forecasts made before the election of the Conservative government with those made by the same forecasting organisations just after the budget of June 1979. In this way, Barker was able to assess both what the forecasters thought the effects of the policies would be and how accurate their predictions were.

Once the budget measures had been announced the consensus among the forecasters appeared to be that the new government's policies would be deflationary, with all the forecasts for economic growth being revised downwards. There was much less consensus on the severity of this downturn, however, and none of the revised forecasts seem particularly close to the actual outcome. For example, both the Liverpool University Research Group and the NIESR failed to predict the recession at all. Instead they forecast that, although the rate of growth would slow, it would not become negative and that the economy would not therefore contract. The official figures record a 2.8 per cent contraction in output in 1980 and a further 2.3 per cent in 1981.

Of the others, Cambridge Econometrics forecast the onset of recession a year early and underestimated both its depth and severity, forecasting falls in output of 0.5 per cent in 1980 and 0.3 per cent in 1981. The LBS correctly forecast the onset of recession in 1980, but like Cambridge Econometrics, underestimated its depth and duration. In contrast, the CEPG correctly forecast the onset and duration of the recession but overestimated its severity, forecasting a contraction of 6.1 per cent, against an actual outcome of -2.8 per cent.

Evaluating these forecasts is made more difficult by the fact that the different forecasters tended to get different parts of the forecasts wrong. Thus, although the CEPG overestimated the depth of the recession, their forecast for unemployment was one of the best. Similarly, although the Liverpool team, with a very different model, underestimated the negative effect of the policies on output, they none the less produced one of the best inflation forecasts. As a result it is very difficult to decide who made the best forecast, as no single model or forecast or theory explains everything better than the rest. Indeed, the conclusion drawn by Barker is that none of the models predicted the effects of this major policy intervention with any accuracy.

The miners' strike of 1984

Later studies show a similar pattern of partial success and partial failure. In 1987 the ESRC Macromodelling Bureau at Warwick published its third review

of the UK macroeconomic models (Wallis *et al.* 1987). Chapter Four of this edition contains a detailed *ex post* analysis of one- and two-year-ahead forecasts made in the autumn of 1983. These forecasts are particularly interesting as March 1984 saw the beginning of a national strike by miners which lasted approximately twelve months and, according to official figures, reduced output growth by 1 per cent in 1984. In contrast to the well-publicised economic reforms of the Thatcher government, the industrial action of the mine-workers was an unexpected 'shock' to the economy and one that could not, therefore, have been taken into account when forecasts were being prepared.

Perhaps surprisingly, it turns out that although the miners' strike was a common source of error for all forecasters, it does not explain the forecast mistakes. In particular all the forecasts, apart from the Liverpool team's forecast for the 1984 growth rate of expenditure measure of GDP, *underestimated* the actual growth rate.⁴³ For example, the NIESR predicted a sustained slow down in growth of the output measure of GDP where in fact the growth rate increased from 3 per cent to 3.4 per cent. Both the LBS and the NIESR were overly pessimistic about inflation, predicting higher rates than were actually recorded. In contrast the Liverpool forecasters appear over-optimistic, consistently under-predicting inflation. The NIESR's prediction of a sustained slow down in GDP growth led them to over-predict the increase in unemployment during the period to the end of 1985 by about 200,000. In contrast, the LBS and Liverpool, who were relatively more optimistic about output, under-predicted the final unemployment totals by 400,000 and 800,000 respectively. As unemployment actually increased in both 1984 and 1985, the NIESR's pessimism about the growth in GDP, which led it to forecast that unemployment would rise steadily, now seems more warranted.⁴⁴

The conclusions drawn by Wallis *et al.* (1991) are as follows. Liverpool and the LBS are adjudged to have produced the most accurate forecasts for the *level* of GDP, with the LBS producing the more accurate forecasts for the *growth* of GDP. The LBS and the NIESR produced the best inflation forecasts, although both tended to over-predict. Liverpool on the other hand tended to under-predict, but by a bigger margin. Finally, the NIESR and the LBS produced the most accurate forecasts for the number of unemployed, but they made large mistakes in forecasting the number of people actually in work. Liverpool, although wayward in their estimates of unemployment, made broadly 'equal and opposite' mistakes forecasting employment.

The 1980s 'boom'

In a paper published in 1991, Wallis and Whitley repeated this analysis for forecasts published during the period 1984-8.⁴⁵ The conclusions they reached are similar to those for the 1983 forecasts. For example, they found that all the forecasting groups had a tendency to underestimate the rate of growth of output during this period, with only four out of forty-six forecasts appearing over-

optimistic. They also compared the mistakes made by the different groups and found that certain models were more likely to make certain types of forecasting errors. Thus, the demand-based models of the LBS and the NIESR tended to be more pessimistic than the supply-side models of Liverpool and the City University Business School when it came to forecasting economic output. The LBS and the NIESR had average errors of about 3 per cent for the level of output and 1.5 per cent for the growth rate.⁴⁶ The Liverpool and City University forecasters tended to do much better, recording errors of approximately 1 per cent and 0 per cent for the level and growth of output respectively.

Inflation forecasts and their associated errors were less systematic, although the Liverpool forecasts tended to under-predict while the others generally overpredicted, with the LBS forecast errors changing sign. If the variables are considered individually, Liverpool and the City University produced the best forecasts for the level and growth of output, the LBS the best forecasts for inflation and the NIESR the best forecasts for unemployment. If all four variables – rate of output growth, level of output, inflation and unemployment – are considered to be equally important, then the best one-year-ahead forecasts (defined as having the lowest average root mean squared error) were produced by the LBS. The best two-year forecasts, however, were produced by Liverpool.

Summary

Comparing economic forecasts with outturn data seems to tell us one thing: it is very difficult to establish a single best model. Instead it seems that different models will be better at different parts of the forecasts. Were a forecast to be worse than the others on all variables, it might therefore be thought to be wrong. In practice, however, this poor performance would probably need to be repeated over a period of years before the credibility of the model was seriously under threat.

So far a brief history of the development of macro models in the UK has been set out, and their relative performances evaluated. First, a general overview was presented and some of the theoretical similarities and differences between the research groups were outlined. Next it was shown that, using forecasting performance as a criterion, no one model or modelling team appeared to have a significantly greater degree of success than the rest. Despite this, forecasting and modelling activity and research continue to grow apace. The current ESRC funding round has allocated £3.2 million to macro-modelling research.⁴⁷ The Treasury's monthly survey of economic forecasts lists over thirty contributors, and this list is by no means exhaustive. The question this book addresses is not how this diversity can be reduced. This is clearly a matter for economists. Rather it is how this diversity comes about, and how it can be managed within a policy framework that cannot afford to wait for the economists to reach agreement.

CHERISHED BELIEFS AND T-STATISTICS

How economic models are estimated

Introduction

Economic models are attempts to quantify the relationships identified by economic theory by using statistical techniques, such as regression analysis, to examine the relationships between different measures of economic activity. This chapter begins the task of analysing the sociology of economic modelling by introducing some of the key concepts and techniques which economists use to understand the economy. It is, however, more than just a recapitulation of textbook economics. Rather, the analysis shows how applying basic econometrics to actual data is complicated in practice and how additional expertise, developed through participation in professional economic life, is needed to estimate even the most basic of econometric equations.¹

The argument begins by introducing the basic accounting and theoretical ideas that underpin the specification of econometric equations and models and constitute what might be thought of as the ‘ontology’ of the economic modeller. Next it shows how an equation representing one of these elements – the consumption function, which models consumption expenditure – can be estimated, and illustrates some of the problems that arise when attempting to do this. By attempting to replicate econometric work done by Giles Keating (1985) in the early 1980s, the analysis shows how statistical significance tests are interpreted in the context of wider concerns and commitments, and examines the stability of the coefficients in an econometric equation over time.² The final section assesses whether a commitment to economic theory improves economic modellers’ ability to forecast the economy, by comparing the forecasting performance of three different consumption functions.

National income accounting and IS-LM models

Understanding that economists think about the economy in ways that are specific to their discipline is a vital first step to analysing what they do. Indeed, understanding the economy in this way is an important part of what it means

to 'be' an economist. The two main elements of this macroeconomic understanding are the national income accounts, which define the basic accounting relationships used to measure economic performance, and the theoretical IS-LM model used by economists to analyse these accounts.³

National income accounting

The national income accounts are the framework within which economic performance is discussed and analysed. Although national income estimates have a long history, perhaps dating back to the seventeenth century, their current format can be traced to the developments in statistical methodology and economic theory that took place in the inter-war years.⁴ The easiest way to understand the national income accounts is in terms of a circular flow of incomes and expenditure around the economy.⁵ For example, if we imagine a simple economy which consists only of households and firms, the households will use income they receive in return for their labour in order to purchase the goods produced by the firms. There is therefore a flow of income, which economists call 'factor payments', from firms to households (e.g. wages) and there is another flow of expenditure from households back to firms (e.g. revenue from the sale of products to the households). If the economy is in equilibrium, then the flow of expenditure from firms to households will be matched by the flow of expenditure from households to firms.⁶

The economy shown in Figure 2.1 is an extension of this basic idea. The principle differences are that a tax-levying government sector and a financial sector have been added. The government is assumed to use the taxes it raises to supply public services and to redistribute income. Starting from the right hand side of the diagram in the box labelled 'Firms', and following the flow of payments clockwise around the loop, we can see that firms have a flow of expenditure (Y). These are the factor payments and are made up of wages, dividends and so on. However, because the government levies direct taxes (T) and redistributes income through benefit payments (B), the actual disposable income available to households consists of the factor payments (Y) minus direct taxes (T) plus benefits (B): $(Y - T + B)$.

Basically, households spend some^d of this disposable income and save the rest. Thus, households will use some of their disposable income to finance their planned expenditure (C) but will also transfer some to the financial sector in the form of saving (S). As these savings represent a deduction from households' possible consumption expenditure they are said to be a leakage from the circular flow of payments. However, once in the financial sector, these savings will flow into the investment spending (I) of both firms and the government and are thereby returned to the circular flow of payments. Similarly, government expenditure on goods and services (G) also contributes to the flow of payments. The sum of household expenditure, investment expenditure and government expenditure ($C + I + G$) is therefore the aggregate

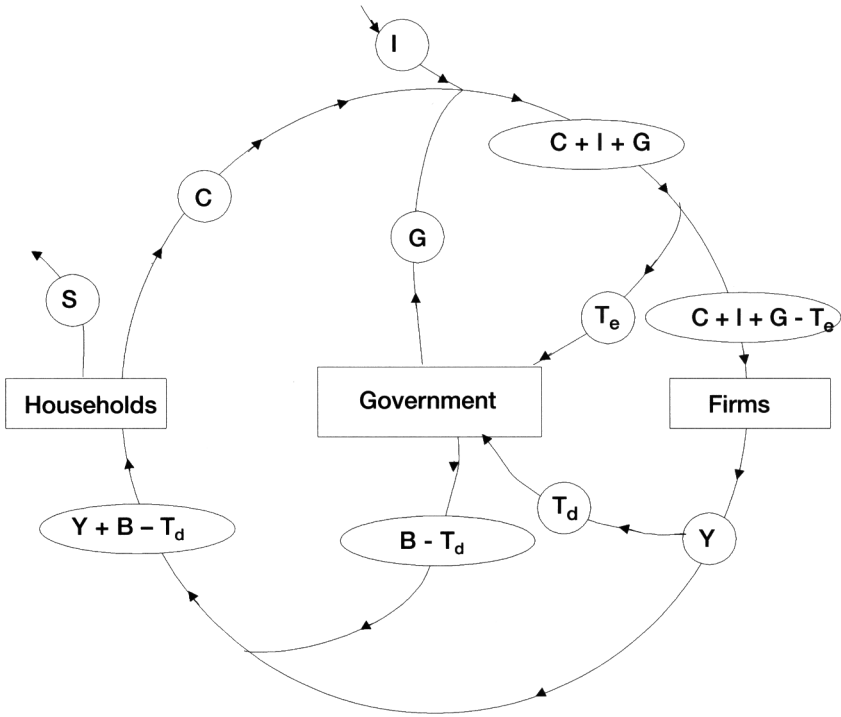


Figure 2.1 Flows of income and expenditure in a simple economy with a tax-raising government.

Source: Reproduced with the kind permission of McGraw Hill Publishing Co. Begg, D., Fisher, S. and Dornbusch, R., *Economics*, Third edition 1991 p. 361.

expenditure on ‘final products’ and is one way of measuring national income. In fact,

$$GDP = C + I + G$$

is the definition of GDP at market prices for the economy shown in Figure 2.1.

However, because many categories of expenditure are subject to indirect taxation (T) not all of the expenditure by government, households or investors that is received as income by firms can be kept by them: a proportion goes to the government. This means that the income available to firms, which is equal to their factor payments (Y), is given by $(C + I + G - T)$. For the economy shown in Figure 2.1, this is the definition of GDP at factor cost.

Finally, it should be noted that this simple economy has no trading links with any other economy. If international trade were to take place, then exports (X)

would be represented as an injection into the flow of payments, that is, a flow of income into the domestic economy from the rest of the world. Imports (M) would be represented as a leakage, that is, a flow of expenditure from the domestic economy to international economies. This would mean that the total amount of factor payments (Y) made by firms is given by:

$$Y = C + I + G + X - M - T$$

This basic model defines GDP at factor cost (Y) as the sum of its parts and describes how the flows of income and expenditure within an economy are interrelated. However, it does not tell us how the level of national income is affected by changes in the economic environment, and nor does it have anything to say about factors that determine the distribution of its components. The national accounts are thus only part of the story. It is also necessary to have a theory that links the components of aggregate expenditure to the items that influence the expenditure decisions of economic agents, for example interest rates and money. This is what IS-LM analysis provides.

The IS-LM model

The IS-LM model is the theoretical starting point for almost all macroeconomic models and provides a way of analysing the effects of monetary and fiscal policies on aggregate demand.⁷ However, precisely because it deals with the demand-side determinants, of the economy the IS-LM framework is frequently augmented by adding equations such as the Phillips Curve (which relates to a trade-off between wages and unemployment) to represent the supply-side forces which also act on the economy.

In the simple case of the closed economy shown in Figure 2.1 (i.e. an economy with no foreign trade) there are two basic markets: the market for goods and the market for money. The IS-LM model provides a framework which can be used to analyse the conditions affecting supply and demand in each of these markets. If the theory can be related to economic data, it can then be used to identify the unique combination of national income and interest rates at which both the money and goods markets will be in equilibrium at the same time.

Equilibrium in the goods market

The IS curve defines the conditions for equilibrium in the goods market and is concerned with the relationship between aggregate demand and income. In a closed economy (i.e. one with no imports or exports) there are two sources of demand for goods: the demand for consumption goods by households and the demand for investment goods (e.g. machines and buildings) by firms. Equilibrium in the goods market therefore occurs when the planned expenditure

of firms and households (i.e. aggregate demand) equals their actual income. In other words, the goods market is in equilibrium when Income (Y) equals planned investment (I) plus planned consumption (C):

$$Y = C + I$$

By rearranging this, an alternative formulation can be obtained in which the goods market will be in equilibrium when planned investment equals actual income minus planned consumption:

$$I = Y - C$$

If it is noted that savings (S) are, by definition, that part of income not devoted to consumption (i.e. $S = Y - C$) then the equilibrium condition for the goods market becomes:

$$I = S$$

In other words, the market for goods will be in equilibrium when planned investment equals planned savings. But what happens to investment and savings when the level of income rises? Because economic agents are assumed to always save a certain proportion of income, as income rises, savings rise too. This means that if equilibrium is to be restored in the goods market, investment must also be induced to rise. In the closed economy, planned investment is inversely related to the rate of interest, so the rate of interest must be lowered to increase investment so that it once more equals savings. In other words the IS curve, which is the set of combinations of income and interest rates for which the goods market will be in equilibrium, has a negative slope.

Equilibrium in the money market

For the money market to be in equilibrium the demand for money (L) must equal the supply of money (M). If the supply of money is determined exogenously (e.g. by the government), then the economic theory or model must explain the demand for money. According to Keynes, the demand for money has two main components. First there is the demand for active balances, which are used to finance transactions and to cover unforeseen expenditure. Active balances are thus an increasing function of income. Second there is the demand for idle balances. Idle balances are held by speculative investors and are inversely related to the rate of interest. The total demand for money is thus the sum of the demands for active and idle balances.

If income increases then the demand for active balances will also increase (as more money is needed to finance the increased level of transactions). However, because the supply of money has been fixed by the government, any

increase in the demand for active balances must be offset by a decrease in the demand for idle balances. For this to happen, interest rates will have to rise. In other words, for a fixed money supply, increases in the level of income will be met by increases in the rate of interest. Because of this the LM curve, which shows the combinations of interest rates and income that are compatible with a given supply of money, must have a positive slope.

The IS-LM model

If the IS curve represents a series of equilibria for the goods market in a closed economy, then the LM curve represents the same for the money market. When the two curves are plotted on the same axes, as shown in Figure 2.2, the intersection gives the unique combination of income (Y) and the rate of interest (r) for which both the goods market and money market will be in equilibrium.⁸ The intersection of the IS and LM curves is thus the equilibrium point for the economy as a whole.

Although relatively simple, the basic IS-LM model can be readily expanded to include extra details and 'real world' features. For example, the effect of government spending (G) and tax raising (T) can be included in the specification of the IS curve, with the effect that the equilibrium condition becomes:

$$S + T = I + G$$

If this is done, the government is able to influence both the LM curve (through its control of the money supply) and the IS curve through its control of fiscal policy and spending programmes.

In order to make the IS-LM model suitable for analysing an economy with foreign trade, imports and exports are incorporated into the IS curve in a similar

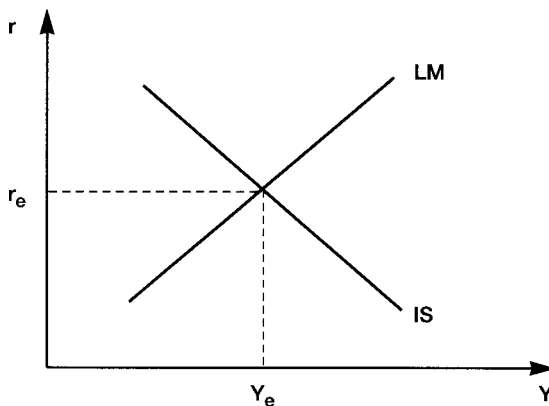


Figure 2.2 Simple IS-LM model

way to the government sector. For the LM curve, exchange rates and the balance of payments have important effects on the money supply. However, these are usually written as separate equations, which at equilibrium must pass through the same point in the (Y, r) plane as the intersection of the IS-LM curves. They thus take the form of extra conditions that must be fulfilled for an overall economic equilibrium.

The role of econometrics

Although the framework of IS-LM analysis is clearly important in suggesting the ways in which aggregate demand will respond to changes in interest rates, it does not offer any information on the size of the response. However, exactly this type of information is typically required by economic policy makers. For example, although economic theory predicts that households' expenditure will be less than their income (i.e. the marginal propensity to consume is less than one), the exact proportion of income that is saved is not specified. However, a government cutting taxation in the hope of boosting consumption would find it valuable to quantify this, and thus know how much of the extra income made available to households will actually be used to finance expenditure.

The value of econometrics is that statistical techniques (principally regression analysis) can be used to obtain quantitative estimates for just these types of items. Having derived estimates for, say, the marginal propensity to consume, the econometrician is then in a position to advise the government how much additional consumption will result from the additional income released by the tax reduction. This is at the core of policy analysis using a macro-econometric model. By first modelling the economy and then running simulations it is hoped that the effects of economic policy decisions can be estimated and policies refined so that they have the best possible chance of achieving their targets. However, the veracity of this advice will clearly depend on the quality of the econometric equations used to produce it.

Econometric testing and estimation

As all the economic forecasts and policy recommendations discussed later in this book are derived from macroeconomic models, it is important to understand not only their use but also their construction. This section traces my efforts to gain an insider's understanding of economic modelling, based on an attempt to replicate the consumption function estimated by Giles Keating in his 1985 book *The Production and Use of Economic Forecasts*. This is primarily a practical book, intended to equip the reader with sufficient skills and information to produce an economic forecast. It contains full listings of all the equations used by Keating to make his economic forecasts, detailed references to data sources and even some blank tables for readers to photocopy and use in preparing their own forecasts.⁹ It also describes in considerable detail the methods by which

an econometric model is specified, and illustrates the discussion with a series of highly detailed examples. It is therefore an excellent starting point for a sociologist (or anyone else) wishing to examine the theory and methods upon which econometric equations and models are based.

The intention here is not to argue that Keating's method is in any way inadequate: in fact it is the opposite. Keating is undoubtedly a competent and skilled econometrician, and his methodology repays careful study. The aim is to use Keating's method as a guide to good practice, and to use his results to validate (or provide grounds for questioning) my own results. As in all scientific endeavour, it may not be obvious a priori to what degree of accuracy Keating's results should be replicated in order to prove the the competence of the replication.¹⁰

The account which follows is intended to make two main points. The first illustrates one of the central themes in the sociology of science: the nature of knowledge transfer. The sociology of scientific knowledge argues that knowledge is transferred by means of socialisation. The appropriate way to proceed in science therefore is by learning through participation: the process cannot be encoded in a set of rules.¹¹ In the case of economics, it is argued that the interpretation of statistical significance tests is particularly sensitive to social context and convention. By following Keating's methods it is shown how a commitment to a particular theoretical understanding of the economy guides the interpretation of statistical tests and shapes the final structure of the model.

In addition to its value as an exposition of econometric methodology, Keating's book can be used to shed light on another, perhaps more fundamental, question: how stable are the econometric relationships? By collecting the data identified by Keating and repeating his analysis it is possible to see how additional data which has become available since 1980 influences the regression equations described in his book. In this way the analysis directly addresses the possibility of an econometric modelling science. If econometric relationships are not stable over time, then extrapolating from estimated relationships, no matter how statistically sophisticated the techniques used, will be difficult to justify.

Replicating econometrics

This section describes in some detail how I worked through the analysis outlined by Keating, and examines how closely it was possible to reproduce his results. It therefore focuses on the estimation of one of the most fundamental of econometric equations: the consumption function.

The most general form of Keating's consumption function is given in Equation 2.1. It uses 12 explanatory variables plus a constant term.¹² The variables used are the first, second and fourth lags of consumer expenditure (C), real personal disposable income (RPDI), short term interest rates (RLB) and the rate of inflation (given by change in prices (PC) plus trend real growth).¹³

Equation 2.1: General consumption function

$$\begin{aligned}
\log(C_t) = & \alpha_1 \log(C_{t-1}) + \alpha_2 \log(C_{t-2}) + \alpha_3 \log(C_{t-4}) \\
& + \beta_1 \log(\text{RPDI}_{t-1}) + \beta_2 \log(\text{RPDI}_{t-2}) + \beta_3 \log(\text{RPDI}_{t-4}) \\
& + \gamma_1 \text{RLB}_{t-1} + \gamma_2 \text{RLB}_{t-2} + \gamma_3 \text{RLB}_{t-4} \\
& + \phi_1 [\log(\text{PC}_{t-1} / \text{PC}_{t-2}) + 0.0057] + \phi_2 [\log(\text{PC}_{t-2} / \text{PC}_{t-3}) + 0.0057] \\
& + \phi_3 [\log(\text{PC}_{t-4} / \text{PC}_{t-5}) + 0.0057] + \kappa
\end{aligned}$$

Using data from 1955Q1 to 1992Q4, the regression software RATS was used to estimate coefficients for this formula.¹⁴ Table 2.1 shows Keating's coefficients (obtained from data running from 1956Q4 to 1980Q4) and mine, together with their appropriate t statistics.¹⁵ However, before comparing results several caveats should be noted. First, Keating's estimates are derived using data based on 1980 prices, while mine use data based on 1990 prices. If re-basing was all that had happened to the data, then the constant term should have changed but the coefficients should have stayed the same. However, economic time series are not only re-based, they are also subject to revisions which can literally rewrite history. This is particularly important in econometrics, and it is not uncommon to find that major papers can be sensitive not just to the sample period used, but also to the date at which the data was published.¹⁶ In other words, because of the potential effect of revisions we should not expect the coefficients to be exactly the same and it is possible that even expecting the signs and statistical significance to be unaltered is also unwarranted.

From Table 2.1, it can be seen that the estimated values of the regression coefficients do differ, although the first two lags of consumer expenditure (C_{t-1} and C_{t-2}) are statistically significant in both data sets.¹⁷ However, there are differences in both the signs and the significance of other variables. For example, the fourth lag of consumer expenditure (C_{t-4}) is, for my data, both negative and significant, although only just so. Keating¹⁴ on the other hand finds the fourth lag to be positive but statistically insignificant. Similarly, real personal disposable income is significant for my data, but not Keating's. Although other differences are apparent (e.g. the sign on the second lags of interest rates and prices) none of these coefficients are statistically significant. This means that neither Keating nor myself are able to reject the hypothesis that the coefficients are actually zero, something that that would appear to be contrary to economic theory.

The data in Table 2.1 thus indicates that previous levels of consumption remain an important influence on consumers' expenditure decisions. If anything, the weight attached to these factors has increased. The table also shows that, for the extended data set, real personal disposable income has a statistically significant effect on consumers' expenditure decisions. Of course it is possible

Table 2.1 Unrestricted regression coefficients

<i>Variable</i>	<i>Keating coefficient</i>	<i>Keating t statistic</i>	<i>Evans coefficient</i>	<i>Evans t statistic</i>
$\log(C_{t-1})$	0.614	5.13*	0.7246	6.79*
$\log(C_{t-2})$	0.305	2.29*	0.4157	3.30*
$\log(C_{t-4})$	0.0687	0.55	-0.1944	-2.04*
$\log(\text{RPDI}_{t-1})$	0.170	1.80	0.2136	2.64*
$\log(\text{RPDI}_{t-2})$	-0.0867	-0.82	-0.2142	-2.39*
$\log(\text{RPDI}_{t-4})$	-0.0465	-0.57	0.0673	0.86
RLB_{t-1}	-0.00340	-2.28*	-0.0016	-1.55
RLB_{t-2}	0.00127	0.73	-0.0005	-0.46
RLB_{t-4}	0.00015	0.13	0.0005	0.66
$\log(\text{PC}_{t-1}/\text{PC}_{t-2})$ + 0.0057	-0.250	-0.93	-0.0777	-0.50
$\log(\text{PC}_{t-2}/\text{PC}_{t-3})$ + 0.0057	-0.0239	-0.09	0.0494	0.32
$\log(\text{PC}_{t-4}/\text{PC}_{t-5})$ + 0.0057	0.049	0.24	-0.0226	-0.18
K	-0.224	-1.25	-0.1244	-1.13

* statistically significant at 5% level

that the differences that can be seen between my estimates and Keating's reflect not a change in economic behaviour during the 1980s (and hence the econometric equations) but a difference between the data sets. One way to check this would be to restrict my data set to match the sample period used by Keating and re-estimate the parameters. If the differences between the two sets of estimates are owing to differences between the two data sets, then this equation should be broadly similar to the one estimated on the full data set. If, however, the differences between my estimates and Keating's are owing to changes in economic behaviour that have occurred since 1980, then the new equation should be broadly similar to Keating's.¹⁸

Table 2.2 gives results from the same equation as in Table 2.1, estimated over two different sample periods. Columns 2 and 3 contain parameter estimates obtained when using the full data set, and columns 4 and 5 parameter estimates based on a sample restricted from 1956Q4 to 1980Q4 (the same sample period as used by Keating).¹⁹ As can be seen, the coefficients and *t* statistics estimated using the smaller sample are generally closer to those reported by Keating. For

Table 2.2 Unrestricted regression using full data set and data from 1956Q4 to 1980Q4

<i>Variable</i>	<i>Coefficient</i> (1955Q1 to 1992Q4)	<i>t statistic</i>	<i>Coefficient</i> (1956Q4 to 1980Q4)	<i>t statistic</i>
$\log(C_{t-1})$	0.7246	6.79*	0.3646	1.88
$\log(C_{t-2})$	0.4157	3.30*	0.3918	2.01*
$\log(C_{t-4})$	-0.1944	-2.04*	0.0718	0.39
$\log(\text{RPDI}_{t-1})$	0.2136	2.64*	0.2814	1.92
$\log(\text{RPDI}_{t-2})$	-0.2142	-2.39*	-0.0804	-0.50
$\log(\text{RPDI}_{t-4})$	0.0673	0.86	0.0393	0.29
RLB_{t-1}	-0.0016	-1.55	-0.0024	-1.26
RLB_{t-2}	-0.0005	-0.46	-0.0021	-1.01
RLB_{t-4}	0.0005	0.66	0.00003	0.02
$\log(\text{PC}_{t-1}/\text{PC}_{t-2})$ + 0.0057	-0.0777	-0.50	-0.2167	-0.83
$\log(\text{PC}_{t-2}/\text{PC}_{t-3})$ + 0.0057	0.0494	0.32	0.0668	0.24
$\log(\text{PC}_{t-1}/\text{PC}_{t-2})$ + 0.0057	-0.0226	-0.18	-0.1523	-0.72
\mathbf{K}	-0.1244	-1.13	-0.7156	-1.44

* statistically significant at 5% level

example real personal disposable income is no longer statistically significant and although some differences between the statistically insignificant coefficients persists, the important point is that we remain unable to reject the hypothesis that they are zero.

Of course, these comparisons with Keating's estimates cannot resolve the question whether the reasons for the difference in my estimates are revisions to the data or changes in behaviour, although some evidence for the latter can be drawn from Table 2.2. Table 2.2 does however show quite dramatically the differences that emerge when the same set of variables are regressed over *different* sample periods within the same data set. In particular, four independent variables that were not significant for data up to 1980 become significant with the additional data. From having only one statistically significant variable for data up to 1980, the regression equation based on data up to 1992 has five significant variables. The differences between columns 2 and 4 illustrate quite clearly that the quantitative relationships between economic variables change

over time. This is perhaps unsurprising. Nevertheless it is important, as it is the reason why economic forecasting is never simply a matter of extrapolation. Thus the changing coefficients of Table 2.2, although not fatal to the use of econometric equations for forecasting, should be used with caution: is the equation still the 'right' equation?

It is possible to argue that macro-econometric models are used not simply for forecasting but to test economic theory. Indeed some economists believe that placing too much emphasis on forecasting is detrimental to macroeconomic modelling because it diverts effort from the more important task of using the models to test and develop economic theory. (See, e.g., Wren-Lewis 1992.) From this point of view it is possible to argue the actual parameter values are not so important. What matters is whether or not a particular coefficient turns out to be significant. Of course economic forecasters who identify themselves as economists want their equations to be defensible on both grounds, and the next section outlines how economic theory can be used to refine the general consumption function shown in Table 2.1.

Econometrics and the experimenter's regress

This section shows how economic theory can be used to interpret econometric evidence and shape the final form of an econometric equation. It therefore follows the procedures outlined by Keating as he refined his first, general regression equation (Equation 2.1) into a more compact and theoretically coherent consumption function which could be included in his macro-econometric forecasting model. The section works through his analysis and argues that it illustrates a well-known sociological phenomenon known as the experimenter's regress, which arises because an experiment can only be said to be successful when it yields the correct results.²⁰ However, the correct results can only be obtained from competent experiments. When the appropriate answer is known then the regress disappears, since it is obvious when a mistake has been made. In contrast, when what counts as the correct answer is unknown, deciding whether or not a mistake has been made is much more complicated. In most of what follows the econometrics is being used to test a standard economic theory and so the 'correct' answer is well known. However, it is argued that the potential for change inherent in social systems means that closure is especially problematic in econometric modelling, and even within the most conventional of economic theories room for doubt can creep in.

Testing theories with restricted regressions

The basic methodology for hypothesis testing in econometrics is to specify some sort of restriction on the regression equation, re-estimate it and see if the coefficients have the appropriate test statistics. Typically one would use the theory to specify an equation in such a way as to test the null hypothesis

that a particular variable has no effect. If the coefficient turns out to be statistically significant then the null hypothesis is rejected and the alternative, that the independent variable does have an effect on the dependent variable, is accepted.

The first restriction imposed by Keating is to drop all the fourth lag variables on the grounds that they are not statistically significant.²¹ Interestingly, however, he retains the first and second lags of both RPDI and the inflation term, as well as the second lag of the interest rate term (RLB) despite the fact that these are also statistically insignificant at the 5 per cent level. No explanation is offered for this decision, although it seems likely that the central importance of prices, income and interest rates in economic theory means that, for the model to be an 'economic' one, these variables have to remain even if they do no apparent work. Note that for my data set the first and second lag of RPDI are statistically significant and their inclusion is not therefore problematic. It seems that prior theoretical commitments to interest rates and inflation mean that they must be included in the model, despite the fact that over twenty years worth of economic data say they should not, although this is not articulated by Keating.²²

The abbreviated equation is then re-estimated and Keating's analysis moves on to consider the coefficient on the second lag of inflation rate, which is shown in Table 2.3. Because this coefficient is both small and statistically insignificant, the second lag of the inflation term is dropped from the equation. This is also the case for my data set and so I, like Keating, have good grounds for also excluding this variable from the equation.

After removing the second lag of the inflation rate, the consumption function has as explanatory variables the first and second lags of consumer expenditure, RPDI and interest rates, as well as the first lag of the inflation rate plus a constant term. The next stage of specification testing involves rearranging the consumption function. First the dependent variable is changed from the level of consumer expenditure to its first difference (i.e. $(\log(C) - \log(C_{t-1}))$). The explanatory (or independent) variables remain unaltered, so when the equation is re-estimated the coefficients should remain the same. The only exception to this should be the coefficient on lagged consumer expenditure, which should be exactly one less than it was before. The coefficients are shown in Table 2.4 and it can be seen that this is indeed the case.

Table 2.3 Coefficient on price level in first restricted regression

<i>Variable</i>	<i>Keating coefficient</i>	<i>Keating t statistic</i>	<i>Evans coefficient</i>	<i>Evans t statistic</i>
$\log(PC_{t-2}/ PC_{t-3})$ + 0.0057	0.0111	-0.05	-0.0013	-0.01

Table 2.4 Rearranged regression equation (Evans's estimates)

<i>Variable</i>	<i>Dependent variable</i> <i>= log(C)</i>	<i>t statistic</i>	<i>Dependent variable</i> <i>= log(C_t)</i> <i>- log(C_{t-1})</i>	<i>t statistic</i>
$\log(C_{t-1})$	0.7249	7.07*	-0.2751	-2.69*
$\log(C_{t-2})$	0.2647	2.62*	0.2647	2.62*
$\log(\text{RPDI}_{t-1})$	0.2324	3.07*	0.2324	3.07*
$\log(\text{RPDI}_{t-2})$	-0.2062	-2.60*	-0.2062	-2.60*
RLB_{t-1}	-0.0013	-1.40	-0.0014	-1.40
RLB_{t-2}	-0.0005	-0.46	-0.0005	-0.46
$\log(\text{PC}_{t-1}/\text{PC}_{t-2})$ + 0.0057	-0.0314	-0.29	-0.0314	-0.29
K	-0.1515	-1.64	-0.1515	-1.64

* statistically significant at 5% level

The next rearrangement is to replace the second lag of consumer expenditure by a difference (i.e. the first lag minus the second lag), and the lagged values of RPDI by a difference and a 'new' variable formed by subtracting income lagged once from expenditure, also lagged once. However, as this rearrangement of C and RPDI uses only the previously existing independent variables, the coefficients on the interest rate and inflation terms should be unchanged; the others alter so as to offset the effects of the rearrangement. The results of this regression are shown in Table 2.5.

The purpose of this rearrangement is to make it possible to test the long-run form of the equation. The long-run form of the consumption function should contain all the variables shown in Table 2.5, apart from the lagged level of consumer expenditure. If its coefficient is not significant, then this variable can be dropped from future regressions and the equation will have a form that is consistent with economic theory.

As can be seen from Table 2.5, the *t* statistic for the lagged value of consumer expenditure is 1.83, which is not significant at the 5 per cent level. The variable is therefore excluded and the re-estimated equation is shown in Table 2.6.

In the long run the growth rates of real consumer spending and income, and the levels of interest rates and inflation, are all by assumption constants. Keating further assumes that spending and incomes rise at the same rate, so the constants are, respectively, *k*, *r* and *p*. From Table 2.6, this gives the long-run consumption function shown in Equation 2.2.

Table 2.5 Regression with further rearrangement

<i>Variable</i>	<i>Dependent variable</i> $= \log(C_t) - \log(C_{t-1})$	<i>t statistic</i>
$\log(C_{t-1})$	0.0157	1.83
$\log(C_{t-1}) - \log(C_{t-2})$	-0.2647	-2.62*
$\log(\text{RPDI}_{t-1}) - \log(\text{RPDI}_{t-2})$	0.2062	2.60*
$\log(C_{t-1}) - \log(\text{RPDI}_{t-1})$	-0.0261	-0.39
RLB_{t-1}	-0.0014	-1.40
RLB_{t-2}	-0.0005	-0.46
$\log(\text{PC}_{t-1}/\text{PC}_{t-2}) + 0.0057$	-0.0314	-0.29
K	-0.1515	-1.64

* statistically significant at 5% level

Table 2.6 Long-run form of equation

<i>Variable</i>	<i>Dependent variable</i> $= \log(C_t) - \log(C_{t-1})$	<i>t statistic</i>
$\log(C_{t-1}) - \log(C_{t-2})$	-0.2711	-2.66*
$\log(\text{RPDI}_{t-1}) - \log(\text{RPDI}_{t-2})$	0.2430	3.14*
$\log(C_{t-1}) - \log(\text{RPDI}_{t-1})$	0.0207	0.33
RLB_{t-1}	-0.0009	-0.99
RLB_{t-2}	0.0002	0.16
$\log(\text{PC}_{t-1}/\text{PC}_{t-2}) + 0.0057$	-0.0904	-0.86
K	0.0177	2.91*

* statistically significant at 5% level

Equation 2.2 Long-run consumption function

$$k = (-0.271 + 0.243)k + 0.021(\log(C) - \log(\text{RPDI})) \\ + (-0.0009 + 0.0015)r - 0.091p + 0.018$$

Since k , r and p are all constants, the equation shows that there is a constant difference between $\log(C)$ and $\log(\text{RPDI})$. In other words there is a constant average propensity to consume. To reflect the standard economic theory that proportionally more savings are needed at higher rates of inflation, in order to maintain the real value of savings, this constant should be low at high values of p . For this condition to be satisfied, the sign on $[\log(C) - \log(\text{RPDI})]$ must be the same as that on p .

This can be seen by rearranging the function to express the ratio of expenditure to income. Equation 2.3 shows the results based on my parameter estimates, and Equation 2.4 the same equation as presented by Keating and based on his parameter estimates.

Equation 2.3 Average propensity to consume, derived from Evans's estimates

$$C/\text{RPDI} = \exp.[(1.028k - 0.006r + 0.091p + 0.018)/0.021]$$

Equation 2.4 Average propensity to consume, derived from Keating's estimates

$$C/\text{RPDI} = \exp.[-(0.163k - 0.00187r - 0.179p + 0.00960)/0.148]$$

It can be seen that as p increases, the right-hand side of Equation 2.3 also increases, as the sign on the coefficient is positive. This implies that consumers do not raise savings as rates of inflation rise, but actually save less when inflation is higher. In contrast, in Equation 2.4 the coefficient on p is negative, which is what the theory would suggest. It is clear that there is something wrong with my analysis; my econometrics have produced an incorrect result and would probably be discounted by the majority of economists. An economist would probably try to test the 'robustness' of this anomalous result by changing the variables or the sample period.

I did exactly this. Initially I restricted the sample period to the one used by Keating, but I found that, although both the coefficients had changed sign, they were still different. I therefore experimented with a few different sample periods, and found that extending the sample period by four observations (i.e. to run from 1956Q4 to 1981Q4) was sufficient to bring the estimated equation back in line with the conventional theory. Of course, it should be noted that the coefficient on the inflation rate was never statistically significant in either Keating's analysis nor any of my own regressions, so all this could well be regarded as pointless. Strictly speaking, what Keating's econometrics and my own actually tell us is that, at the levels of statistical significance conventionally

used in the social science, the coefficients on the inflation terms are indistinguishable from zero.

However, it is now clear why Keating chose his particular strategy. The aim of the exercise was to produce a compact and theoretically coherent consumption function, and the theory defined in advance the form of this consumption function. Certain statistically insignificant variables were also economically insignificant and could be discarded. On the other hand, other equally statistically insignificant variables were economically very salient indeed. This latter group of variables had to be retained in order that the equation could be presented in the appropriate form. The reasons for ignoring certain t statistics and respecting others are now clear.

Testing further restrictions and hypotheses

Apart from testing that the long-run form of the equation matches the theory, Keating tests a further hypothesis. He argues that, in addition to maintaining the real value of asset holdings, consumers will tend to increase these asset holdings to allow for real growth in income. The reason is that if they failed to do so, over time their holdings of financial assets would become either a very high or very low proportion of their income. On this assumption, the average propensity to consume equals a constant times the sum of the inflation rate and the trend real growth rate. This implies some further restrictions on the regression equation, and these are tested in two stages.

In the first stage, the steady state growth rate (2.3 per cent per annum, or 0.57 per cent per quarter) is subtracted from the dependent variable and from the right-hand side consumption and income growth variables. The coefficients for this equation are shown in Table 2.7. The first stage of the test is to see

Table 2.7 Regression with steady state growth

<i>Variable</i>	<i>Dependent variable</i> <i>= log(C_t) - log(C_{t-1})</i> <i>- 0.0057</i>	<i>t statistic</i>
$\log(C_{t-1}) - \log(C_{t-2}) - 0.0057$	-0.2711	-2.66*
$\log(\text{RPDI}_{t-1}) - \log(\text{RPDI}_{t-2}) - 0.0057$	0.2430	3.14*
$\log(C_{t-1}) - \log(\text{RPDI}_{t-1})$	0.0207	0.33
RLB_{t-1}	-0.0009	-0.99
RLB_{t-2}	0.0002	0.16
$\log(\text{PC}_{t-1}/\text{PC}_{t-2}) + 0.0057$	-0.0904	-0.86
κ	0.0118	2.00*

* statistically significant at 5% level

whether or not the constant term is statistically significant. If Keating's hypothesis is correct, then the constant term should not be significant.²³ However, from Table 2.7 it can be seen that the constant term is statistically significant at the 5 per cent level ($t = 2.00$).

In this case, the idea of the experimenter's regress is more pertinent, as it is not clear what counts as the correct answer. The implication of the statistically significant constant term is that consumers are no longer raising their asset holdings to allow for growth in real income. Arguably this is what happened during the 1980s when consumers as a group greatly increased their financial liabilities with the result that their asset holdings fell relative to income. One of the more distinctive features of economic behaviour in the 1980s was the decline in the savings ratio (which fell to about 5 per cent in 1988).²⁴ Taken together, these observations suggest that consumer behaviour may well have changed during the 1980s. It is therefore at least possible that the statistical significance of the constant term is reflecting this change.²⁵ Such a behavioural change might be either permanent or temporary. In either case a competent economic evaluation might reasonably be expected to pick it up. In other words, this situation is quite different to the long-run case discussed above. Replicating Keating's result would not prove correctness, nor would failure to replicate it suggest incompetence. Interestingly, even at the time Keating was writing the data were not entirely consistent with the hypothesis.

As mentioned above, testing the hypothesis involved two stages. The first was to confirm that the constant term in the regression equation that includes the steady state growth rate was not statistically significant. The second stage was to re-estimate the equation without the constant term, and with the second lag of interest rates replaced with a difference. If the hypothesis were correct, the coefficient on the level of interest rates should be statistically insignificant.

The coefficients from Keating's equation and my own are shown in Table 2.8. As can be seen, the hypothesis that the coefficient on the level of interest rates is zero is rejected at the 5 per cent level for Keating's data set. It should therefore be retained in the equation. As Keating points out, however, a t statistic of 2.48 is not statistically significant at the 1 per cent level, so the interest rate variable can be dropped from the equation. In other words, by selectively invoking more stringent criteria, the hypothesis derived from economic theory is supported.

For my data the hypothesis is rejected at the 5 per cent level, though the meaning of this is unclear as in order to maintain comparability with Keating I dropped the constant term even though it was significant.

One alternative at this point is to declare oneself unconvinced by the story about changing consumers' behaviour. Another is to manipulate the data in an attempt to provide better support for the hypothesis. For example, it would be possible to argue that the t statistic for the constant in Table 2.7 was barely significant at the 5 per cent level ($t = 2.00$) and was certainly not significant at

Table 2.8 Regression with interest rate difference

<i>Variable</i>	<i>Keating coefficient</i>	<i>Keating t statistic</i>	<i>Evans coefficient</i>	<i>Evans t statistic</i>
$\log(C_{t-1}) - \log(C_{t-2})$ - 0.0057	-0.215	-2.21*	-0.1914	-2.01*
$\log(\text{RPDI}_{t-1})$ - $\log(\text{RPDI}_{t-2})$ - 0.0057	0.0547	0.67	0.2101	2.74*
$\log(C_{t-1})$ - $\log(\text{RPDI}_{t-1})$	-0.182	-3.42*	-0.0724	-1.69
RLB_{t-1}	0.00186	-2.48*	-0.0006	-1.38
$\text{RLB}_{t-1} - \text{RLB}_{t-2}$	-0.00183	-1.44	-0.0002	-0.23
$\log(\text{PC}_{t-1}/\text{PC}_{t-2})$ + 0.0057	-0.211	-1.17	-0.0907	-0.85

* statistically significant at 5% level

the 1 per cent level. Therefore the constant should be excluded on the grounds that it failed to pass the stricter significance test. From Table 2.8 it could be argued that the coefficient on the interest rate level was also insignificant and that this too should be excluded. Such tactics could make the data appear consistent with the hypothesis that consumers raise their asset holdings in line with income growth.

This then is how the experimenter's regress can manifest itself in econometrics: valid results are obtained only from appropriate econometrics, but the authority of the econometrics is only established by producing the correct results. In order to know how to interpret Tables 2.7 and 2.8 it is necessary to know in advance the 'correct' answer. Should it be accepted that the constant term is statistically significant, or should more stringent tests be used? Have assets become a low proportion of consumers' income or not? The answer to each question depends on the answer to the other.²⁶

The implication of this is that one needs a reasonably clear idea about the workings of the economy in order to specify a model in the first place. Indeed, it seems that one needs this set of theoretical commitments largely because the econometric evidence is so weak. A researcher who believed in the power of statistics, as opposed to the power of economic theory, would probably produce a very different sort of model (cf. Sims 1980).

The failure of econometrics to support economic theory might seem surprising, as econometric textbooks often refer to the ease with which statistically significant regression equations can be produced. For example, in

the documentation to his econometrics software package *PC-Give*, David Hendry writes:

Even at an elementary level a critical appreciation must be engendered, especially for time series econometrics . . . Challenge the class to select any two variables . . . and you guarantee to produce a correlation of over 0.95 between the two variables after at most one transformation on each variable . . . You would be unlucky to lose this challenge.

(Hendry 1989)

Hendry implies that there is such a profusion of statistically significant relationships that finding support for a theory is all too easy. My own experience of repeating Keating's analysis suggests that this overstates the case. In fact, from my account, it looks as though theory is needed not to discriminate between the important and the statistically significant, but rescue the important from the statistically insignificant.

Of course it might be argued that Keating is unusual in the strength of his prior commitments, and that his example is not typical of economists or econometricians.²⁷ This is not the intention here. As was emphasized earlier, Keating's book is being used as a guide to good practice. The status of Keating's econometrics is therefore crucial to the credibility of the argument. It should be stressed that there is nothing unusual about Keating's approach to econometric modelling. On the contrary, Keating's example is remarkably consistent with something I discovered very early on in my study of macro-econometric modelling. In my very first interview with an econometrician I learned that it is considered quite normal to retain statistically insignificant variables if they are theoretically interesting:

Hudson: The model builder has to have a theory about what goes into an equation. He doesn't just look for t statistics and significance in an equation because that tends to lead to bad forecasts. If you've got something which is statistically significant and makes theoretical sense, then you are unlikely to be picking up chance correlations.

Evans: So in some cases you would keep in [explanatory variables] that perhaps weren't as statistically significant as you would like.

Hudson: Often.

Evans: You would say: I think it should be in there anyway, so I'm going to leave it in even though the t statistics say I should drop it.

Hudson: Yes.

(Author's interview, 15 January 1993)

Because of the ambiguity of within-sample econometric tests, out-of-sample forecasting is also important to economic modellers. The intuition is that out-

of-sample testing will quickly expose any spurious relationships not eliminated during econometric testing.

Forecast tests

Before this process of testing is discussed in more detail, it will be useful to outline a typology of possible forecast tests and to introduce some terminology.

A typology of forecasting

Economic models can be made to forecast in a variety of ways. These methods can be ranked in a hierarchy, with each representing a more difficult challenge than the one that came before. The range of possible tests is illustrated in Figure 2.3.

The first simulation test that can be performed is an *ex post* simulation test in which the model or equation is made to forecast the endogenous (left-hand side or dependent) variables for some portion of the sample period. *Ex post* simulations can be either static or dynamic. In a static simulation, the lagged endogenous variables take their actual values, while in a dynamic simulation, the values generated by the model are used.

Next in the hierarchy is *ex post* forecasting, in which forecasts are generated for the period between the end of the sample period and the present day. Like *ex post* simulations, an *ex post* forecast may be either a static (or one-step ahead) forecast or a dynamic forecast. *Ex post* forecasts are more difficult than simulations because information about any changes in the economic environment after the sample period cannot be reflected in the coefficients.

The final and most difficult forecasting is *ex ante* forecasting, in which forecasts are generated for the future values of the endogenous variables. Unlike dynamic *ex post* forecasting, where at least the values of the exogenous variables are known from the historical data, *ex ante* forecasting requires the forecaster to predict the future values of all these variables before the forecast can be produced. *Ex ante* forecasting therefore takes both the model and the modeller into the unknown.

Testing the consumption function

In Chapter Four of his book Keating briefly discusses the out-of-sample testing of the consumption function he has derived. By comparing the forecast ten quarters ahead with the known outturn, he is able to compute a test statistic (he does not define which) according to which the forecast values are close enough to the actual values for the errors to be considered random. However, because the test is based on a comparison of within-sample errors to out-of-sample errors, it is possible for an equation which fits the historical data badly to pass the test, not because it forecasts well, but because the error terms in

HOW ECONOMIC MODELS ARE ESTIMATED

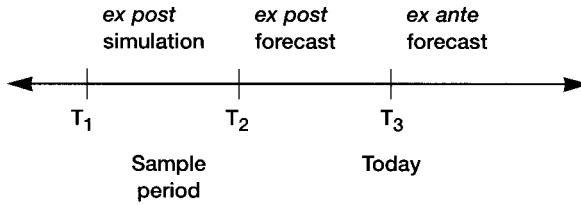


Figure 2.3 A typology of forecasts

Source: adapted from Hudson and Dymitou-Jensen, *Modelling a Developing Country: A Case Study of Cyprus*, p. 83.

the past have also been large. Keating checks for this possibility by examining the standard error of the estimated equation, which provides an estimate of the square root of the variance of the error terms. In Keating's case this figure is 0.01177 which, because the dependent variable is measured in natural logarithms, implies about 1.2 per cent. As Keating admits:

There is no formal measure of whether this is a large or small standard error, but a judgement can be made by comparing it with the average change in consumers' expenditure over the estimation period. This suggests that the standard error is neither exceptionally large nor very small, implying that the forecast test is a reasonably good guide to the forecasting ability of the equation.

(Keating 1985: 83)

In the case of my own equation it was not possible to compute the test statistic, but the comparable standard error figure to the one quoted by Keating is 0.01250, which seems reasonably similar to Keating's estimate. It was possible to carry out some forecast tests. In what follows, the out-of-sample forecasting performance of the consumption function estimated by Keating is compared with my own version of the same function, and a third equation (shown in Table 2.9) is derived using a simple statistical rule.²⁸

Table 2.9 Equation derived by application of statistical rule

<i>Variable</i>	<i>Dependent variable</i> <i>= log(C_t)</i>	<i>t statistic</i>
log(C _{t-1})	0.698102447	6.4878*
log(RPDI _{t-1})	0.301617075	2.8206*
RLB _{t-1}	-0.002760012	-3.9310*

* statistically significant at 5% level

The purpose of including this third equation was to examine the ‘value added’ by the economic theory. If economic theory does explain how consumers behave, it seems reasonable to expect that an equation based on this theory will prove a better forecasting model than one derived from an atheoretical data-fitting methodology, especially over long periods.

Figure 2.4 shows fitted values (static *ex post* simulations) and dynamic forecasts for each of the three equations, all of which were estimated on data up to the end of 1980. The lines labelled ‘Keating’ and ‘Evans’ show the *ex post* simulations and forecasts for the consumer expenditure equation, calculated using the specification procedures outlined previously. The line labelled ‘Rule’ shows *ex post* simulations and forecasts calculated using the equation estimated by the rule: exclude the least significant variable and re-estimate.

From Figure 2.4 it appears that the specification procedure which used economic theory as a guide has had the following effects. First, as might have been hoped, it has produced an equation that is robust with regard to the vintage of the data set and the measures of interest rates used. Both equations fit the past data very well, and the out-of-sample forecasting performance appears to be similar. Less positively, both equations seem to forecast a more or less constant rate of increase in consumers’ expenditure throughout the 1980s, missing both the downturn at the beginning and the boom in the middle. However, by the end of the decade the downturn caused by the recession has brought the actual values back into line with those predicted by the model.

The performance of the equation derived from the data-fitting rule is rather different from that of the equations derived from economic theory. The mechanically-derived equation correctly forecasts the downturn and is closer to the actual values of consumer expenditure during the first few years of the decade. Thereafter it too under-predicts the growth in consumer expenditure but seems more responsive to changes in its other independent variables (real personal disposable income and short-term interest rates) and finishes the decade with the highest forecasts. The impression gained from Figure 2.4 is that the rule-based model tracks the upturn in consumer expenditure more closely and does a better forecasting job than the equations derived from economic theory.

However, Figure 2.4 is not necessarily the best way of examining the relative merits of the three equations as forecasting models. It is more useful to look at the actual forecasting errors. These are shown in Figure 2.5 and tell a similar story to the one outlined in the previous paragraphs. The theory-based equations derived by Keating and myself make a similar pattern of mistakes, with the over-predictions at the beginning of the decade shown as positive errors and the under-predictions of the boom years as increasingly large negative errors. The recession at the end brings forecast and reality back together. In contrast, the forecasts using the rule-derived equation are close to the actual outcome at the beginning of the decade, and for short-term forecasting, at least, it is clearly superior to either of the equations derived from economic theory.

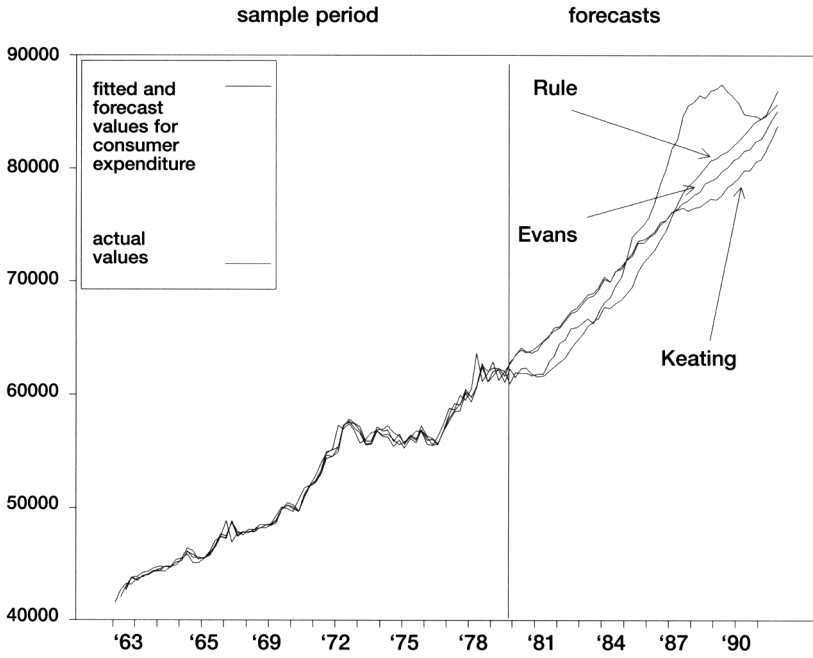


Figure 2.4 Fitted and forecast values for consumer expenditure

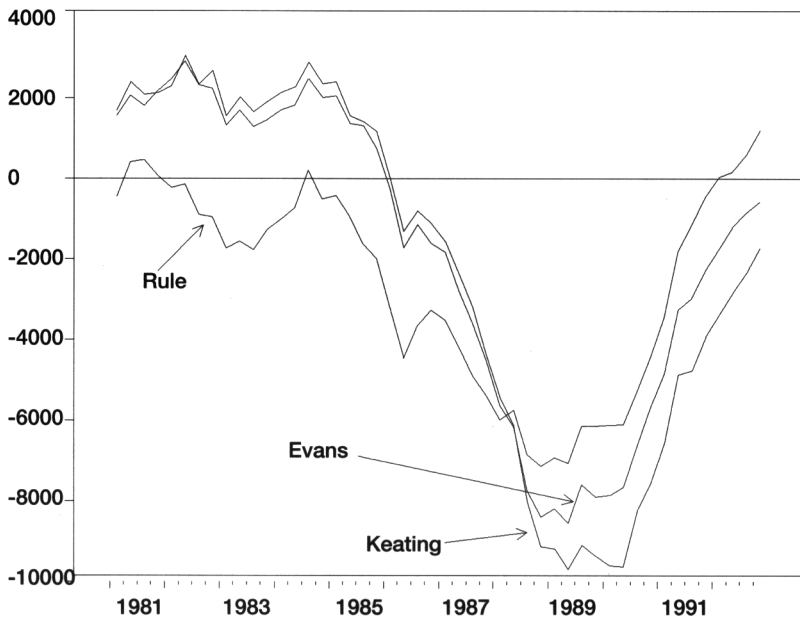


Figure 2.5 Forecast errors for equations based on theory and data fitting

Again, this is not necessarily an unusual phenomenon. For example, Wallis and Whitley (1991) found that over short-term forecasting horizons vector autoregressive models performed at least as well as structural econometric models. It was only over longer-term forecasting horizons that the structural models that incorporated economic theory were unambiguously superior.

However, in the case of the forecasts shown in Figures 2.4 and 2.5, this does not appear to be the case. In fact, from Figure 2.5 it looks as if the atheoretical data-fitting model was superior throughout almost the entire forecasting period. This intuition can be tested more formally, and a common way is to calculate the root mean square error (RMSE) of the different functions throughout the forecast period.²⁹ The RMSEs for each of the three consumption functions are shown in Figure 2.6.

From Figure 2.6 it is apparent that, with the exception of the middle portion of the graph (approximately the period from 1987 to 1988) the consumption functions based on economic theory forecast less well than the atheoretical version. This difference is particularly marked for the first five years or so of the forecast period. Thereafter the RMSE increases quite dramatically for all three functions, but from about 1989 onwards the forecasts produced by the data-fitting method are superior. Indeed it seems that using economic theory to inform the specification of the consumption function has actually produced a worse result, and it is this aspect which is the most troubling. It seems from Figure 2.6 that, for the two- to three-year time frame in which most forecasters

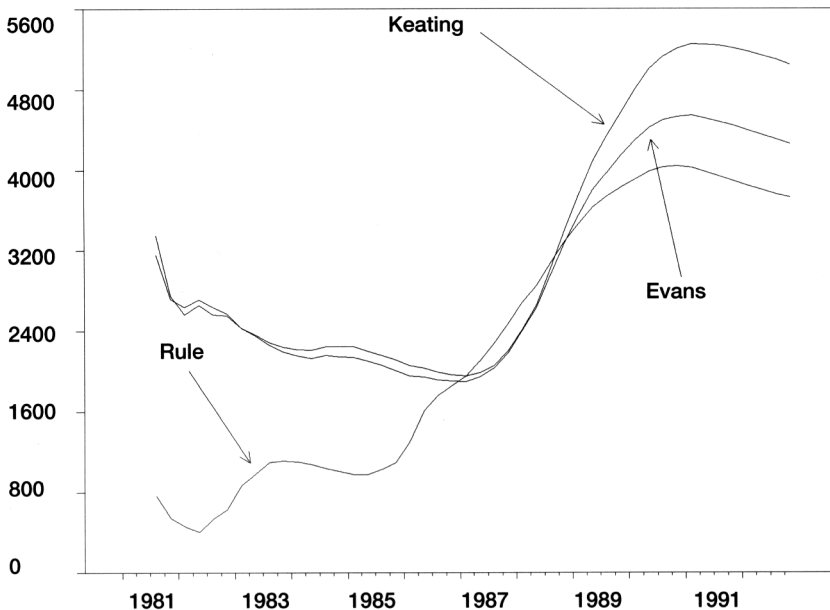


Figure 2.6 Root mean square errors for equations based on theory and data fitting

are really interested, economic theory is detrimental to the forecasting ability of the model.

As has been remarked earlier, it can be argued that there are other reasons for wanting economic theory in one's model: for example, tractable long-run properties and developing a theoretical understanding of the economy. However, a sceptic would surely maintain that if these theories are neither supported by the data nor useful for forecasting, their relevance to practical matters must surely be in doubt.

Conclusion

This chapter has discussed the estimation of a single equation and shown how economic theory, econometric tests and the economist's professional judgement are interwoven in the specification of an equation. From the very beginning of the analysis, when statistically insignificant variables were retained and apparently equally insignificant ones excluded, it was the economist who made these judgements. When testing restrictions, the crucial decision was the level of statistical significance that was appropriate. The purpose of working through the estimation of a single equation has been to highlight the various points at which decisions must be made, and the ways in which these decisions can be negotiated.

In particular, the discussion has tried to draw out the questions that economists must ask of themselves and of their data if they are to produce economic equations and models that their peers will find credible. It is not claimed that I possess this expertise myself, nor is it claimed that, by reading this chapter, the reader will have acquired it. Rather, the claim is that in order to be recognized as competent and professional, an economist must be able to use the reference points provided by economic theory and empirical econometrics to make these decisions correctly. The sociological point is that this sort of knowledge and expertise can only be acquired by participating in the community of economics.

But this was only one equation, using just four variables. A typical macroeconomic modeller may use several hundred variables and estimate several dozen equations, each of which will need the same combination of econometrics, economics and conviction as the consumption function detailed above. This chapter has shown that the foundations of macro-econometric modelling are set not just in the (relative) certainty of econometric procedures and statistical theory, but also in the social practices and shared convictions of economists. However, this foundational role is not the only way in which the social enters into macro-econometrics; it is merely the beginning.

EXTRAPOLATIONS AND EXPERTISE

How economic forecasts are made

Introduction

The previous chapter examined the specification and estimation of a particular version of the consumption function, one of the many econometric equations in a typical macroeconomic model. The conclusion drawn from the interplay of theory, data and judgement analysed was that the foundations of an econometric model are sunk not just in the analytic insights of economic theory and the observed regularities of economic data, but also in the shared standards and conventions of economists. In this chapter this argument is extended to include not just the specification of econometric equations but also their use in econometric forecasting. The chapter thus makes one major claim: economic forecasts are not extrapolations of the past into the future, but considered judgements about what is most likely to happen.

These points are illustrated by demonstrating how a simple macro-econometric model can be used to produce an economic forecast. Of course, if, as is claimed, expertise and judgement do matter, then the plausibility of the forecast is crucially linked to my own economic expertise. I have tried to approximate the training and experience needed to produce independent economic forecasts by deciding in advance that my forecast will be the same as the average of the forecasts made by the government's Panel of Independent Forecasters at their first meeting in February 1993.¹ However, the implication of the analysis is that by taking different judgements about the economic prospects I could have used the same model to produce very different economic forecasts. The fact that I have reproduced the average therefore reflects a more or less arbitrary decision on my part, made possible in part because, as a sociologist, I have no commitment to any particular economic theory or model.

The chapter is therefore not simply about a particular economic forecast. However, neither is it a denunciation of macro-econometric modelling in general. Rather, it is about the types of expertise that are needed to make plausible economic forecasts. As such it provides a critical (in the academic sense) re-evaluation of the basis of economic modellers' claims to knowledge

in the light of a more sociological understanding of knowledge and expertise. In particular, if we understand economic forecasts as products of considered judgement, and not of esoteric econometric virtuosity or the inevitable consequence of the application of immutable laws and forces, then attitudes to economic forecasting, especially as it impinges on the making of economic policy, will need to be re-examined. For example, with the role of judgement now foregrounded, it seems important to ask whether macroeconomic forecasters, with their very particular and distinctive perspective on the economy, are the only people qualified to make these judgements.

The answer is more complex than it might seem. On the one hand, it is obvious that economic modellers and forecasters are particularly knowledgeable about the workings of the economy, and that this knowledge and their modelling activity reinforce each other. Constructing, maintaining and updating macroeconomic models is a full-time job which compels a small group of people continuously to monitor, analyse and talk about the economy, and their expertise is a natural result. However, this does not imply that they are the *only* experts. Indeed, precisely because of the unusual way in which economic forecasters acquire their expertise, mediated through the twin filters of economic theory and econometric models, it seems reasonable to ask whether, and to what extent, this expertise is (necessarily) partial and incomplete.

The argument of this chapter begins by showing how an economic model is 'completed' by the judgement of the economist, and how the relatively private and technical achievement of the model is turned into a publicly available and (hopefully) plausible economic forecast. In the following section the economic model used to produce the forecast is outlined. Next the question how the initial extrapolation of this model can be transformed into a more plausible economic forecast is examined in some detail, paying particular attention to the decisions that have to be made about the interpretation of ambiguous economic data. The chapter concludes by suggesting that a significant aspect of economic forecasting is the judgement made by economists on events that are not well captured by the model.

A simple economic model

This section outlines the basic properties of the macroeconomic model that will be used to produce the economic forecast. The model is based on the one outlined by Keating (1985) referred to previously, and includes the consumption function described in Chapter Two. (A full listing of all the equations and details of the data sources used are given in Appendix A.)

The basics of economic modelling

Perhaps the easiest way to appreciate why a multi-equation economic model is needed is to consider the alternatives. According to a popular textbook on

econometrics (Pindyck and Rubinfeld 1991) there are three basic ways in which econometric modelling and forecasting can be done. The first and most simple way to predict the future path of an economic data series is by extrapolating from its own past values. The advantage of such a technique is that it is relatively quick and easy to do. Extrapolation of this sort is, therefore, a useful way of generating short-term forecasts for time-series data. This technique is not much use for detailed policy analysis, however, as there are no explanatory variables other than the variable being forecast. It is therefore best suited to applications in which theory is either undeveloped or simply not needed.

The second technique is to use a single regression equation but include some additional independent variables. Thus, to take a simple example, in the case of consumer expenditure (C), we may hypothesise that this is a function of income (Y), and test this by estimating the following regression equation:

$$C = K + \alpha Y$$

We could of course make things more complex, as was done in Chapter Two, by introducing additional independent variables, such as the price level or the rate of interest, and by including past values of any or all of these variables. This single-equation approach is perfectly satisfactory when we are only interested in one variable, and it can be used to test and develop quite complex theories of economic behaviour. However, economic policy is usually concerned with the economy as a whole, and therefore with a range of variables simultaneously (for example, economic growth, inflation, unemployment, public borrowing, balance of trade and interest rates). Although separate equations could be used for each policy variable, this makes it difficult to model their interdependence, that is, the way in which changes in one variable are related to, and influenced by, changes in the others.

As a result, the third and most common approach amongst macro-econometric modellers is to use multi- or simultaneous equation models which can represent both the individual elements of economic activity and the interrelationships between them within the economy as a whole. Thus, a simple model of a closed economy (i.e. one with a government (G) but no international trading links) might include an equation for consumer expenditure (C), one for investment (I) and an identity defining national income (Y). An example of such a model is shown as Model 3.1.

As can be seen, Model 3.1 defines income (Y) as being the sum of its parts. Consumption in the current time period (C) is related to the level of income (Y) and also to its previous values (C_{t-1}). Investment (I) is related to the change in income (Y - Y_{t-1}) and also to the rate of interest (R_t). Note that in each case the level of income determines C and I and is determined by them through the GDP identity. By solving the three equations together, and ensuring that the same values of Y, C and I are used in each equation, the economic modeller

Model 3.1 A simple model of a closed economy

<i>GDP Identity</i>	$Y_t = C_t + I_t + G_t$
<i>Consumption</i>	$C_t = \alpha_1 Y_t + \alpha_2 C_{t-1} + \varepsilon_{\alpha}$
<i>Investment</i>	$I_t = \beta_1 (Y_t - Y_{t-1}) + \beta_2 R_t + \varepsilon_{\beta}$

<i>Symbols</i>	<i>Meanings</i>
Y	national income or GDP
C	consumer expenditure
I	investment
G	government expenditure (not determined by the model)
R	interest rates (not determined by the model)
ε	error term

can produce a forecast which recognises both the determinants of the individual data series and their interactions.

Although the model in Box 3.1 is very simple, it is the basis of many larger macroeconomic models. By collecting more data, using powerful computers and sophisticated regression techniques, it would be possible to disaggregate the equations, expand them to include more independent variables and combine them into a complex model of the economy. For example, a macro-econometric model used by members of the Panel of Independent Forecasters used about five hundred variables and approximately one hundred estimated equations, plus a considerable number of accounting identities.²

The model to model all models?

This section explains, as non-technically as possible, the economic model that was used to produce the economic forecasts described below. The principal equations that define the model are shown in Box 3.2 and are described in detail in Keating (1985: 50-68). Briefly, the equations in Box 3.2 show that the demand for money, the first equation, is related to the level of income, defined now as the expenditure-based measure of gross domestic product (GDPE) and interest rates (RLB). The demand for money rises as GDPE increases and people need more cash to finance the increased number of transactions, and falls as interest rates, and hence the cost of holding cash, rise (i.e. the LM curve). The second equation, which is the IS curve, shows that output (i.e. GDPE) will tend to fall when real interest rates are high and when domestic prices (PC) are high relative to world prices (WWPI) - that is, when imports are relatively cheap. The third equation relates domestic prices

Model 3.2 A simple linear-in-logs model of an open economy

$$LM \text{ curve} \quad \log(MO_t/PC_t) = \alpha_1 \log(GDPE_t) - \alpha_2 RLB_t + \alpha_3 + \epsilon_{\alpha t}$$

$$IS \text{ curve} \quad \log(GDPE_t) = \beta_1[(RLB_t/400) - \log(PC_t/PC_{t-1})] \\ - \beta_2[\log(PC_t) - (\log(WWPI_t) - \log(EER_t))] + \beta_3 + \epsilon_{\beta t}$$

$$UK \text{ prices} \quad \log(PC_t) = \gamma_1 \log(PC_{t-1}) + \gamma_2[\log(WWPI_t) \\ - \log(EER_t)] + \epsilon_{\gamma t}$$

$$Exchange \text{ Rate} \quad \log(EER_t) = \log(XEER_t) + (RLB_t - RSW_t)/400$$

Symbol *Meanings*
(variables are endogenous unless otherwise stated)

MO_t money supply (MO), exogenous policy variable

$GDPE_t$ gross domestic product, expenditure measure

RLB_t UK short term interest rates

PC_t domestic price index

$WWPI_t$ world price index (exogenous)

EER_t UK exchange rate (foreign currency per unit of sterling)

RSW_t world short term interest rates (exogenous)

$XEER_t$ Expectations of the exchange rate in the next time period

ϵ Error term

to their own past values but also to world prices which means that domestic prices will eventually be brought into line with world prices. Finally the fourth equation, which is based on uncovered interest rate parity and models the exchange rate (EER), suggests that any expected appreciation in the domestic currency (XEER) must be matched by domestic interest rates being below world interest rate (RSW) by the same amount.

From Model 3.2 it can be seen that Keating's is essentially a demand-side model. By this it is meant that supply-side factors relating to the working population or the productive capacity of the economy do not appear. Although this would be important to economists, it is not particularly salient in this context as it could be rectified by re-specifying the IS curve to include the appropriate data. From a sociological perspective, the specification of the model is relatively unimportant: it is simply an example of an econometric model.³

In addition to the equations that make up the mathematical version of the economic model, it is also possible to represent the model graphically by using

a flow chart like that shown in Figure 3.1 in which the lines joining the boxes show the links within the economy. The arrows then indicate the direction (but not the magnitude) of ‘causality’. Where a line splits or branches to influence two or more variables this is represented by a small triangle at the junction. If no triangular symbol is present, this means that the lines simply cross and do not interact with each other in any way.

The advantage of this type of diagrammatic representation is that it allows one to see relatively easily both the variables that make up the model and the ways in which they are interrelated. In Figure 3.1, which shows my implementation of Keating’s economic model, all variables enclosed in ellipses are endogenous, which means that their future values are forecast by the model. Variables enclosed in rectangular boxes are exogenous, which means that they are not forecast by the model and any future values must be forecast by some other method. One way is to input the values directly (this is done for the expected exchange rate and interest rate); another is to use a simple univariate autoregression to extrapolate forwards from the base values (this is used for the majority of exogenous variables).⁴

In addition to elliptical and rectangular boxes, there are also three rectangular boxes with rounded corners (interest rates, in the upper left quadrant of the chart, and the balance of payments and the public sector borrowing requirement at the bottom). These boxes are used to identify variables that, in Keating’s original specification, were endogenous but which, in my version of the model, are exogenous. In the case of interest rates this decision was made in order to follow the format adopted by the Treasury’s Panel of Forecasters in their reports. The dotted line linking prices to interest rates shows the channel of influence which has been cut as a result of this decision. The PSBR and balance of payments were made exogenous for rather more pragmatic reasons: the equations given by Keating did not fit my data at all well (possibly due to changes in tax structure and rates etc. during the 1980s). As a result, forecasting these variables using autoregressive equations proved to be far more successful than attempting to reproduce or update the specifications used in Keating’s model. Once again, these problems, which would be an issue for a professional economic modeller, need not concern us here as the emphasis is on how forecasts are produced. The preceding chapter, which discussed the production of individual equations in detail, should however give a general idea how such problems might be resolved.

Other changes from the model specified by Keating concern the variables that influence imports. In Keating’s model, imports are influenced by several ‘world variables’ (e.g. world GNP, prices and the exchange rate). These variables are not included in the specification of my import equation as I was unable to obtain appropriate data; once again the dotted lines reflect the channels of influence that one might ordinarily expect to find in a macro-econometric model.

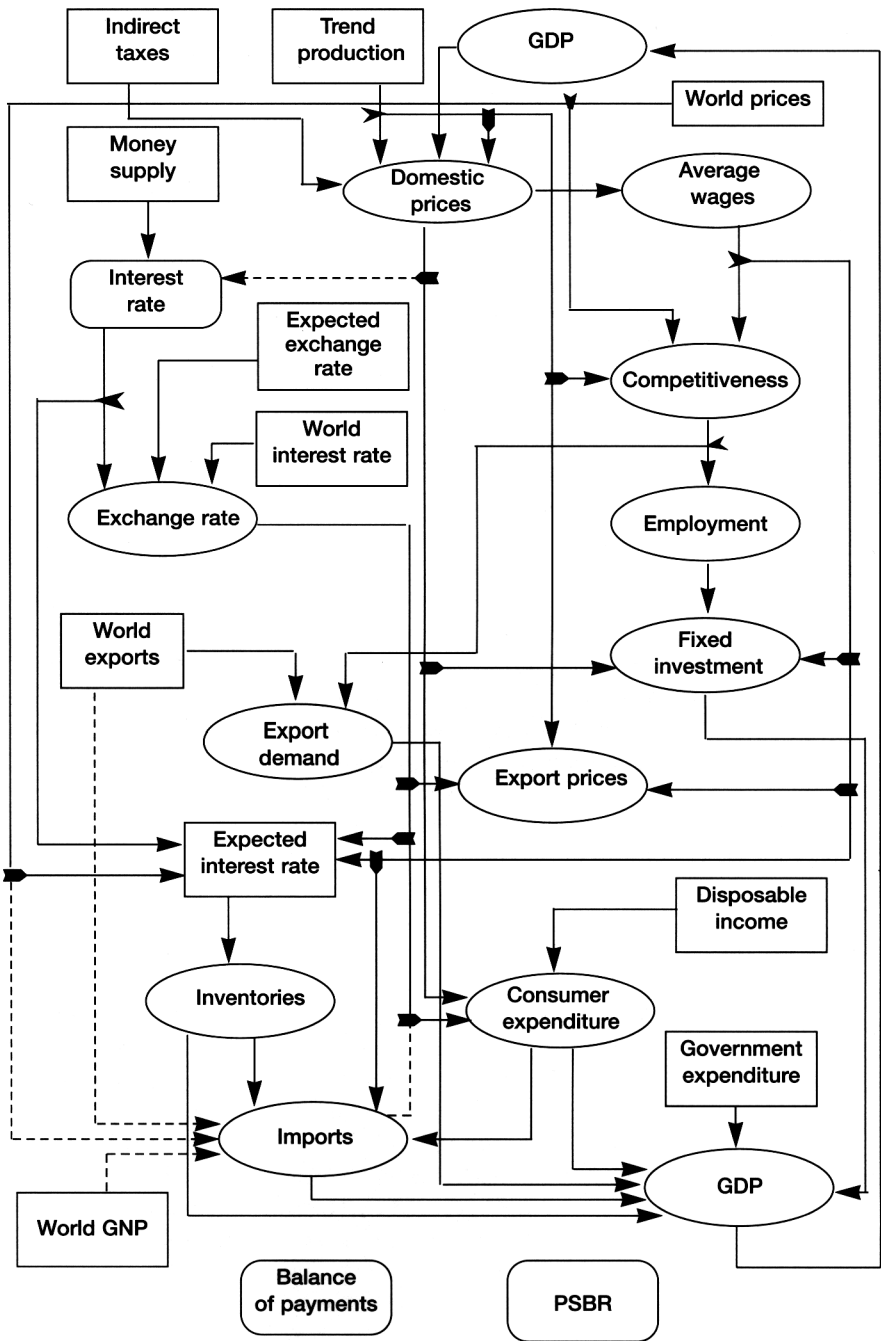


Figure 3.1 Flow chart of macro-econometric model

In the model, the single most important variable is domestic prices, which are determined by the level of GDP from the previous iteration or time period and several exogenous variables (indirect taxes, trend production and world prices). Once forecast, prices directly influence average wages, fixed investment and consumer expenditure. Through their effect on wages, prices also influence competitiveness, export prices, the expected interest rate, inventories and imports. In addition, competitiveness, which is responsive to prices because of their effect on wages, also influences export demand.

In Keating's model, the influence of prices is even greater. Remember that the interest rate should be endogenous, and should therefore be partly determined by prices. If this linkage had been maintained, there would be an additional set of effects due to changes in prices. In particular the exchange rate would be responsive to changes in prices and these effects would, in turn, impact upon the expected rate of interest (and hence inventories) as well as imports.

Figure 3.1 thus shows the basic structure of the econometric model, and by following the connections between variables it is possible to see how a change in one variable can propagate through the system. For example, an increase in prices might push up wages, thereby reducing competitiveness and hence employment and fixed investment. The increase in wages might also be expected to raise export prices and to increase the expected rate of interest (which would have to increase to maintain the real rate of interest at a particular level). In addition to increasing the expected rate of interest, a rise in prices would also increase the actual current or nominal rate of interest and hence the exchange rate (because sterling deposits become increasingly attractive due to their higher rate of return). This would happen directly in Keating's model, and indirectly in my own as the implications of the change in prices were taken into account when setting the exogenous variables. Finally, the rise in prices would also have an impact upon consumer expenditure decisions (for example by encouraging precautionary savings).

We can see therefore that, even using the rudimentary the structure given in Figure 3.1, it is possible to develop quite rich and complex stories about economic events. The problem is that although we can say that the increase in prices will push up wages, reducing competitiveness and thus employment, we cannot say by how much, nor how soon. For governments and businesses alike these are often the really important questions. A government planning to raise additional revenue through increasing indirect taxes (one of the variables which directly influence prices) would obviously like to know the consequences of choosing a particular tax-raising policy. Similarly, a firm planning its investment or marketing strategy would like an indication of the likely level of consumer expenditure and the rate of interest applied to any loans it might have to repay. Unfortunately the flow chart cannot provide this kind of detail. Thus, although it may be a convenient way of organising one's thoughts it is not much use for the sort of decisions which people who rely on economic forecasts are called

upon to make. These people need numbers, and this is what an econometric model provides.

Put simply, an econometric model is a mathematical version of the flow chart shown in Figure 3.1, in which the lines linking variables are replaced with numerical weights. In addition, a time dimension can be added by allowing (several) previous values of a particular variable to appear in each equation. Thus the effects of prices on wages and ultimately employment might be seen very quickly (e.g. by including their values in the previous time period) or relatively slowly (e.g. by including their values from several previous periods). The remainder of this chapter illustrates how such a model is used to produce an economic forecast.

Forecasting the UK economy in 1993

An economic forecast is produced by first running the model to produce an extrapolation, then adjusting the individual equations within the model so that their output becomes more plausible as an economic forecast. Of course, what counts as a 'plausible' economic forecast is to some extent negotiable. In this chapter, 'plausible' is taken to mean 'consistent with the average of the forecasts produced by the Treasury's Panel of Independent Forecasters'. There is no particular significance attached to this forecast; it is simply the one that has been selected as an example. Indeed, given the importance of judgmental adjustments in shaping the outcome of economic models, it would have been possible to use the same model to reproduce any of the Panel's Forecasts. In other words, even if it were possible for economic modellers to agree on one unique model of the economy (in this case the one used by Keating and myself) they could still disagree about the future prospects. Thus, although the use of different models certainly reflects differences of both opinion and economics between economists, it would be a mistake to think that the models were the cause of these differences, and that if only they could agree on a model, they would agree on everything else.

The purpose of the present exercise is therefore to show how, for the orders of magnitude which matter to economic forecasters, the model does not determine the forecast.⁵ Throughout the emphasis will be on the thought processes, arguments and judgements that I, as an 'economic forecaster' was forced to make as I compared what my model was telling me with other information available at the time.⁶ What follows is thus a reflexive and ethnographic, if somewhat personal, account of how an economic forecast is produced.

Preliminaries to forecasting

Before using the model to forecast it was necessary to estimate the equations, group them together into a single economic model and debug the computer

program that did this. This actually took much longer than producing the forecast and involved all the usual trials, tribulations and frustrations associated with programming a computer. For example, on one test run, the model forecast expected interest rates of over 230 per cent and an unemployment figure of about 5.7 million. A closer inspection of the code eventually revealed that the formula being used to calculate the expected real rate of interest bore absolutely no relation to the estimated equation from which it was supposed to have been derived! Other smaller mistakes included incorrect signs on some coefficients. Eventually, after these (and several other) mistakes were corrected the forecast for the expected interest rate improved to a rather more plausible 6.5 per cent and the model code appeared to be working satisfactorily.

Beginning to forecast

The first part of any forecast is a summary of the point from which it is to begin. In this case the time was February 1993 and the situation in the UK economy could be summed up as follows:

Since the middle of 1990, the UK has experienced a prolonged and severe recession. GDP fell in 1992 for the second year in succession, and unemployment has risen to about 3 million. Since the Government suspended its membership of the Exchange Rate Mechanism (ERM) on 16 September [1992] it has been able to relax monetary policy by cutting interest rates and the exchange rate has fallen.

(HM Treasury 1993a para. 1)

The challenge facing economic forecasters is to predict what will happen as a result of these changes in economic policy.

The first forecast

The first forecast an economist makes is obtained simply by running the model forwards and seeing what happens. This forecast would be unlikely to be published, but the extrapolation offers a rough guide where the economy might be going if past trends were to repeat themselves. No adjustments of any kind would be made at this stage, and very little expertise is required to produce this sort of forecast. It is not even necessary to use a large econometric model, as a relatively simple vector auto-regressive model (in which everything is regressed on to everything else) can provide a similar type of extrapolation with less time and trouble.

Of course, such extrapolations are generally not much use as forecasts. In order to turn them into a plausible forecast the economist must use his or her

expertise to adjust the model in the light of what is known about the economy. In other words, the economist will have to sift and analyse economic data and stories as they are released, and feed the judgements distilled from this process into the model, to transform the initial extrapolation into a persuasive and coherent economic forecast.

As an illustration of the different sorts of forecasts that can be made, and the variations in their credibility, consider Table 3.1, which shows a selection of forecasts that could have been produced in February 1993. The first column shows the average of the forecasts produced by the Treasury's Panel of Independent Forecasters and published in their February Report to the Chancellor (HM Treasury 1993). The second shows the model-only forecast produced by my model - a much less sophisticated economic model than that used by any of the Panel of Forecasters, and as yet unimproved by the addition of any information not contained in the data on which it was estimated. The third column shows the forecast produced by a vector auto-regressive model, subject to the constraint that GDP growth in 1993 must be 1.1 per cent (i.e. the same as the Seven Wise Men average in order to enable comparisons).

From extrapolation to forecast

In comparing the Panel's average forecast with that produced by the VAR model, it can be seen that the judgement of the Seven Wise Men was that interest rates and prices would be rather lower than a simple extrapolation would suggest. In addition, the balance of payments deficit and the PSBR were expected to be higher than the extrapolated figures. In other words, for the same GDP growth rate, the economists were more optimistic than the VAR model about prices and interest rates but more pessimistic about net trade and the PSBR.

Another way to look at this difference might be to ask whether or not economic forecasters are conservative, in the sense of tending to underestimate year-on-year changes and over-emphasize the inertia or momentum in the economy. To address this question it is necessary to compare the changes forecast with the outturn data for 1993. In 1992, prices increased by 3.7 per

Table 3.1 A selection of forecasts for 1993, together with outturn data

	<i>Panel of Forecasters average</i>	<i>Simple structural model</i>	<i>VAR model</i>
GDP % growth	1.1	-1.0	1.1
Price Index (1993Q4)	3.9	2.8	7.9
Interest rates %	5.4	6.33	8.4
Current account (£bn)	-15.7	-10.1	-11.1
PSBR (£bn)	47.0	32.8	43.9

cent, short term interest rates stood at 7.5 per cent, the current account balance was £ -11.7 billion and the PSBR was £37.3 billion (HM Treasury 1993: Table B1, Seven Wise Men Averages). In 1993, the comparable figures were as follows: prices increased by 2.7 per cent, interest rates finished the year at 5.8 per cent, the current account deficit was £ -10.7 billion and the PSBR was £45.9 billion. We can see therefore that for the same growth in GDP, the economic forecasters were not consistently more conservative than the economic extrapolation produced by the statistical model. Although they forecast less of a change in prices, they forecast bigger changes in interest rates, the current account and the PSBR.

With regard to the extrapolation produced by my structural model, it can be seen that the initial forecast was not particularly encouraging, either for me or for the economy. In particular, the model was forecasting a contraction in GDP of around 1 per cent, despite the devaluation and reductions in interest rates that had occurred in the preceding few years.⁷ This relaxation in monetary policy should have provided a considerable boost to economic activity, which should have been manifested in increasing GDP growth. However, it was not and this is a clear indication that something was wrong with my model. To try to find out where the error lay it is necessary to look at the individual components of GDP.

Searching for errors

As was discussed in Chapter Two, GDP is the sum of several different categories of expenditure. To understand why the model forecast a fall in GDP, despite the anticipated positive effects of recent policy changes, it is necessary to examine the individual forecasts for each element of GDP. In my model, GDP was forecast using the identity given in Equation 3.1.

The forecasts for the individual components of GDP are shown in Figure 3.2. It can be seen that the majority were actually increasing throughout 1993.

The only exceptions to this trend were imports and stockbuilding.⁸ However, imports are a negative contribution to GDP, so all other things being equal, a

Equation 3.1 Identity for GDP

$$\begin{aligned} \text{GDP} = & \text{Consumers' Expenditure} \\ & + \text{Gross Fixed Investment} \\ & + \text{Stockbuilding} \\ & + \text{Government Expenditure} \\ & + \text{Exports} \\ & - \text{Imports} \\ & + \text{Net Oil Exports} \end{aligned}$$

Forecasts for 1993					
ENTRY	GDPE	Consumers' Expenditure	Fixed Investment	Stockbuilding	
1992:04	134536.00000000	85801.911075366	31801.004613527	2530500.0000000	
1993:01	128858.46830563	86091.574708815	32556.474937994	2530646.5658070	
1993:02	127918.64186875	86921.726089594	29554.121505269	2530985.8214860	
1993:03	129900.50225242	87761.774399129	29133.228947722	2531427.4053428	
1993:04	133185.27236814	88667.403850992	30129.930785551	2531922.6789842	
ENTRY	Government Expenditure	Exports	Imports	Oil Exports	
1992:04	30028.000000000	32925.729388517	36165.000000000	-100.729388517	
1993:01	30100.811494758	33773.298662270	35854.533769487	-97.306911365	
1993:02	30202.969834087	34602.714937693	35784.505666221	-92.895784248	
1993:03	30300.377648518	35420.756194853	35138.231526046	-89.904444952	
1993:04	30400.959803183	36234.442110861	34537.021610576	-87.314583375	
ENTRY	Adj. to Factor Cost				
1992:04	17771.838931040				
1993:01	17858.416624360				
1993:02	17824.744726369				
1993:03	17929.082823690				
1993:04	18118.401629843				

Figure 3.2 Model-only forecasts for GDP and its components

fall in imports would increase GDP. Consequently, the too-small forecast for GDP could not be attributed to the declining forecast for imports. Although the fall in fixed investment does have a negative effect on GDP, and may therefore be to blame for the fall in GDP, boosting the forecast for fixed investment to increase GDP was not an attractive option as it made the forecast less plausible. This was because fixed investment was generally expected to be negative during 1993: although the range of values forecast by the Panel of Forecasters is large (from -3.1 per cent to +2.6 per cent for the six which use the same definition) the average forecast was for a fall of 1.9 per cent.⁹ As a result of these considerations, I decided against 'fixing' the GDP forecast by increasing the fixed investment forecast

If fixed investment is not the problem, what was causing GDP to contract? The cause could not have been any of the variables which contribute positively to GDP as these were all forecast to increase during 1993. It follows that the most likely explanation for the contraction must have been an overly large negative contribution from any (or perhaps all) of imports, net oil exports or

the adjustment to factor cost. We have already noted that imports were falling, so they could not be the cause. Although the forecast for net oil exports was negative, the numbers involved were very small and were, in any case, consistent with the notion that the UK was a net oil importer (which I think was the case). This left the adjustment to factor cost, which is a type of balancing item subtracted from GDP at market prices to define the expenditure measure of national output, GDPE.¹⁰ Although it was forecast by an equation in the model, it has to be admitted that this equation did not appear to be very good, since at the end of 1992, the fitted value estimated by the equation was approximately 8,000 more than the actual value. (See Figure 3.3.)

Given this, it would seem reasonable to begin adjusting the model by improving the performance of the equation that calculates the adjustment to factor cost. In particular, because it was known that this equation had overpredicted in the past it would have been appropriate to attempt to offset this known mis-specification with a residual adjustment. By calculating that the fitted value was (to the nearest 100) 8,200 too high at the end of 1992, the decision was made to reduce the adjustment to factor cost by this amount for each quarter in 1993. This might not have been the pattern of residuals adjustments used in the final forecast but it was a good first approximation.

Indeed, revising the adjustment to factor cost did have the intended effect, and with the adjustments in place the model now forecast GDP growth in

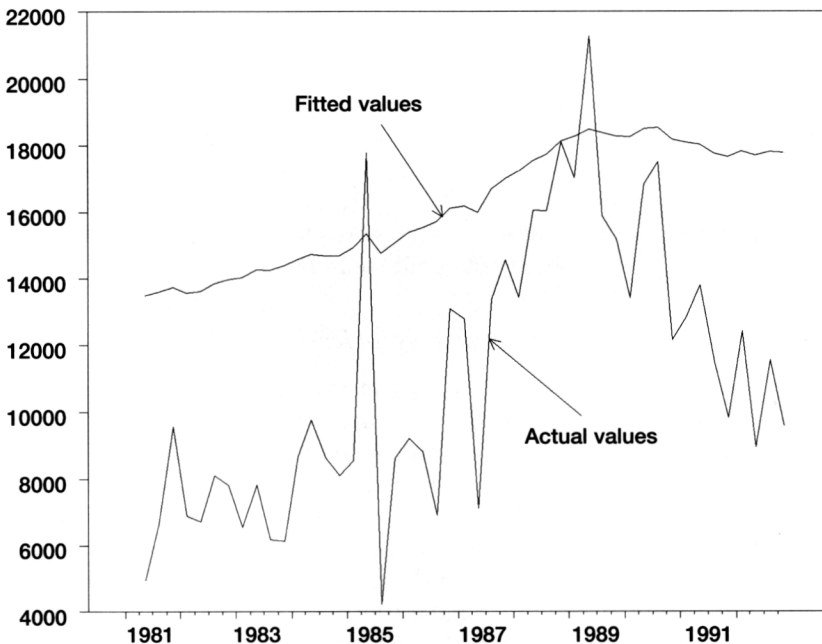


Figure 3.3 Actual and fitted values for the adjustment to factor cost

1993. The adjustment therefore improved the model forecast in two ways. First, it now seemed more consistent with the general expectation of other forecasters. Second, it was also more consistent with the story being told by the model itself, which was forecasting growth in most categories of expenditure.

Once the general direction of the forecast has been determined, the next stage is typically to examine the other components of GDP in more detail. However, before making further adjustments to fine-tune the model I noticed that my assumption about interest rates was at odds with the central expectation of the other forecasters. My initial assumption was that interest rates would remain unchanged throughout 1993. It was now necessary to ask whether this assumption was plausible in the light of what was known about the economic situation in the UK.

First, although some recovery was expected during 1993, the average of the Panel's forecasts was only for below trend growth of 1.1 per cent. The recovery was not therefore expected to be strong. Moreover, one of the reasons for the anticipated weakness of the recovery was the belief that consumer expenditure would be restrained by the high levels of debt caused by recession and the fall in house prices. Keeping interest rates higher than they needed to be would only compound this problem, and had therefore to be regarded as an unlikely economic policy. In addition to holding back consumer expenditure, a high rate of interest would also tend to push up the exchange rate (by making sterling deposits attractive to investors). However, export growth, which is important for economic recovery, would be held back if the exchange rate were unnecessarily high. In sum, there were several reasons to believe that the rate of interest was likely to fall during 1993. I therefore changed the exogenous projections for interest rates to show a constant decline throughout 1993, reaching the Seven Wise Men average of 5.4 per cent in the fourth quarter.¹¹ Once these changes had been made, the model was run again and another set of forecasts for 1993 produced. Now the task of forecasting the economy in detail could begin.

Forecasting prices

Given the central importance of prices in the model, the first forecast I decide to check was the one for the price level at the end of 1993. Although the initial model-only forecast (shown in Table 3.1) was for prices to increase by 2.8 per cent this had since changed. This is because prices are partly determined by previous values of GDP, and increasing the forecast for GDP, which is what the adjustment to the adjustment to factor cost effectively did, increased the forecast for the price level. In fact the new forecast for prices was for the consumer price index (the measure used in my model) to increase by 3.9 per cent, from 113.97 in 1992Q4 to 118.36 in 1993Q4.

It then needed to be asked how plausible this forecast was. On the one hand, the UK had been through a prolonged recession and inflationary pressures should have been particularly weak, suggesting perhaps that a simple extrapolation might over-predict. On the other hand, the recent devaluation and other relaxations in monetary policy suggested the opposite. Thus forecasting prices for 1993 meant identifying which, of all the factors that cause prices to change, were going to be the most important in this particular economic cycle. In their report, the Panel of Independent Forecasters described the decision in terms of the balance of two opposing tendencies:

On the one hand, the fall in the exchange rate since September will inevitably put upward pressure on retail prices in the short term. On the other, the considerable slack in the economy is a powerful disinflationary force . . . The extent to which any rise in inflation due to the recent fall in the exchange rate proves temporary will be determined by the behaviour of wages. [The Panel] all agree that there is some risk that inflation could rise in 1994 as the recovery gathers place. But [they] have different views about the likelihood of this occurring. Most of [them] think that low headline RPI inflation (due in part to recent cuts in mortgage rates) and the high level of unemployment should keep the rate of earnings growth to below 5 per cent in 1994. But [two] expect that these factors will be outweighed by the effects of stronger activity.

(HM Treasury 1993a paras 12-14)

It is clear that a judgement was needed on how the effects of the devaluation would be offset or cancelled out by the recession that preceded it. Economic forecasters are routinely called upon to make this sort of judgement. It is known that there are two opposing forces acting upon the economy. The past data show how things have worked out in the past. The econometric model tells us how things will work out in the future, based on an aggregation or averaging of what happened before. The economic forecaster has to first recognise the general case (e.g. a devaluation) of which the present circumstances are an instance, then identify the factors that make the present situation unique and incorporate them into the analysis and forecast.

In the case of my forecast, the consumer price index (the measure used in my model) was projected to increase by 3.9 per cent in 1993, from 113.97 in 1992Q4 to 118.36 in 1993Q4. This is exactly the same as the average of the Panel's forecasts reported in Table 3.1. Although this might seem like a satisfactory outcome, it should be taken into account that, although the numbers were the same, the forecasts were for different price indices. My forecast was for the consumer price index (which is calculated from the ratio of consumer expenditure in current prices to consumer expenditure in constant prices). The Panel, on the other hand, forecast levels of the retail

price index (RPI) and the retail price index excluding mortgage interest payments (RPIX). According to the Bank of England, 'The RPI is intended to measure the cost of purchasing a representative basket of goods and services. It includes expenditure on consumption only; expenditures on investment and saving are excluded' (*Bank of England Quarterly Bulletin*, February 1993 vol. 33 no. 1:12).

However, the government's anti-inflation policy targeted the RPIX measure of inflation and not the RPI. This qualification is important as different price indices change at different rates. Thus, while the Panel of Forecasters expected RPIX to increase by around 3.9 per cent in 1993, the RPI (i.e. the more inclusive measure of prices) was expected to increase by only 2.6 per cent in the same time period (HM Treasury 1993a: Table B1).

Thus, in evaluating the adequacy of my own forecast for a 3.9 per cent increase in the consumer price index it is necessary to consider which, of these two measures of inflation, is most like the consumer price index used in my model. Because consumer expenditure (the data series from which the consumer price index is constructed) includes 'accommodation in owner-occupied homes' then it seemed likely that the standard retail price index, which includes MIPS, was the more appropriate proxy.¹² However, at this early stage of the forecasting process, the decision was made that the best strategy would be to hedge my bets and go for an inflation forecast somewhere towards the bottom end of the range marked out by the RPI and the RPIX measures. In order to achieve the desired outcome, and restrain the growth in prices during 1993 to a slightly lower level, the residual adjustments shown in Table 3.2 were made.

The result of these adjustments was to reduce the growth in prices to about 3.5 per cent in 1993. While there remains a concern that this might not be enough to fully reflect the weight of housing costs in the retail price index, and hence in my consumer price index, it should be remembered that adjustments to other equations in the model would also impact on prices. The final forecast was still therefore undecided and the adjustments made thus far are best thought of as the first approximation in an iterative process that may run for several more loops.

Of course, some justification for these adjustments was also needed. In particular, were there any reasons for thinking that inflation in 1993 would be

Table 3.2 Residual adjustments to price equation

<i>Date</i>	<i>Adjustment</i>
1993 Q1	0.000
1993 Q2	0.000
1993 Q3	-0.001
1993 Q4	-0.002

lower than usual? In fact, several reasons can be adduced. As was noted when making the projections for interest rates, it was generally recognized that any recovery that did occur in 1993 would be weak, and that consumers would be unusually reluctant to spend the economy out of recovery. The implication for prices was that producers would be unable to pass on the increased costs associated with the rise in import prices owing to the devaluation. In other words, 'imported inflation' might have been expected to be lower in this cycle than in previous ones. In addition there were grounds for believing that the large pool of unemployed workers would keep a firm downward pressure on wages and so this force, which would tend to increase inflation, was also unusually muted. Taken together these observations suggested that there were several distinctive features in the economic situation which made it reasonable to believe that prices would rise by less than an extrapolation of past trends would suggest.

After having thus set an initial and plausible path for prices, my attention turned to the components of GDP. The first to be considered was consumer expenditure, principally because it is the largest.

Forecasting consumer expenditure

From the forecasts shown in Figure 3.3 it can be seen that the initial model-only forecast was for consumer expenditure to increase during 1993 by around 3.4 per cent. As has been pointed out before, it was generally believed that the economy was particularly weak and it was possible that an unadjusted forecast would be unduly optimistic. The Panel of Independent Forecasters confirmed this suspicion by writing that:

[they] agree that consumer spending is likely to be restrained by balance sheet problems (for example, the large overhang of debt), the weakness of the housing market, rising unemployment and a squeeze on real incomes caused by rising import prices.

(HM Treasury 1993a: 5)

At this point in the forecast there were several options available. For example, one could have argued that in order to restrain the growth forecast by the model the government would be forced to raise interest rates in 1993 and not let them fall, which was the usual expectation. As already noted, however, this would have led to an appreciation of sterling on the foreign exchanges which would have been very damaging to exports, so it was one of the last things the government was expected to do. Indeed, in his submission to the Report of the Panel of Independent Forecasters Gavyn Davies explicitly warned of the dangers of too-high interest rates and argued that reducing rates should be the priority, with sterling being allowed to slide if necessary. Patrick

Minford and Wynne Godley advocated similar policies, although for different reasons.

Given this analysis, reducing the forecast for consumer expenditure by returning interest rates to the higher-than-necessary values previously assumed did not seem a particularly plausible solution. In addition, if consumers' debts were behind the unusually high saving ratio, increasing the cost of this debt more than necessary seemed a particularly unlikely government policy. In other words, the interest rate assumption seemed perfectly plausible and was not therefore a particularly appealing candidate for change.

Another possible way in which the behaviour of consumers might be influenced is through the effect of wages. As was noted when forecasting prices, there was a case for arguing that the large pool of unemployed workers would tend to keep wage increases low. Although wages do not appear directly in the consumption function used in my model, disposable income does. One solution to the problem of overly high consumers' expenditure forecasts might therefore have been to revise the exogenous projections of disposable income down, in the light of the expected low level of wage settlements. At this stage, disposable income projections were produced by a simple univariate autoregression equation. This is hardly a sophisticated forecasting tool and so the use of a judgmental adjustment to the exogenous projections could have been justified. An alternative would have been simply to impose a residual adjustment on the consumer expenditure equation to represent both the unusual reluctance of consumers to spend and the unusually low level of wage increases which were expected to lead to a relatively small growth in disposable income.

I suspect a professional modeller would first revise the exogenous projections for disposable income to make them compatible with the assumptions and forecasts about wages, and only then adjust the consumer expenditure equation. Revising the exogenous database has the presentational advantage of not appearing in any list of residual adjustments. It also means that any residual adjustments that are necessary will look smaller. In my case, for ease of programming and presentation, I chose to make the adjustments to the consumer expenditure equation itself. The adjustments are given in Table 3.3 and had the effect of reducing the growth in consumer expenditure to around 1 per cent. However, reducing consumer expenditure also reduced GDP and the lower levels of GDP fed back into the price equation, causing the forecast for prices to fall slightly. It was therefore possible that the residual adjustments made to the price equation would have to be reset to zero in the next iteration.

Gross fixed investment

It was noted in discussing the model-only forecasts shown in Figure 3.2 that the range of uncertainty surrounding the forecast for gross fixed investment was very large. Some members of the Panel of Forecasters were anticipating

Table 3.3 Residual adjustments to consumer expenditure equation

<i>Date</i>	<i>Adjustment</i>
1993 Q1	-0.001
1993 Q2	-0.007
1993 Q3	-0.007
1993 Q4	-0.007

an increase in the level of fixed investment while, others were expecting a decrease. The range of plausible outcomes was such that the judgement made about the level of investment could mean the difference between a forecast for GDP growth and a forecast for continued recession. Nevertheless, a forecast needed to be made for fixed investment and this is the task to which we now turn.

After the adjustments made so far, the model was predicting that fixed investment in 1993 would increase by around 11 per cent. This was clearly too high, and was well outside the range of values forecast by the Seven Wise Men, my proxy for the boundaries of plausible economic beliefs. However, before making any residual adjustments to the equation that forecasts gross fixed investment, it was necessary to consider the possibility that the high forecast was caused by an error somewhere else in the model, which fed into the investment equation where it became apparent.

From the flow chart (Figure 3.1) it can be seen that fixed investment is determined by two factors: wages and employment. It is therefore important to ensure that the forecasts for these two series are plausible before the investment equation is altered. Because wages influence employment, the forecasts for wages will be considered first. At this stage wages were forecast to remain virtually static throughout 1993, with the index of wages and salaries increasing by 0.1, from 115.4 to 115.5.

As was noted when discussing prices, one effect of the ending of the recession was expected to be an upward pressure on wage demands. On the other hand, it was noted that high levels of unemployment would tend to work in the opposite direction. In addition, the judgement that prices would be increasing at an historically low rate also implied that wage increases would be relatively low. Against this, however, the recession had ensured that wage increases in the previous few years were very low. There was therefore a belief that even the weak growth in GDP that was expected to occur might have been sufficient to trigger an increase in the rate of earnings growth. In addition, the possible inflationary effects of the devaluation might have acted to reinforce this tendency.

As a result of these considerations, the range of forecasts for average earnings by the Seven Wise Men ran from 2.8 to 6.4 per cent. Thus, although some saw wages as a bigger problem than others, an increase in the level of

wages was nevertheless the expectation of every forecaster. In light of this, my forecast was clearly in need of some adjustment, and it was possible that the equation itself would need respecifying in a more fundamental manner. However, the immediate priority was to ensure that the forecast for wages showed an increase throughout 1993. Any respecification would have to wait until another day. By making the adjustments shown in Table 3.4 to reflect the wage pressure due to the upturn in economic activity and the devaluation, as well as to correct the perceived mis-specification, the forecast for the index of average wages could be made to show an increase of about 4 per cent.

Having thus achieved a plausible forecast for the increase in average wages, we must now turn our attention to the forecasts for employment, which is also a determinant of fixed investment. The existing forecast was for employment in the economy to fall during 1993. On first inspection this seemed quite plausible. For example, the Panel of Independent Forecasters expected unemployment to increase by around 300,000, from 2.9 million to 3.2 million throughout 1993 (HM Treasury 1993a: Table 1) and employment to fall by about 2 per cent (*ibid.*: Table B4). Thus, although the forecast fall was rather large (it implied unemployment of 4.5 million if all the newly unemployed were added to the jobless total) the direction was correct and, by using the residual adjustments shown in Table 3.5, it could be adjusted back towards this range.

Having ensured that the inputs to the fixed investment equation were as good as possible, I was in a position to examine the forecasts for investment. Unfortunately the changes implemented had made things worse. The forecast was now for fixed investment to increase by 13 per cent. However, by imposing non-zero residuals, the forecast could be made to show a fall of 2 per cent. This was consistent with the range being forecast by the Panel of Independent Forecasters and seemed like a reasonable first approximation.

Stockbuilding

Like fixed investment, Stockbuilding is a volatile component of GDP, and forecasting it is made difficult by the general uncertainty that surrounds business spending. The forecast produced by the model was for Stockbuilding to make a positive contribution to growth in GDP in 1993. This was in the right general

Table 3.4 Residual adjustments to wage equation

<i>Date</i>	<i>Adjustment</i>
1993 Q1	0.000
1993 Q2	0.012
1993 Q3	0.015
1993 Q4	0.020

Table 3.5 Residual adjustments to employment equation

<i>Date</i>	<i>Adjustment</i>
1993 Q1	0.022
1993 Q2	0.022
1993 Q3	0.022
1993 Q4	0.018

direction, but the actual forecast was for stockbuilding to increase by an amount which would have added just over 1 per cent to GDP, which was higher than any of the forecasts produced by the Panel of Forecasters.¹³ In the absence of a strong reason for expecting a high level of stockbuilding, a risk-averse forecaster would revise this forecast closer to the average.

Using residual adjustments to reduce the forecast seemed particularly attractive since the reasons for making adjustments corroborated a cautious approach. For example, the tentative nature of the recovery and the general belief that it was far from firmly established suggested that businesses might be reluctant to invest in stockbuilding until economic conditions made a the return on this investment more certain. Accordingly the adjustments detailed in Table 3.6 were made, and the forecast for stockbuilding was reduced slightly so as to show a positive contribution to GDP growth of 0.25 per cent. (The average forecast by the Panel of Independent Forecasters was 0.28 per cent.)

An unintended consequence of this adjustment was a fall in fixed investment by more than seemed justified. The residual adjustments made for that equation therefore needed to be revised before the forecast could be finalised. For the time being, however, it was decided to continue with the first iteration of the forecasting process. Only after the whole forecast provided a satisfactory first approximation would the fine-tuning of individual components make sense.

Net trade

Net trade is the contribution made to growth in GDP by exports minus imports plus net oil exports. The average of the forecasts made by the Panel of

Table 3.6 Residual adjustments to stockbuilding equation

<i>Date</i>	<i>Adjustment</i>
1993 Q1	0.0000
1993 Q2	-0.0001
1993 Q3	-0.0001
1993 Q4	-0.0001

Independent Forecasters was that net trade would contribute 0.4 per cent to growth in GDP during 1993. My model forecast was very similar, with an initial negative contribution to GDP changing into a small positive one by the end of the year. This was, in fact, pretty much what one would expect to see as a result of the devaluation. As imports become relatively more expensive and exports relatively cheaper, one would expect exports to rise.

A closer reading, however, revealed that all was not well with my forecast. Although the devaluation was expected to improve net trade, the general expectation was not that the absolute level of imports would fall, only that there would be a fall in their rate of growth. The Panel of Forecasters expected both imports and exports to grow during 1993, with exports growing faster and thus making a positive contribution to GDP. In my model, although the final forecast was plausible, my projection that the absolute level of imports would fall was not in line with my exemplars.

The average of the Panel's forecasts (excluding Godley, who forecast a different variable) was for exports to grow by 5.5 per cent and imports by 4.7 per cent in 1993. However, if these projections were applied to my data values for 1992Q4, then imports remained larger than exports and so the contribution of net trade to GDP was negative. I suspected the problem lay with the data for imports and exports. These series were very difficult to obtain and had to be constructed by combining other data series, so it was quite likely that an error had been made during this procedure. The best solution to the problem, therefore, was not to adjust the equation, but to obtain another data series for non-oil imports and exports for the purpose of comparison, and to make any corrections that this process brought to light. Alternatively, it would have been possible to correct the suspected data errors by making adjustments to the import equation, and then offsetting these effects in the equation for net trade. In practice I did not do either, but simply flagged the potential problems, leaving my forecast for net trade to make a small positive contribution to growth in GDP in 1993.

The forecast so far

Completion of the first round of adjustments to the forecast provided an appropriate point to take stock of the situation. As a result of the adjustments most of the forecasts for the components of GDP were reasonably plausible, in the sense that they were within the range set out in the Panel of Independent Forecasters' Reports. The forecast for prices had fallen slightly as a result of the adjustments to the other equations and now stood at a 3.3 per cent increase over the year. However, as noted above, the forecast obtained for prices was perhaps slightly too high initially so this was not a cause for concern.

More significantly, the adjustments made in other parts of the model had affected the forecast for fixed investment, which now showed a bigger fall than

was intended. This suggested the residual adjustments made to the fixed investment equation were too large and needed to be reduced. By resetting the residuals to the values shown in Table 3.7 the forecast was made to show a fall of 1.8 per cent.

With these adjustments, the forecasts for prices, wages, employment and the individual components of GDP were now in reasonable agreement with the average of Panel's published forecasts. It was now necessary to check that the forecast for GDP as a whole was satisfactory.

The forecast for total GDP growth in 1993 was in fact rather too high at approximately 3.5 per cent. As the forecasts for the components were all acceptable it was necessary to make the adjustment in the adjustment to factor cost. At the beginning of the forecasting period, the residuals on the factor cost adjustment equation were set at a constant -8,200 through the period. To make the forecast for GDP lower, the adjustment to factor cost needed to be made larger, by making the residual adjustments smaller. Could this be justified?

Fortunately several justifications could be given. It is apparent from Figure 3.3 that the adjustment to factor cost is a volatile series with relatively frequent changes in value. It was therefore most unlikely that it would remain virtually constant throughout 1993, as was implied by the initial pattern of residual adjustments. It is also clear from Figure 3.3 that the adjustment to factor cost was on a downward trend in the year leading up to 1992Q4. Consequently, to set a pattern of decreasing residual adjustments for the period 1993Q1 to 1993Q4 would introduce a turning point into the series which would be more or less consistent with the previous data. On the basis of these arguments, the residuals on the factor cost equation were reduced as detailed in table 3.8, thus increasing the size of the adjustment. Reducing the adjustments suggested

Table 3.7 Residual adjustments to fixed investment equation

<i>Date</i>	<i>Adjustment</i>
1993 Q1	-0.025
1993 Q2	-0.025
1993 Q3	-0.045
1993 Q4	-0.045

Table 3.8 Residual adjustments to adjustment to factor cost

<i>Date</i>	<i>Adjustment</i>
1993 Q1	-6700
1993 Q2	-6100
1993 Q3	-6000
1993 Q4	-5900

that the performance of the equation was not as bad as initially believed. These changes produced a forecast for growth in GDP of 1.1 per cent in 1993.

The forecast, which was now consistent with average of the Panel of Independent Forecasters' forecasts, is shown in Table 3.9. There were still some problems that needed rectifying, particularly over the forecasting of imports. If the series of data for imports had been boosted to a level closer to that of exports, the negative contribution made to GDP by net trade in the first three quarters of 1993 would have been smaller and the overall forecasts for GDP growth larger. This suggests that the residuals set on the factor cost equation should have been set at an even lower level.

These problems could no doubt have been solved by further rounds of the adjustment process and additional econometric work, and they should not detract from the main purpose of this exposition. The aim has been to illustrate how an economic forecast is produced using a macro-econometric model and to show the importance of the economist's judgement in producing a plausible forecast. The implications of the example are discussed in the next section.

Conclusions

The aim of this chapter has been to illustrate the ways in which the apparently precise and quantitative output of an econometric model is the product not just of a sophisticated computer model, but also of a process of interpretation and adjustment. In particular it has been shown that forecasting the economy relies on the skill and reasoning powers of economists. Thus, the raw output of the model is not an economic forecast. Even after the economist has specified and estimated the model (a process that is itself influenced by a variety of prior beliefs) its output still needs to be assessed, interpreted and its plausibility in the wider community established.

In fact, forecasting the economic future seems to rely even more heavily on the economist's powers of judgement and reasoning than the estimation of the

Table 3.9 Summary of forecasts for 1993 by R. Evans and the Panel of Independent Forecasters

	<i>Evans</i>	<i>Panel</i>
GDP % growth	1.1	1.1
Consumer expenditure	1.1	0.91
Gross fixed investment	-1.8	-0.7
Stockbuilding*	0.25	0.31
Government	1.2	1.01
Net Trade**	0.7	0.4

* Excludes Minford.

** Contribution to GDP growth per cent.

model. As a result, it is difficult to separate the model from the economist, and forecasts appear more negotiable than their apparent grounding in a complex set of econometric equations might suggest. What is more, if judgement and interpretation are to play an important part in economic forecasting, is it not reasonable to suggest that judgements and interpretations from outside economics might also play a valid role in forecasting and policy making?

In other words, it is clear that whenever an econometric model is used to project the future, a degree of judgement will be required from the forecaster. For example, the values of exogenous variables must be provided by the forecaster, and although past values may be available, future values will inevitably depend on the forecaster's judgement. The values chosen will embody expectations which are inextricable from the forecaster's expectations about the forecast itself. For example, if world trade were expected to decline, or grow comparatively slowly, this would have negative consequences for predictions of the domestic economy, which would be exporting into a shrinking market place. Similarly, projections for interest rates are listed as assumptions (that is, they are exogenous to the model) in the reports of the Panel of Forecasters, but such projections must inevitably be based upon a prior judgement about the future state of the domestic economy.¹⁴

This is not intended as a criticism of economic forecasting, however. It is inevitable that in the future existing practices will evolve and new ones emerge which will challenge and change established relationships and patterns.¹⁵ Any attempt at forecasting must therefore incorporate a significant degree of judgement. Even the extrapolation of past trends requires a prediction that nothing significant will change during the forecast period. In real-life circumstances, forecasters cannot avoid making broad assessments of how events are likely to unfold, in order to produce forecasts plausible enough for government policies and business investment decisions to be based upon them.

The question that arises is, how are these judgements about the future to be assessed, evaluated and warranted? In the case of estimating individual econometric equations as discussed in Chapter Two, the professional standards and codes of the economics community provide training and tend to ensure that judgements are appropriately in line with prevailing expert opinion. However, when it is necessary to make judgements about the effectiveness of government policy decisions, the impact of new technology on working practices, or the likely response of employees to a devaluation, the boundaries of 'relevant' expertise are not so clearly marked. It is arguable that organisational sociologists, occupational psychologists, civil servants, business administrators, industrial managers and trade unions have valid perspectives on these issues.

In practice economic forecasters participate in, and draw expertise from, a limited set of professional communities. Predominant among these is obviously the professional community of economists, and forecasters and modellers clearly

acquire experience, judgement and information through the 'grapevine' of informal meetings among colleagues. However, economic modellers and forecasters are distinguished from more theoretical economists by their attention to day-to-day developments in the economy. Their specific knowledge and expertise is developed by studying surveys, press releases and prices. It is also shaped by their interaction with the consumers of economic forecasts, who are primarily non-economists.

Of course economic forecasters also participate in social networks outside the professional community of economics, but it is questionable to what extent they see these as a source of information and relevant expertise. There are also social and professional networks with which most economists have at best very limited contact. In other words, because the sociometric worlds and connections of economic forecasters are limited and to some extent partial, so must their expertise be limited and partial. A common criticism, for example, is that 'academic' economists have a limited understanding of the needs of business and industry. They will to some extent be aware of the tensions and concerns that characterize life outside their own social networks, and of the general 'public mood', but their expertise in this regard is neither unique nor especially secure.

It should not necessarily be concluded that macro-econometric modelling is not quite scientific. A central message of the sociology of science is that in all science it is to some degree necessary to negotiate meaning and evidence, although this aspect of scientific endeavour is in general given little attention. Macro-econometric modelling is not unusual in this respect. Rather the point is that we need to think carefully about the expertise needed to make economic forecasts. Economists have the expertise needed to estimate models, but it by no means clear that they are also the sole custodians of the expertise needed to use them. Arguably this is both the strength of economic modelling (in that it is not as narrowly focused as some critics suggest) and its weakness (in that it is undervalued by economists and neither emphasized nor exploited by them). The sociological point is that this kind of interaction is important and needs to be recognised as such. The critical question, therefore, is whether or not existing institutions make adequate provision for wider sources of knowledge, judgement and expertise to be drawn upon in the process of constructing economic models and forecasts, and using them to shape economic policy.

SOOTHSAYING OR SCIENCE?

Choosing between economic models

Introduction

It has already been shown that macro-econometric modelling methodologies permit a range of theoretical specifications to co-exist. This chapter develops this theme in two main ways. First, it examines the statistical properties of macro economic models in order to give some insight into the reasons for this interpretative flexibility. Second, it shows why neither econometric testing nor forecast performance appear to be decisive in proving or disproving an economic theory.¹ It therefore shows why it is difficult for both economists and policy makers to choose between economic models.

The analysis concerns macro-econometric modelling in the most general sense, and the issues raised apply more or less equally to all macro-econometric models. The intention, as with the rest of the book, is to take a sociological perspective on the skill and expertise of economic forecasters and their social practices.

Taken as a whole, the chapter illustrates the considerable diversity of the macro-econometric models and theories at the disposal of policy makers, and argues that none appears to be significantly better than the rest. Moreover, the possibility of ranking models must remain distant as the most basic questions (for example, when a mistake has occurred in a forecast) cannot be answered.

Statistical uncertainty in economic models

Chapter Two described in some detail how one of the equations in a macro-econometric model, the consumption function, could be estimated using regression techniques. The analysis highlighted the ambiguity of the regression analysis used to estimate the equation and showed how the experimenters' regress (Collins 1992) that arose as econometrics were used to test a theory could be countered by the 'cherished beliefs' of the economics community and the economist's judgement.² Another aspect of regression analysis, not discussed thus far, is the statistical uncertainty that needs to be attached to the results before they can properly be understood.

Chapter Two included several tables in which the coefficients in regression equations were given to three, four and sometimes five decimal places, which certainly looks very precise.³ However, this degree of precision is illusory if it is not interpreted in the context of regression analysis. At its most basic, when just one independent variable is being used to predict the dependent variable, regression analysis calculates the equation for the straight line that best fits the available data points. 'Best fit' is defined as the estimated regression equation which produces a line from which the sum of the (squared) deviations of the data is as small as possible. If two independent variables are used to explain or predict the dependent variable, then regression analysis calculates the 'plane of best fit'. (See Wallace and Silver 1988: 92 note 3.) Although regression analysis becomes increasingly difficult to visualize as the number of explanatory variables increases, the underlying principles remain: the regression equation is the best estimate of the relationship between the independent and the dependent variables.

This means that, if the regression equation is used to predict values of Y , given values for the independent variables ($X_1 \dots X_n$), the answer will not be the observed value of Y but a number close to Y . If the estimated regression equation is used to predict all the values of Y associated with all the input data, on average the difference between the predicted and actual value will be zero. (This is why regression estimators are said to be unbiased.) However, for any particular value of Y it is most unlikely that the value of this deviation or error term will be zero. Thus, for each regression equation there is an associated set of error terms given by the difference between the actual values of Y and the values estimated by using the equation.⁴

The apparently precise appearance of a regression equation, in which coefficients are calculated to many decimal places, is therefore potentially misleading because the error term has not been shown. This error term, which can be thought of as a random 'draw' from the distribution of possible error terms, could be either positive or negative. Its potential range of values is determined by the closeness of the fit between the estimated values of Y and the original data. In the best cases, these errors will be small relative to the value of Y_i ; in the worst they will be of the same order of magnitude.

In other words, every point forecast produced using a regression equation comes with the caveat that the actual value will most likely be in the range given by the estimated value plus or minus twice the standard deviation of the distribution of the error term. The information value of the forecast must therefore depend crucially on the size of the standard deviation of the error term relative to the dependent variable. It is important to remember that all econometric equations have this property: it is a necessary consequence of the statistical methods by which they are produced.

Estimating uncertainty

As was shown in Chapter Three, a macroeconomic model is a system of linked equations. Some of these equations are accounting or definitional identities, which are true by fiat, but others are behavioural equations that attempt to model the actions of economic agents in the economy. These behavioural equations can be estimated individually and then combined into a whole model, or the whole model can be estimated in one go using a systems technique such as three-stage least squares. In theory, systems techniques are the best for estimating multi-equation models, because they take into account the co-variances between equations owing to dependent variables in some equations appearing as independent variables in others. In addition, using a systems estimation technique enables the standard error of the model to be calculated. This is similar to the standard error of an individual equation and, if it were known, would mean that any point forecast could be accompanied by an accurate estimate of the range of values implied by the fit of the model to the data. The statistical uncertainty attached to the model could then be taken into account when interpreting and presenting economic forecasts.

If the equations are estimated separately and only later combined into a single model, the model standard error cannot be calculated directly and must be estimated by some other means. In practice this is generally what happens as most macro-models are composed of equations estimated individually using partial information techniques such as instrumental variables.⁵ Because of this a direct measure of the model standard error is not available, and if one is needed it must be calculated indirectly. This is usually done by conducting a stochastic simulation in which the error term in each equation is allowed to take the range of values specified by the equation's standard error. For each error value, a simulation run is made and the forecast recorded. By repeating this process for literally thousands of runs, the distribution of forecast values can be plotted and the error associated with the model estimated.

From simulations such as these it transpires that the standard errors, however they are calculated, tend to be of the same order of magnitude as the actual outcomes. The presentation of forecasts to one or even two decimal places is therefore misleading. For example, a forecast for the GDP growth rate over the next twelve months might be surrounded by an uncertainty band of about 1.25 per cent either way. In other words, a forecast for growth in GDP of 2.5 per cent over the next year really means that it is most likely the outcome will be between 1.2 per cent and 3.8 per cent, but there remains a 5 per cent chance that it will be above or below this range. When looked at this way it seems that economic models do not tell us very much, especially over the longer term, and this is indeed the case:

Britton: [A]fter two [years] I think you've got to the stage where the information content of the forecast is probably about nil. In other words, that the standard error in the forecast is as large as the standard deviation of what you are trying to forecast, whilst over one year you roughly halve it, which is clearly useful.

(Interview 29 April 1993: transcript p. 27)

Once the uncertainty which surrounds an economic forecast becomes apparent it is also clear why, despite their theoretical differences, macro-econometric models are all more or less compatible with the economic data. A wide range of outcomes are compatible with any given forecast, and 'the differences that are often argued about are really well within sampling error' (Ken Wallis, interview 21 March 1993: transcript p. 5).

This uncertainty derived from the use of statistical techniques is not the only type of uncertainty that must be taken into account when interpreting an economic forecast. For forecasting, as opposed to the analysis of a historical data set, the projected future values of any variables not forecast by the model itself (i.e. the exogenous variables) must be obtained. These exogenous projections may be derived statistically and/or predicted by the economist based on his/her judgement. In either case they will add further uncertainty to the forecasts, and, to the extent that they are based on judgement, it is not clear how their uncertainty could be quantified.

This is not the only way in which the unquantifiable uncertainty of human judgement enters economic forecasting. As was shown in Chapter Three, forecasters may fine-tune their forecasts by setting the error terms in various equations to non-zero values. Research by the ESRC Macromodelling Bureau suggests that such practices are fairly commonplace and that these adjustments generally reduce the error of the final forecast.⁶ Although this is clearly desirable, it means that even if the model standard error were known, it would not be possible to assess the 'uncertainty band'. Interestingly the feeling among macro-econometric modellers is that, because most standard error estimates are so large, they are not worth publishing at all:

Ormerod: Well, in fact it's been technically feasible to calculate the mean and standard errors for many years. In fact, we could have done this at the National Institute in the mid '70s, but we suppressed it on the grounds that the standard errors were so large, that it would have been difficult for non-specialists, you know people using the models, using the forecasts, to appreciate.

(Interview 6 March 1993: transcript p. 2)

Minford: The trouble is that these stochastic simulations draw on a very restricted bunch of errors - you've got the errors in the exogenous variables as well, which are not generally included in these stochastic simulation

exercises.

Evans: And if they were, the outcome would be even worse.

Minford: Oh absolutely, that's why it's absolutely pointless to publish these forecast error bands because they are extremely large . . . I'm all for publishing full and frank statements but you see the difficulty [with] these standard errors is that they're huge. If you were properly to draw out the uncertainty surrounding a forecast, it's huge.

(Interview 31 March 1993: transcript pp. 2-4)

Communicating uncertainty

It would be quite wrong to conclude from this that macro-econometric modellers are engaged in a plot to conceal information about the uncertainty surrounding an economic forecast. Rather the emphasis is on communicating the error in such a way as to strike a balance between full disclosure and undermining one's credibility. Apart from the technical criticisms mentioned above, stochastic simulations also take no account of the input of the modeller. This aspect is particularly important because, as noted previously, the ESRC Macromodelling Bureau's *ex post* analyses have shown that the residual adjustments made by the modellers tend to improve the accuracy of the published version (Wallis (ed.), Andrew, Fisher and Whitley 1984). In assessing the uncertainty surrounding a forecast, some way of accounting for this added value needs to be found. The most frequently mentioned compromise is therefore for the modelling team to look back over its own past record:

Wallis: The best you can do, I feel, is to look back at what your forecasting record has been in the past. Obviously it will slowly change over time but hopefully only slowly. I would argue that a lot of the other forecasters ought to do what the Treasury does, which is report some summary statistics about past performance, so that the point forecasts are taken with a grain of salt.

(Interview 21 March 1993: transcript p. 5)

In fact this is what many forecasters actually do. For example, in the *Economic Outlook* the LBS includes details of its previous four forecasts; the NIESR forecasts contain a box detailing the mean errors of its one- and two-year-ahead forecasts for a selection of key variables. Another approach is to discuss a range of outcomes in the text. The common feature of these strategies, and their advantage from the forecaster's point of view, is that they evaluate the finished forecast and not just the model which is partly responsible for it. However, this changes the issue from the confidence limits surrounding a forecast, to the past accuracy of the model and modeller combination.⁷ These problems would remain even if the standard error of the model were known precisely, as the input of the modeller would still have

to be taken into account. In other words, as long as modellers make residual adjustments and exogenous projections, uncertainty bands will remain uncertain.

Nevertheless, given the importance of the uncertainty bands that could, and perhaps should, be drawn around an economic forecast, it seems pertinent to ask how far economists go in drawing attention to these issues. The general public are probably unaware that the apparently precise figures quoted in the media are merely the mid-points in a large range of probable outcomes.⁸ However, it remains the case that models are usually specified and estimated for a particular person and at the request of a specific funding organisation. The target audience influences both the style and the content of the published results:

Minford: In the early '80s people who took notice of our inflation forecasts made a lot of money on gilts, which was nice for them, and very important. You've got to remember who are the customers for forecasts. There are two major customers, the City and to a much lesser extent as a matter of interest, business. Business is not that interested in forecasts . . . but the City people want forecasts the whole time.

(Interview 31 March 1993: transcript p. 19)

This emphasis on customers is particularly important for those forecasting groups receiving little or no public money, that is, almost all but the NIESR, although they too have some commercial operations based on their forecasting activities. The difference that appealing to different customers makes is clearly evidenced in the following remarks:

Britton: So [there] was an attempt to become more open and more formal, and we got support in that from the ESRC, who took over the financing of model-building here and also at the LBS and one or two other places. The fact that we were doing it for a Research Council, instead for a government department, had a lot to do with the way in which it was done, and particularly the way it got written up. It became more academic, more scientific, and rather less 'Believe me, I know'.⁹

(Interview 29 April 1993: transcript p. 35)

Minford: What audience are we trying to convince about these models? Whose beliefs do we weigh the highest in this business? People who actually put their money in support of them, or academics who really don't give a damn about them?

(Interview 31 March 1993: transcript p. 19)

In addition to the more obvious difference in orientation the existence of private funding groups also creates areas of privileged information, known to some subscribers but not to others.¹⁰ For example, the LBS publishes the values of residual adjustments in the *Economic Outlook*, but does not give either the specification of the relevant equation or the reasons why the adjustments were felt to be necessary. This reflects the fact that the publication is intended for readers primarily interested in the forecast rather than the means by which it is produced. When the same forecast is written up for the LBS's forecasting consortium, this information is provided because this group is seen as 'model users'.

Social change and macro-econometric modelling

The previous section highlighted the ways in which the statistical uncertainty associated with regression analysis causes any given economic forecast to be compatible with a wide range of data. This is a proposition with which most economic forecasters would agree, and indeed they seem to take it for granted. Although it may seem to an outsider that this uncertainty invalidates the whole enterprise, it is argued that the increase in accuracy brought about by the forecasters' adjustments and fine-tuning is sufficient to make the funding of the joint project (i.e. model plus modeller) worthwhile.

Moreover, if the unavoidable statistical uncertainty were the only reason why economic forecasts are not more than approximately correct, the continued funding of model development would eventually reduce this uncertainty (as the sample size gets bigger) and thus, over time, forecasts would become more accurate. One analogy is weather forecasting, in which a continued investment in meteorological research has improved accuracy. Unfortunately, it is not a valid analogy because the economy is a social system and the processes that generate the data change over time, while meteorologists are always trying to model the same system, even though their understanding of it may change.¹¹

The following sections detail how economic forecasters resolve the tension between their use of models which presuppose continuing economic processes and relationships, and their awareness that the real economic environment is altered by policy and world events. In weather forecasting additional data can reduce uncertainty, but in economic modelling more data can mean more changes to the system, bringing about more uncertainty. The discussion begins with a brief example illustrating just how sensitive econometric models can be to changes in the data set used for their estimation.

Practical macro-econometric modelling

On one of my first fieldwork visits I arrived slightly early, and found the economist I was due to interview working with a colleague on the re-

estimation of his model. It seemed that one equation, for a series called 'SSR', was causing particular problems. In the newly estimated equation, the coefficient on one variable was given as 0.6, but the economist felt it ought to be nearer 1.¹² Initial attempts to solve the problem involved re-estimating the equation using a different statistical technique. However, because this involved removing the correction for correlation between the error terms, the re-estimated equation failed to satisfy the most basic econometric tests (the Durbin-Watson statistic being too small).¹³ Several further attempts were made to resolve the problem by re-specifying the equation, adding new variables and changing the lag structure, but these too were not successful.¹⁴

At this point the economist changed the equation back to its original formulation in order to explain to me in more detail what was going on. This time the estimated coefficient was just over 1, exactly the value the economist had been trying to achieve. A closer inspection revealed that this equation was in fact slightly different from the original version. Although the variables were the same, the sample period over which it had been estimated now began in 1970, and not 1973 as in the original case. The extra data was enough to bring the wayward coefficient back into line.

'Strong' and 'weak' econometrics

This sensitivity of coefficients and equations to changes in data illustrates one of the central problems faced by all economic modellers: the sensitivity of models to changes in the sample period over which they were estimated. Although in the example just given, historical data made the difference, it is possible for future data, added subsequently to a model, to have a similar effect. If this is the case, what warrant is there for basing economic policy on extrapolations of past relationships?

Although these concerns are well known to economists, perhaps receiving their strongest expression in the Lucas critique (Lucas 1976), economic modellers seem relatively unmoved by this seemingly fundamental flaw in their approach. Basically, Lucas's criticism is that the parameters (i.e. the coefficients) in an econometric model are a function of the policies enacted during the period over which the model has been estimated. If the model is to be used to evaluate a new policy, then parameters must be changed to reflect the effects of the new policy. However, it is just these effects that the model is supposed to be predicting.¹⁵ In principle the Lucas critique should apply to (and undercut) all macro-econometric model-based policy evaluation, although its original targets were Keynesian models which routinely imagined economic policies as affecting economic activity but not the fundamental parameters of the model. However all the econometric modellers to whom I spoke were keen to minimise its significance.¹⁶

Minford: We obviously have to accept that the Lucas critique applies to the models. We use them with our heart in our mouths, because obviously we're really betting against there being some change in the environment having mucked up our parameters. We're doing this the whole time, it's very judgmental
(Interview 3 March 1993: transcript p. 26)

Britton: I think that it evolves rather than changes abruptly, and it's based on a sort of weighted average of the recent past. The recent past might be twenty years in some cases so I don't want to exaggerate the extent to which things change. . . . The economy won't change overnight; unless we have a revolution it's not going to become a totally different economy. So the model is nearly right all the time, but it's not exactly right.
(Interview 29 April 1993: transcript pp. 4-5)

Evans: So you don't think it [the Lucas critique] applies to your kind of model?

Godley: I don't think it applies to it at all. Not seriously at all.

Evans: Even though according to Lucas all the behavioural equations in your model would change as a result of the policy change?

Godley: Well, I don't think they would.

(Interview 5 April 1993: transcript p. 1)

Thus, although the economists thought that there were ways to work around the potential problems caused by social change, there was also some disagreement about how much these changes mattered. The real issue, therefore, concerns how quickly the processes that generate economic data change. If the economy stays more or less the same, then the model parameters should remain reasonably constant too. If the economy changes more quickly, then economic models must also change as they are estimated for new data, and perhaps change quite drastically. Thus even within the conceptual space where econometric modelling is seen as worthwhile, different methodologies are possible.

This distinction between the different rates at which the processes that generate economic data change is also reflected in the relative weights given to economic theory and economic data. Those economists who believe that models should remain stable tend to have a strong commitment to a particular theoretical understanding of the economy. Proponents of this perspective are engaged in what could be termed 'strong' macro-econometric modelling.¹⁷ In contrast, those economists who allow the economic data to play a greater role in determining the specification of the model can be regarded as adopting a more pragmatic approach and being less attached to a *particular* economic theory. These economists are engaged in what may be termed 'weak' econometric modelling.

From the strong perspective, according to which the processes that generate economic data change only slowly, the criticism made of the more data-driven methodologies is that changes in the economy are neither as great nor as frequent as weak models suggest. For example:

Ormerod: If you track an individual model over time, the results which you get out of it vary enormously, which must further undermine their credibility . . . In any individual model, the multiplier will often vary from say 1 to 2 in a matter of a couple of years. Now, if that's the case, then I simply don't believe what comes out of the model. It can't be right, that it can vary so much.

(Interview 16 March 1993: transcript pp. 4, 16)

Minford: I think the properties of their model are far too influenced by little wiggles in the data. They don't think through what it is they are trying to say and have a stable theoretical framework.

(Interview 31 March 1993: transcript p. 14)

In contrast to this pragmatic approach, strong econometric modelling assumes that most of the variation in economic behaviour can be accounted for by the error terms in equations. The theoretical understanding, and hence the actual specification, thus remains relatively stable, as Minford explained:

Minford: My starting point for this is that economics is perfectly obviously not a precise science in the normal sense of the word. It is highly stochastic – relationships are very hard to pin down precisely and they shift because of all problems that Lucas drew attention to. Nevertheless, there are certain fundamental laws, basically the laws of supply and demand, which regulate economic behaviour, particularly over the long term.

(Ibid. p. 5)

In other words, the stability of the economy is mirrored in the constancy of the model. It follows from this that a good econometric modelling methodology is one in which the structure of the model remains constant over time:

Minford: We have, subject to re-estimating on quarterly data, which we did a few years ago, preserved the structure and just changed some of the parameter detail. We've not felt it was worth changing model parameters for the sake of a few wiggles in the data. The only basis on which we change model parameters is if there are major upsets to our forecasts or some major evidence that the parameters are going wrong.

(Ibid. p. 12)

In contrast to this belief in a clear and stable economic structure, other macro-econometric modellers are equally adamant that changes do occur. They argue that forecasting is still possible because the economic system changes only gradually, so the forecast period will be sufficiently similar to the recent past for the model to give a reasonable approximation to reality. However, because these gradual changes are occurring all the time, frequent re-estimation, and if appropriate re-specification, is both a necessity and a virtue:

Britton: I was surprised for a moment when you said that the trouble with the Institute model is it changes too often. In some senses the trouble with the National Institute model is that it doesn't change often enough . . . I mean, the economy changes a lot and that's the basic difficulty of economic forecasting as compared to say weather forecasting.

(Interview 29 April 1993: transcript pp. 3-4)

Within this pragmatic econometrics tradition the aim is to develop less a single 'true' model than a portfolio of analogues against which events and changes in the real economy can be compared. However, because econometric models are only informative to the extent that history repeats itself, novel events are extremely problematic, and it is in circumstances where no comparison can be made that a macro-econometric model will be of least use. Thus:

Wallis: The place where you're most susceptible to find the model incorrect is where the nature of the change is completely new.

(Interview 21 March 1993: transcript p. 9)

Using 'science' in economic modelling

Although there is clearly a diversity of opinion among economic modellers about how economic modelling should be done, there is relatively little disagreement about how these debates should be resolved. On the whole macro-econometric modellers subscribe to the traditional norms of science and believe that hypotheses should be tested against empirical data whenever possible. The mainstream view of economic modelling as conforming to traditional science values is well illustrated in the following quote, which contrasts the objectivity of scientific forecasts with the subjectivity of what would otherwise be no more than 'assertions of faith':

Britton: The feeling certainly was that the Institute was likely to attract more support from the academic community, and would actually perform a better function in contributing to the debate, if it made its approaches as scientific as they could be. Therefore everything we do should be capable of replication -

anybody who wants our database can have it; anyone who wants to run our model can have it – and we ought to give statistical criteria for what we'd decided what to do, rather than just saying 'It felt like that'.

(Interview 29 April 1993: transcript p. 4)

The other important science value highlighted in this quote is that of replication. Although replication in all sciences is frequently problematic, it raises particular problems for macro-econometric modelling and forecasting (see Collins 1992). The reason is that, for an experiment to be replicable, the technology, skills and tacit knowledge essential for its success must be diffused among the community. In terms of macro-econometric forecasting this means that the model must be made separate from the modeller. In other words, for replication to be possible the econometric model must appear as uncontroversial to economic forecasters as a voltmeter would to an engineer.

Other difficulties also make replication a problem. First, the techniques used to test hypotheses in econometrics do not appear powerful enough to resolve the disputes. Economic models are rarely uncontroversial: rather, they can be the subject of long running controversies. For example, consider the implications of the distinction between strong and weak econometrics for the characteristics of the model as a whole.

A strong macroeconomic model aims to encode the rules of behaviour or laws of economics which govern economic activity, because 'if there are any statistical relationships it must be due to something happening out there in the real world' (Britton, interview 29 April 1993: transcript p. 25). 'Science' is seen as providing the methods by which theories about the world can be tested and false ones discarded. This in turn explains the importance and popularity of regression analysis in macro-econometric modelling.¹⁸ By specifying a regression equation it is possible to identify apparent relationships between variables, or more precisely to reject, at a given confidence level, the null hypothesis that there is no relationship.

The 'strong' econometric methodology assumes there is only one true model of the economy, and that the purpose of economic modelling is to eliminate chance correlations and isolate these elusive relationships. Of course this is not to say that developing an economic model and using it to produce forecasts is a straightforward or easy task. However, it does suggest that the problems encountered will be primarily technical, associated with the limited amount of data available, the quality of this data and so on.

A second implication of the strong approach is that judgmental adjustments are not a central part of the forecasting process. They are used only as a way of incorporating information about policies to be enacted, or as a temporary way of making up for the shortcomings of the model. (See, e.g., Turner 1990.)

In contrast, in weak econometric modelling judgement is an important part of the whole forecasting process and not something added at the end to fine-

tune the forecast. Models do not supplant judgement, but externalize some of the steps taken by the modeller and preserve the structure of the overall argument. The split between the model and modeller is blurred or even dissolved.¹⁹

Britton: I think there are really two rather different approaches. One is to say that this is a branch of science and that everything must be based on objective criteria which people can understand. The other is to say that is just too inflexible, and that there's something called judgement, intuition if you like, which has its place in the sciences and that it's the people who are intuitive who are successful. There's some truth in both, I guess.

(Interview 29 April 1993: transcript p. 35)

Within the weak methodology, there are those who see the model-modeller split as at best a heuristic device that relates to the correspondence between the modeller's ideas and what is actually in the model. Others see it as wrong in principle:

Godley: The forecasts I publish are not the result of a model working independently of my mind.

(Interview 5 April 1993: transcript p. 5)

The pervasive influence of judgement in weak macro-econometrics makes replication virtually impossible. If a forecast resembles a well-reasoned argument, albeit developed with the help of a computer and deploying empirical results where appropriate, it is clear that although we may come to agree or disagree with the point of view being expressed, we would not talk about replicating it.

Weak econometric modelling also puts limits on the range of application of econometric models. It has already been noted that the usefulness of economic models tends to be limited to circumstances in which the current or expected variation in a particular data series is no greater than that which has been experienced in the past. Models can be further limited by the fact that they have been commissioned, specified and estimated for a specific task. There is not therefore a general macro-econometric model:

Wallis: There is never a true [model], but there is an economy out there and we have a lot of different representations of it and approximations to it.

(Interview 21 March 1993: transcript p. 2)

Finally, from the perspective of weak econometrics the economy has a shifting and ephemeral quality, which means that econometric models must be used with considerable caution. In particular, modellers must continually ask whether their model is still a valid approximation to the economy, or

whether change has rendered it obsolete. The precise timing and nature of any change may not be predictable, but the economic forecaster must always be looking for signs that a change has occurred so that its effects can be taken into account:

Ormerod: To say that the system will change, [and] may change at any point in time, means that it's very hard to anticipate. But I think that macro models really help to destroy thought. People should be thinking about these sorts of things all the time. I don't think it's an accident that people like Tim Congdon and Wynne Godley, who don't have 'elaborate' models which back them up, spend more of their time thinking about what are the key issues *now*.

(Interview 16 March 1993: transcript p. 8, emphasis added)

In addition, the goals of the forecasting exercise are now rather more subtle than the deterministic futurology implicit in the strong programme:

Godley: I think tables of numbers are the enemies of good forecasting. [Forecasts] should be judged by whether or not they give a good idea of what the whole situation is going to be like, what character it will have, otherwise you wouldn't bother to write anything.

(Interview 5 April 1993: transcript p. 15)

Agreements and disagreements

As was noted in the preceding section, the fact that economic models are designed for specific purposes can to some extent account for differences between them. However, similarity and difference are often matters of perspective. For example, the Treasury model of the economy differs from many others in that it has an unusually detailed representation of the public sector, because this is an area of particular interest to the government. In other respects it is a fairly standard econometric model, employing the same set of economic theory, data and methods as other mainstream models.

All the economists I interviewed were engaged (or had been engaged) in modelling the macro-economy with the aim of evaluating economic policy and producing short- and medium-term forecasts for major economic indicators. To this extent all the models had been developed for the same task, and differences between them cannot be explained on a task-oriented basis. This section examines what the modellers see as being the areas of similarity and difference between their models. In other words, despite the difficulties of econometrics, what are the features of the economy, and of economic models, that economic modellers agree about?

Macro-economic models are virtually all based on the same three major relationships. This therefore provides a common conceptual framework within

which disagreements are over orders of magnitude, lag structure and other differences of degree:

Minford: The competition we're talking about here is competition very much at the margin. It's almost like different sorts of camera, whether they catch the light in a more or less appealing way. All the features are very similar.
(Interview 31 March 1993: transcript p. 25)

Wallis: There is a kind of consensus, a general framework, which most people subscribe to. The argument really is about orders of magnitude and relative strengths, not whether anyone has a computer system that does or does not represent a particular channel of influence.
(Interview 21 March 1993: transcript p. 18)

However, not all the economists I spoke to share this area of consensus. Some question the demand-side emphasis of the mainstream IS-LM models:

Ormerod: These macro models, their problem is that the theory which lies behind them, I think, is basically wrong. The economy isn't driven by demand, it's more supply side driven; it's driven by productivity and level of profitability and investment. It's the whole approach that's wrong.
(Interview 16 March 1993: transcript pp. 6-7)

Others believe that the problem lies with the way in which IS-LM models treat the supply side of the economy:

Godley: Straightforward textbooks would have IS-LM plus a supply side - IS-LM deals with aggregate demand, but there is also aggregate supply. I would accept the IS-LM part of it, up to a point, but I wouldn't accept the aggregate supply part.
(Interview 5 April 1993: transcript p. 2)

Another, possibly more contentious, issue in macroeconomics is the extent to which explanations of macroeconomic phenomena should be grounded in the optimizing behaviour of individual economic agents; or 'micro foundations'. In macro-econometric modelling, this idea usually manifests itself as the rational expectations hypothesis.

Of the economists I interviewed, Patrick Minford is probably the most well-known UK advocate of the rational expectations approach. He believes it is one of the defining characteristics of the new consensus in macroeconomics:

Minford: All these models have IS-LM curves and a Phillips curve - you have

three major relationships. In an open economy context it's a little bit more complicated, but it's essentially an adaptation of that closed economy framework. The disputes have been on the degree of rational expectations. But that, in itself, shows you how far economists converge . . .

[In over 10 years] there's been no big challenge to what you describe, this IS-LM, Phillips curve plus rational expectations, new classical/new Keynesian framework, which they all operate in to differing degrees

(Interview 31 March 1993: transcript pp. 24, 26)

On the other hand, of the two mainstream economists I spoke to, David Currie had stopped using rational expectations altogether, and the other, Andrew Britton, was keen to differentiate himself from the full-blooded rational expectations methodology of the Liverpool group:²⁰

Britton: [I]n 1985 we introduced RE [rational expectations] in a very different way to the way in which Patrick Minford and the Liverpool model had them. What we were doing actually was taking account of output expectations in order to get a *marginally*, I mean it's not a vast difference, to try and get a marginally better explanation of employment behaviour, stockbuilding and one or two other things. In other words, decisions about volumes. It was very different to the sense in which RE was being discussed, particularly in the Liverpool model, where they were assuming that the world corresponded to a very simple model in which events were totally transparent.

(Interview 29 April 1993: transcript pp. 6-7)

Thus, despite Minford's claim that rational expectations is a defining characteristic of the consensus in macroeconomics, the mainstream econometric modellers appeared distinctly lukewarm about the idea. Other modellers were more forthright in their rejection of the need to ground macro-econometric models in microeconomic theories:

Ormerod: I think that the aim of building up macro relationships from individual maximizing behaviour is fundamentally flawed. Because of feedback in the system, peoples' behaviour is altered by observing other peoples' behaviour, and the behaviour of the macro economy may be quite different from the behaviour of an hypothesized individual on a desert island. It simply cannot be aggregated from individual behaviour.

(Interview 16 March 1993: transcript p. 14)

Godley: When people speak of micro foundations they tend to mean a very special thing, which is that it's all deducible in terms of the optimizing behaviour

of individual rational agents, and I don't accept that as an appropriate concept.

(Interview 5 April 1993: transcript p. 7)

In summary, economic modelling exists with diverse opinions about many aspects of its practice. These disputes range from the basic ontological conception of the economy (changing or stable) to the appropriate theoretical tools to use in understanding it. The existence of this heterogeneity suggests, as shown in Chapter Two, that econometric testing does not discriminate between economic theories. Of course, econometrics is not the only way in which an economic theory can be tested. Indeed there is a strong tradition in economics (usually attributed to Friedman 1953) of testing theories by the accuracy of the predictions which they make about the world. If regression analysis is, for various reasons, unable to discriminate between economic theories, can forecasts be interpreted as testable hypotheses about the economy and forecasting performance used to evaluate econometric models?

Falsification: econometrics and crucial experiments

This section examines how ideas of falsification enter into economic forecasting, and shows how economists are able to maintain their state of chronic disagreement while simultaneously upholding their scientific ideals.²¹ In particular, it tries to understand what persuades economists to adopt one specification rather than another, and what they see as being necessary in order to falsify a model or an equation. The discussion focuses on the extent to which economic forecasts are regarded as experiments, and the *ad hoc* strategies used to defend these apparently rejected hypotheses.

Attitudes to forecasting errors

It may seem intuitively obvious that if a forecast is wildly in error, that is evidence that the model is mistaken. Some economists indeed accept this, as the following quote shows:

Minford: There are two main ways in which the profession changes its view. One is if a theory makes palpable nonsense, they jettison it, or if another theory comes and makes sense they may accept it . . . The other things that prove irresistible are big forecast errors.

(Interview 31 March 1993: transcript pp. 7-8)

Testing forecast data is important because as has been noted, the modelling process tends to make it difficult for fit to past data to be used as a test of the model's validity. In a nutshell, 'it is [not] possible for econometrics to actually test economic theories over short periods of time, or on actual data' (Minford, interview 31 March 1993: transcript p. 5):

Ormerod: People can design models which may have quite important differences in their policy conclusions, but all of them are quite capable of producing models which satisfy these design criteria [i.e. econometric tests].

(Interview 16 March 1993: transcript p. 12)

Minford: One is reminded that there are quite a lot of models which are observationally equivalent in terms of their variances, but which nevertheless are quite different representations of the world.

(Interview 31 March 1993: transcript p. 30)

Perhaps unsurprisingly, not everyone agrees with this assessment:

Wallis: What one does try to do, from an econometric point of view, is to ask how good the model was, or how good the model is. And we have ways of doing that. I would be much happier taking those analyses to be the tests of the theory, rather than the failure of the published forecasts. I think the evidence in the economics community is that, by and large, they don't take forecast failures as crucial experiments.

(Interview 21 March 1993: transcript p. 14)

There is also a more sophisticated argument in which the stochastic nature of econometrics is stressed: forecasting mistakes are to be expected and are nothing more than bad luck as there is always a chance (typically 1 in 20) that the evidence will not conform to the theory.

Britton: [Forecasts] are all probability statements. All econometrics is based on the idea of probability criteria, that the likelihood of this result not being consistent with theory is below 5 per cent. You don't get the one definitive experiment which shows you that the speed of light is the same in all directions.²²

(Interview 29 April 1993: transcript p. 17)

Alternatively, the idea of a quantitative falsification can be rejected as inappropriate:

Godley: I don't really agree with that way of looking at it. If you're going to look at the forecasting operation as being GDP going up 3 per cent and

that kind of thing, I wouldn't accept that . . . The concept of judging a forecast in terms of a number and its relationship to another number [is wrong]. A forecast ought to convey the whole character of the forthcoming period; it shouldn't be thought of as being an entirely quantitative thing.

(Interview 5 April 1993: transcript p. 3)

The argument thus turns on the appropriate test for an econometric model. The remainder of this section demonstrates that whichever option is chosen, econometrics or forecast mistakes, the conclusions are always ambiguous because *ad hoc* strategies are always available. To give an example and set the scene, consider what (if anything) was proved by the UK's exit from the Exchange Rate Mechanism (ERM) in September 1992. Was it that the UK had entered the ERM at the wrong rate? Was it that fixed exchange rates never work? Or was it that the Bank of England and the government made a series of tactical errors?

Ad hoc responses to econometric tests

As has already been noted, it is extremely difficult to get a 'clean kill' on a theory through the econometric testing of models. Economists justify this in various ways. First there is the sensitivity of econometrics to the sample data:

Minford: I think most econometrics is rubbish, frankly. There's an awful lot published in the journals, where people purport to have tested something, but it's baloney because there are five other runs on the same data with slightly different specifications, with slightly different sample periods, that have either supported the [null] hypothesis or only marginally discounted it.

(Interview 31 March 1993: transcript p. 6)

Second there is the way in which the variable is measured or used within the model:

Ormerod: Even within the same political framework, what is a relevant definition of wealth in a consumption function? There are many different ways of looking at it. For example, is it important to consider the impact of inflation on income or not?

(Interview 16 March 1993: transcript p. 12)

Britton: If you look at the theory which says that real interest rates should influence investment, it's very difficult to find many studies which convincingly show it does. This is not necessarily to say it has no effect, but just that for practical purposes this effect, which theory would

emphasize, is actually rather difficult to see in the data. Of course, there are always reasons [and] the way in which the falsification of one hypothesis generates the next one makes it a little more complicated - it's not just the real rate of interest, it's the post-tax real rate of interest, and so on.

(Interview 29 April 1993: transcript p. 18)

Finally, there is the limited amount of data available:

Britton: I think the reason why these statistical methods we use don't produce success more often is that we're always dealing with a sample which we know is too small. If you asked, you know, what size of sample would you like to have in order to estimate the relationship between imports and activity using quarterly observations, I'd say about 300 years would begin to be adequate. We've actually got about thirty.

(Ibid. p. 25)

For these and many other reasons, econometric evaluation is always open to re-interpretation at the hands of a determined sceptic. This is not to say that there is necessarily anything wrong with this. Indeed, that scientists use *ad hoc* strategies to defend their beliefs is common currency in many philosophies of science and should surprise no one. The point is simply that, taken alone, econometric evidence is unlikely to persuade anyone to hold or reject a particular theory.

Ad hoc responses to major forecast mistakes

The rhetoric of the 'big forecast mistake' has straightforward down-to-earth appeal, in contrast to the technical and evasive qualifications of formal econometrics. The argument is that because econometric tests are known to be sensitive to the sample period used, economists are relatively unimpressed by them. In contrast, a model that can reliably predict the future is a model that commands respect.

An obvious prerequisite for applying the 'big forecast mistake' test is to tell when a forecast is clearly wrong. This can be rather difficult in practice. To begin with, some forecast mistakes are more important than others. For example, when asked why the Liverpool model had not been falsified when it completely failed to predict the 1980-1981 recession, Patrick Minford replied:

Minford: I'm talking about the big errors. Now of course, we didn't call the recession we saw in 1980. We forecast that the Thatcher policies would deliver a mild recession, a growth recession, but a tremendous drop in inflation. Other people were saying there'd be something more of a recession but

no drop in inflation.

(Interview 31 March 1993: transcript p. 6)

Needless to say, there are those who would however argue that predicting that output would remain constant when, in fact, it contracted by nearly 3 per cent must surely count as a big forecast mistake.²³

The 'rightness' too of a forecast has to be established and is open to dispute. It is possible for a numerically correct forecast to be discounted because it was right for the wrong reasons:

Minford: Cambridge Economic Policy Group . . . got unemployment right because they were very bad on output. They were too optimistic on their unemployment-output relationships, so they got unemployment right but they got the mix wrong, rather than unemployment, like everybody else. So I don't think people were terribly impressed. They just felt they were far too pessimistic on demand and output and that they had got unemployment right by mistake.

(Ibid. p. 11)

Again this perception is not shared by everybody:

Godley: Cambridge Economic Policy Group was very good.

Evans: For unemployment, but Patrick Minford's group was very good for inflation. There's an article by Ken Holden.²⁴

Godley: Our forecast for inflation was very good.

Evans: But if you compare the errors, who did the best ones, then apparently Liverpool are the best for inflation.

Godley: I don't believe it's true.

(Interview 5 April 1993: transcript p. 9)

In addition to being the lucky result of mistakes elsewhere in the econometric model, a correct forecast can be produced by chance. This confers no credibility on either the model or the modeller. For example, Tim Congdon is generally credited (at least by fellow monetarists) with calling the inflation of 1989 and 1990 very accurately. However, non-monetarists are less convinced, arguing that he had forecast the inflation earlier but it had not happened. As a result, it is possible to question the eventual success of the later forecast.

Even in the case of an acknowledged forecast failure, there are a number of reasons why a discrepancy between the forecast and the outturn will not necessarily lead to the rejection of the model.²⁵ First, the data used to estimate the model may subsequently be revised, thus changing the definition of the recent past and hence the parameters of the model. Second, the projections made for exogenous variables may be wrong, and these may invalidate the

forecast. Again, this does not imply that the model itself was at fault: had the exogenous conditions been fulfilled the forecast might have been correct.²⁶ Third, some of the judgmental adjustments made might, with the benefit of hindsight, be regarded as inappropriate.²⁷ It is only after all these possibilities have been exhausted that the mis-specification of the model becomes an issue. Thus, even if the modellers agree that there has been a forecast mistake, *ad hoc* rescues are always possible, particularly if the economists are very committed to their model or theory.²⁸

Finally, even if it does eventually have to be conceded that the model is mis-specified this need not reflect too badly on the modelling team. In discussing the NIESR's failure to predict both the boom and recession phases of the last economic cycle in the UK Andrew Britton explained:

Britton: [It's possible that the model was always wrong] but in fact that wouldn't have explained why forecasting performance had deteriorated. The fact that virtually all the models, all the formal fully developed models failed to predict, suggests that it was not that our model was particularly bad, but that the underlying economy had changed.

(Interview 29 April 1993: transcript p. 11)

In other words, the problem is that the economy no longer matches the model. This observation brings us back to the beginning of this section where it was noted that change in the economy can render econometric models obsolete as representations of the economy.

Achieving closure

Thus far, this chapter has shown how the fluid and open nature of macro-modelling is maintained. Because econometric testing is chronically ambiguous no one model, theory or specification can be shown to be unambiguously superior to the rest. In addition, forecast mistakes are similarly ambiguous and even when acknowledged do not force any particular course of action on the modelling team. This means that economic modellers are trapped within an experimenters' regress from which they are unable to escape. Because they are unable to agree what the correct outcome should be, they can never know when an experiment has been carried out competently, thus establishing the veracity of the original knowledge claim.

In this situation, economists must resort to non-econometric methods to buttress their arguments. This raises the question: what persuades economists to adopt one course of action rather than another if the 'formal' procedures are indeterminate? Are there other sorts of reasons for believing an economic forecaster, and if so what are they?

Minford: I think the econometrics is important, [but] it's only one fairly moderate

constituent in the whole process of fielding a competitive modelling effort. You have to have a theory that is at once decent, consistent and respectable; persuades you and others. It also has to be sufficiently simple to be implementable. It has to be reasonably stable over time so it's recognizable. And then it has to pass these much more informal tests of can you tell stories about what's going on? Can you, if you have made a forecasting error, can you say why? What is it that you got wrong? Obviously it's got to avoid major forecasting catastrophes, that's absolutely crucial.

(Interview 31 March 1993: transcript pp. 22-3)

It seems therefore that, as McCloskey (1986) has argued, the supposedly scientific part of econometric modelling, econometric testing, does not count for much among economists. It is McCloskey's belief that economists are ultimately persuaded not by statistics, but by a well-reasoned argument, clearly and lucidly expressed. From my own interviews it seems clear that this view is shared by others:

Evans: What actually does resolve disputes in economics?

Godley: Nothing!

Evans: They just go on. Well, they certainly seem to.

Godley: Successful rhetoric is what resolves issues.

(Interview 5 April 1993: transcript p. 17)

Conclusions

This chapter began by examining the statistical uncertainty associated with an econometric model and its forecasts. It was argued that if the 'error bars' which bound an econometric forecast were to be formally estimated they would enclose a wide range of economic outturns, and that this range might be large enough to undermine the credibility of the whole enterprise. It was then shown how this uncertainty is reduced and managed by economists, principally through re-defining the question into one about past errors and limiting the dissemination of information.

The second theme developed in this chapter has been the interpretative flexibility of macro-econometric modelling. Interview quotes have been used to demonstrate the openness and variety of discourses within macro-modelling, and to illustrate how economic data are capable of sustaining a variety of alternative hypotheses. The aim throughout has been to show how each claim has its counter-claim, how each position in every argument is capable of supporting an alternative hypothesis. Thus, econometric models are seen by some as objective and replicable; to others they are inextricably bound up with the modeller. As with other branches of science, tests are subsequently seen to be inadequate and falsified hypotheses are rescued. However, and perhaps this

is where econometric modelling differs from other branches of science, the situation in which many different models are equally compatible with the evidence and no one specification is unambiguously superior to another seems to have become an institutionalized way of life.

Were economic modelling a less important science, this might not matter too much. However, given the importance of the economy, and of economic policy, for everyday life, economists and policy makers routinely have to find some way of choosing between economic models (and modellers). The next three chapters discuss one response to this situation: the UK government's Independent Panel of Forecasters. First appointed in 1993, the Panel's membership reflected the full range of economic views and, if nothing else, ensured that economic policy advice was based on the full range of available analysis.

DIFFERENT MODELS

Economic forecasts in February 1993

Introduction

Chapters Two to Four discussed how economic models and forecasts are made. The analysis now turns to how they are used in the hands of professional economic forecasters charged with advising the government. This and the succeeding two chapters examine the forecasts produced by the Panel of Independent Forecasters, the ‘Seven Wise Men’, during 1993, and show how the uncertainty, judgements and econometrics described in the preceding chapters affect policy making. They highlight both the problems that arise when economists are asked to give policy advice and the ways in which the Panel sought to resolve them.

It will be helpful to identify the main dimensions along which the different economic analyses and recommendations put forward by the individual Panel members can be distinguished. Although there are a great many economic variables that could be considered, this discussion will focus on the relationships between a few: the trade-off between GDP growth and inflation, the relationship between unemployment and inflation, and the accounting identity that links debts in the private and government sectors to trade in the international export markets.

After briefly outlining the main interactions highlighted in these relationships, the chapter goes on to show how they were interpreted and used in the forecasts for 1993 produced by the Panel of Independent Forecasters. The aim is to show the consequences for economic policy advice and policy making of the interpretative flexibility of econometric evidence and forecast tests. In particular, it will be argued that economic theory, practice and evidence are compatible with two distinctive (and to some extent contradictory) positions.

Main economic relationships

Although economic models are undeniably complex achievements, it is important to remember that much of their apparent complexity derives from

attempts either to disaggregate the analysis to more useful levels of detail, or to transform data gathered for some other purpose into operationally appropriate measures of economic concepts (Holly 1991). Lying beneath this complexity is a much smaller number of economic relationships, and it is these that are the focus here.

The relationships that structure economic forecasts are essentially simple ones, at least in principle. It is possible, therefore, to understand quite a lot about the economy, and the difficulties of economic forecasting and providing policy advice, by considering a relatively small number of variables. Growth in GDP, unemployment, inflation, the trade deficit, government borrowing and the financial surplus held by the private sector together form the basis of three important relationships used in macroeconomic analysis and forecasting. They therefore provide a convenient way into understanding the different views of members of the Panel of Forecasters and these relationships can be summarised as follows:

- 1 *The track-off between the GDP growth rate and the rate of inflation.* This is particularly important for policy makers as the success of economic policy is often judged on the extent to which economic output is maintained or increased without causing inflation.
- 2 *The relationships between inflation and unemployment* which link prices and output to behaviour in the labour market. This is another key area for policy makers and economists. From a sociological perspective it is through this relationship that social institutions such as trade unions and the welfare state enter into economic analysis.
- 3 *The national income accounting identity* which links debts in the private and government sectors to net trade performance via the balance of payments on the current account (often referred to as the 'trade deficit' when it is negative). This is important because it links the financial behaviour of the domestic economy to its performance in international markets.

As with the discussion on econometrics and forecasting in Chapters Two and Three, it is not suggested that these three relationships comprise the whole of economic analysis. Clearly variables such as growth in GDP or unemployment are measuring economic activity at a high level of aggregation, and must be underpinned by the analysis of, for example, consumer spending, commercial investment decisions and labour market performance. The advantage of focusing on this more general level of analysis is that the key differences which economists have yet to resolve are more readily apparent.

The growth-inflation trade-off

Of these three relationships, the trade-off between growth and inflation is perhaps the most widely covered in the news media. The fundamental

relationship is very simple: increasing the rate of growth of GDP tends to raise the rate at which prices increase, while keeping inflation low tends to depress the growth in GDP, so the policy goals of healthy growth in GDP and low inflation are to some extent in opposition. The goal for macroeconomic policy is to ensure that there is enough economic growth to maintain prosperity without generating excessive inflation and thus to avoid the 'boom and bust' of deep economic cycles and, ultimately, recession. Taxation, spending, interest rates and other policy instruments are manipulated in order to achieve this end.

The difficulty is not just that a good outcome on one measure tends to imply a poor performance on the other: it is that it is necessary to judge the level of compromise required in order to maintain both measures within acceptable limits. In particular, some idea of how much GDP can grow before inflation reaches 'unacceptable' levels would clearly be very useful. Unfortunately this is exactly what economic forecasters are unable to provide. It will be shown that a key difference between the Panel members is over the rate of inflation which is consistent with a given growth rate.

Inflation and unemployment

The relationship between the rates of inflation and unemployment is slightly more complicated than the growth-inflation trade-off as it involves two separate effects. First, there is a relationship between unemployment and inflation which is known as the Phillips Curve. The Phillips Curve is based on the idea that there is a rate of unemployment at which the labour market would be in equilibrium and at which inflation would be steady. This rate of unemployment is also known as the non-accelerating inflation rate of unemployment, or NAIRU. If the rate of unemployment goes above this steady-state rate the competition for available jobs will increase, subduing wage increases and holding back price inflation more generally. If unemployment falls below this rate, firms must compete harder for the available workers with the result that wages increase, wage costs are passed on to customers, and prices increase. The policy issues thus concern identifying the rate of unemployment at which inflation will be more or less stable and estimating the effect of any deviations from that rate. Unfortunately there is little agreement among economists either on the level of equilibrium or on the impact of deviations from it.

The second factor that links the rates of inflation and unemployment is the rate of inflation itself, or perhaps more accurately, the *expected* rate of inflation. As inflation rises, or is expected to rise, so wage claims are also expected to increase, leading to a spiral effect as expected inflation and wage increases reinforce each other. The result is that the rate of unemployment increases as workers begin to find themselves 'priced out of jobs'. On the other hand, if inflation is expected to be low, this will tend to reduce wage

inflation and maintain employment. From this perspective the challenge for economic policy makers is to set credible inflation targets and create plausible mechanisms for achieving them. This has been one of the key concerns of UK economic policy and, for example, was a core argument for joining the European Exchange Rate Mechanism.

Balancing the books: the budget deficit, net trade and the private sector

The link between net trade, private sector debts and the government deficit rests on the definition of these terms:

$$\text{Balance of Payments} = \text{Budget Deficit} - \text{Private Sector Surplus}$$

There can be no disagreement about the way in which the balances held in different sectors of the economy are related to each other through the national income accounts. However, there is certainly disagreement over the ways in which those balances are, or should be, distributed.

Because of the way in which these terms are defined, if the private sector's financial surplus were to be stable (which it used to be), then any increase in the government's borrowing must necessarily be matched by an increase in the deficit on the net trade account and vice versa. This insight, that net trade was a key issue for government policy, was the basis of much of the New Cambridge economics of the 1970s.

However, the assumption that the behaviour of the private sector is more or less stable (or at the very least, that any changes will be predictable) is crucial. Since the 1970s not only has the size of the private sector surplus changed quite dramatically, it has also proved very difficult to predict when and by how much these changes were going to occur. As a result although the identity is obviously still true, applying it to policy analysis is problematic. For example, if the government deficit were small a significant trade deficit could still result if the private sector surplus were large enough. The question which policy makers need to address is whether this outcome should concern them. Perhaps unsurprisingly, this was also an issue on which the Panel of Forecasters was split.

Summary

The three major relationships that have been identified, each of which economists can draw on when interpreting economic data and making forecasts, suggest three stylised facts about the economy. First, high levels of economic growth are accompanied by increasing inflationary pressures. Second, low rates of unemployment or high inflationary expectations will increase the rate of inflation. Third, to the extent that private sector financial

surplus is stable, increases in the level of government debt will be matched by a deterioration in the balance of payments on the current account. The problem for policy makers and analysts is that although these relationships feature in one form or another in all macroeconomic models, they are all quantified in different ways. As a result, their application to any specific economic situation invariably leads to widely differing accounts, forecasts and recommendations. Just how divergent these accounts can be is the subject of the rest of this chapter.

Forecasting 1993

The Panel of Independent Forecasters met for the first time in February 1993. Their terms of reference required them to produce economic forecasts and policy analysis and to make policy recommendations to the Chancellor. At this time their forecasts for the year ahead ranged from reasonably optimistic to rather bleak; for example, for GDP growth in 1993 they ranged from a high of 2.0 per cent to a low of 0.2 per cent. In addition, and perhaps more interestingly, the kind of growth forecast differed quite dramatically. For example, some of the Panel forecast that the source of the increase in output would be the expansion of domestic demand, with exports either contributing little to the recovery or having a negative effect. Other Panel members predicted that growth in GDP would be attributable to increasing success in export markets rather than activity in the domestic economy. As a result, the Panel's February 1993 Report commented: 'The average of our forecasts for *domestic demand* is a rise of 3/4 per cent in 1993 but the range stretches from -1 to 1 3/4 per cent' (HM Treasury 1993a p.5 para. 6, emphasis in original).

One reason for this variation was the considerable uncertainty that remained about what had actually happened the previous year. For example, in February 1993 the figures for domestic demand in 1992 ranged from -0.5 (Godley) to 0.2 (Congdon), and for net trade they ranged from -1.4 (Congdon) to -0.4 (Godley). In other words, although there was a general agreement that GDP had contracted by about 0.8 per cent in 1992, there was much less consensus about where the contraction had occurred. Looking forward to the year ahead, these differences continued to be important.

Forecasts for growth in GDP

Domestic demand

Domestic demand is made up mainly of consumer expenditure but also includes government spending and business investment. This last is particularly volatile and is especially difficult to forecast.

The most optimistic assessment of domestic demand was provided by Andrew Britton of the NIESR, which had forecast an increase of 1.8 per cent. In his submission, Britton emphasized that based on past experience an economic recovery was due. The cuts in interest rates that had occurred since September 1992 were seen as continuing a series which stretched back over two years, and were interpreted in this context. Their effect was to add strength to the nascent economic recovery:

Normally we would not expect cuts in interest rates to act at all quickly to stimulate domestic demand, either from consumers or business. If there is indeed the beginnings of a revival in the housing market and some sectors of consumer spending this is now long overdue in view of the cuts in interest rates (from 15 per cent to 10.5 per cent) made between 1990 and 1991. The effects of the more recent reduction (from 10 per cent to 6 per cent) should be reinforcing recovery throughout this year and beyond.

(Britton 1993b: 18 para. 6).

Similarly optimistic forecasts were produced by David Currie and Gavyn Davies (1.6 per cent and 1.4 per cent respectively), although it should be noted that Gavyn Davies's optimism was conditional upon the government taking further action, in particular cutting interest rates still further, to boost economic activity. Without this action by the government, Davies's analysis was much closer to his more pessimistic colleagues who did not expect a significant recovery in domestic demand.

Davies shared the concern of other Panel members that the unusually high levels of indebtedness in both the corporate and household sectors would prevent domestic demand from recovering in line with past experience. One key difference between the forecasts was the allowance made for this factor. Britton seemed to make very little allowance, forecasting that the economy would respond in a more or less typical manner. Wynne Godley, in contrast, argued that the debts would hold back both household consumption and business investment for the rest of the year:

House prices have continued to fall in real terms and little recovery is in prospect in view of the stock of unsold, often repossessed, houses still on the market. As houses provided so much of the collateral for household indebtedness, the fall in prices will act as an additional bromide on borrowing, while having a negative wealth effect on personal consumption.¹

(Godley 1993b: 59 para. 2(b))

There were thus two conflicting forces acting on the economy: the cuts in interest rates following the devaluation, which would have tended to boost

activity, and the high levels of debts following the recession, which would have tended to depress it. Unfortunately for the Panel of Forecasters, this situation was unprecedented, which made forecasting particularly difficult:

Trends in consumer spending have been notoriously difficult to predict in recent years with traditional forecasting methods underestimating both the strength of the consumer boom in the late 1980s and the weakness of consumer spending in the recession. Consumer spending decisions are currently the subject of two conflicting influences. High consumer indebtedness, coupled with the weakness of the housing market is acting to inhibit spending while the historically low level of interest rates now established should encourage higher spending. As this combination of circumstances has not occurred before, its outcome is particularly difficult to predict.

(Sentance 1993a: 77 para. 15)

A similar effect made forecasting investment equally difficult. Even as late as July 1993, the Panel continued to disagree about what would happen that year: the July Report contained forecasts from Congdon, Godley and Sentance suggesting that investment growth was likely to be weak alongside forecasts from Britton and Currie which drew the opposite conclusions from the same data. The investment optimists stressed the potential for the manufacturing part of the total investment picture and discounted the negative effects of the recession on the construction part. They thus came to a fairly positive conclusion.

Even though surveys suggest that companies are at present operating below capacity, our view is that new investment will be profitable and will go ahead on this basis. We are forecasting a rise in whole economy investment next year of as much as 7 per cent.

(Currie 1993b: 40 para. 16)

The pessimists, on the other hand, minimized the importance of manufacturing and emphasised the weak state of the construction industry:

There is an overhang of excessive commercial building - a legacy from the property boom of the late eighties - which will act as a deterrent to new investment. In addition corporations are over indebted and banks still have weak balance sheets as a result of bad debts acquired during the eighties boom which makes borrowing for business investment in fixed and working capital unusually difficult.

(Godley 1993b: 59 para 2(e))

It is interesting to note that the reasons are almost mirror images of each other. For the optimists, manufacturing dominates construction; for pessimists, construction dominates manufacturing. Similarly, for the optimists, the survey data is relatively unreliable and does not accurately reflect the economic situation. Thus, for example, Andrew Britton explained the NIESR forecast as follows:

Britton: We are saying that the surveys have been too pessimistic and that when they filled those [CBI] surveys in, the firms didn't realise the extent of the recovery that was coming along . . . I think that the construction sector has been particularly hard hit in the recession, and there was over-investment in the 80s, particularly concentrated in commercial property, so that it may turn out to be the sector which lags behind . . . [But] I don't think that the problem with commercial property need hold back the other sectors.

(Interview 12 July 1993: transcript pp. 10-11)

In contrast, Andrew Sentance (who worked for the CBI at the time) took a rather different view. When asked why he did not think that the increasing confidence of manufacturing firms would be enough to turn the investment total around, it was also survey evidence that he cited:

Sentance: You are going to have to get a very big increase in manufacturing investment, but we are not seeing that in our surveys yet . . . The last time we did a survey of investment intentions, there were still more manufacturers planning to cut investment than increase it. So I would want to see more evidence from our surveys to convince me that investment was about to rise.

(Interview 13 July 1993: transcript p. 14)

It would now be possible to determine whether it was the pessimism of Sentance and Godley that was warranted or the optimism of Britton and Currie, but the important point is that the same data was used to justify both conclusions.

Net trade

The second element in the forecast for GDP growth was net trade. Although the forecasts produced by the Panel arrived at different conclusions, they were generally informed by similar considerations: one, the effects of the devaluation on the UK's competitiveness' and two, conditions within the markets to which the UK typically exports.

The effects of the devaluation were not disputed and in fact were hardly mentioned at all in the report. The mainstream view from which nobody seemed to deviate or dissent was given as follows:

The 15 per cent devaluation provides an important impetus, boosting export volumes and holding back imports.

(Currie 1993b: 34 para. 7)

As on previous occasions when sterling has fallen sharply, we expect the gain in relative cost competitiveness to result in improved profit margins for exporters as well as a substantial gain in export volume . . . Devaluation also provides an incentive for import substitution. It is encouraging therefore that import volumes (on a balance of payments basis) fell in the fourth quarter.

(Britton 1993a: 17 paras 3, 5)

The second factor determining the forecasts for net trade was the conditions in the export markets. Although the world economy was growing, which might seem to augur well for the UK's export markets, the European economies into which the majority of UK exports are sold were expected to be very weak in 1993. This meant that the demand for UK exports might not be particularly strong, despite the fall in the exchange rate. Andrew Sentance summed up the situation as follows, using the same 'conflicting forces' metaphor as for domestic demand:

[G]rowth in the OECD area - which dominates world trade - is subject to two conflicting pressures. Growth in the US appears to be gathering momentum while some key European economies - notably Germany - are sliding into recession with little sign yet of any significant reduction in German interest rates. The pace of growth in UK export markets depends crucially on which of these two influences dominates.

(Sentance 1993a: 77 para. 13)

The consensus amongst the Panel was that the weakness of European markets would tend more or less to cancel out the gains from increasing exports to the rest of the world. This is reflected in the fact that five out of seven forecasts lay in the range -0.1 per cent to +0.3 per cent. In other words, most of the Panel expected that net trade would make no more than a minor contribution to GDP growth during 1993. This is made clear in the following comments:

Although the recent devaluation of sterling will assist British exports, the developing recession in Europe will depress the demand for UK exports - probably by more than the recovery in US growth will boost it.

(Godley 1993b: 60 para. 2(f))

Net trade is expected to make a positive contribution to growth over the next two years, adding 0.3 per cent to GDP in 1993 and 0.7 per cent in 1994 . . . In 1993, the growth of exports will be held back by sluggish European markets – especially Germany – which will reduce the growth of world trade despite signs of a strengthening recovery in the United States.

(Sentance 1993a: 75–6 para. 6)

Slow growth in the world economy this year will moderate the pace at which UK exports can expand.

(Britton 1993b: 17 para. 4)

However, there were two significant exceptions to this view: Patrick Minford, who forecast a positive contribution to GDP of 1.3 per cent, and Tim Congdon, who forecast a positive contribution of 0.7 per cent. This was not merely a numerical difference reflecting the operation of statistical and other uncertainty in the ways outlined in Chapter Four. Econometrics allows several different interpretations of economic data to coexist and the differences between Minford and Congdon and the rest of the Panel are differences of economic theory. They identify a different set of causal links between the various parts of the economy and suggest a very different agenda to the policy makers.

In particular, Congdon and Minford made much more use of the idea of equilibrium. They emphasized that the recession that had preceded (if not precipitated) the devaluation had pushed the UK economy a long way from its long-run trend position. By this they meant that it had created a gap between the measured output of the economy and an extrapolation of what that output would have been had the economy enjoyed average economic growth in the previous few years. Their judgement was that any increase in the rate of economic growth that closed this output gap would not lead to an increase in inflation because it was merely correcting the disequilibrium in the economy. By projecting a future in which this output gap was reduced by a relatively strong export performance, Minford and Congdon also projected a relatively optimistic future for the economy as a whole. The key policy question is whether the policy makers should have believed them or the less optimistic majority.

Unemployment/wages

The second important consideration for both economists and policy makers is the rate of unemployment. In February 1993 the forecasts for unemployment were broadly similar, predicting a slight increase from 2.9 million at the beginning of 1993 to between 3.1 and 3.4 million by the end. For 1994, the

Panel forecast that unemployment would fall slightly, but with one exception they nevertheless expected it to remain at or above 3 million.

The reason the majority were not expecting a significant fall in 1993 and 1994, despite the increase in economic activity that they were all expecting, was the effect of any fall in unemployment on inflation. In particular, they expected that any expansion of demand that would be sufficient to reduce unemployment would also increase inflation to such an extent that deflationary policies would be required to contain it. Effectively the majority of Panel members were claiming that the rate of unemployment at which inflation would be stable was only just below the level currently being recorded. The government's scope for manoeuvre was therefore very small and unemployment was, as David Currie observed, an 'intractable' policy problem:

Unemployment is likely to rise to 3.25 million this year and to decline only slowly thereafter, dipping only marginally below 3 million in 1996. This is an intractable policy problem. The danger is that the policy response will be to go for growth to bring unemployment down, raising inflation and pushing the UK into another inflationary boom/bust cycle.

(Currie 1993a: 34 para. 10)

If Britton, Congdon, Currie, Davies and Sentance represented the central tendency of the group, then Wynne Godley and Patrick Minford were the outliers. Godley's was the highest forecast for unemployment and it was consistent with his bleak forecasts more generally. Minford represented the more optimistic view. In the short term, he thought that unemployment would continue to rise as economic growth remained weak during 1993 in the absence of interest rate cuts:

Unemployment is rising rapidly still, and companies are cutting costs sharply, having finally given up on any sort of rapid recovery.

(Minford 1993a: 67 para. 1)

However, in the longer term he took a much more optimistic stance based on his view of the UK labour market reforms enacted by the Conservative government during the 1980s, combined with the effect of the recession on inflation expectations. In particular, Minford believed that the level of unemployment at which inflation would be stable was much lower than the majority of the Panel were suggesting. He argued that it was about one million compared with the 2-2.5 million implied by the other Panel members.

As with the discussion of growth and inflation, this was not so much a matter of different judgements, as of different interpretations of the same data. As Minford himself made clear, both estimates of the stable-inflation rate of

unemployment agree that competitiveness in the labour market is determined by the same factors:

There are two available types of analysis of this [i.e. the labour] market. One (as in my own work) assumes that at least in a significant part of the labour market there is a competitive supply and demand for labour: there are other parts that are either monopolised by unions or monopolised by firms, but anyone failing to find work in those must look for it in the competitive part. The other type of analysis (pursued in many papers by Professors Steve Nickell of Oxford and Richard Layard of LSE) assumes that throughout the labour market firms bargain with unions or other worker groups for wages and set prices in response to given wages . . . Both types produce similar conclusions: that unemployment benefit (its rate and availability), taxes and union power are the principal determinants of unemployment.

(Minford 1993a: 68 paras 8-9)

The controversy, or disagreement, arises because there are different ways of understanding this economic data which give very different answers about the competitiveness of the labour market. In particular what set Minford apart from the rest of the Panel, who more or less followed the Layard-Nickell line on unemployment, was his assessment of the effectiveness of government policies in changing labour market behaviour in the UK. Minford believed that these policies, which included trade union and welfare benefit reform, had made a major difference to the UK economy:

One is struck by how massively the labour market environment in this sense has changed since 1979, when both approaches suggested that the UK natural rate of unemployment would be high - up to 3 million or 11 per cent. Cuts in benefit rates relative to productivity-linked wages, tougher eligibility for benefit including the renewed Beveridge 'worktest' as in the 'Restart' scheme, union laws, cuts in marginal tax rates, and so on. The Liverpool estimate of the natural rate that results from applying these ideas is down to around 1 million - approximately 3.5 per cent of today's labour force.

(Minford 1993a: 68 para. 10)

In other words, Minford believed that the competition for employment was very intense and that as a result significant economic growth could be achieved before labour market and wage inflation created the need for policy interventions. The rest of the Panel (with the possible exception of Congdon) believed that these labour market policies had not had much effect and that, for a variety of reasons, the natural rate of unemployment was around two to

three million. In other words, even relatively minor economic growth would be sufficient to precipitate a spiral of wage and price inflation.

Again, policy makers were faced with a choice between economic theories that economists themselves had been unable to make. Specifically, how effective had the labour market reforms initiated by previous Conservative governments been in changing labour market behaviour? The importance of the different views expressed can be seen even more clearly when we consider the forecasts for inflation, the trade deficit and the budget deficits.

Inflation

Although there were differences between the Panel on almost all issues, the forecasts for the rate of inflation showed the greatest differences. The majority of Panel members were generally pessimistic about the future, forecasting relatively high inflation at the same time as low rates of GDP growth. The more monetarist members (Minford and Congdon) were rather more optimistic. As has been shown, they saw the high level of unemployment and low level of output created by the recession as exerting a powerful downward pressure on any potential increase in inflation. Indeed, Congdon's central conclusion was that 'above-trend growth can be reconciled for several years with low inflation' (Congdon 1993c: 25 para. 2).

To support this argument Congdon drew on both a theoretical analysis - 'the model of wage determination developed in Friedman's 1967 presidential address to the American Economic Association' (ibid.) - and econometric evidence - 'an inflation equation used at Lombard Street Research' (ibid).

However, the theory and econometrics do more than support his inflation forecast. They also provide a way of filtering and weighing up other evidence. For example, in the following discussion, the positive indications from other sources are subordinate to the effects due to the output gap: However, the January CBI survey (with a positive balance of companies on the 'output expectations' question) and the December Building Societies Association press release (with a sharp increase in seasonally-adjusted mortgage commitments) point to a resumption of growth in the first half of 1993, although at a beneath-trend rate. Although the pound may fall further and the devaluation will have an impact effect on the price level, the negative output gap will continue to increase again and underlying inflationary pressures will continue to weaken.

(Congdon 1993c: 26 para. 5)

Thus, despite some growth in the economy during 1993, Congdon forecast that the negative output gap would continue to increase, albeit at a slower rate, and that inflation would therefore remain low and probably fall further (to just

1 per cent) during 1994. Minford's analysis is basically similar, although it relied more on the idea of excess unemployment rather than the output gap. He was also less optimistic than Congdon in the short term.

The rest of the Panel were distinctly pessimistic, although some were more worried than others. What they shared – the reason the group as a whole was pessimistic – was the expectation that there would not be any great improvement in the economic situation before inflation became a problem. In other words, for this group the growth–inflation trade-off was much more adverse than it was for Minford and Congdon. In forecasting terms, this pessimism was manifested as the expectation that inflation would remain above 3 per cent even though unemployment was also expected to remain high and economic growth to remain low.

Let us consider these more pessimistic forecasts in more detail. The highest forecast for inflation at the end of 1993 was produced by Wynne Godley, who expected it to rise to 4.8 per cent in the final quarter. However, this increase mainly reflected the temporary effects of the devaluation and Godley's forecast for inflation at the end of 1994 was slightly lower at around 4 per cent. Although this was closer to the average of the other forecasts (3.6 per cent) it should be noted that this was in the context of below-trend growth and rising unemployment. Taking the forecast as a whole, Godley was therefore deeply pessimistic.

Andrew Britton and David Currie both forecast a similar combination of continued inflation and weak growth. In particular, both predicted a rise in wage demands following the devaluation, and both saw temporary breaches of the official inflation target of 1–4 per cent as likely during 1994. Currie's comments were:

We see inflation remaining low over the next year, but then picking up as a consequence of devaluation. Headline RPI inflation averages about 2.5 per cent this year, partly as a result of cuts in mortgage rates, but then picks up. The Chancellor's target indicator, RPI excluding MIPs, remains at the top end of the target range during 1993, but then rises above it in 1994.

(Currie 1993a: 34 para. 8)

The predicted rise in wages which led to the relatively high inflation forecast was described as follows, with a heavy emphasis on the 'typical' as opposed to the 'particular' or 'unusual':

If higher inflation feeds into higher wages, there is the danger that the competitive advantage of a lower pound is eroded in a wage/price spiral: this occurred in the three years after the 1967 devaluation, and is the characteristic response of the British economy. Over the next year, with unemployment high and rising,

we see no such danger. But in 1994 and beyond, with unemployment stabilising and growth at its trend rate, we see earnings responding to higher inflation, and higher interest rates are needed to stem rising inflation.

(Ibid.)

Thus, Currie was clearly pessimistic about the ability of the economy to expand without an increase in inflation sufficient to require deflationary policies.

Britton's inflation forecast was slightly higher than Currie's for 1993 but lower for 1994. However, like Currie he forecast headline inflation of above 5 per cent in 1994. Again, it is interesting to note the prominence given to the 1967 devaluation in the NIESR forecast, although some allowance was made for the exceptionally high unemployment in 1993:

Devaluation has already raised import prices by 9 per cent in the fourth quarter. That in turn must add to wage pressure, although not necessarily straight away. After the 1967 devaluation wages were held back for a year or so by a successful episode of incomes policy, but there was a sharp rebound in 1970. By the end of 1971 the whole of the devaluation gain in competitiveness had been reversed. The conditions of 1993 are of course very different, not least because unemployment is so much higher. Even so we would expect to see wage settlements beginning to rise again in the course of this year, provided that the output recovery is significant and sustained. This is the reasoning behind our forecast that the target band for inflation may well be exceeded.

(Britton 1993b: 18 para. 7)

Again, the policy choices facing the Chancellor could not be starker. The majority of the economists on the Panel suggested that inflationary pressures were already building and that rises in interest rates might be needed to prevent further inflation. The monetarist minority suggested that the weakness in the economy, was such that policies to boost growth were still urgently needed. Who was right? Should interest rates have gone up or down? Or was there some compromise position that offended neither and kept the best parts of both?

Balance of payments and government debt

The final set of economic relationships which the policy makers had to weigh up consisted of the links between the balance of payments on the current account, the deficit (or surplus) on the government's own finances and the financial surplus held by the private sector. Although the Panel members could

not disagree how these numbers were related, as it is a matter of national income accounting, there was room for disagreement about how they should be interpreted. In particular, there was considerable variation about the future of the public sector borrowing requirement (PSBR) which is the politically salient measure of government debt in the UK.

Before discussing the forecasts for the PSBR, it is worth explaining that it represents the amount by which the gross government debt increases during the financial year. It is not therefore the same as the government debt, although it is clearly related to the deficit or surplus (income less expenditure) of the government. The relationship is not straightforward, however. In addition to buying goods and services, the government also makes loans and sells assets (although the latter are also presented separately). Thus during the year some assets might be sold or privatized, a number of new loans made and some of the existing ones repaid. It is however broadly true that in the absence of any significant sale of government assets, a large PSBR implies an increasing government debt.

In February 1993 the debates about the future size of the PSBR related to the changes that were expected to occur as the economy made the transition from recession to recovery. The increase in public spending during the recession, which had brought about a large PSBR, was generally seen as wholly appropriate in that context. However, with recovery now under way, what mattered was the extent to which this government borrowing would be reduced by the increase in economic activity.

It was generally agreed by economists that the large borrowing requirement was created at least in part by the recession, but there was less agreement on the degree to which it would be removed by the end of the recession. One argument was that it would evaporate without any need for action as the economy expanded: in economic jargon it was 'cyclical', purely a product of the economic cycle. The opposite argument was that the level of the PSBR was structural, and that without policy action it would remain at its post-recession level despite renewed economic growth. Most economists accepted that the PSBR as a whole had both cyclical and structural components.

The distinction between the structural and cyclical components of the PSBR needs to be seen in the context of the expected economic recovery. For those who forecast a relatively slow recovery (Sentance and Godley), any reduction in the deficit owing to that increase in activity would also be small. In this case, the deficit would appear to be mostly structural. In contrast, if growth were expected to be faster, then the reduction would be greater, and the deficit would appear more cyclical. However even if the recovery was not strong the PSBR could be reduced if taxes were increased and/or public expenditure restrained or even reduced. If these policies were applied with sufficient determination (as for example Minford assumed) the PSBR could be virtually eliminated with only a fairly modest expansion in output. Forecasting the level of the PSBR thus required the forecaster to judge how

the economy would grow, how the government would act, and how these factors would interact with each other.

In the February meeting, nobody held the view that the PSBR was purely structural, although Patrick Minford argued that it was almost entirely cyclical. This conclusion was based on his estimate of the natural rate of unemployment (around one million) and an estimate that put the 'negative output gap' at around 10 per cent. However, it is worth noting that Minford also assumed a fall (of 3 per cent) in government consumption in 1993-94.² Consequently the PSBR was, or at least could be made to be, cyclical, and as a result no special measures beyond an assumed restraint in government consumption and investment were considered necessary to eliminate it.

As was noted when discussing the forecasts for unemployment, however, many of the Panel believed that unemployment could not fall this much without creating the sort of inflation that would necessitate deflationary economic policies. Thus, they believed that the PSBR was only partly cyclical and that although the increase in economic activity would reduce it, it would not eliminate it completely. For example:

In framing his March and December Budgets, the Chancellor has to steer between the problems of a tentative recovery and rising unemployment and the problems posed by the PSBR heading towards £50 billion, or 7 per cent of GDP, in 1993-94 and 1994-95. We reject the view that most of this public sector deficit is a cyclical phenomenon that will disappear with recovery.

(Currie 1993a: 35 para. 17)

The majority of the Panel thus saw a significant structural component in the PSBR that would remain despite the increase in activity. Because of this most of the Panel recommended that taxes should be increased in order to ensure that government debt was reduced. However, even within this group of economists there were important differences over when the new fiscal measures should be introduced and why. Everyone except Tim Congdon argued that any tax increases should be deferred until the recovery was clearly under way. The reason for this caution was their view that the recovery was still uncertain and increasing taxes too soon risked it stalling completely. In particular, the more cautious forecasters were concerned that both households and business were already extremely reluctant to spend and were saving a significant proportion of their income because of their large debts. As the economy expanded this unusually high propensity to save might well come down. However, if taxes were to be increased too soon then the capacity of the private sector to spend would be reduced still further and this would be most undesirable at such an early stage of the recovery.

It is in this context that the identity that links the budget deficit to the trade deficit and the private sector surplus is so important. By definition the budget

deficit is equal to the sum of the trade deficit and the private sector surplus. According to Minford and Congdon, net trade would make a positive contribution to GDP growth, and they therefore expected the trade deficit to reduce during 1993. In the absence of any change in the private sector surplus this also implied a fall in the budget deficit. Any reduction in the private sector surplus would have tended to reinforce this effect.

However, for the rest of the Panel neither the trade deficit nor the private sector surplus was expected to fall during 1993. If anything the trade deficit was expected to widen. As a result, the chances of reducing the PSBR were expected to be similarly small. Over the longer term there was a slightly different picture: the private sector surplus was expected to fall back to more 'normal' levels, but net trade, and its impact upon the balance of payments on the current account remained a key concern for some:

The current account deficit is expected to worsen this year, rising to about £20 billion . . . In 1994, the beneficial effects of the devaluation should work through to check any further deterioration during 1994 due to growth, and we see a small improvement in 1995, helped also by inward direct investment to the UK. But the current account deficit will remain stuck at about 2.5-3 per cent of GDP in the longer run. There are technical reasons for thinking that the true deficit is overstated, but nonetheless it is of concern.

(Currie 1993a: 34 para. 9)

The majority of the Panel shared this concern and was similarly concerned that a persistent deficit on the current account of the balance of payments might turn out to be a 'problem'. Their concern was that if the current account was in deficit then the transactions in external assets and liabilities (the other component of the balance of payments account) would be forced to show a surplus. This point is at the heart of the dispute whether a deficit in the balance of payments on the current account matters. Godley - probably the economist who places the greatest emphasis on the current account - believed that the deficit must matter because a persistent deficit implied that the UK was reducing its assets or increasing its liabilities, a process which could not continue indefinitely. In other words, even countries have credit limits. For example Gavyn Davies, chief economist at Goldman Sachs, wrote that:

'Automatic' financing [of the trade deficit] will only occur for as long as foreigners are content to accumulate UK assets . . . However, as soon as growth falters, or inflation threatens to rise, the existence of a large trade gap becomes quite menacing for the economy. Suddenly the flow of foreigners willing to acquire UK assets, and lend to British citizens can dry up. Sterling can then fall sharply,

forcing up interest rates. Or the sudden unavailability of foreign lending can lead to a severe dislocation as domestic spending has to be curtailed. Either way, the correction of the trade deficit involves a recession.

(Davies 1993b: 54 para. 30)

These arguments were not accepted by everyone. As was noted previously the two monetarist economists, Minford and Congdon, were relatively optimistic about the prospects for net trade. This was reflected in their forecasts for the deficit on the balance of payment on the current account: both showed some reduction in 1993. One consequence of this was, of course, that they could be more optimistic about government debt even in the absence of any major change in private sector behaviour.

Congdon would go further than this and raise as a point of principle the question whether a deficit on the balance of payments of the current account is a legitimate concern of policy makers. In contrast to Godley, Davies, Sentance and several of the other Panel members, he does not believe that a deficit is necessarily a problem. So long as the government's own finances are in order, Congdon's view is that it does not matter if the recovery is accompanied by an increasing current account deficit, as this would simply reflect the actions of private economic agents.³ Thus, he wrote:

If the Budget were indeed roughly balanced on a cyclically-adjusted basis, the Government should not be concerned about the behaviour of the current account of the balance of payments. The recovery, when it finally emerges, may be accompanied by a widening of the current account deficit. But, if that widening were to reflect the free decisions of private-sector agents, it would not necessarily be a problem for public policy.

(Congdon 1993c: 27-8 para. 11)

To sum up, the forecasts for the PSBR, which are at the heart of any economic policy making process, are a focus for all the uncertainty and choice of economic modelling and forecasting. First it is necessary to assess how far the economy is from equilibrium (a concept with which Godley would probably disagree), that is, from its potential level of output and natural rate of unemployment. As we have seen, when made explicit the estimates of the output gap were between 3 and 10 per cent, and of the equilibrium rate of unemployment varied from one million to around 2-2.5 million.

Next it is necessary to consider the effects of any expected growth. As the economy expands and the output gap is closed, so unemployment falls, tax revenue rises and state expenditure can fall. In addition tax rates and spending plans may also be changed, but this depends on what is politically feasible. Taking all these things together, the majority of the Panel forecast

that the government's finances were likely to remain a problem. More technically, they forecast that the budget deficit was at least in part structural, and that additional tax revenue would be needed to reduce it. The only real dissenter to this view was Minford. Minford argued that the deficit was not a problem because in his estimates the economy was so far from its trend levels of output and unemployment that the budget deficit would be eliminated automatically.

The forecasts for the deficit of the current account of the balance of payments showed a similar story, with the majority of the Panel forecasting a sustained deficit (the counterpart of their projection of a structural budget deficit). However, Minford's outlying position was reinforced by Congdon who argued that the only legitimate concern of policy makers was the government's finances. If the actions of private agents resulted in a persistent deficit on the current account of the balance of payments, then so be it.

Again, it was left to policy makers to decide on issues that economists collectively had been unable to resolve. Partly these decisions related to issues already discussed such as the growth rate, the labour market and their effect on inflation. They also involved questions such as how long the UK could continue to finance a trade deficit by selling assets, whether the decisions of private sector agents mattered to government, and whether the government should intervene to influence them.

Different models, different futures

So far this chapter has examined the forecasts for several important economic variables made by the Panel of Independent Forecasters in February 1993. Each section has considered one aspect of the forecast and shown how the forecasters thought that part of the economy would develop during 1993. Of course, as was shown in Chapter Three, the economy is not made up of discrete components. This final section draws together the different elements and shows how they are combined in coherent and integrated economic models, narratives and policy recommendations.

As we have seen, different members of the Panel saw different factors as being important. Their views also diverged according to whether their analysis focused on the devaluation or on the effects of the recession that preceded it. This latter classification is developed here in more detail.

The devaluation/conventional group of economists

The members of the group for whom the devaluation was the key event in defining the characteristics of the economic future were Andrew Britton, David Currie, Gavyn Davies, Wynne Godley and Andrew Sentance. In terms of the conventional classifications of economics, they are generally representative of the mainstream neo-classical orthodoxy. However, Wynne Godley's inclusion

means that a simple identification with mainstream economics would be inappropriate.

As well as seeing economic growth being stimulated by the devaluation this group shared to varying degrees the concern that, given the way in which the UK economy had responded to devaluations in the past, the recovery from recession would be slow and difficult. The main feature of this group's economic forecasts was thus that any short-term optimism attributable to the boost from the devaluation was quickly tempered by medium-term problems. The analysis is summarized by David Currie as follows:

The past two years have seen the PSBR move from balance towards a deficit of around £50bn, interest rates cut from 15 per cent to 6 per cent, and the pound devalued by 15 per cent. This is a very large relaxation of policy: because the economy responds with a substantial delay, we have yet to see its major impact on demand, output and inflation . . . The 15 per cent devaluation provides an important impetus, boosting export volumes and holding back imports . . . Low interest rates are expected to lead to growth in consumer spending, though slowly because of rising unemployment, uncertain employment prospects and continued risks about house prices.

(Currie 1993a: 33-4 paras 3, 7)

As the forecasts turned from 1993 and into 1994 and 1995, however, a rather more difficult future was expected. In particular, they were concerned that unemployment could not fall very much before inflationary pressures would increase. They therefore predicted that the need to reduce government borrowing would combine with the deficit on the balance of payments of the current account to make any significant economic growth very difficult to achieve.

Past and present within economic forecasts

Although this group of economists shared the expectation of a problematic economic future, it would not be fair to say that they agreed on every issue, nor even on how the current situation should be understood. They had significant disagreements over the relative weights to be given to precedent and present. The NIESR in particular, but also the London Business School, gave considerable weight to the past performance of the UK economy in predicting what was likely to happen in the future. The appeal to precedent, and hence (implicitly) to the econometric equations of their macroeconomic model, can be seen quite clearly in the following quote from Andrew Britton:

[The] National Institute's forecasts for a recovery in the economy do not rest mainly on the indications of an improvement in

confidence or in the levels of economic activity around the turn of the year. They rest rather on our analysis of the effects of the relaxation in monetary policy beginning from last September, especially the devaluation of sterling.

(Britton 1993b: 17 para. 1)

An interesting feature of this version of the devaluation analysis is that the persuasiveness of the forecasts now rests upon the response of the UK economy to previous devaluations. Both Britton and Currie made reference to the response of the UK economy to the 1967 devaluation, as a guide to what would happen in 1993 and beyond. In other words, the emphasis was shifted from the specific nature of the present case to a more abstract discussion of devaluations in general. The effect of this strategy was to base the plausibility of the forecast upon the validity of historical precedent as a guide to future events.

One function of this emphasis on historical precedent was to distinguish the NIESR's optimism from that of others who had forecast the end of the recession only for output to continue to contract.

Other forecasters on the Panel who shared many of the same concerns were less confident that the impact of the devaluation could be predicted with any certainty. For example, in February 1993 Andrew Sentance believed it was still too early to be sure of the implications: 'There has been a very significant relaxation of domestic economic policy over the last six months, the consequences of which are still unclear' (Sentance 1993a: 77 para. 14).

Nor did all the economists on the Panel who emphasized the importance of the devaluation also emphasize the importance of historical precedent in forecasting its effects. Some were much more concerned about the effects of the economic present. For example, the devaluation was clearly very significant for Wynne Godley, who had for many years argued that the UK economy could grow no faster than was warranted by its performance in world trade. Indeed, for Godley the long-term deterioration in the competitiveness of UK exports was the crucial factor in explaining UK economic performance over the previous twenty years. Thus, to the extent that the devaluation had lowered the exchange rate and improved the UK's competitiveness, he saw it as a step in the right direction. However, Godley saw other problems in the UK economy, which in his judgement meant that the devaluation would not be enough to ensure an economic recovery. In particular, what was important about 1993 was not that there had been a devaluation comparable to that of 1967, but that the 'present conjuncture contain[ed] a number of well known features which, taken together, make it very different from the later stages of all previous recessions' (Godley 1993b: 59 para. 2).

In other words, for Godley it was the uniqueness of the situation that had to be appreciated if it was to be properly understood, and not its similarities to previous events. The principal factors that Godley identified as making 1993

different were financial deregulation and the high debt levels associated with the credit boom of the 1980s. Thus, he wrote:

All earlier recoveries were assisted, if not initiated, by the relaxation of credit controls which caused a discontinuous increase in spending on durables. For instance, in the first quarter of 1983, following the removal of credit controls in mid-1982, consumption of durables was over 25 per cent higher than a year earlier. The same thing cannot happen this time as there are now no credit controls to relax. Moreover, while the flow of net credit to the household sector has fallen dramatically (by at least 10 per cent of disposable income) since 1988, it remained positive, at least until the middle of last year, so that total household debt is still nearly 100 per cent of disposable income - three times the proportion reached in 1974, at the peak of an earlier notorious credit boom.

(Godley 1993b: 59 para. 2(a))

In addition to the high levels of household debt, and the weak balance sheets and continued high saving which this implies, he identified another change to the UK economy:

Another structural problem arises because of the distortion in the composition of demand which has taken place during the last decade. Compared with the seventies, the share of domestic demand in GDP has risen by about six percentage points - the straightforward counterpart of the move from surplus to deficit in the balance of trade. Within domestic demand, the share of GDP taken by fixed investment, after rising sharply in the late eighties, has fallen sharply during the recession, and is now back to the very low levels of 1981-2. By contrast, the share of personal consumption, which rose extremely fast during the boom to quite unprecedented levels in the late eighties, has hardly fallen back at all, although the credit boom was broken in 1990.

(Ibid.: 61 para. 9)

Again this implies that the economic circumstances of 1993 were quite different to those of the previous devaluations and that these changes needed to be recognized if developments in 1993 and beyond were to be properly understood and anticipated. This position contrasts quite clearly with the appeals to precedent of Britton and Currie.

Recognizing the unusual nature of the present also manifested itself in other differences between these forecasters. Davies also emphasized the problems of the present and, drawing a parallel with the US experience, argued that interest rates might need to be cut further. In contrast Britton and Currie, who saw the

devaluation as more or less the same as 1967, and therefore also thought that enough had been done to stimulate growth, thought it possible that the next move in interest rates might be upwards (to ward off inflation owing to imports and wage inflation).

In summary, although the devaluation economists emphasized the importance of the fall in both the exchange rate and the interest rate in stimulating growth, they differed about several key issues. Unfortunately for policy makers, the devaluation story was not the only one on offer. As a result, they were faced with a choice not just between different economists, but between different economics.

The group of classical economists

The other way of analysing the economy drew more heavily on what might be thought of as the 'toolkit' of classical economics. In particular, the ideas of equilibrium and of natural rates of output and unemployment featured prominently. There was also an emphasis on markets rather than policies. In terms of economic theory the members of this group - Tim Congdon and Patrick Minford - are both identifiable as monetarist economists. However, as we shall see, there are some quite important differences between them.

The most straightforward application of the concepts of classical and monetary economics to the UK economy was to be found in the analysis and advice provided by Tim Congdon. For Congdon, what was important was that the recession had pushed the economy a long way from its long-run trend position and that the disequilibrium thereby created should ensure recovery. Although the devaluation was important in this account, it was not seen as the cause of the recovery; if anything, the disequilibrium was the cause of the devaluation. The distance of the economy from its trend or longrun position is measured by the difference between actual output and trend output. According to Congdon this output gap was, in 1993, 'exceptionally large':

In the last two-and-a-half years, the British economy has been through a severe recession. Although the recession has been less intense than that of 1980 and 1981 (i.e. the average quarterly fall in output has been smaller this time), it has lasted significantly longer. On the assumption that underlying trend growth has remained in the normal 2-2.5 per cent area, the excess of trend output over actual output - the so-called 'negative output gap' - must be exceptionally high. Calculations at Lombard Street Research Ltd. suggest that the negative output gap (expressed as a proportion of trend output) is currently about 6 per cent. This figure is not far from the OECD's estimate in its *December Economic Outlook* that the UK's negative output gap in the fourth quarter of 1992 was 6.9

per cent. The excess of trend output over actual output at present is probably the largest in the post-war period.

(Congdon 1993b: 25 para. 1)

The existence of a large negative output gap meant for Congdon that the economy might grow at an above trend rate for several years without the trade-off between growth and inflation becoming adverse. Of course, once the negative output gap had been closed, then growth would need to remain at or about the trend level, unless productivity improved dramatically. However, given the size of the output gap, GDP growth of 3.5 to 4 per cent would be consistent with three to four years of non-inflationary economic growth. In fact, according to Congdon, inflation was not the problem at all: so strong were the deflationary forces acting upon the economy that it was possible the price level might actually fall unless the government acted to ensure economic growth.

Interestingly, unlike Britton and Currie, Congdon believed that February 1993 was 'still rather early to assess the effects of the large fall in interest rates since Britain's departure from the European exchange rate mechanism on 16 September [1992]' (ibid.: 26 para. 5). Of course Congdon's own analysis is also open to the same sort of deconstruction. For example, the negative output gap, which according to Congdon was possibly the largest since the Second World War, might actually have been much smaller than he believed:

The economy is now clearly working some way below capacity. A simple extrapolation of a 2 per cent growth rate since the second half of 1990 suggests that GDP in 1993 will be some 7 per cent below trend. However, it is possible that there has been an erosion of plant capacity in the course of the recession; Goldman Sachs' very rough estimates, based on CBI Survey data and other sources, suggests that the 'output gap' may in fact be no more than 3-4 per cent.

(Davies 1993a: 47 para. 22)

If this estimate were correct it would have meant that inflationary pressures were not nearly as weak as Congdon argued and that the prospects for growth were correspondingly weaker.

The other monetarist on the Panel was Patrick Minford. Like Congdon, Minford believed that the economy was a long way beneath trend levels of output and unemployment and was likely to stay there unless prompt action was taken to ensure recovery. Consequently, 'The latest Liverpool forecast is for very weak growth in 1993 in the UK, relying on further falls in interest rates to the 5-6 per cent range' (Minford 1993a: 67 para. 1).

However, Minford's reasons for believing this were slightly different to Congdon's. Rather than focusing on the gap between actual and trend output, Minford's analysis was based on the difference between the level of

unemployment and its 'natural rate'. Minford believed that because unemployment was above the 'natural rate', inflationary pressures were weak. Clearly, however, the ideas are linked – as economy grows, the negative output gap is closed and the rate of unemployment falls towards its natural rate – and when the output gap is closed then the level of unemployment is (by definition) at the natural rate.⁴ The most important (shared) idea is thus that of movements away from equilibrium generating powerful forces which tend to restore that equilibrium. The (shared) weakness, of course, is that both depend on having correctly identified the natural rates from which deviation is being measured.

Minford's analysis was different to Congdon's in other ways as well. When discussing the devaluation-based forecasts it was noted that there were differences in the weight given to precedent and present. Within the classical camp there were also differences on this issue. Thus Congdon, who used the standard concepts and exemplars of his own preferred branch of economics, can be seen to share with Britton and Currie an emphasis on precedent in understanding the economy. In contrast, and perhaps somewhat ironically given their different views on just about everything else, Minford and Wynne Godley both emphasized the features of the contemporary economic cycle. Particularly important in Minford's argument was the United States economy where debts were very high, balance sheets very weak and interest rates had had to be cut to very low levels in order to get any kind of recovery going. Taking this as his guide to the UK experience, Minford therefore argued that the Chancellor should cut rates further in order to ensure that the recovery was sustained:

In the absence of US-style monetary policy this weakness will continue, with the downside risk ever-present. The argument is now shifting to fiscal policy where some are arguing for a rise in taxes. However, this would be a mistake. It is the weakness of the economy mainly that has destroyed the Government's finances. The appropriate remedy is to maintain tax rates and to proceed with medium term plans to reduce public spending.

(Minford 1993a: 67 para. 2)

Conclusions

To summarize, the Panel was divided over whether the future for the UK economy was good or bad. The most optimistic was probably Tim Congdon, although Patrick Minford was also optimistic about the longer-term future. The reservations both economists expressed about the pace of growth reflected their beliefs, particularly Minford's, that the government would not take the necessary policy decisions to ensure recovery. In contrast, the rest of the Panel were basically pessimistic. Although Britton, Currie and Davies all forecast growth of between 1.5 and 2.0 per cent it is important to remember that this was still

beneath trend, and therefore that the output gap would still be increasing. However, this is not why the group was pessimistic. Like Sentance and Godley, who forecast weak growth in 1993, this group did not believe that the recovery could continue for long without inflation rising. The reasons for this pessimism varied slightly but centred on either the 'natural rate of unemployment' or the high proportion of consumption in GDP.

However, other differences cut across this distinction and could have produced different classifications of economists. For example, the Panel differed on how much they emphasized particular features of the 1992-93 economic situation, and on how much they were prepared to base their analysis on what had happened in previous devaluations. Britton and Currie, who probably have the largest econometric models, paid the most attention to historical precedent. Godley paid most attention to the factors that made 1993 a unique moment in time.

From the policy makers' perspective, however, it seems likely that the first set of differences are the most salient. In particular, no matter how they arrived at their forecasts, the Panel was offering two quite different sets of analysis and advice. On the one hand, the future was bleak. On the other, reasonable economic growth was achievable. The problem was that choosing between these different forecasts also meant choosing between different economic policies. Asking the Panel to provide recommendations to the Chancellor certainly sharpened the questions and made the choices clearer. It did not provide an answer, however, and the policy choice thus remained an political as much as an economic one.

DIFFERENT FUTURES

Controversies and disputes in July 1993

Introduction

Economists have a reputation, perhaps undeserved, for disagreeing. They do disagree about some things and, as we have seen, these disagreements matter to both lay people and policy makers. In February 1993 a range of economic analyses of, and forecasts for, the UK economy were available, based on the different models used by the individual members of the Panel of Independent Forecasters. This chapter examines the debates about unemployment, the public sector borrowing requirement and the deficit on the balance of payments of the current account in order to exemplify the controversies that occur in economics.¹ However, the analysis does more than provide an account of the interpretative flexibility present in economics: it also highlights the links between ideas and social actions. Just as recognizing the existence of germs requires certain actions on the parts of doctors and nurses, so too recognizing the existence (or not) of hysteresis requires a certain type of policy response from the government.

It is these larger issues which make the existence of uncertainty in economic science important. Decisions about how to understand the economy have ramifications outside the world of academic economics. What is more, this outside world can rarely wait for scientists to resolve their disputes and establish a collectively endorsed theory. For example, the controversies in economics referred to in the previous chapter have lasted for around twenty years. Similar observations about interpretative flexibility could be made about other fields of science too, some politically important, some not so. It is most certainly not peculiar to economics, and dealing with economics therefore has more in common with managing other sorts of science than might initially be thought.

In other words, the aim of this chapter is not to argue that the disputes and controversies referred to in the previous chapters are the product of pathological stubbornness peculiar to economists. Instead it is to show how they are the consequence of a reasonable disagreement about the meaning of the available data, and to explore the consequences of this for policy making. In particular,

it examines the different futures offered, and potentially legitimated, by the Panel of Independent Forecasters. It is argued that acting upon the models and forecasts requires, in addition to choosing between different sorts of economics, making a commitment to a particular sort of social and political culture.

Reducing unemployment

It was noted in Chapter Five that at their first meeting in February 1993, the Panel of Independent Forecasters expected the rate of unemployment to remain at its current level for the whole of 1993. Indeed, for the majority, a further increase in the number of people out of work was likely. However, this was not how things turned out. In February 1993 the number of people out of work and claiming unemployment benefit fell by 26,000. This fall was repeated in March and followed by further falls in April and May. Both the timing and the scale of these falls were completely unexpected, and because of this their interpretation was far from straightforward. This section shows how these falls in the number of people claiming unemployment benefit were interpreted either as a statistical aberration or as reflecting the new and more flexible characteristics of the UK labour market. By following the Panel's analysis of unemployment data as it was gathered during 1993 the analysis shows how the uncertainty was reduced and an explanation produced.

The mainstream view

The Panel of Forecasters included proponents of two different models of the labour market. In this they were representative of economists more generally. Of the two explanations, one remains true to the tradition of the classical economists and focuses on the adjustment of wages, which is in turn influenced by institutional factors such as the power of trade unions, labour mobility and the benefit system. The other explanation, which is of more recent origin, supplements this story with an additional factor known as 'hysteresis'.² The main distinction therefore is between those who believe in the idea of hysteresis and those who do not.

Although the relative newcomer, it is the hysteresis model, developed by Professors Steve Nickell and Richard Layard (Layard, Nickell and Jackman 1991, 1994), which is the mainstream representation of the labour market. Often referred to as the 'Layard-Nickell' model, it is based on the observation that after there has been a recession unemployment does not fall back to the pre-recession level but appears to remain stuck at a new higher level. The basic application of the ideas behind the model to the UK economy can be summed up as follows:

In the 1980s . . . union membership and influence declined.
Unemployment benefits also became less generous in relation to

incomes in work. These factors should have pushed down the equilibrium rate of unemployment. But their influence was offset by a rising total of labour market ‘outsiders’: long-term unemployment rose steadily, with the number out of work for over a year accounting for almost half the total. As a result, unemployment of over 3 million exerted little downward pressure on inflation, which settled at an underlying rate of 5 per cent in the mid-1980s.

(Sentance 1993a: 86–7 para. 17)

The members of the Panel use the idea of hysteresis slightly differently. The majority (Britton, Currie, Davies and Sentance) subscribe to something like the following, again expressed by Andrew Sentance:

Sentance: My line of argument is that the genuine natural rate is probably 1 to 1.5 million . . . but overlaid on that you have this hysteresis effect, which makes it appear that you can’t get unemployment down below 2 to 2.5 million without inflation picking up.

(Interview 13 July 1993: transcript p. 15)

This situation arises because the effect of hysteresis is to restrict the available supply of employable people to a subset of the total number of unemployed. This might happen for a variety of reasons – deskilling, disillussionment and so on – which may have very little to do with economics. However, if a number of people are excluded from the labour force the supply of available labour is reduced and this does have economic consequences. It is basic economics that if the supply of something is restricted then its price will rise, and that a rise in the price of something will simultaneously act to reduce demand for it. What hysteresis does therefore is to create a new equilibrium in the labour market at which the price of labour will be higher and the demand for labour lower than would otherwise be the case. Of course, the unemployable labour force does not go away.³ As a result, another effect of hysteresis is that the natural rate of unemployment, at which wages and prices are stable, appears to be raised. In other words, for the majority of the Panel, the total number of people unemployed is made up of two components. There are those who remain out of work because of the institutional and other features of the economy (the ‘natural rate’) and those who remain out of work for other reasons (the hysteresis effect). These causes are quite separate and require different sorts of policies to tackle them, as is made clear by Andrew Sentance:

Sentance: [It is important] not to confuse the long run natural rate of unemployment with this hysteresis effect. I think the hysteresis

effect can be tackled with the right labour policies and sensible macro management. The long-term natural rate may be the product of fairly deep structural things, the bargaining structure, union membership and so on, which can't readily be changed by economic policy and may be very difficult to shift altogether.

(Interview 13 July 1993: transcript p. 16)

The idea of hysteresis is important for macroeconomic policy because it implies that being unemployed, particularly long-term unemployed, effectively renders people unemployable unless special training programs are created. Given the implications of perpetual unemployment for the public finances (not to mention the lives of those consigned to a life on benefits) economists who accept the idea of hysteresis also tend to favour fairly active labour market policies. The following sentiments are more or less typical:

Britton: My interest [in unemployment] has been involved by wider social considerations. What is wrong with the state of the nation seems to me to have a lot to do with a sustained period of high unemployment. So my interest in trying to think how full employment could be restored is not just a question of trying to make the economy grow faster and have more real wealth to distribute, but also feeling that socially it is divisive to have so many people unemployed. They ought to be able to have a role to contribute to society beyond simply claiming their dole - not just so many mouths to feed, but actually somebody with a useful contribution to make.

(Interview 12 July 1993: transcript p. 7)

Sentance: [You would need] things like better training programs for the long-term unemployed. I would actually have an employment program for the long-term unemployed and link this very closely into the benefit system. A report of this type doesn't really give you chance to expand all your ideas. These are my personal ideas, and I'm not sure how robust they are, but my notion is that you have to break into this cycle where people apparently appear to drop out of the labour market and come to exist on benefits for a long period of time . . . [I think you need to] have a set of macroeconomic policies that enable you to have above trend growth and then you need a 'supply-push' policy in the labour market with active training for the long-term unemployed.

(Interview 13 July 1993: transcript pp. 15-16)

The (new?) classical view

The alternative to the mainstream account is that of the classical economists. This is the basis of the approach favoured by Minford and Congdon. According to this view, hysteresis does not exist and the high levels of unemployment seen in the UK are the result of policy mistakes. However, as there is a difference in the importance which Minford and Congdon attach to the labour market, their views are discussed separately.

Congdon's view is perhaps the more simple. It is his belief that the responsibilities of government do not include the maintenance of full employment (interview 2 June 1994: transcript p. 1) and he seems to think about the labour market only to the extent that it affects his forecasts for other variables. In this analysis he relies heavily on the concept of the 'natural rate' (defined, as before, as the rate of unemployment at which prices would be stable).

Like the rest of Panel, Congdon believed that at 2.9 million unemployment in the UK was above the natural or equilibrium rate. (Congdon 1993b: 20). From this it followed that the competition for jobs had to be strong and that the pressure on wages, and hence on prices, had to be correspondingly weak. This is all perfectly consistent with basic monetarist theories and so, to the extent that he discusses the labour market at all, Congdon's story is fairly straightforward:

Following the argument of Friedman's 1967 presidential address to the American Economic Association and the associated literature, unemployment above the natural rate should cause a progressive amelioration in the unemployment/inflation trade-off as inflation expectations decline. It is this amelioration, combined with the scope for above trend growth because of high unemployment and abundant spare capacity, which justifies optimism about the medium-term outlook.

(Congdon 1993d: 29 para. 2)

However, Congdon did not forecast a particularly significant fall in unemployment: in fact his forecast was for virtually no change during either 1993 or 1994.⁴ The important point about his analysis was that he did not use the idea of hysteresis and consequently believed that inflation would remain low despite relatively strong economic growth.

Patrick Minford also uses this classical type of framework, although he elaborates it in a different way (Minford 1985). He was the only one of the seven who forecast a significant fall in unemployment. Minford explicitly rejected the idea of hysteresis and argued that the perceived association between inflation and unemployment during the 1980s was the result of interpreting the data out of context. Thus, according to Minford, if one takes into account the

headline inflation figures of the 1980s, wage behaviour was not particularly aggressive:

According to our story [the acceleration of average earnings growth from 8 to 10 per cent] reflected rising inflationary expectations in the monetary context of 1987-90. It is actually remarkable how little wage settlements reacted to a sharp rise in inflation (from 5 per cent to 10 per cent on the RPI and around 8 per cent on 'underlying' measures); we explain this by our view that *unemployment was above not below the natural rate*. For all this period real wages were growing by substantially less than the 4.7 per cent 1980s average growth in manufacturing (let alone still higher general industrial) productivity. The behaviour of expected real wages remained moderate even when unemployment had fallen to 1.6 million; this is inconsistent with a natural rate of 2.5 million and above.

(Minford 1993b: 76 para. 17)

However, it remains the case that unemployment and prices both rose in the late 1980s and did so at unemployment levels supposedly in excess of the 'natural rate'. If hysteresis was not the reason then what was? It was Minford's contention that the economic policies of the 1980s, particularly the decision to keep the UK exchange rate fixed to the Deutschmark, were to blame. In typically flamboyant prose he described the policies of the 1980s as a violent assault on the UK economy:

Why did unemployment drop only to 1.6 million and why has it risen to 3 million-plus? Our answer is that owing to our tragic errors in monetary policy we had to hit on the head an economy which otherwise could have remained on a sustained growth path of some 3 per cent. After we had so hit it on the head we joined the ERM proper and continued raining blows on its prostrate body. The resulting deep recession has produced an unemployment 'excess' (over the natural rate) of over 2 million. In short it is recession, not the trends of a poorly-performing labour market, that has delivered us this apparent ratchet.

(Ibid.: 77 para. 19)

Finally, Minford turned the spotlight back onto hysteresis. One of the most controversial claims of the hysteresis school is that the very fact of being unemployed can make individuals unemployable. According to the mainstream view:

Rises in unemployment create a pool of labour market 'outsiders' who exert little influence on the process of setting wages and hence

on inflation. They also become unattractive to employers as their skills degenerate and as their motivation to work ebbs away. After the deflationary shock has worn off, high unemployment therefore ceases to exert any further downward pressure on inflation.

(Sentance 1993b: 86 para. 15)

Minford flatly denies that this is the case. Minford's objections to hysteresis are based on two main factors. The first is that there is no convincing (at least to him) econometric proof:

Minford: All the evidence on hysteresis suggests that the most you can find in the unemployment series is a root of 0.7 or something like that. There is slow adjustment, but there is not a unit root. When unemployment goes up, it does not stay up, it slowly declines. The sort of equations that people have been quite clearly of this form:

$$u = a u + \dots$$

where a is less than 1. It is only if a is equal to one that you get the strong sort of hysteresis. And no one can find it. It's just not there. There are exhaustive studies by Steve Nickell and co. on this issue and they have said, basically, that there is slow adjustment. Well I've got slow adjustment in my model, it's no big deal.

(Interview July 1993: transcript p. 13)

However, Minford is prepared to concede that the econometric evidence about hysteresis is not entirely conclusive. The crucial test for hysteresis is to look not at the relationship between inflation and unemployment, nor at the speed with which unemployment adjusts, but at what happens to the long-term unemployed. If hysteresis exists then the unemployed, particularly the long-term unemployed, ought remain excluded from the labour market. According to Minford this is not what happened, and the long-term unemployed were far more active in the labour market than the mainstream economists acknowledged:

Then we must query the lack of pressure from 'outsiders', in the form of long-term unemployed. Those unemployed more than a year had dropped by end-1990 to 0.5 million from 1.3 million in 1987. Furthermore the turnover rate in the labour market has risen to around 0.3 million per month, approximately 14 per cent of the labour force per year (against 9 per cent in 1988). Hence some 50 per cent of the labour force may have 'quit' jobs and experienced a spell of unemployment in the last four years; even allowing for double and even more frequent spells among these this high rate of activity suggests a wide experience of unemployment in the labour force. This is not a picture of supine labour market behaviour

by the unemployed, not even those with the misfortune to become 'long-term' unemployed.

(Minford 1993b: 76-7 para. 18)

What this meant to Minford was that:

Minford: There is no evidence that once people are out of work they stay out of work for ever. Indeed there is positively contradictory evidence. If you believe in anything remotely Popperian, you cannot sustain the view that the long-term unemployed do not get jobs, because it is flatly contradicted by one important event – they did get jobs and they stopped being long-term unemployed.

(Interview July 1993: transcript pp. 13-14)

As a result, he saw no reason why unemployment could not fall substantially (provided, of course, that the right monetary policies were pursued):

If unemployment can fall to 3 per cent or so from its projected 1993 rate of 11 per cent, then output can rise approximately 10 per cent from its depressed 1993 rate: the reasons lie not merely in the greater employment but also in the likely ratio of output to employment which tends to rise in the upswing as productivity increases.

(Minford 1993b: 75 para. 13)

Thus, in Minford's economics the labour market is much the same as any other market: the price varies so as to equilibrate supply with demand. In the case of the labour market, the price is the wage and the supply is the number of people available for work. At the equilibrium rate of unemployment, the price of labour is such that there is neither excess demand for, nor supply of, labour. In other words, the labour market is the mechanism through which wages adjust and individuals price themselves into (or out of) work. The goal of economic policy is to create the conditions in which this adjustment can take place as efficiently as possible. What this means in practice is stable economic growth and social policies to reduce the potency of the factors which adversely affect this adjustment, principally state benefits and union power.

In addition, because Minford's analysis rejected the idea of hysteresis, he also believed that the sort of active labour policies favoured by the mainstream panellists were mistaken. In particular, because the long-term unemployed were not excluded from the labour market, government training programs were unnecessary. Instead market forces would, as they have done in the past, ensure that appropriate training programmes were provided:

Minford: Whenever we have had skills problems in the past they have turned out to be very short lived . . . One famous skills shortage was computer software writers. Well they simply raised the wages and people came into software writing just like there was no tomorrow. And now, of course, there is a glut of computer software writers, which is fair enough. That is the way markets work. So I am extremely sceptical of all this stuff, because it is economics without markets in my opinion. I mean training is very important, [but] it responds to market forces, like anything else does.

(Interview July 1993: 6)

Thus, in Minford's world, unemployment will automatically fall to the natural rate fairly quickly but can be held back by government or trade union intervention. In addition, employers and employees can be relied upon to arrange the appropriate training for their needs. As a corollary, government intervention to assist the long-term unemployed is unnecessary and might actually make things worse by hampering the operation of the markets for education and training.

For or against hysteresis: a summary

Hysteresis is a controversial topic in labour market economics. What is more, it is a controversy that has important policy implications. The majority of the Panel believed that it exists and that simply being long-term unemployed is sufficient to render some people (although not all) unemployable. As evidence for this, they pointed to the behaviour of unemployment and inflation over the preceding ten to fifteen years and concluded that unemployment could not fall much below two million without wage pressure leading to a surge in inflation. Consequently this group recommended that measures be introduced to increase the turnover in the labour market and to minimize the number of long-term unemployed. In this way the supply of employable labour should increase, and the stable inflation level of unemployment fall. Without such measures the future would be bleak.

In opposition to this, Minford argued that there was no such thing as hysteresis. The apparent ratchet-like rise in unemployment was, on his account, the product of policy mistakes destroying growth and creating recession. The long-term unemployed were not unemployable, as evidenced by the fact that they did get jobs. In Minford's world, the solution was to reduce benefits (and the marginal tax rates faced by those coming off benefits) so that it was worth working. In fact, Minford believed that the labour market policies already enacted had made most of the necessary changes, and that in 1993 the natural rate of unemployment was below one million. The future was therefore potentially very promising for Minford, provided that the right policies were followed. The contrast between the two views is nicely summarized by Minford as follows:

Mimford: They are saying that the labour market will heat up very rapidly the moment the slightest [let-up occurs]. I believe that we are going to see a slow recovery, with unemployment probably falling again, and that we are going to see wages going on plunging . . . They are saying that the labour market rigidity and the natural rate are very high, because of insider-power or whatever it might be, and I don't believe there's a shred of evidence for it.

(Interview July 1993: transcript pp. 6-7)

Applying the theories: analysing unemployment data in July 1993

Having set the scene by outlining the main dimensions of the controversy over how unemployment ought to be understood in fairly general terms, let us now consider how the unemployment data produced during 1993 was interpreted. As noted in the introduction to this chapter, unemployment fell rather unexpectedly in the early months of 1993. How were the alternative frameworks outlined above applied to this data?

In February 1993 none of the economists was forecasting a significant fall in unemployment in the forthcoming year. There was nothing particularly unusual about this as unemployment typically lags behind the economic cycle and therefore continues to rise for some time after economic growth begins to pick up. For example, after the 1981 recession unemployment continued to rise for a further two or three years. This was not what happened in 1993, however. After rising quite sharply at the end of 1992, unemployment fell in the early months of 1993. As the Panel's July report made clear, the fall in unemployment was a clear deviation from past trends:

The falls in unemployment in the last four months have come as a surprise to all of us. They have come at a much earlier stage in the cycle than would have been expected in the light of past experience, and the turnaround from the rises at the end of last year was unprecedentedly large.

(HM Treasury 1993b: 6 para. 11)

The problem for the forecasters is thus to give specific reasons why 1993 was different from similar stages in previous economic cycles. In other words, what were the specific features of 1993 which could both preserve the general model (be it hysteresis or classical) and account for the perceived discrepancy between that model and the economy? The report contains several hypotheses and these are discussed in more detail below.

The least controversial explanation was that the falls represented a correction in the labour market:

At least part of the reason for the falls in unemployment is that firms took a very gloomy view in the second half of [1992] and laid off more workers than the subsequent conditions dictated.

(HM Treasury 1993b: 6 para. 12)

Thus, the anomaly was not the *fall* in unemployment in early 1993, but the *rise* at the end of 1992, which was explained as a collective misjudgement on the part of employers. If this was the case, then the recent falls were less problematic as they represented a move *towards*, and not *away from*, a more normal state of affairs.

This sort of equilibrium story is no doubt appealing to many economists, but as the passage quoted above concedes, it was not sufficient to explain all the fall in unemployment. The reason was that the increase in employment recorded over the same period was smaller than the fall in unemployment; hence the use of the qualifier 'At least part of the reason' in the passage quoted. Additional reasons suggested included lay sociology, demographic changes and falling labour costs. They were explained in the Panel's report as follows:

Other possible explanations [for the falls in unemployment] include:

- the claimant count may overstate the underlying fall, for example if benefit offices are finding it easier to prove that applicants are 'not actively seeking work' now that the economy has turned the corner and there are more jobs around;
- with unemployment rates more evenly distributed across regions, unemployment may react more quickly to increases in employment opportunities, and thus could fall at a lower level of vacancies than in past episodes;
- the fall in employment might reflect an increase in firms' desired employment levels due to falling real unit labour costs.

(Ibid.)

Of these three reasons, the first is the most interesting from the perspective of a sociology of science. Advanced by Andrew Britton, it effectively sought to problematize the unemployment data by emphasizing its socially constructed nature. According to Britton the unexpected falls in unemployment might not have represented a change in the way the labour market responded to the economic cycle. Instead, the falls in the claimant count simply represented a change in the number of people eligible for a particular state benefit.⁵

As a way of justifying this, Britton referred to the different ways in which data for the number of people in or out of work were collected. The ILO *Labour Force Survey*, from which the employed total is derived, also includes a figure for unemployment. However, the *Labour Force Survey* includes as

unemployed anybody who has looked for work in the previous month, regardless of their eligibility for unemployment benefit. On this arguably more inclusive measure, unemployment did not fall so dramatically, and was therefore expected to match up more closely with the estimated increase in employment:

Britton: The first reason given in the main body of the report . . . is to the effect that this is a change in the claimant count, which is probably not reflected in what will become the ILO measure. You will probably find that the fall in unemployment on that measure has not been so great.

(Interview 17 July 1993: transcript p. 11)

However the claimant count did fall significantly, and by more than employment increased, so there remained a discrepancy to be accounted for. Where had the people gone who stopped claiming unemployment benefit, if they had not obtained jobs and did not appear in the other measures of unemployment? One possible explanation was that they had left the labour force altogether so were defined neither as employed nor as unemployed:

Britton: We know that there have been a lot of people leaving the measured labour force over a period of a year or two, particularly men approaching retirement age and also people staying on in education or training for longer. They are not either employed or unemployed. This is the point that was made in the [NIESR] Review, and is backed up by the *Labour Force Survey*, which runs up to the early months of this year.

(Ibid.)

As Britton admitted, it was nevertheless unlikely that ‘the number of claimants suddenly started dropping [because] a lot of people voluntarily left the labour market in February, March, April and May’. (ibid.: 11-12). Rather, he was suggesting that changes within the Department of Employment had resulted in a greater than usual success rate in ‘getting some of the people they had coached in how to do job interviews and so on . . . to the front of the queue’ (ibid.: 12). This could have happened without increasing the overall size of the labour force (however measured) because:

Britton: They would have got the jobs that were going, in preference to the people who might have come in from outside the labour force. What has normally happened in previous upturns has been that a lot of the jobs at the beginning go to, say, married women looking for part-time work . . . The other possibility is that they got a bit tougher on the availability of work test. It is a bit easier to enforce that test when there are jobs to point

people to. As long as there is no work to be had, there is no way of proving that somebody is not looking for work, [but] if they start refusing offers of employment then, they may lose their benefit.

(Interview 17 July 1993: transcript p. 12)

Thus, in order to explain unexpected falls in unemployment, economists can problematize the data by highlighting the normally taken-for-granted procedures by which it is produced: in this case, the reaction of benefit agencies to social and political pressures. However, this explanation was not widely supported by the other Panel members.

There was a similar lack of widespread support for a second idea: that because unemployment was less concentrated in any one region than had been the case in previous recessions, new vacancies were more likely to be filled. Unlike the 'claimant count' explanation, which was based on the premise that nothing much had changed, the demographic explanation clearly asserted that this feature differentiated the 1990 recession from previous ones. In this case Britton was among the sceptics:

Britton: If you look at actual localities, you still have the blackspots, and people don't seem to travel to work that much. You can get neighbouring areas which have very high and very low unemployment. So that wasn't particularly my idea. It seemed to me quite an original stab at an explanation, but not one that I would particularly want to back.

(Ibid.)

Despite the apparent novelty of the data, the Panel was reluctant to accept that anything unusual was going on. Instead they tended to view the data itself with some scepticism. The majority took the view that despite the fall in the claimant count the labour force was not responding more quickly to the growth in output than it had done in the past. The attitude of Gavyn Davies is more or less typical:

Considerable uncertainty surrounds the immediate outlook for unemployment. The declines of 26,000 per month in February and March may have been partly due to seasonal adjustment problems, but there have been some confirmatory signals of an improving labour market in other information, including business surveys, and the further fall of 26,000 in May is obviously significant. Although recent figures may have been favourably affected by the fact that too many jobs were shed by many firms last autumn, I assume that the trend has now genuinely turned, and my forecast shows the claimant count falling to 2.9 million at the end of 1993 and to 2.7 million a year later.

(Davies 1993b: 50 para. 20)

Not all Panel members shared this judgement. Patrick Minford, who favoured the explanation that the falls in unemployment were the counterpart of a rise in employment caused by falling real wages, saw the data as indicating an important change in the UK economy. Minford's view of flexible labour markets naturally lends itself to this sort of story, in which wages adjust quite rapidly to move the economy back towards its equilibrium path:

Minford: I'm very encouraged by what you might call the supply side developments - the fact that unemployment's fallen . . . I think it is people pricing themselves into jobs, in the sense that they are so desperate to get work. They are saying to firms, 'For God's sake, anything you say.'

(Interview July 1993: transcript p. 7)

This view suggested that the recovery in 1993 would be different from previous economic upturns. The falls in unemployment were interpreted not as deviations from the norm but as confirmation of a change in the underlying features of the economy. In the flexible economy of the Liverpool model there was every reason to expect that the falls would continue. Of course, this interpretation too was not without its sceptics. As Andrew Britton pointed out:

Britton: If that is the interpretation, that the people who are unemployed now are more flexible, then the outlook for getting unemployment down over the next year or so would also be better. It is not just an explanation of the first few months, but it would be a reason for optimism [more generally]. But the group as a whole are not particularly optimistic about unemployment.

(Interview 12 July 1993: transcript p. 13)

***Closing the debate: October 1993 and agreement on
how to interpret the data***

As has been shown, the falling unemployment figures recorded in the early months of 1993 caused some difficulties for the Panel, and a variety of explanations for them were on offer at their July meeting. When they next met in October 1993 the falls in unemployment had stopped, and the Panel were all forecasting that unemployment levels would remain fairly stable for the rest of the year before falling slightly during 1994.⁶

In addition to the levelling-off of the unemployment figures, the passage of time resolved the debate over the causes of unexpected earlier falls in unemployment. The collective view of the Panel was:

Developments in the labour market now look much less odd than they did in July. Then we saw fairly large falls in *unemployment* virtually coincident with the upturn in activity in the first quarter of 1993. But unemployment has since levelled off, and the profile of non-oil GDP has been revised to show it rising steadily from three quarters earlier than before.

(HM Treasury 1993c: 9 para. 19)

Part of the reason for this improved consensus was the revised profile for GDP that had appeared in the economic statistics published in September 1993. In July, interpreting the unemployment data was difficult because the falls had appeared to occur earlier in the economic cycle than would usually have been the case. However, according to the newly released economic data, the recovery had started sooner than was previously thought. As a result, the falls in unemployment now appeared later in the economic cycle.

This change also brought a new complication. Although there was now a generally accepted explanation for falls in unemployment in the early part of 1993 their speed and scale still needed to be accounted for. In fact it turned out that the falls were not the aberration: rather, the aberration was the rise in unemployment that took place in the months before Christmas 1992. During this period, when it must be remembered there were no economic data to suggest recovery, the Panel believes that there was 'A collective loss of business confidence . . . which resulted in many firms either bringing forward labour shedding or delaying hiring. This led to fewer redundancies, and more hirings, in early 1993, and consequently a fall in unemployment' (*ibid.*).

Taken together these two explanations enabled the majority of the panel to normalize the previously puzzling unemployment data by re-establishing the link between the economic cycle and the rate of unemployment. With the unusually rapid rise and fall in unemployment now accounted for by the actions of the business community, unemployment is restored to its more usual relationship with output and economic orthodoxy is restored. However the consistent mavericks, Wynne Godley and Patrick Minford, disagreed with both the mainstream and each other.

Godley was keen to stress that the revised data was consistent with his view that the UK's economic problems stemmed from a long-term decline in net exports. Thus, he claimed the situation was 'well in line with the strategic predictions which [he] was making, with Cambridge colleagues, throughout the seventies and early eighties, although the violent fluctuations which have occurred have tended to distract attention from the underlying trends' (Godley 1993c: 61 para. 1). Like the others, he saw the rise in unemployment in the autumn of 1992 as an aberration which had to some extent been corrected. However he did not accept that the contribution of the benefit administration to the claimant count measure of unemployment could be dismissed so lightly:

[One explanation for the fall in claimant count unemployment] derives from the fact that the claimant count is, in the words of the *Employment Gazette*, ‘a by-product of the administrative system used for paying benefits’. It seems clear that people who cease to be employed may find themselves inside or outside the categories which make them eligible for arbitrary reasons . . . there are reasonable grounds for supposing that, of those people who have lost their jobs over the last three years (including many self-employed), a greater proportion than normal have left the labour force and therefore that unemployment, properly understood (that is, the number of people who are not employed and are seeking jobs) is higher than the number successfully claiming unemployment benefit. This picture is confirmed by the fact that employment has fallen rapidly over the last year and has hardly increased at all during the last few months, while the population of working age has continued to increase.

(Godley 1993c: 63 para. 4)

Thus Godley’s approach was to increase the complexity of the analysis and to deconstruct the simple relationships between unemployment and employment that the others aimed to restore. The other Panel members did not address the issues of the definition of unemployment and the size of the labour force in their submissions, so Godley’s arguments remained marginalized.

Minford also continued to doubt that things were as normal as the mainstream Panel members made out. He still believed that unemployment had ‘fallen rather earlier in the cycle than in previous upturns’, and he interpreted this as a sign that labour market behaviour is changing’ (Minford 1993b: 74 para. 12). As a result of his belief that the UK labour market was much more flexible than in previous economic cycles, he saw the change in labour market behaviour not as a peculiarity which needed to be explained away, but as an indication that policies had worked. The mainstream economists generally doubted the efficacy of these policies.

Here lies the importance of the debate on how to interpret the data. If Minford had been correct, the government should not have been considering policies to increase participation and turnover in the labour market. If he were wrong, such policies should have remained a high priority. The year’s worth of economic data did not prove the case for or against Minford’s view, so the government’s policy dilemma – to intervene or trust the market – remained.

Balancing the books: the budget deficit, trade deficit and policy options

The other important topic discussed at the July meeting was the medium term outlook for the UK economy. Over this too there was considerable

disagreement. Although the debate was ostensibly about projections for the public sector borrowing requirement (PSBR) over the next few years, it touched on other important issues, including the rationale for macroeconomic policy itself.

The budget deficit

At their first meeting in February the forecasts for the PSBR were discussed in some detail by the Panel, with the debate centring on how much of the deficit was cyclical and how much was structural. The consensus at that time was that there were elements of both and that taxes would have to rise in the future, although only David Currie and Gavyn Davies wanted the measures to be spelled out in advance. By July, the Chancellor's March budget had implemented the kind of post-dated tax increases favoured by some Panel members. As a result, although the PSBR for the year 1993-4 was forecast by the Panel to be between £41 billion and £52 billion, a deficit of this size was not itself perceived to be a problem. With the recession over, the expectation was that the PSBR would fall as the economy picked up. This general process was accepted by the Panel much as it is in the wider economic community:

Public finances lag the cycle. Thus, despite the evidence that the recovery is under way and that employment prospects may be improving, we all expect a worsening of the *PSBR* in 1993-94. However, the average of our forecasts is below the Government's projection of £50 billion. Cyclical improvement, coupled with the tax changes already announced, mean that we all forecast a reduction in the *PSBR* in 1994-95, but only Minford expects a dramatic change. His forecast is conditioned by an assumed reduction in public spending in 1993-94 followed by severe restraint.

(HM Treasury 1993b: 9 para. 19)

Detailed projections about the future level of the PSBR led predictably to more difficulty and disagreement. Part of the problem arose from the scale of the uncertainties involved in estimating the impact of the tax increases already announced. Other more conceptual or theoretical difficulties arose because of the links between the PSBR and other economic factors. As a result, the discussion on the budget deficit quickly expanded into much wider discussions about the overall performance of the economy and particularly the balance between net exports, investment and consumption within GDP.

Forecasts for the PSBR

In general, the Panel did not think that the measures proposed by the Chancellor in his March budget had done enough to ensure an adequate reduction in the

public deficit over the medium term. Four out of the seven called for additional fiscal tightening:

Congdon, Currie, Davies and Sentance all advocate further discretionary tightening in policy to reduce the deficit by 1–1.5 per cent of GDP. This would provide greater certainty that the debt position will become sustainable. For some of this group, the measures should be phased in gradually.

(HM Treasury 1993b: 14 para. 36)

Of the remaining three, Britton was sympathetic to this view, but did not feel that pre-announcing policies was economically justified. (He accepted that there could be political reasons, in the run-up to an election):

Britton: The natural buoyancy of the revenue in the recovery, even if it is not a very long recovery, should get us back to the position where, although we are still borrowing, we are not borrowing at an explosive rate. So I'm not pressing for further tax increases . . . I am quite prepared to accept that the government may prefer to put up taxes. I don't feel that they have to, but they may prefer to up taxes, in which case it is very important that they should accompany that with a cut in interest rates, in order to maintain the strength of the recovery.

(Interview 12 July 1993: transcript p. 5)

Those who did advocate fiscal tightening accepted to some degree that because of uncertainty in the PSBR projections, this was not an entirely sufficient reason for pursuing such policies. For example, Gavyn Davies wrote:

On my central projections for GDP growth, the PSBR would 'automatically' fall to about 4 per cent of GDP by 1997/98 - not far from a reasonable medium term target, which I suggest should be around 3 per cent of GDP (with a debt/GDP ratio of 60 per cent). This suggests that the need for further fiscal action to reduce the PSBR may be rather small, and is certainly swamped by the huge uncertainty in the forecast . . . I argued in my last Panel report that a risk-averse Chancellor would introduce now measures to limit the PSBR in later years . . . If these measures eventually proved unnecessary to control the PSBR, they could be offset by income tax cuts later, in which case a desirable economic reform would have taken place.

(Davies 1993c: 6, 45, 58, paras 4–44)

Of the other two economists, Minford remained adamantly opposed to future tax increases, arguing that the deficit was cyclical. By extending the horizon of the discussions beyond the budget, the July report exposed the rather contrived nature of the consensus produced at the previous meeting:

Minford: [In February we were fairly unanimous] about not having taxes this year, but having them next year. Although I didn't want them next year, we at least agreed we shouldn't have them this year, which is what seemed relevant at the time, with the Budget coming up. But this time [i.e. July 1993] we were in next year, as it were, and obviously could not patch over that one any more, and it came right out of the woodwork. Some people were in favour of big tax increases; others saying no, what's done is enough; others saying no, you shouldn't increase taxes at all, you should cut spending over a reasonable period of time.

(Interview July 1993: transcript p. 5)

Finally, Wynne Godley also opposed tax increases, but for very different reasons. Based on his position that the UK economy was constrained by its inability successfully to export and that domestic demand was very weak because consumers were saving and paying off debts, he argued that reducing the disposable income of consumers through increases in taxation would only further weaken demand, thereby threatening what was an already precarious recovery. His view was that measures to improve export performance were vital. Fiscal policy should only be tightened at the point where consumers' increased expenditure began to increase imports:

The PSBR has become largely structural because Britain's foreign trade performance is unlikely to warrant growth fast enough to have much impact on the public finances. However, taxes should not be put up while the present unusual excess of private income over expenditure persists. When the private sector surplus does fall, fiscal policy will have to be tightened if a larger balance of payments deficit is threatened. The eventual scale of this tightening might have to be very large indeed.

(HM Treasury 1993b: 14 para. 37)

This link between the trade deficit and the budget deficit was also made by several of the other forecasters and is referred to as the 'twin deficit' problem. As might be expected, the Panel differed over the importance of the two deficits, but several were concerned.

*Not one deficit but two: increasing the complexity of
the policy problem*

The twin deficit debate was perhaps the most contentious of all the publicfinance-related debates, as it brought together a wide range of issues, evidence and controversies. If tax increases were simply needed to reduce the PSBR, then they could easily be offset by tax cuts if the deficit disappeared more quickly than expected. In contrast, the problem identified by Godley, and endorsed by several of the others, demanded action which could far less easily be reversed if the analysis proved to be mistaken.

The proponents of the twin deficit problem essentially argued that if past patterns were repeated, an increase in GDP sufficient to bring unemployment down below two million and reduce the budget deficit would have one or both of two adverse effects. It could increase inflation (thus requiring deflationary polices), or it could increase imports, and hence the trade deficit, to such an extent that interest rates would have to rise to prevent a 'sterling crisis'. On the other hand, if GDP growth were restricted sufficiently to avoid these pitfalls unemployment would not fall. This would place additional demands on public funds, the PSBR would continue to grow, and further tax increases would be needed to fund it. The issue therefore brought together the disputes about unemployment, net trade, and fiscal policy.

In the discussion of unemployment and the importance (or not) of hysteresis, it was shown that the majority of the Panel thought growth in output would be limited because of the inflation that would occur as unemployment dropped below two million. As a corollary, if unemployment remained high, then so would the PSBR. On the other hand, for those who believed that unemployment could fall to around one million without inflation picking up the deficit problem did not exist either. As Andrew Britton remarked:

Britton: The question about how far [the public sector finances] will improve is of course entirely bound up with how far you think the recovery can go – the optimists on the natural rate of unemployment are also optimists on public borrowing; the pessimists on sustainable growth are pessimists on public borrowing.

(Interview 13 July 1993: transcript p. 5)

Patrick Minford was the least impressed with this argument. He disputed its basic premise, that labour market rigidities were such that unemployment could not fall significantly without inflation rising:

Minford: I mean the sorts of factors that David Currie and Andrew Britton are talking about, the labour market being difficult, I believe that in the next six months we are going to see that that's completely wrong.

(Interview July 1993: transcript p. 5)

Similarly he saw no reason why the UK could not compete effectively in world markets. Although he forecast a stable rather than a decreasing current account deficit, this was in the context of sustained and non-inflationary economic growth. Minford summarized his position as follows:

Minford: Our forecast for the current account is running at levels not dissimilar from the present levels against a background of quite sustained growth: 3.5 per cent, for a long time to come . . . [But] the reason the economy is growing, is that it is driven by potential output and the natural rate [of unemployment] being lower than the actual rate. So people are getting back to work and producing.

(Interview July 1993: transcript p. 15)

The other optimist on the balance of payments was Tim Congdon. Like Minford's his story was based on the belief that the UK was a long way below potential output and above the natural rate of unemployment. The growth/inflation combination figures he forecast reflected this. However Congdon's forecasts for the balance of trade were also low, principally owing to a fall in imports which the more pessimistic members of the Panel did not foresee (Congdon 1993a). For example, Wynne Godley noted that for Minford and Congdon's forecasts to be consistent they 'are (must be) assuming a growth in net export demand far in excess of anything that has ever occurred in the last fifteen years' (Godley 1993c: 66 para. 7 fn. 3).

*Appropriate policy targets: the allocation of resources
within the economy*

The twin deficit problem is not just a matter of economic forecasting: it also contributes to debate about the appropriate boundaries of economic policy. In particular, Congdon is on record as saying that the balance of trade deficit should not be a matter for government concern provided its own finances are in order (Congdon 1993a). It was his belief that the trade deficit represents the actions of free economic agents and should not be subject to political interference. The pessimists disagreed: not only did they forecast a widening trade deficit, asserting that the general patterns of the past would be repeated, they also believed, *pace* Congdon, that this would be a problem. Godley, the strongest proponent of this view, articulated his concerns as follows:

Whether or not a balance of payments deficit 'matters' seems, unfortunately, to still be a vexed question. To my eye it is obvious that it does matter if only because no country can get into debt to an unlimited degree.⁷

(Godley 1993c: 66 para. 7)

In Godley's economics, the balance of payments was crucial and his policy advice was that 'the government should attach extremely high priority to achieving a rapid expansion in net export demand' (ibid. 69 para. 17).

The mainstream economists on the Panel tended to agree with Godley that poor export performance and a high propensity to import were serious weaknesses in the UK economy. They also agreed that this was a suitable area for government action although they did not give the same priority as Godley to net trade performance. Rather they framed the problem in terms of the 'balance of the economy': to use a slicing-the-cake metaphor, if one person has too much there is not enough left for everyone else. Expenditure on consumption was, they argued, crowding out net exports and investment. Their policy solution, which was quite independent from the sustainability of the PSBR, was to act to reduce consumption as a proportion of GDP, increasing the portion of the national cake available for exports and investment. Policy interventions of this sort were backed by Andrew Britton, David Currie, Gavyn Davies and Andrew Sentance.

In order to achieve this redistribution of resources within the economy, Currie and Davies proposed additional taxation on top of the increases announced in the March Budget. Currie was the more moderate of the two, justifying a fiscal tightening of 1.5 per cent of GDP as follows:

Total consumption, counting both private and public is at too high a level . . . With so much of GDP devoted to consumption, there is insufficient room for investment and net exports. For last September's devaluation to be made to work, in terms of channelling more resources to the balance of payments to reduce the external deficit, total consumption needs to be held back to make room for net exports. This may be done by cutting public consumption or curtailing private consumption by tax increases . . . Public spending cuts are a natural choice, but the danger is that they can be indiscriminate, cutting into areas of spending that are important for promoting longer term growth, for example, spending on education and training, R&D, and infrastructure. Indeed, it can be argued that in these areas there is a need for increased spending to promote the international competitiveness of UK industry . . . Because of this, the Chancellor may have to consider increases in direct taxes, along with other measures.

(Currie 1993b: 40-1 paras 18-20)

Davies foresaw the need for a fiscal tightening of 'at least 2-3 per cent of GDP', one half going to reduce the PSBR and the other to fund special employment training and other infrastructure programs:

It is now extremely well known that the trade deficit is unusually large for the present stage of the economic cycle . . . The share of

personal consumption in GDP is correspondingly large – around 66 per cent, compared with a long-term average prior to 1985 of about 60 per cent. There is a similar ‘excess’ of real personal disposable income in GDP at the present time . . . Ideally, we need to shift resources out of short term consumption (public as well as private), and into both exports and investment . . . Since the government has recently re-affirmed its medium term spending targets at unchanged levels, higher taxation would need to bear the brunt of this action.

(Davies 1993c: 59–60 paras 51–3)

Summary

The discussions about public finances exposed some interesting differences between the views of the Panel of Forecasters. Some believed that the forecasts for future borrowing were sufficient to justify further tax increases, but Minford drew the opposite conclusion. A second set of arguments interpreted the PSBR figures in a different way, linking the forecast government borrowing to other factors, particularly the trade deficit and consumption, but still came down on the side of further policy interventions. These were disputed by Tim Congdon. The claim was made that regardless of the uncertainty surrounding the forecasts for the PSBR, the distribution of resources within the economy was wrong, with consumption attracting too high a share of national income and investment and net exports too little. As a result, actions were needed to prevent the trade deficit from holding back economic recovery, including increases in taxation to reduce consumption and improvements in education and training and incentives to increase investment and research and development. Only in this way could both the budget deficit and the trade deficit be reduced.

The limits of economic policy

The forecasts for, and recommendations on, unemployment, government borrowing and the balance of payments can be grouped into three main strands. The first did not see a major problem and projected low unemployment, a balanced budget and acceptable trade figures. The second group considered the fiscal tightening proposed in the budget to be insufficient to ensure the sustainability of the PSBR and recommended additional measures. The third strand accepted that the PSBR projections did not fully justify further tax increases but found other reasons why they were desirable. These sets of arguments about fiscal policy, together with their proponents, are summarized in Table 6.1.

A majority of the Panel advocated policies to improve competitiveness and hence net exports (increased training etc. for most, increased demand for

Table 6.1 Views on fiscal policy

<u>No problem</u>	<u>Structural deficit remains</u>	<u>Balance of demand is wrong</u>
Minford	Congdon Currie Davies Sentance	Godley Davies Britton Currie Sentance

Godley). Although they suggested a variety ways of achieving this, they all focused on three points:

- 1 Policies were needed to retrain the long-term unemployed, reduce long-term unemployment (and hence hysteresis) and restrain wages.
- 2 Training and skill levels in the rest of the economy needed to be improved to increase productivity in the economy as a whole.
- 3 The spending and taxation priorities needed to be changed so that the first two points could be achieved.

There were of course variations in the recommendations for specific policies and actions. However, all the recommendations were for long-term policies which required a change in the political culture if they were to succeed.

The two economists who have been most involved with these policy issues are probably Andrew Britton of the NIESR and Andrew Sentance of the CBI.⁸ Interestingly both argued that increases in taxation were not necessary to bring about the desired changes. Rather, what was needed was a change in the culture and priorities of government spending:

Sentance: I think that if we want more [Research and Development or investment], we can't just say that the overall scale of government activity has to increase further in order to do it. I think there has to be some re-orientation of priorities within existing government programmes. Now I think many of the, some of the members of the group take it as a sort of given that it is very difficult to restrain government spending, and you can't do that, but I just don't take that view.

(Interview 13 July 1993: transcript p. 12)

At this stage I would not argue for tax increases beyond those already announced, or spending cuts beyond those necessary to keep within the totals already agreed. What is required however is a change in the structure of both spending and taxation to encourage

growth of output and especially of employment. Other objectives might have to be sacrificed if that aim is given priority.

(Britton 1993c: 26 para. 25)

This challenge to policy makers to change the priorities of public spending was not endorsed by all Panel members. In conclusion, it is clear that there was fundamental disagreement among Panel members, in their analysis of the economic system, in the forecasts they derived from this analysis, and in the policy recommendations they made as a result.

The issue that must now be considered – and which is the subject of the next chapter – is the appropriate response to this situation. The dispute is really on two levels at once. At one level there is the economic issue, identifying the correct model of the economy, and at the another level there is a policy question, identifying the most appropriate action in the light of the forecast situation. In this situation, it is important to ask how the role of expert economists as advisors should be interfaced with the role of politicians as policy-makers, and at what point input should be obtained from experts in other relevant fields.

DIFFICULT CHOICES

Policy recommendations in October 1993

Introduction

Should economists (or other scientists for that matter), who disagree profoundly at all levels from theoretical analysis to practical recommendations for action, present policy makers with consensus recommendations? If so, how can this be achieved? And if not, how can disagreements best be explained and differing recommendations presented?

This final chapter about the meetings of the Panel of Forecasters addresses these issues. It highlights the differences which existed between the Panel members. It explores their conviction that consensus recommendations were needed if they were to influence economic policy and looks at the ways in which they worked together to produce these.

The empirical material focuses on the policy recommendations made by the Panel to the Chancellor in the run-up to the UK's first ever unified budget in December 1993. It is suggested that although the Panel's achievement of consensus was aided by the relatively non-doctrinaire nature of the problem, it was also due at least in part to the determination of some Panel members to be seen to achieve it.

The aim is to use the way in which the Panel reached consensus to open up questions about the boundaries of expertise. There is, as has been demonstrated earlier in the book, a great deal of judgement involved in making economic forecasts and recommendations. Economic science and econometric tests have a part to play, but the actual recommendations produced also draw on more social and political attitudes and opinions. It could be argued that business economists, trade union representatives and training agencies, for example, have relevant viewpoints and experience. At what point should this be introduced into the process? Similarly to what degree are social and political choices delegated to economists, and to what degree should they be? How can adequately informed decisions be made by elected and accountable representatives of society rather than by a professional elite?

The chapter examines the policy recommendations made by the Panel and compares them with the policies subsequently announced by the Chancellor.

The first section outlines the nature of the policy consensus that the Panel appeared to reach despite their different theories, models and forecasts. The second part of the analysis focuses on the perhaps unintended consequences of this decision. In particular, it explores how, in their efforts to present a unified front, the Panel removed from public and political debate decisions on issues which they themselves felt to be important for the future prosperity of their fellow citizens.

Policy recommendations in October 1993: reaching consensus

When the Panel met for the third time in October 1993 their discussions were clearly mindful of the fact that the the UK's first ever unified budget would be presented to Parliament on 1 December (just six weeks after the publication of the Panel's October report).¹ As was the case in February, when the Panel also focused on policy recommendations for a forthcoming budget, the extent to which they could sign up to a coherent and agreed set of economic policy recommendations was important for many commentators in assessing their worth. As we shall see, Panel members consciously took such an aim on board as they took part in the discussions and drafted the report.

It is an important part of the argument that there were potential disagreements between the Panel over the necessary economic policies. These views obviously stem from the different models and futures discussed in the preceding two chapters. The important difference is that the focus has moved from disputes about economic theory to those about policy. Further, where in previous chapters the focus was on the medium term, here it is on the short term. The core concern is with the way in which the Panel moved from a range of forecasts for the medium term to a consensus over short-term recommendations to the Chancellor.

The opening part of this section is concerned with the policy recommendations themselves, and examines the range of views that lay beneath the surface of the Panel's collective viewpoint. Having established that there was in fact a range of options, the chapter goes on in its latter part to examine the social processes that lay behind the consensus. In particular, it looks at the extent to which the consensus was made to happen by the Panel members themselves.

Deconstructing consensus: what the Panel said to the Chancellor in October 1993

The starting point for the Panel's policy recommendations in October 1993 is their diagnosis of the current economic situation. In particular, the recommendations were held together by their agreement that 'the overriding imperative of policy is to ensure a sustainable fiscal position' (HM Treasury 1993c: 15 para. 36). As a result:

[The Panel] would therefore all support the announcement of tax or spending reforms, desirable in their own right, which would secure a net reduction in the PSBR [Public Sector Borrowing Requirement]. If it subsequently becomes clear that the PSBR will fall by more than is required, taxes can then be cut or public investment increased.

(HM Treasury 1993c: 15 para. 38)

Because the Panel disagreed about the strength and duration of any future economic growth, they could not be sure that the recovery would bring about an increase in tax revenues large enough to reduce the PSBR to an appropriate level. The consensus view was that the ‘public finances are probably just about on or within the border of sustainability’ (ibid.: 13 para. 34) but that inaction in 1993 would be harder to correct later on. In other words, it was *not* a consensus that some form of fiscal tightening was necessary. In fact, as the report made clear, ‘there is no consensus whether further fiscal tightening will be necessary in practice’ (ibid.: 15 para. 36). The wording adopted was that the Panel:

would all support further fiscal tightening over the next three to four years *if it became necessary* to ensure sustainability.

(HM Treasury 1993c: 15 para. 36)

This may seem little more than a statement of the economically obvious: an unsustainable budget deficit is surely a bad thing, and what economist could recommend an unsustainable, or oppose a ‘necessary’, policy? Consequently, it might be argued that the consensus on the public finances had little if any real content.

To some degree Panel members accept this:

Britton: I think it is true, just from the arithmetic of the budget and what is a sustainable deficit and what is not, that this is a non-doctrinaire sort of issue.

(Interview 27 October 1993: transcript p. 9)

However, this conclusion is not entirely justified. As Gavyn Davies pointed out, although there were some ‘motherhood-and-apple-pie’ statements in the report about the importance of fiscal sustainability, ‘you would not have got most Keynesians to write that down ten years ago’ (interview 27 October 1993: transcript p. 16). Thus there was some real significance in the consensus that the size of the government debt and its interaction with the economic cycle were problems. Moreover, even if the underlying analysis drew on little more than the IS-LM curve, in which increases in taxation or reductions in public spending are offset by a relaxation of monetary policy, the

recommendations derived from it constituted a significant consensus. As Gavyn Davies put it:

Davies: There are big issues about what you want the overall stance of macroeconomic policy to be. Do you want it to tighten or not to tighten? Usually you will not get economists to agree on that kind of thing . . . I noticed in the public response to the last report, some people have said 'You could only agree about the obvious'. Well, that may be true. But I still think that there is a hell of a debate going on in Britain at the moment about whether the Chancellor should put up taxes in the budget or not. This Panel has gone away and thought about it and come up with an answer.

(Interview 27 October 1993: transcript pp. 16-17)

The aim here is not to repeat the ill-founded criticism that the Panel agreed on nothing of substance. Rather it is to explain the rationale behind the policy statements, and to show how their production depended on the reasonableness of the Panel members and their own efforts to achieve consensus. In order to show just how much the Panel had to converge in order to produce even the limited agreement of the October Report, their range of views will be outlined. These illustrate the 'gaps' which had to be closed, or at least papered over, if consensus was to be reached.

Why the PSBR could not be left to chance

The policy recommendation put to the Chancellor by the Panel was that in the December budget a 'prudent' Chancellor would 'introduce a package of tax and spending reforms, desirable in themselves, which would reduce the PSBR over a number of years' (HM Treasury 1993c: 3). In their individual submissions some Panel members went into more detail, and specified the sorts of reforms they would like to see and when they ought to be implemented. It is at this more detailed level that the apparent consensus starts to break down and the need for a general formulation becomes apparent.

Consider for example the recommendations for fiscal policy. The majority of the Panel recommended some sort of fiscal tightening, with the most straightforward exposition coming from Tim Congdon who stated that 'there is no doubt that public sector borrowing is unsustainably high and that steps must be taken to reduce it' (Congdon 1993e: 35 para. 15). Although quite substantial tax increases had been announced in the March 1993 budget, it was Congdon's belief that the 'structural Budget deficit probably remains at over 3 per cent of GDP (i.e. roughly £20 billion)' (ibid. p. 35 para. 16). He stated that 'three years is surely quite long enough' (ibid.) to reduce this to zero.

At one level this concern was shared by the other members of the Panel. For example David Currie also wrote that:

There is a need in the Budget to curb the PSBR, which we estimate at just under £50 billion this financial year . . . we see the PSBR next year at £38 billion, still some 6 per cent of GDP . . . At this level of borrowing, the interest paid on new borrowing cumulates rapidly, leaving a heavy burden of debt interest to be financed in the future.

(Currie. 1993c: 41 para. 8)

However a more detailed look at Currie's analysis and recommendations rapidly reveals the gulf between his position and Congdon's. Congdon's monetarist analysis of the economy was based on the concept of deviations from natural rates tending to restore equilibrium. The economically more mainstream members of the Panel tended to introduce additional factors such as hysteresis in the labour market and the effects of the trade deficit on economic growth (both discussed in the previous chapter). As a result, they saw the economic future as rather more problematic. The effect of hysteresis is to produce a much poorer trade-off between growth and inflation than Congdon's analysis suggested. The resultant prediction that inflation would rise sooner rather than later had an impact on financial markets, so proponents of this view argued that the Chancellor needed to be attentive not just to the situation in the UK, but also to the perception of that situation abroad.

Currie commented:

High borrowing leaves interest rates more vulnerable to shifts in sentiment in financial markets, and may make it more difficult for the authorities to avoid an interest rate rise, particularly if the inflation background deteriorates.

(Ibid.)

This more cautious estimation of the future was also reflected in his policy recommendations. He was less optimistic about the potential for growth in output than Congdon, and he therefore recommended that measures be 'phased in to avoid undue impact on [the] recovery during 1994' (ibid.: 41 para. 10). In addition, he was concerned about export performance and wanted to see a shift in the balance of macroeconomic policy in order to control inflation while encouraging exports. This related to the twin deficit problem discussed in the previous chapter. The idea was that the Chancellor needed to place more emphasis on fiscal policies in order to control inflation, while simultaneously facilitating lower interest rates and a more competitive exchange rate:

Loose fiscal policy and tight monetary policy make it more difficult to maintain a competitive level of the pound without inflationary pressures emerging, impeding adjustment of the external current account deficit.

(Ibid.)

Currie's view is thus more complex than that of Congdon, who saw the question primarily in terms of reducing the PSBR as quickly as practical. In practical terms, Currie recommended 'Budget measures to curb borrowing amounting to some £3-4bn over and above [those] announced by the previous Chancellor [in the March budget]' (ibid.: 41 para. 10). Like Congdon, he recognized that they could take the form of either additional tax revenues or reduced spending, and he identified several ways in which the reduction in borrowing could be achieved. For example:

One option for the Chancellor is [to] take full advantage of the new integrated Budget to lower the public expenditure Control Total set by the Cabinet in July, to reflect lower inflation and the continued policy of tight control on public sector pay. This could cut public borrowing by some £2bn, requiring only modest additional action to raise revenues.
(Currie 1993c: 41 para. 10)

However, Currie was concerned that such a policy (which Congdon doubted the government had the 'political will to implement' (Congdon 1993c: 35 para. 15) would fail to address the long-run problems in the British economy. Thus, he continued:

A preferable alternative would be to maintain the Control Total and redeploy resources within the total from public consumption to public investment. This would allow support of those areas of public spending that help strengthen the longer run supply side performance of the UK economy, including education, training and R&D. In that case, he will need greater action on the revenue side. What should be avoided on the expenditure side is cuts in public investment to maintain public consumption.

(Currie 1993c: 41-2 para. 10)

Currie was not alone in these concerns: several other Panel members provided fairly similar analyses and recommendations. There were some differences within this group, however. For example, Andrew Sentance also argued 'that the medium term outlook for public borrowing is too high' (Sentance. 1993c: 81 para. 13) and like Currie identified the opportunity created by lower than expected inflation to reduce public expenditure. But unlike Currie, he regarded the option of reducing public expenditure in this way as a reasonable policy choice:

Just as higher than expected inflation created overruns in spending in the late 1980s and early 1990s, it would seem appropriate to try and take advantage of a lower inflation climate to claw back some of these increases in the years ahead.

(Sentance 1993c: 81 para. 12)

Sentance was also one of the economists who believed that the UK economy devoted too large a share of its output to consumption. It is not therefore surprising to find that, like Currie, he argued that ‘a further fiscal tightening is [needed] to change the balance of fiscal and monetary policy in a way which is favourable to sustaining a higher level of investment and net exports’ (ibid. 82 para. 14).

This analysis was also important for Gavyn Davies who likewise argued that ‘a further shift in the fiscal/monetary [mix] looks desirable in order to control consumption, boost investment and maintain a competitive real exchange rate’ (Davies 1993d: 47 para. 4). In terms of practical action Davies suggested, as he did in July, that the Chancellor should introduce ‘consumption-reducing measures [which] should probably build up to at least 2-3 per cent of GDP over the next 4 years’ (ibid.: 56 para. 35). However, unlike Currie, Sentance or particularly Congdon, Davies believed that there was only a ‘modest case’ for implementing these measures in terms of the sustainability of the PSBR alone:

On central economic forecasts, the PSBR should fall to 3.6 per cent of GDP by 1997/98. This compares with an appropriate target of about 2.5-3 per cent of GDP . . . Given the immense uncertainty in all projections of this type, there is obviously no compelling case from a PSBR/financing point of view for further fiscal tightening.

(Davies 1993c: 47 para. 4)

Andrew Britton supported this aspect of Davies’s argument, which emphasizes the uncertainty surrounding any projections of the PSBR. In fact Britton went further and was one of the few Panel members who thought that changes to fiscal policy did not need to be announced in the December budget. On the basis of the NIESR’s forecasts he argued:

Our projections of the public sector financial position do not suggest the *need* for any further tax increases in the November Budget. The level of debt and borrowing implied for the medium term – in so far as this can be calculated with any degree of accuracy – does not appear imprudent.

(Britton 1993d: 28 para. 29)

In interview, Britton argued against immediate action as follows:

Britton: You should wait because the recovery may stall and you already have a big tax increase coming along in the early months of next year. In the case of our particular forecast there is also a bit of anxiety about the effect of that on the price level. We could go over the top on the price level simply because of the indirect taxes [and] that would ruin your plans for reducing interest rates.

(Interview 27 October 1993: transcript p. 6)

However, in the report itself he did not dissent from the view that it would be 'prudent' for the Chancellor to announce tax and spending reforms in the budget. Thus, even though announcing tax and spending plans in advance was not something he personally advocated, he was prepared to concede that this could be a reasonable strategy for the Chancellor to follow. The reasons he gave for this were as follows:

Britton: In the Panel meetings themselves there was a lot of discussion about the relative risk. It wasn't so much a question of whether you have to, but whether perhaps you nevertheless ought to . . . Should you, in some sense, try and play safe? Would you feel safer if you had a tax increase under your belt that you could always give away again? Well, I certainly accept that argument . . . I wouldn't mind so much if he's pencilled in something for later years, because I regard this pencilling in as a rather abstract exercise which can always be undone at the next budget if you don't like it . . . [But] if you have got firm plans - you know that you want to widen the VAT base as soon as possible or you want to abolish mortgage interest relief - there is no great harm in announcing those plans. It might even reassure the markets that you have got the situation under control.

(Interview 27 October 1993: transcript pp. 6-7)

Finally, Patrick Minford's contribution to the debate was interesting not least because his forecast for economic growth was very different. As noted in previous chapters, Minford believed that the PSBR was almost entirely cyclical. As a result, he expected that:

first the public borrowing we see would melt away [and] secondly there would be no need for the public spending programmes allegedly needed to boost competitiveness. Thirdly, interest rates, currently at around 4 per cent adjusted for inflation, could come down without risk in order to underpin the still-fragile recovery.

(Minford 1993c: 75 para. 17)

Minford also did not accept the claim, made explicitly by Britton in the quote above and implied in the consensus view of the Panel, that increases in taxes could be undone later on if it turned out that they were not necessary. He argued:

reversing damaging tax and spending decisions is not easy, because they create vested interests in their continuation; spending ministries always find good ways of spending tax revenues and spending programmes acquire lives of their own. Even if reversal could be

guaranteed there is still the loss of output meanwhile, as stagnation is unnecessarily prolonged.

(Ibid.: 75–6 para. 18)

To sum up, behind the consensus that a prudent Chancellor ‘would introduce a package of tax and spending reforms’ to ensure control of the PSBR were the following distinct, and to some extent contradictory, pieces of advice:

- Tim Congdon arguing that the PSBR was unsustainable and that fiscal reforms to reduce it should start straight away.
- David Currie and Andrew Sentance arguing that the PSBR was unsustainable and that measures to correct it needed to be phased in.
- Gavyn Davies arguing that the uncertainty of the forecasts meant that it was very difficult to judge whether the PSBR presented a problem, but that the balance of policy needed to shift to boost exports and investment (an argument also supported by Currie and Sentance).
- Andrew Britton arguing that no policy announcement was needed in 1993 because the PSBR appeared to be under control.
- Patrick Minford arguing that the PSBR was not the problem: it was ensuring the recovery that mattered.

How the policy consensus was reached

There is no reason a single set of policy recommendations derived from these disparate views was either necessary or achievable, and at least one of the economists on the Panel disputed the suggestion that they should try to reach one:

Congdon: I don’t think the purpose of the Panel is to be consensual, and that certainly isn’t what we were told. That certainly isn’t my understanding of its purpose. It is to bring together these individuals so that they can express their views. It may happen that on some things they agree, but it isn’t the purpose to reach consensus . . . I certainly don’t see the Panel in those terms, and if I don’t agree with what the rest of the Panel is saying I will indicate that I don’t agree with it . . . But there can hardly be any disagreement with the point that in the end, the growth of the public debt must be brought under control.

(Interview 27 October 1993: transcript pp. 13–14)

Thus for some, as noted at the beginning of this section, the reason for the consensus is the non-doctrinaire nature of the problem. Patrick Minford also remarked:

Minford: On this occasion, the disagreements between the Panel about what exactly the policy would be were not very great in fact . . . I think most people

assumed that they would cut interest rates in response to a fairly weak economy, and most people also assumed that they would take some steps to tighten fiscal policy one way or another.

(Interview 25 October 1993: transcript p. 9)

However, this remark (which focuses more on the assumptions than on the recommendations) downplays the significance of Minford's own reasonableness in the meetings. Minford was one of the outliers, so it was crucial to producing the consensus that it should encompass his stance. In particular, he had to accept the issue of fiscal sustainability and the risk that the budget deficit had a significant structural component. He was able to do this because he saw other reasons why the recommended policies were desirable. Thus, as Andrew Sentance pointed out, Minford's reasons for endorsing the policy recommendation were rather different to the rest of the Panel's:

Sentance: Patrick believes that, later in this Parliament, the Chancellor, assuming he follows our advice, will have a lot of scope for reducing taxes in the way that Patrick would like to see. So he is relaxed about it. He doesn't see this as being necessary for fiscal tightening means. He sees that other people want it for fiscal tightening means, but that's not why he's wanting it, although he has agreed to put his name to it.

(Interview 29 October 1993: transcript p. 14)

In other words even this limited consensus called for flexibility on the parts of Britton and Minford.

Some members of the Panel saw the consensus as a simple recommendation for the economics of fiscal sustainability, but others were more attentive to the wider social context and the pressures that this created. For example, in an early interview Minford commented that one of the 'interesting things' about the Panel's first ever meeting was that 'We were all trying to be nice I think . . . [to] make an effort [for] our community of economists, and the community at large . . . there was that sort of spirit about it, which was quite interesting' (interview 31 March 1993: transcript p. 20).

Indeed Minford's view was that if the group was to survive and become truly useful, it needed to develop its own 'group dynamic where people are willing to actually confront evidence and modify their positions over time . . . Otherwise it is simply going to be a bunch of people who disagree the whole time' (interview July 1993: transcript p. 2). As a result of this, he believed that:

Minford: The only way it will be successful is if it develops a group ethos. At the moment [July 1993] it hasn't . . . but, in a way, that doesn't matter because it can still develop over time . . . I think we have all got different views but,

at the end of the day, there are important public interest issues to be settled. We can't afford to let our vanity or whatever it is get in the way of a sensible discussion.

(Interview July 1993: transcript p. 9)

By October 1993 Gavyn Davies could see that progress was being made in developing this group ethos:

Davies: I do think the Panel is showing some signs of developing in ways which are separate from the three-meetings-per-annum cycle. I think we are going to have additional meetings about different subjects, more subject-related than forecast-related. So those three meetings will exist, but I think in addition there will be other things, which I would think is going to be good . . . I still think we are kind of groping our way towards a role, but the last set of meetings were actually pretty good. They were good meetings in themselves and I think there was more of a corporate spirit . . . People are getting to know each other and feeling part of the same operation, as opposed to just visitors to a meeting. I think more of us are buying into the idea that we want to make this thing work.

(Interview 27 October 1993: transcript pp. 12-13)

The social context of the Panel – the fact that they were a group of people in an important position – clearly had an effect on the way that the Panel members acted.

*Reasons for agreement: social pressures and
budget arithmetic*

It has been shown that there were several different views on offer at the October meeting, and that there was no obvious way in which they reduced to a single policy. That they did so is due at least in part to the Panel's efforts to make it so. Certainly there was a small but significant movement towards agreement during the meeting, which was noted by Gavyn Davies:

Davies: I think there was more agreement coming out of that meeting than there was going into it, which suggested that there was a willingness to compromise in order to reach a common objective. . . . I think there was a genuine tendency for people to see each other's point of view and amend their own position as a result of the discussion. I don't want to overstate this. I don't think there was that much disagreement going in, and I don't think there was 100 per cent agreement coming out, but there was a tendency to move towards

agreement, which I don't think was there in the previous two meetings.

(Interview 27 October 1993: transcript p. 16)

Why did at least some of the Panel members feel that it was more important to reach an agreement than to stick to the distinctive positions derived from their different economic theories? One reason might be the sense of collective or institutional identity that several of the Panel members felt was developing. There was also a view that if the Panel was to be judged useful, then it would be important for the members to minimize their differences and agree a single strategy for the Budget.

Several Panel members made this point quite explicitly in interview, although each stressed slightly different reasons for wanting consensus. For example, Andrew Britton highlighted the role of press and public opinion:

Britton: I think you are right about seeking consensus. We felt, particularly after the July Report, which got rather a bad press, that people found the arguing in public a bit tedious and that they would prefer us to emphasize the agreements rather than the disagreements.

(Interview 27 October 1993: transcript p. 8)

Andrew Sentance agreed that there was an effort towards reaching agreement, but 'not to placate the media' (interview 29 October 1993: transcript p. 13). Rather:

Sentance: there was a feeling that here was a man we were supposed to be reporting to - the Chancellor of the Exchequer - and he was going to have to take some tough decisions in the Budget. He was going to look at this report and he was going to say 'What is this Report telling me about what I should do in the Budget?' And if we couldn't come to some sort of strategy that really came out of the views of the Panel then I think, quite legitimately, he would say 'This is all very well, but it is not helping me a great deal'. [So] it was an attempt by us to do our job, which is to advise the Chancellor.

(Interview 29 October 1993: transcript p. 12)

The important point is that the Panel actively worked to bring about consensus, for reasons related in various ways to the perception of others: the media, the Chancellor, other economists and so on. As Gavyn Davies pointed out:

Davies: I think that the Treasury has given us a massive opportunity, both as individuals and as representatives of economics outside the Treasury.

(Interview 27 October 1993: transcript p. 13)

It seems that this responsibility was one taken seriously by the Panel and was one of the reasons, they were able to find ways of working around their differences and speaking to the Chancellor in October 1993 (and to the public) with a single voice.

Effectiveness: what the Chancellor actually did

The final section of this chapter briefly examines the effectiveness of the Panel of Independent Forecasters in shaping economic policy. The measure of effectiveness used is simply the extent to which the Chancellor appeared to heed the advice given to him by the Panel. The previous section outlined the individual and collective policy recommendations made by the Panel members. This section begins by briefly outlining the background to the December budget and the policies announced by the Chancellor.

Chancellor Kenneth Clarke's maiden budget was presented to Parliament on 1 December 1993. It was unusual in that it was the second budget of 1993. The first, which was announced in March by Norman Lamont, also followed a report by the Panel which made broadly similar recommendations about the importance of ensuring a net reduction in the PSBR. Lamont's budget echoed this advice: it imposed tough spending limits and increased taxes over a period of years. The budget was economically successful, in that it provided some reassurance that government borrowing was under control, but it was politically controversial, and Lamont resigned as Chancellor shortly afterwards.

In December the challenge facing Clarke was to keep the confidence of the economic markets while restoring the electoral prospects of the Conservative government. His budget essentially continued Lamont's commitment to control government borrowing and fight inflation. Clarke began his Budget speech:

My first priority has been to sustain the economic recovery now underway and to create the right climate for growth and jobs. I have been determined to take no risks with inflation. To achieve these objectives, the task of my first Budget has been to set the governments finances on a sustainable path for the rest of the decade.

(Quoted in *The Times*, 1 December 1993)

From this it seems obvious that the Panel were successful in setting, or perhaps rather maintaining, the policy agenda. The similarity is clear between the task the Chancellor set himself and the Panel's advice 'that the overriding imperative of policy is to ensure a sustainable fiscal position' (HM Treasury 1993c: 15 para. 36). Not surprisingly the Panel gave the budget a relatively warm reception Lisa Vaughan, writing in the *Independent*, reported the Panel

as having ‘applauded [it as] “brave”, “skilful” and “an appropriate start”’ (Vaughan 1993).

Despite this positive reaction, the budget did not wholly reflect the Panel’s priorities. Among the points of agreement were the decision not to raise direct tax rates, the reduction of tax relief on mortgage interest repayments to 15 per cent and the widening of the tax base to include new taxes on air travel and insurance. However, in their report the Panel listed as ‘an important caveat’ that they would ‘caution against anything more than a modest further tightening of the fiscal position for 1994–95 because of the large tax rises already in place’ (HM Treasury 1993c: 15 para. 40). Clearly the Chancellor did not accept this concern, and as a result several of the Panel were concerned about the budget’s effect on the recovery and feared that it might slow growth, despite the 0.5 per cent cut in interest rates announced the week before. Thus, Andrew Britton was concerned that the negative effects of the extra taxes announced by Clarke would not be offset by increases in economic growth: ‘By raising taxes, he has reduced the amount people can afford to spend . . . The measures he has announced could clearly cause hardship unless the economy expands more rapidly than we are forecasting’ (quoted in Vaughan 1993).

As a result of the budget’s deflationary impact some of the Panel reasserted their recommendation that ‘any further tightening of fiscal policy should be offset with lower interest rates’ (HM Treasury 1993c: 15 para. 42). For example Wynne Godley, while accepting that the Chancellor was right to take a tough line, was concerned that it might result in what little recovery there was coming to a premature end. He therefore warned that ‘I do think that it will cause a very severe check to the expansion, which in any case is not very robust . . . Interest rates ought to be cut again’ (quoted in Vaughan 1993).

In a slightly different vein, Patrick Minford also felt that interest rates could be lowered as a result of the budget. Although he had no worries about the government borrowing, which he had always regarded as mostly cyclical, he was concerned about the weakness of economic growth. By taking action that ensured the PSBR was firmly under control, he felt that the Chancellor had now removed that last ‘fiscal excuse’ for not cutting interest rates and boosting growth (quoted in Vaughan 1993). Thus, although the Panel were influential in terms of the basic budget guideline (to ensure fiscal sustainability) both individual members and the consensus differed from the Chancellor over the extent to which monetary policy should be adjusted to compensate for the measures announced.

There were also more significant ways in which the Panel’s priorities were not fully reflected in the Chancellor’s speech. The Panel was almost unanimous that the PSBR should be reduced through increases in taxation. Although the Chancellor did announce tax increases in his budget, they were partially offset by increased expenditure. In fact, of the £5.5 billion reduction in the PSBR

announced (far larger than anything the Panel had considered necessary or even possible), only £1.75 billion came from the changes to the tax and benefit system. The majority of the saving came from a reduction in expenditure of about £3.5 billion, which was made possible by halving the contingency reserve.

This decision to reduce expenditure was clearly counter to the views of the Panel members who had explicitly argued that increases in certain categories of expenditure were needed. For example, Gavyn Davies had recommended that taxes be increased, with half the extra revenue being used to ‘boost public investment, education and training’ (Davies 1993c: 47 para. 5). A similar economic priority was to be found in David Currie’s submission. Indeed, while he correctly anticipated that the lower than expected inflation could be used by the Chancellor to reduce public expenditure, Currie had counselled against this. He argued that the government should maintain its spending but ‘redeploy resources . . . [to] support of those areas of public spending that help strengthen the long run supply side performance of the UK economy, including education, training and R&D’ (Currie 1993c: 42 para. 10). A similar concern was expressed by Wynne Godley:

neither tax increases or public expenditure cuts are needed at the moment and [that] to implement them immediately would be unnecessarily destructive. Indeed there remains a case . . . for actually increasing public investment.

(Godley 1993d: 65 para. 10)

These arguments, made individually by the majority of the Panel members, but never collectively endorsed by the group as a whole, suggested that a more radical or interventionist set of economic policies were needed and focused on increasing training, investment, and research and development. However, they were not reflected in Clarke’s budget. This is interesting because in simple quantitative terms, changing the priorities of public spending was supported by a greater number of the Panel than the rather more technical argument about the future size of the government’s debt.

***Rattling the network: cherished beliefs and
boundary objects***

As was noted in Chapter Six, which discussed the twin-deficit problem in more detail, Panel members put forward two separate sets of reasons to justify fiscal reform. One, that the PSBR was too large for it to be reduced to a sustainable level by economic growth alone, was backed by Congdon, Currie, Davies and Sentance, and was more or less adopted by the Chancellor. The other, backed by Britton, Currie, Godley, Davies and Sentance and centred around the need to improve the supply-side performance of the economy, reduce the share of

consumption and increase investment and exports, did not make it through to either the Panel's collective recommendations or the political agenda of the Chancellor.

In the case of the sustainability of the PSBR, it has been shown that the Panel managed to reach a consensus, in spite of underlying differences of opinion. In October only Congdon, Currie and Sentance felt sure that additional measures were needed; Britton and Davies thought the requirement questionable provided growth turned out as they expected; Minford thought the deficit was cyclical and would disappear without additional action.² Clearly, if these differences had been pursued the consensus could not have been achieved. Because the budget deficit can also be defined as a technical rather than a theoretical concept it was also possible for the Panel to differ about its value while agreeing how it should be understood, and this distinction lies at the heart of their consensus. In particular, by limiting their consensus the Panel were able to sketch out a fairly general position which committed them, and the Chancellor, to achieving a sustainable budget deficit. They did not however agree whether existing policies would achieve this. The two sides of the debate are summarized in Table 7.1, which shows how the disagreements could be contained as different judgements about the risks associated with particular outcomes and the effectiveness of existing policies.

In this way, the idea of the budget deficit functioned as a sort of 'boundary object' (Star and Giesemer 1989) which enabled the different theories and models to unite around a single issue, even though it probably meant different things to each. Thus the apparent consensus on policies to reduce the budget deficit is not based on forecasts for the PSBR but on more general economic arguments about fiscal sustainability and the intrinsic desirability of certain sorts of tax reforms. As a result, it is easy to understand why:

[The Panel] would therefore all support the announcement of tax or spending reforms, desirable in their own right, which would secure a net reduction in the PSBR.

(HM Treasury 1993c: 15 para. 38)

In contrast the twin deficit issue was a controversy over which the Panel did not achieve this sort of resolution. If knowledge is seen as a network of beliefs and actions, then part of the explanation for this must surely lie in the relative positions occupied by the two controversies in the economic models. The difficulty of compromising over the twin deficit issue was that the question raised much more fundamental challenges about the way in which different economists understand the economy than the relatively technocratic consensus of PSBR sustainability. In many ways, debates about the PSBR were about questions such as, 'will the government debt in three to four years' time be 61 per cent of GDP or 59 per cent?' Arguments about

Table 7.1 Arguments for and against tightening fiscal policy

<i>For taxes now</i>	<i>Against taxes now</i>
A sustainable budget deficit and certain tax reforms are economically desirable things.	A sustainable budget deficit and certain tax reforms are economically desirable things.
Forecasts suggest that the PSBR will fall slowly. This means that, in the absence of further measures, the scope for future increases in borrowing will be small.	Forecasts suggest that GDP will grow slowly. The means that, if additional measures are implemented, the scope for increases in investment will be small.
It is better to tighten fiscal policy now and ensure that a sustainable budget deficit is achieved. If necessary, taxes can always be cut later.	It is better not to tighten fiscal policy now and ensure that a reasonable economic recovery is achieved. If necessary, taxes can always be raised later.
The right thing to do is to announce, but not implement immediately, economically desirable fiscal reforms. If we are wrong, and they are not needed, then they can be offset at a later date with further tax cuts.	The right thing to do is to announce, but not implement immediately, economically desirable fiscal reforms. If we are wrong, and the PSBR turns out not to be sustainable, then they should be implemented in full.

the balance of the economy, the provision of training and the need for government interventions were arguments about social priorities and political cultures.

The strategies that resolved the PSBR debate are much harder to apply to the twin deficit controversy. In particular, the risks and costs of mistakes are higher, but it is intellectually much more difficult to move from one side of the debate to the other. Changing sides is more like changing paradigms or shifting from one form-of-life to another. The polarities which characterized the debate are outlined in Table 7.2.

This perspective makes it easier to understand why the Panel were not able to agree on even a conditional compromise. For example, on the left-hand side of Table 7.2, hysteresis exists in the labour market and the size of the deficit on the current account of the balance of payments is a legitimate concern for public policy. On the right hand side neither of these propositions is true.

However, choices between training programmes for the long-term unemployed, or policies to encourage industrial investment and research and development, are surely just as important as the choices about fiscal policies to reduce the budget deficit to a conventionally accepted level. Unfortunately, because the Panel could not reach agreement on how to phrase the issue, there was nothing that could function as a boundary object and link the models.

Table 7.2 Arguments for and against intervention to 'solve' the twin deficit problem

<i>For action to solve twin deficit problem</i>	<i>Against action to solve twin deficit problem</i>
Sustainable economic growth requires improved export performance and a small PSBR.	Sustainable economic growth requires improved export performance and a small PSBR.
Because of hysteresis effects in the labour market, inflation will rise soon after economic growth begins, hampering any recovery and ensuring that, in the absence of any extra fiscal tightening, the PSBR stays high.	Because unemployment is above the natural rate, inflation will not rise as economic growth begins and although it is possible that some extra fiscal tightening will be needed, the PSBR will be reduced.
The government therefore needs to take action to ensure extra investment and training so that the UK economy can compete successfully in export markets. If they do not do this then trade deficit will hold back economic growth.	There is no need for the government to take action to ensure extra investment and training because the UK economy is already able to compete successfully in export markets. The trade deficit is not therefore a problem for policy.
The right option is to change the priorities of economic policies and public investment. Without these policies the government will not achieve the economically desirable outcomes of a sustainable budget and sustainable trade deficits.	The right option is to continue with the existing priorities of economic policies and restraint of public investment. With these policies the government will achieve the economically desirable outcomes of a sustainable budget and sustainable trade deficits.

Instead there was a stark choice between different economics: and because there was no compromise, neither option featured in the Panel's collective recommendations. Instead of 'one voice' there was 'no voice' speaking to the Chancellor.

It seems that at this point we have run up against the boundaries of economic expertise. However, the fact that economists are unable to decide on an issue - the dissension within the Panel of Independent Forecasters is echoed throughout the professional community - does not reduce its importance. In practice, because the Panel's recommendations focused on those relatively non-controversial areas of economics where they did agree, these inevitably became the core of their recommendations.

The point is not that we need one economic model about which there can be no disagreement, or that we need lots of economic models, competing with each other in the policy market place. To think this way is to hand the problem

straight back to the same economists who cannot resolve it. Rather, what is needed is social legitimacy. In the context of the UK policy-making process, the potential advantage of the Panel was that it took economic policy debates outside a partially discredited government body and opened up assessment of the choices, opportunities and problems to a much wider range of economists. By choosing to speak to the Chancellor with one voice, however, the actual practice of the Panel tended to work against this outcome.

This then is the difficult choice referred to in the chapter title. For the Panel, it is the choice between having some influence on the answers to the existing questions and changing the questions themselves. Within the context of a given institutional framework, it is clearly important to ensure that existing institutions and bodies do their jobs well. Indeed, one reason why the Panel tried to speak with one voice was that they wanted to be influential and ensure that the Chancellor did take heed of their advice. As Alan Budd observed:

Budd: Before the last Budget [i.e. at the meeting in October 1993], to a large extent, they produced a consensus report. Now that, in itself, is very interesting, because they know that they are going to be more influential if they agree.
(Interview 22 March 1994: transcript p. 10)

But, although reaching agreement has benefits in terms of influence, it has costs associated with it. By not including the controversy in the policy recommendations – by not making the Chancellor choose – the Panel left important issues off the policy agenda. The disappointment must be that problems which some, possibly even the majority, of the Panel diagnosed as important for the future of large numbers of their fellow citizens were simply not addressed in the budget. They were neither affirmed nor denied. Consequently the structural problems that they believed they had identified may still remain unresolved, potentially holding back economic growth in the UK and prolonging the waste of human lives. The problem therefore is that rather than attempting to answer this question, the Panel chose not even to ask it.

ECONOMIC MODELS, ECONOMIC POLICY AND SCIENCE STUDIES

Introduction

The previous chapters have examined economic modelling and forecasting from a variety of different perspectives. This chapter concludes the discussion by giving the forecasters' own evaluation of their performance. In particular, how did the Panel feel their forecasts measured up to the economic outturns? What sorts of mistakes did they feel they had made, and what did these mistakes tell them about their economic models?

Economic activity, and hence economic forecasts and models are also, of course, significant to policy makers, managers in business and industry and ordinary people with jobs and mortgages to worry about. This chapter also explores the implications for these groups of the sociological analysis outlined in the preceding chapters.

In particular it is argued that if scientific experts are unable to agree on an explanation, representation or model, and accordingly on policy recommendations then other forms of expertise need to be brought into the process of advising policy makers if it is to be perceived as credible.¹ If this does not happen, institutions and policy decisions are likely to be seen as favouring or promoting the interests of one group over another.

Keeping the faith: evaluating the 1993 forecasts

When the Panel met for the fourth time in February 1994, much of the subsequent report dealt with forecasts for the forthcoming year and beyond. Perhaps to mark the anniversary of the Panel's appointment, the report also included a retrospective analysis of 1993. This aspect of the report is the topic of this section. The focus is on the ways in which the Panel interpreted the data for 1993 and judged their own performance. As will be shown although all Panel members admitted that they had made mistakes and misjudgements, none of them saw anything in the events of 1993 that forced them fundamentally to rethink their underlying theoretical ideas.

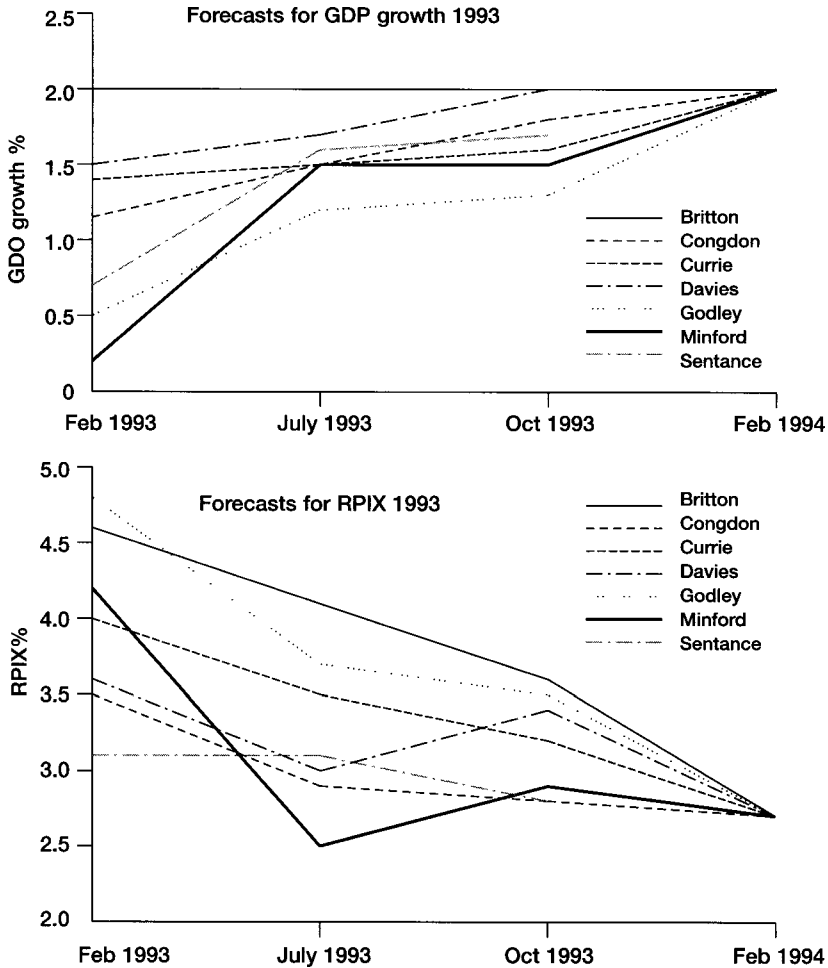


Figure 8.1 Forecasts and outturns for GDP growth and inflation in 1993

Figure 8.1 compares the Panel’s forecasts for GDP and inflation with the outturn values. As can be seen, the general trend was for the forecasts to under-predict economic growth and over-predict inflation: the forecasts converged on the GDP growth outturn from below and on the inflation outturn from above.

As was noted in Chapter Five, the trade-off between growth and inflation plays an important role in economic modelling and forecasting. Figure 8.1 shows that the vast majority of the Panel did not forecast this trade-off correctly, and predicted more inflation and less growth than actually occurred. It could also be said that because the Panel were relatively pessimistic about inflation, their policy recommendations to boost growth were more cautious than they would

have been had estimate of the trade-off been better. In addition, the trade deficit was smaller than most of the Panel expected and they failed to predict the falls in unemployment.

The suggestion that the inflation–output trade-off turned out to be better than the Panel had forecast, particularly in the short term, was clearly acknowledged by them in their 1994 report. However, none of the Panel members saw the various forecasting failures that have been detailed as a reason to reconsider their theories and models. Overwhelmingly, they were able to explain the results in terms of their existing paradigms:

The events of [1993] do not cause any of us to change our (differing) views of the UK economy fundamentally, and in the main have not led us to make substantial changes to our forecasts for 1994 and beyond. The happy combination of steady growth and falling inflation is not unprecedented in the early stages of recovery.

(HM Treasury 1994: 4–5 para. 3)

The rest of this section examines the responses of the individual forecasters in more detail, and the next section discusses some of the issues raised by them.

The ‘devaluationists’

In Chapter Five, it was argued that the Panel’s forecasts fell into two main types: one stressing the importance of the devaluation in forecasting the economy, one relying on equilibrium concepts such as the natural rates of output and unemployment. The basis of the ‘devaluationist’ account was that ‘Without the stimulus from the devaluation of sterling and the cuts in interest rates, output would hardly have recovered at all last year [1993], and unemployment would have risen well above 3 million’ (Britton 1994: 20 para. 5).

The devaluationists had to account for the facts that even with the devaluation, growth in 1993 was higher than they generally forecast, and inflation was lower. In addition, unemployment fell sooner and more rapidly than in previous economic cycles and the net trade figures were surprisingly good. In short, the pessimists appeared to have been too pessimistic.

However, although they would probably agree that forecast mistakes were made, the proponents of the devaluation story did not give up or modify their general model. They explained 1993 as a particularly odd instance of the devaluation cycle, in which a combination of external events muted the inflationary effect. The implication was that the factors that lay behind their initial pessimism – for example the low levels of investment and the exclusion of the long-term unemployed from the labour market – remained as unresolved problems for the future.

Taking first the forecasts for GDP growth, Andrew Britton, whose initial forecast of 2 per cent turned out to be exactly right, appeared to be in the enviable position of having very little to explain:

[In February] the Institute was at the top end of the Panel range with a forecast of 2 per cent growth . . . This forecast now appears to have been exactly right . . . The Institute's projections of domestic demand were also substantially correct, with consumer spending rising a little faster than we expected (despite the Panel's worries about debt ratios) and fixed investment picking up a little.

(Britton 1994: 19 paras 1, 2)

This apparent accuracy masked some problems, however. The forecast made by the NIESR in February 1993 was based on economic data standardized to 1985 prices. During 1993, though, two things happened. First, in summer 1993 the method of generating import and export figures was changed to bring UK data on European trade in line with the requirements of the European Single Market. As a result the volumes of both imports and exports appeared to fall dramatically, although trade with non-EU countries, which was still being measured by the old system, showed no such change. The introduction of the new trade date therefore created a discontinuity in the EU trade data. It remained uncertain to what extent real changes in trade patterns had occurred.

Second, in September 1993 the national income accounts for the UK were revised and rebased using 1990 prices. The main effect of this was to change the recorded figures for GDP in 1992, with the result that the recession was now seen to have ended before the UK left the Exchange Rate Mechanism in September 1992. For many of the Panel, this revision to the profile of GDP was an important factor in explaining why they had under-predicted growth during 1993. However, the NIESR's situation was rather different, Andrew Britton explained that at the NIESR:

Britton: What we tended to do was think that, probably, the provisional output figures were correct and the trade figures and stockbuilding were wrong. So when we got the figures with more growth in ... we put that in as a preliminary outturn [for net trade] because that was the published figure, but we offset the effect on stockbuilding so that we weren't changing our GDP forecast.

(Interview 1 March 1994: transcript p. 6)

David Currie typifies those who used the data revisions as explanation. He conceded that the LBS 'under-estimated the extent and pace of recovery during the year', but argued that:

In large part, this was due to the substantial revision to the estimates for GDP in 1992. Instead of the weak numbers first estimated, the revised figures (including rebasing) show that GDP was at its lowest in the first quarter of 1992, and was rising slowly throughout the rest of the year. With that starting point for our forecasts in 1993, our view of growth during the year would have been more positive.
(Currie 1994: 49 para. 26)

For both NIESR and LBS forecasts, another important factor is the judgemental adjustments that were made by the forecasters. With hindsight, some of these were viewed as mistaken. Currie claimed that the LBS forecasting mistakes were at least in part owing to the forecasting team giving too much weight to the views of the members of their Consortium Panel when revising their provisional forecasts (Currie, interview 24 February 1994).

Other Panel members identified different factors. For example, Wynne Godley did not seem to think that the data revisions had a significant effect on his forecasting performance. Instead he highlighted the unusual way in which export prices rose following the devaluation and the unexpected boom in the stock exchange in late 1993 (Godley 1994: 73 para. 18).

As was noted in Chapter Five, one of the key judgements needed at the start of 1993 concerned the effect (if any) of the unusually high levels of private and corporate debts on the response of the economy to the devaluation. Like many of the other economists, Godley identified this feature as a reason his forecasts underestimated GDP growth:

Godley: Following the slump there was an unusually large private sector surplus, which I take to be the consequence of people having acquired debt and then trying to repair their balance sheets. . . . And therefore they had very low expenditure relative to their incomes. The position was quite abnormal – nothing like it had ever happened before – and the forecasting problem was to know when this financial surplus would fall. . . . I took the view that it would take a year or two longer to start falling than it actually did. But it was always a great uncertainty to me.

(Interview 9 April 1994: transcript pp. 1-2)

He saw no reason to think his model fundamentally flawed because this judgement, always uncertain, proved incorrect. However he did concede that the time frame over which certain variables changed might need to be reconsidered:

I am not yet sure whether or by how much I shall want to change my model in the light of some of the things which happened in 1993 (in particular to inflation and unemployment) which were surprising

to me. There certainly seems some evidence of increased flexibility in a number of areas.

(Godley 1994: 73 para. 19)

In summary, the events of 1993 did little to make Godley doubt that his analysis was essentially correct. He was surprised less by events than by their timing: consumption started to rise sooner than he had expected but he did not interpret this as a challenge to his fundamental understanding of the economy:

Godley: I haven't really changed my model, or my way of thinking, at least not about income-output determination. It is just that I didn't get the timing of it right.

(Interview 9 June 1994: transcript pp. 2-3)

The devaluationists also needed to account for the surprisingly low inflation figure, and again a pattern emerged in which unusual events were identified and used to explain the difference between the initial forecast and the outcome. Thus, although both Britton and Currie accepted that the devaluation did not have the inflationary effect they had anticipated, neither felt it necessary to alter his views fundamentally.

Britton argued that based on the values of the residuals in the wage and prices equations in the NIESR model, wage behaviour was unusual during the recession and the period following. The residual acts as a quantitative indicator of closeness between the actual value and the computed value: the smaller the residual the more typical the instance, and the larger the residual, the more unusual or atypical. By examining the residuals produced by the NIESR model, Britton was able to construct a table of numbers which:

Confirms that wage inflation in particular was, relative to our model, 'unusually' high in 1990 and 1991 . . . By last year [i.e. 1993], however, the residuals in the wage equation ha[d] changed sign, suggesting that the wage increase was, relative to our model, 'unusually' low.

(Britton 1994: 24 para. 21)

Britton provided various explanations for these differing values. The unexpectedly high wage inflation of 1990 and 1991 was in part, he suggested 'because the depth and duration of the recession was not at that time fully appreciated by wage bargainers' (ibid.). As the economic cycle turned and the recovery was accompanied by lower than expected inflation, he argued that:

Weight was being given to the very low figures for the headline RPI [inflation] figure which does not feature in our model, because of

pay restraint in the public sector or because the labour market was slow to recognise the turning point in unemployment.

(Britton 1994: 24 para. 21)

David Currie's explanation followed a similar structure although he identified a different set of special factors, including falling interest rates, discounts in shops, 'the robust policy of cutting the large fiscal deficit, as well as low inflation in the rest of the world' (Currie 1994: 49 para. 27). In this way, both Britton and Currie were able to maintain their general model as an explanatory structure and locate the errors in the processes that mediated economic activity. They rejected the argument that the low inflation figures reflected a more fundamental shift in the economic structure which their models did not reproduce. Currie recognized this as a candidate explanation but ultimately restated his faith in the original analysis:

There is also the possibility that [the low inflation] reflects changes in the structure of the UK economy leading to improved flexibility of supply performance . . . although we accept this explanation in part, we retain some scepticism as to whether this improvement will be sustained. We therefore see some risk of rising inflationary pressures as recovery continues.

(Currie 1994: 49 para. 27)

In conclusion, the low inflation of 1993 was explained and accounted for by transitory and contingent factors, and the validity of the general model was maintained:

Britton: There have been a lot of special factors that you can identify in the rate of inflation over the last year, particularly in food and household durables, where there have been a lot of price cuts, particularly in the sales. And I think that I am not really saying more than, perhaps, there won't be so much of that in the future.

(Interview 1 March 1994: transcript p. 4)

The classical economists

The classical economists had been more optimistic about the future than the devaluationists, forecasting that growth with low inflation was the most likely outcome over the next few years. Arguably this was what happened, so they were able to claim that they had more or less got it right all along. Thus, for example, Tim Congdon argued:

The February 1993 [Lombard Street Research] *Submission* [to the Panel] argued, on the basis of an equation using the output gap

concept, that 'above-trend growth can be reconciled for several years with low inflation' and that 'the next few years should be good ones for the British economy' . . . Much of this forecast, including its generally optimistic tenor, has proved correct.

(Congdon 1994: 35-6 paras 21-2)

However he did acknowledge that his forecast had erred in three interrelated ways: 'it under-estimated growth in 1993; it failed to foresee the fall in unemployment which did in fact occur, and it failed to forecast the acceleration in broad money growth in late 1993' (ibid.: 36 para. 22).

Like Currie, Congdon believed a large part of the under-estimate of GDP (about half in his case) could be attributed to the revisions to the economic data. However, the rest was owing to a more 'fundamental' reason: the rate of growth in the second half of 1993 was faster than he had expected. In this respect he was in the same position as the devaluationists over their forecasts for inflation. He too was in need of specific explanations. His explanation was that the effect of the lower interest rates had been more significant than he had originally anticipated:

The failure to fully foresee the strengthening of activity in the second half may have been due to exaggerating the tightness of the constraint implied by low monetary growth. In practice, the drop in interest rates may have been responsible for people trying to switch out of interest-bearing deposits into more rewarding assets, such as equities and real assets (including consumer durables and cars), on a greater scale than had been anticipated.

(Congdon 1994: 36 para. 23)

The idea that economic agents will try to maximise their returns is uncontroversial, and Congdon's explanation maintains the general model. Similarly, on the under-estimation of monetary growth, a crucial part of his economics, Congdon was keen to point out that the forecasting errors did not undermine the model. Indeed by attributing the mistake to his 'neglect[ing] the potential capital in-flows into the non-bank sector from abroad' (ibid.: 37 para. 25) he saw the errors as actually confirming the general model:

The faster-than-expected monetary growth in late 1993 contributed to the improvement in balance-sheet strength across the economy and so the greater-than-forecast buoyancy in domestic demand and employment. . . The message of [this] is clear . . . 'in any worthwhile discussion of the macroeconomic situation, we must make an attempt to relate the demand to hold broad money to the quantity of such balances actually in existence'.

(Congdon 1994: 37 paras 25-6)

Patrick Minford, the other member of the Classical camp, had at the Panel's first meeting been both a pessimist and an optimist. He was one of the more pessimistic forecasters for 1993, predicting low GDP growth and recommending large and immediate cuts in interest rates. However, over the longer term (into 1994 and beyond) he was one of the more optimistic, predicting strong growth accompanied by low inflation and falling unemployment. This is not what happened: GDP growth in 1993 was nearly 2 per cent more than Minford forecast in February of that year. In his retrospective analysis, Minford identified three factors that had led to this unexpectedly strong recovery:

There seem to have been three simultaneous and probably connected sources of greater strength: in net exports, recruiting and in consumption. Our model may have underestimated the exchange rate effect on net exports and the real interest rate effect on consumption; and the lags in the unemployment relationship may have speeded up.

(Minford 1994: 84 para. 23)

In interviews it became clear that the low February forecast had also reflected the pessimistic judgements of Minford and his forecasting team. The Liverpool economic model had initially produced a higher forecast, but Minford and his team had 'restrained' it because of their uncertainty over the effects of the unusually high levels of debts. This uncertainty was compounded by the problem of interpreting the 'regime shift' caused by moving from a fixed to a floating exchange rate:

Minford: Had [we] been in a floating regime all along, one would probably have let the model roll. The difficulty was how to interpret this post-ERM regime shift . . . I think we felt that the sharp difficulties with balance sheets - the overhang of the ERM really - wouldn't go away quickly . . . Basically we thought that it would take 1993 to work out of the system and we also assumed that there would need to be interest rate cuts during '93. In fact, they obviously worked out of the system quicker.

(Interview 11 March 1994: transcript pp. 2-3)

Thus, as for Godley, the high levels of indebtedness in the economy caused Minford and his colleagues difficulties. Also like Godley, they over-estimated the time period over which this situation would have a significant effect. However, like his monetarist colleague Congdon's, Minford's forecast for low inflation was more or less correct despite the higher than expected growth, so in this respect he had few worries. As he observed, it was always his view that:

Policy had massively deflated the economy so that the faster slowdown in inflation was not too surprising: we somewhat underestimated it in the short-term, allowing too much for the temporary exchange rate effect on inflation. [But] the evidence has been consistent with our view that the UK supply side has improved and that monetary policy has been highly deflationary.

(Minford 1994: 84 para. 24)

In other words, the lower than expected inflation was interpreted as proof that the model based on monetary influences and the flexibility of markets was confirmed by the data. Finally, with regard to policy, even as 1993 progressed and it became evident that growth was stronger than Minford had anticipated, he never wavered from his original view that interest rates should be cut. Indeed in February 1994 he still thought this was good advice, although economic growth of 2 per cent had been recorded without cuts in interest rates. Minford's argument was that the better than expected growth might have been more a matter of luck than judgement:

These developments any [sic] just have been random 'errors', the result of a lucky draw, a positive event influencing all three sources [of error] . . . Had there been an unlucky draw then the recession would have been yet more prolonged . . . [and] the government have been fortunate that a still poor situation of delayed recovery is not worse still.

(Minford 1994: 84-5 paras 23-5)

In interview Minford reinforced the point:

Minford: What you have to ask is whether you could legitimately have expected to have a robust recovery with those real interest rates. That is really the question, and I think that they were lucky to get away with it. But they haven't really got away with it because it is still a very weak recovery. I suppose another way of putting the point is, suppose I had correctly forecast 2 per cent growth, I would still have taken the same policy position.

(Interview 11 March 1994: transcript pp. 9-10)

Summary

This analysis has now covered one complete cycle of annual forecasts. The forecasters ended the year with essentially the same models as they had at the start of it.

Economics covers so many uncertain factors that it is perhaps no wonder the Panel members could all justify their forecasting failures. It is difficult or

impossible to evaluate their justifications and to decide which model, if any, was wrong. This leaves the question of to what degree policy makers should rely on the models and the recommendations derived from them.

Economic orthodoxy and policy choices

One of the central themes of the sociological analysis of science has been the interpretative flexibility of scientific data in a wide variety of fields and disciplines. Although this has typically been done under the guise of the 'controversy study', the meetings and reports of the Panel of Independent Forecasters tell a similar story.

On this view, the analysis has shown how key judgements enter into the science of model building and testing (Chapter Two), the practice of forecasting (Chapter Three) and the evaluation of forecasts and models (Chapter Four). Chapters Five and Six explored how the different models supported different interpretations of the economic data and led to different policy recommendations. Finally, Chapter Seven showed why despite these differences the Panel members did converge (albeit temporarily) on a consensus policy recommendation in October 1993. The preceding section in this chapter has shown that they did this without fundamentally changing their underlying theories, models and beliefs.

There is some value in emphasizing the connections between the analysis developed in this volume and that to be found elsewhere in sociological literature, but there are also drawbacks. To highlight the similarities between economics and the natural sciences is to mask their differences. The result is that the wider social and political dimensions of the economic controversy - the consequences for the life chances and experiences of the general public - are lost.

The scientific controversy between Panel members was also a potential political controversy. However the Panel themselves masked the points of controversy when they produced consensus reports based on fiscal sustainability. The question remains what impact this had on the process of policy making and ultimately on people's lives.

Economists' views on the prospects for 1993 ranged from the pessimistic to the cautiously optimistic, as we have seen. Their policy prescriptions differed just as greatly. How could economic forecasters offer reliable advice to policy makers when they could not agree what would happen to the economy, or how what had happened should be understood? In a situation of such chronic uncertainty, what was the advantage of bringing together a diverse set of economists and asking them to produce policy recommendations?

There are two main perspectives to consider, although they relate to three sets of actors. First, there are the economists and the policy makers. Most of the time these are the only ones that seem to matter. However, a key insight of the sociology of science literature is that the successful adoption of innovation

and science often involves enrolling less elite groups. The second perspective therefore is that which includes other sources of expertise.

The elite perspective: economists and policy makers

From the perspective of the economists and policy makers, one obvious benefit of the Panel was its value as a form of collaborative professional work, providing an opportunity to exchange and discuss ideas with colleagues. It is therefore important to consider both how the Panel were influenced by each other and how, as a group they influenced the Treasury and the Chancellor.

The primary event that gave rise to the Panel was the departure of the UK from the ERM in September 1992. Without this event it is unlikely that it would ever have been formed. The Panel's existence, remit and role were therefore strongly shaped by the social and political context of that time. In particular, the exit from the ERM was widely regarded as an economic and political catastrophe, and its fall-out damaged the credibility of both politicians and their economic advisers. As Alan Budd explained:

Budd: Associated with [leaving the ERM] was the feeling that the Treasury, in particular, had produced appallingly bad forecasts and that this was one of the reasons why we had made this ERM mistake . . . So what the Chancellor did, to appease the wrath of the people who said he and the Treasury forecasters ought to be sacked . . . [is to say] 'Well, actually we never paid that much attention to our forecasts, and it isn't the only thing we do. But, to demonstrate that we do take account of outside views, I shall have this Panel of Independent Forecasters to supplement what my own guides are saying'.

(Interview 22 March 1994: transcript p. 8)

Of course, cynics could (and did) point out that this was simply a way of shifting attention away from the Treasury and showing that there was an economist for every policy and that most of the time their forecasts were wrong. However there was a real political advantage for the government in taking account of the wide range of views held by the economists on the Panel. One criticism made of the Treasury was that it was too beholden to one particular view of the economy.² If the Panel had done nothing more than make clear the range of legitimate views then arguably it would have been a worthwhile exercise. Further, by lowering the expectations of what economic science and policy could achieve, the Panel might have made it easier for future economists and policy makers to discuss the uncertainties of economic life in a more nuanced manner.

The exchange of views that was mentioned above also counts as a benefit. Panel members actually talked to each other and the Treasury economists. At least from the Treasury's perspective this added value to the process:

Budd: Between them, they have got some very sensible ideas. They cover a range of views and they can raise issues with us that we have not thought of . . . While we are trying to think what [policy] advice to give the Chancellor, we are listening to these guys and hearing what they have to say. It is like having six or seven extra officials at the Treasury, giving us their views [and] widening the range of opinion to which we are exposed and therefore, if you like, the range of opinion to which the Chancellor is exposed. Now of course the Chancellor could always read what they say in the newspapers, but I think that putting them together does genuinely add something.

(Interview 22 March 1994: transcript p. 9)

So the Panel widened the sources of policy advice to some extent; did they also advance economic theory? Perceptions vary, but the overall feeling seems to be that it was worthwhile for the Panel at least to try to understand where and why they disagreed:

Congdon: We might actually have a useful exchange of views, and I think that's, in a way, what the Panel should be doing . . . It may be that just over working together that eventually it does all materialize. You see what I don't do, and what is very important for Godley, is a lot of work on . . . net exports. But nevertheless that work is quite useful and it does force one back on to trying to work out the relationships between different things . . . And David Currie does a lot of work, as does Andrew Britton, on wage-price behaviour which is implicitly in my stuff, but not played up very much. Gavyn Davies and Andrew Britton do a lot of work on the labour market, which I don't really do.

(Interview 2 March 1994: transcript pp. 18-19)

Thus at a personal level the economists on the Panel did benefit from learning from each other. As a result, a modest shifting of positions was possible:

Budd: You might find [that] people will say 'Here is a really convincing story. It seems to fit, and this is how you should think about this'. And people might shift a little bit, and we [the Treasury] would shift as well, and we would have made a little bit of progress.

(Interview 22 March 1994: transcript p. 15)

However, it would be naïve to expect a dramatic conversion and it was always unlikely that the Panel would succeed where the rest of economics had not, and bring about an empirical and theoretical convergence towards a common specification for their models:

Budd: I think what I am more sceptical about is the empirical advances. For example, I don't think that anybody is suddenly going to have a consumption function that works. I think our state of ignorance about this seems to be more or less constant.

(Interview 22 March 1994: transcript p. 15)

If they cannot solve the problems of economic practice, can the Panel solve the problems of economic policy that are, after all, their main objectives? Certainly by emphasizing the beliefs that they share the Panel were able to make a strong case for particular policy recommendations and as was noted in Chapter Seven, this was a strategy the Panel used to increase their influence:

Minford: In order to forecast you have to have your view about what causes things. And also, in order to make policy recommendations you have to set out your reasons. So, therefore, in so far as we can reach agreement about what causes things, that would be helpful, wouldn't it?

(Interview 11 March 1994: transcript p. 15)

Britton: I think, at the moment, the tendency is to try and find a common view, because we feel the Panel is stronger when it speaks with one voice.

(Interview 1 March 1994: transcript p. 18)

From the perspective of the policy makers, however, one important issue remains. Despite the efforts of the Panel to reach consensus, their views about the economy and the future remained different in many important respects. It is questionable whether it would have been possible, let alone desirable, to incorporate these forecasts into a single economic policy. Taking the average of all the forecasts was not necessarily the best solution, not least because it was unlikely that the mean forecast would be the right one.³ As Alan Budd explained:

Budd: You might think that [forecasts] will be like shots at a target: [they] will blow the middle out with a few scattered around . . . [In practice] the truth is liable to lie at one extreme or the other. It is a very peculiar business . . . [So] taking the average may be the best thing to do in the long run, but year by year it is not the right thing to do.

(Interview 22 March 1994: transcript p. 23)

It is very difficult to take the average of two theories, although in some ways this is what the Panel tried to do when they produced their consensus recommendations in October 1993. However, as was noted in Chapter Seven, this strategy had the cost that the direct connections between the theory and the economic events had to be loosened. As a result, the policy

recommendations referred to hypothetical situations – the Panel would support policies if they became necessary – rather than to the specific situation that the UK economy found itself in at the end of 1993. To decide whether or not such policy interventions really were necessary required a decision between the competing economic forecasts and models.

It is here that the Treasury economists are crucial. However their role also raises the central paradox of the Panel of Forecasters. In particular, the role of the Treasury economists is to provide advice to the government. We have already seen that the role of the Panel was to augment that advice with some fresh perspectives and ideas. However, the Treasury economists were still economists and so it was surely inevitable that they would evaluate the Panel's advice in the light of their own experiences, views and, of course, models. Thus, the forecasts of the Panel were not treated as epistemologically equivalent to Treasury forecasts:

Budd: You already have a model of your own, you have your own *a priori* views and then you also weight these individuals according to how coherent their view seems to be and how good their forecasting record seems to be. You lean one way or the other. You have a feeling that X or Y is most likely to be right and you attach less weight to extreme views.

(Interview 22 March 1994: transcript pp. 22–3)

Paradoxically, however, the one qualification that makes the Treasury economists the ideal people to evaluate economic forecasts – their economic expertise – also disqualifies them from taking a perspective which might enable them to solve problems which pure economics cannot solve. Choosing between economists is not only a technical choice, but also a social and political one. Why therefore should we expect economists to be better able to make these choices than other intelligent participants in the policy process? Indeed, by asking economists to choose amongst themselves do we not increase the risk that alternative policies will not receive adequate consideration?

The participatory perspective: economists, alternative experts and policy makers

Although more research by economists is one way to generate economic policy advice – and possibly the only way that might produce consensus amongst economists – while they continue to disagree so profoundly it is not necessarily the best basis for making economic policy in real time. Perhaps a better option is a more secular strategy, which opens out the economic policy debate to include new forms of expertise.

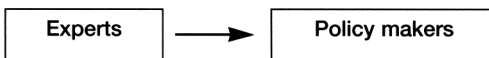
In particular, the insight of the sociologist is that the choices available to policy makers and citizens are political choices between economists and

economic futures. What is needed, in the absence of any scientific consensus, is a social process which can add legitimacy to the policy process. The dominant model, at least in the UK, is for scientists to interact directly with policy makers. The Panel of Forecasters actually broke with this tradition because its reports were made available to both the policy makers (the Treasury and the Chancellor) and other interested groups (including the public), as shown in Figure 8.2.

The insight from the sociological literature is that this latter channel needs to be strengthened if controversial science is to be seen as a legitimate basis for social action. In other words, the question is not whether to have less economists or more, but how to bring about a better understanding of the basis for economic policy decision making. In this context, my own view is that The Panel of Forecasters was a good idea, drawing as it did on a wide range of the available economic arguments. In particular, if the Panel had helped to foster a wider appreciation of the range of legitimate economic views, the uncertainty that surrounds each of them and the opportunities that exist for creating a different society, then this would have been an important achievement.

However, using sociological theories of knowledge can facilitate a more radical approach, recasting the policy problem in a different light. From the sociological viewpoint the key normative question to ask is who are the key actors in the process, or in sociological terms the 'core set' or 'core group' (Collins 1992). The typical approach within UK policy-making institutions is to restrict the membership of this core group to traditionally (usually academically) accredited experts and policy makers. The sociological alternative, which is better developed elsewhere in Europe, is to include in this group some representation from more general groups. For example, in technology assessment and urban planning the inputs of engineers and planners might be supplemented with inputs from representatives of relevant

Elite perspective



Panel perspective

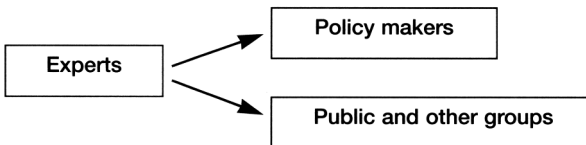


Figure 8.2 The traditional policy-making process compared with the Panel of Independent Forecasters' process

interest groups and perhaps the public. This perspective perceives the unaccredited expertise of farmers, patient groups, pedestrians and motorists as being as valid in the decision-making process as the more obviously accredited expertise of nuclear scientists, doctors, planners and civil engineers.

In other words, there is an alternative to the economists' request for more time and research to clarify, develop and improve their models. This is not to say there is no place for institutions such as the ESRC Macromodelling Bureau, which is the institutional embodiment of this philosophy. It is merely to point out that theirs is not the only methodology which involves using computer models to make decisions. Instead of being used to restrict expertise, models can be used to increase participation. In particular, the range of economic models used by the Panel clearly suggested that a range of economic futures were possible. This could have been used as the starting point for policy making, instead of being treated as a source of difficulty. The implied policy framework is shown in Figure 8.3.

The Panel of Independent Forecasters could have formed the start of this type of process. By showing clearly where the economics stops, it could have initiated a search for new experts from outside economics, and new questions. Perhaps representatives of businesses, workers, local communities and academic disciplines might contribute to policy debates about the labour market, for example. Within such a framework, the relative roles and status of each group – economist, public and policy maker – would change. In particular, the economists would no longer speak directly and solely to the government, but more extended peer groups would also evaluate their ideas (cf. Funtowicz and Ravetz 1993). It would not be the role of the public and/or other experts to decide a policy, any more than it should be the role of

Participatory policy making



Figure 8.3 An alternative policy-making framework

economists. All the parties would feed their own assessments and alternatives to the policy makers who must ultimately face up to the responsibility of making policy.

The advantages of such a process are twofold. First, the lines of accountability would be much clearer and scientists would not be put in the position of having to make policy and take responsibility for decisions that are properly understood as political. Second, when (as often happens) policy makers receive controversial scientific advice, the identity of the expert giving the advice can matter almost as much as their technical expertise. Where there is controversy, and therefore no right answer, the claims of differing experts are inevitably evaluated in terms of the trustworthiness and competence of their proponents. The insight of the sociologist is that it is not necessary to be an economist or a scientist to judge someone's trustworthiness: it is a very everyday skill that nearly everyone possesses. Note that this is not an anti-science argument. Rather it is about recognising science for what it is – the best available advice about one part of the problem – and using it in this context.

Perhaps the key insight of this sort of approach is thus that social systems have meanings to their participants. They are therefore open to change and can be reflexively organized and (re)organized around different goals, values and beliefs. The idea of constructive technology assessment draws on these sociological ideas to offer a new paradigm which explicitly incorporates these goals and values into the decision-making process. It can valuably be applied in the economic context. Policy makers need advice from economists but they also need advice from businesses, workers, the unemployed and the unwaged. They need to discover the real barriers, opportunities, needs and motives of economic agents. Indeed, if they are to take their models forward, then economists need this sort of interaction too.

Conclusions

The sociologically informed conclusion of this study is that the institution of the Panel of Forecasters had the potential to improve the *way* in which economic policy is made. Over the past two decades there has been a shift away from using economic models and forecasts to fine-tune economic policies in order to reach given policy targets. The economic climate now seems much less predictable and policy makers are more cautious, with the emphasis on avoiding mistakes and following rules. The advantage of the Panel in this context was that it had the potential to bring the scientific controversies behind these rules into the open. Indeed, one of the most important things highlighted by the Panel meetings was that different social, political and moral theories are apparently compatible with decent economic growth.

Instead of a narrowly (mis)represented economic orthodoxy setting the policy agenda, the Panel of Independent Forecasters had the potential to repoliticize economic policy by legitimizing debate about different economic models and

theories. In other words the Panel, by bringing a sense of the enormous range of economically legitimate views to the wider and public discussions, could have helped to foster an appreciation of both the uncertainty which surrounds economic policy, and the opportunities which exist for creating a different society. I think that this would have been an important achievement.

In practice, of course, it did not work out this way. The strength of the Panel was perhaps also their weakness: by making the economic uncertainties and choices clear, the Panel put the responsibility for social welfare back to the political process. In May 1997 the new Labour government abolished the Panel and gave operational control of interest rates to the Monetary Policy Committee of the newly independent Bank of England. This decision effectively reversed much of what I see as the Panel's good work by reinforcing the formal separation of 'economics' from 'politics' and reducing the scope of the economic debate.

Certainly many of the criticisms that the Monetary Policy Committee faced in their first year or so in charge of interest rates have been entirely predictable from a sociological perspective which recognizes that policy choices based on controversial science need to be legitimized in other, more political ways. For example, many of the criticisms of the Monetary Policy Committee's early decisions not to reduce interest rates stressed the way in which key social groups (mortgage payers and manufacturing industry) were not represented on the committee. In other words, their interests and values were not represented in the models and thinking of the committee, with the result that they were unwilling to become the 'intended agents' of the economic models.

However, nowhere did the commentators suggest that economic models themselves were a bad thing: their complaint was that the ones being used did not represent certain parts of the economy in an appropriate way. The crises therefore were about the social legitimacy of the interest rate policy - who has the right to speak on my behalf? - rather than of scientific expertise itself. What happens next depends crucially on the extent to which the Treasury Committee charged with monitoring the Bank can keep the different economic models visible in the public domain, and prevent them from slipping back into the privileged 'expertocracy' of economic advisers.

Epilogue

Thursday 6 November 1998

The Monetary Policy Committee of the Bank of England announced that it was cutting interest rates by 1/2 per cent. This was a larger cut than most forecasters were expecting and share prices fell on the assumption that the Bank was trying to avert a recession. Further cuts are expected in the next six months as economic output continues to slow . . .

(*Financial Times* 5 November 1998 and other sources)

APPENDIX A

EQUATIONS USED IN THE ECONOMETRIC MODEL

Numbers in parentheses and beneath each coefficient are the t statistic for each coefficient.

Key

G	general government final consumption plus total public sector fixed investment
GDPE	gross domestic product, expenditure estimate
C	consumer expenditure
X	non-oil exports
M	non-oil imports
O	oil exports net of oil imports
KII	stock of inventories
KI	stock of fixed capital of the private sector
PC	consumer prices index
YD	personal disposable income
RLB	UK bank base rate
EER	Sterling's effective (trade weighted) exchange rate
ET	employed labour force
WSI	index of average wages and salaries
RSW	world short term interest rates
WWPI	world wholesale price index
XWM	index of world exports of manufactures
POILWC	index of oil prices in 'world' currency
WGPN	index of world GNP
M0	money supply, M0, not seasonally adjusted
PSBR	public sector borrowing requirement
DRESV	change in reserves
GOVTB	stock of non-money government debt
BAL	current account balance
OJ	private sector net overseas financial assets

XEER	expectations of the exchange rate one period ahead
COMP	index of competitiveness (i.e. real exchange rate)
PX	non-oil export price index
T	average income tax rate
TREF	average indirect tax rate
TIME	time trend
ICHS	sales of council houses

Equations

Prices

$$\text{PROD} = 1.0057^{\text{TIME} \cdot 32}$$

$$\begin{aligned} \text{PC}_t = & \exp[\log(\text{PC}_{t,1}) - 0.027(\text{PC}_{t,1}/\text{PC}_{t,2}) \\ & (-0.19) \\ & + 0.125 \log(\text{TREF}_t) - 0.090 \log(\text{TREF}_{t,1}) - 0.049 \log(\text{TREF}_{t,4}) \\ & (4.63) \qquad \qquad (-2.68) \qquad \qquad (-2.10) \\ & + 0.001 \log(\text{PC}_{t,1}/(\text{WWPI}_{t,1}/\text{EER}_{t,1})) \\ & (0.08) \\ & + 0.043 \log(\text{GDPE}_{t,1}/\text{PROD}) - 0.467] \\ & (1.68) \qquad \qquad \qquad (-1.52) \end{aligned}$$

Estimation period from 1981Q2 to 1992Q4

$R \text{ bar}^2 = 0.40$

std. error of estimate = 0.005

Durbin-Watson = 2.32

Exchange rate

$$\text{EER}_t = \text{XEER}_t(1 + (\text{RLB}_t - \text{RSW}_t)/400)$$

Wages

$$\begin{aligned} \text{WSI}_t = & \exp[\log(\text{WSI}_{t,1}) + \log(\text{WSI}_{t,1}/\text{WSI}_{t,2}) \\ & + 0.033 (\log(\text{WSI}_{t,2}/\text{WSI}_{t,3}) - \log(\text{WSI}_{t,3}/\text{WSI}_{t,4})) \\ & (0.22) \\ & - 1.070 (\log(\text{WSI}_{t,1}/(1.0057 \times \text{PC}_{t,1})) - (\log(\text{WSI}_{t,1}/\text{PC}_{t,1}))) \\ & (-5.05) \\ & - 0.561 (\log(\text{PC}_{t,1}/\text{PC}_{t,2}) - \log(\text{PC}_{t,2}/\text{PC}_{t,3})) \\ & (-0.60) \\ & - 0.010 \\ & (-1.09) \end{aligned}$$

Estimation period from 1980Q4 to 1992Q4

$R \text{ bar}^2 = 0.49$

std. error of estimate = 0.168

Durbin-Watson = 2.02

Competitiveness

$$\text{COMP}_t = \text{WSI}_t / (\text{PROD}_t \times \text{WWPI}_t / \text{EER}_t)$$

Export demand

$$\begin{aligned} \log(X_t) = \exp[& 0.800 \log(X_{t-1}) + 0.125 \log(X_{t-2}) \\ & (5.02) \qquad (0.81) \\ & + 0.062 \log(\text{XWM}_t) - 0.023 \log(\text{COMP}_t) \\ & (0.74) \qquad (-0.51) \\ & + 0.618 \\ & (0.95) \end{aligned}$$

Estimation period from 1980Q4 to 1992Q4

$R \text{ bar}^2 = 0.98$

std. error of estimate = 0.025

Durbin-Watson = 1.86

Export prices

$$\begin{aligned} \log(\text{PX}_t) = \exp[& 0.857 \log(\text{PX}_{t-1}) - 0.064 \log(\text{WSI}_{t-1} / \text{PROD}_t) \\ & (10.71) \qquad (-2.28) \\ & + 0.054 \log(\text{WWPI}_{t-1} / \text{EER}_{t-1}) + 0.918 \\ & (1.49) \qquad (2.55) \end{aligned}$$

Estimation period from 1981Q1 to 1992Q4

$R \text{ bar}^2 = 0.97$

std. error of estimate = 0.019

Durbin-Watson = 1.64

Fixed investment

$$\begin{aligned} \text{KI}_t = \text{KI}_{t-1} + \text{GDPE}_{t-1} \exp[& 0.923 \log((\text{KI}_{t-1} - \text{KI}_{t-2}) / \text{GDPE}_{t-2}) \\ & (13.99) \\ & + 0.040 \log(\text{KI}_{t-1} / \text{GDPE}_{t-1}) + 1.857 \log(\text{GDPE}_{t-1} / \text{GDPE}_{t-2}) \\ & (0.44) \qquad (1.63) \\ & + 0.042 \log((\text{WSI}_{t-1} \times \text{ET}_{t-1}) / (\text{PQ}_{t-1} \times 1.0057^{\text{TIME}})) \\ & (0.38) \\ & - 0.617 \\ & (-0.52) \end{aligned}$$

Estimation period from 1980Q3 to 1992Q4

$R \text{ bar}^2 = 0.90$

std. error of estimate = 0.062

Durbin-Watson = 2.29

Flow of gross private fixed investment

$$\text{IFP}_t = \text{KI}_t - \text{KI}_{t-1} + 4610(\text{TIME} - 100)$$

Inventories

Expected real rate of interest

$$\begin{aligned} \text{RREAL}_t = & 0.871 \text{ RLB}_{t-1} - 0.383 (400(\text{EER}_{t-1}/(\text{EER}_{t-1} - 1))) \\ & (9.16) \quad (0.81) \\ & + 0.005 (400(((\text{WWPI}_{t-1} \times \text{EER}_{t-2})/(\text{WWPI}_{t-2} \times \text{EER}_{t-1}))- 1)) \\ & (0.40) \\ & - 0.003 (400(\text{WSI}_{t-1}/\text{WSI}_{t-2} - 1) + 157.180) \\ & (-0.28) \quad (0.82) \end{aligned}$$

Estimation period from 1981Q2 to 1992Q4

$R \text{ bar}^2 = 0.67$

std. error of estimate = 1.212

Durbin-Watson = 1.71

$$\begin{aligned} \text{KII}_t = & \exp[\log(\text{KII}_{t-1} - 0.00002 \text{ RREAL}_t + 0.534 \log(\text{KII}_{t-1}/\text{KII}_{t-2})) \\ & (-0.60) \quad (4.37) \\ & + 0.00003 \log(\text{KII}_{t-1}/\text{GDPE}_{t-1}) - 0.0002 \\ & (0.08) \quad (0.06) \end{aligned}$$

Estimation period from 1981Q2 to 1992Q4

$R \text{ bar}^2 = 0.27$

std. error of estimate = 0.0004

Durbin-Watson = 2.13

Employment (whole economy)

$$\begin{aligned} \text{ET}_t = & \exp[\log(\text{ET}_{t-1}) + 0.310 \log(\text{ET}_{t-1}/\text{ET}_{t-2}) - 0.129 \log(\text{ET}_{t-1} \times \text{PROD}/\text{GDPE}_{t-1}) \\ & (2.07) \quad (-3.71) \\ & - 0.0005 \log(\text{TREF}_{t-1}/\text{PC}_{t-1}) - 0.0002 \log(\text{WSI}_{t-1}/(\text{PROD} \times \text{PC}_{t-1})) \\ & (-0.44) \quad (-0.06) \\ & - 0.178 \\ & (-3.55) \end{aligned}$$

Financing the PSBR*Government debt*

$$DGG_t = PSBR_t - (MO_t - MO_{t-1}) + DRESV_t$$

Stock of government debt

$$GOVTB_t = GOVTB_{t-1} + DGG$$

Data sources

This list provides further details of the economic data used to estimate the model. Each data series is identified by a unique four-letter code – for example, consumer expenditure is CAOO – and is cross-referenced to the publication in which it appears, in this case *Economic Trends*. The majority of the data series were downloaded directly from the ESRC's time-series data bank, although some series were not available and were entered manually.

ESRC CENTRE IN ECONOMIC COMPUTING

TIME SERIES DATA BANK - RELEASE DATE 94

List of publications

BB	UK National Accounts, Blue Book
EG	Employment Gazette
ET	Economic Trends
ETAS	Economic Trends Annual Supplement
FSC	Financial Statistics
FSF	Financial Statistics
MD	Monthly Digest of Statistics
PSBR	Public Sector Borrowing Requirement
QA	Quarterly Accounts
RPI	Retail Price Index

DATA RETRIEVAL

Periodicity : Quarterly

Sample period 46 1 TO 94 4 No. observations 196

No. series retrieved 19

Start Base
YPPY YYMM

TABLE QA A06 - General Govt: GDFCF: CP NSA: BPVDL: D0-9

 DFDA DFDA Public Corps: GDFCF: K90 SA: BPVDL: D0-9 62 1 90 0
 Seasonally adjusted
 SIC : D0-9

TABLE ET 2.2 - Consumers' expenditure : Total #m CONS (1990 prices) SA

 CAO GDP(A) at constant market prices (1990 prices) 55 1 90 0
 Seasonally adjusted
 CAAB Consumers' expenditure : Total #m CONS (1990 prices) SA 55 1 90 0
 Seasonally adjusted

TABLE QA A02 - General govt : taxes on expenditure

 DJDG Goods and services: total exports (credits), CONSTANT PR 55 1 90 0
 Seasonally adjusted

TABLE MD 15.1 - Food beverages and tobacco: OTS : Exports by commodity

 BOCD Fuels : OTS : Exports by commodity 70 1 0 0
 Seasonally adjusted

TABLE MD 1.2 - General govt : taxes on expenditure

 DJDJ Goods and services: total imports (debits), CONSTANT PRI 55 1 90 0
 Seasonally adjusted

TABLE MD 15.1 - Food beverages and tobacco: OTS : Exports by commodity

 BODD Imports Fuels : OTS 70 1 0 0
 Seasonally adjusted

TABLE QA X502 - COS: ICCs: TOTAL CAPITAL TRANSFERS

APPENDIX A

FMCD FMCD COS: Increase in stocks and work in progress #m NSA*	66 1 0 0
<hr/>	
TABLE MD 1.8 - Public Corps: GDFCF: CP NSA: BPVDL: D0-9	
DFEB DFEB Private Sector: GDFCF: K90 SA: BPVDL: D0-9	62 1 90 0
Seasonally adjusted	
SIC : D0-9	
<hr/>	
TABLE FSC 1 - Central govt : current surplus or deficit	
AIIX AIIX Consumers' expenditure: Total #m CURR SA	55 1 0 0
Seasonally adjusted	
<hr/>	
TABLE ET 6.1 - Official reserves outstanding : total US\$m	
AJHV AJHV Sterling effective exchange rate index 1985 = 100	75 1 85 0
<hr/>	
TABLE MD 3.1 - Employees in employment (UK) - thousands (EG table 1.1)	
BCAJ BCAJ Employees in employment (UK) - thousands (EG table 1.1)	59 2 0 0
Seasonally adjusted	
<hr/>	
TABLE ETAS 3.1 - Wages & salaries per unit of output index : whole economy	
DNAA DNAA Average earnings (GB) index : whole economy (1990 = 100)	88 1 90 0
SIC : 0-9	
<hr/>	
TABLE FSF 1 - Central govt : current surplus or deficit	
AJIB AJIB Interest on US dollar deposits in London (3 month)	63 1 0 0
<hr/>	
TABLE FSC 1 - Central govt : current surplus or deficit	
AAGE AAGE Public sector finance : notes & coin #m	63 1 0 0
<hr/>	
TABLE ET 6.5 - General govt : financial surplus or deficit	
ABFB ABFB Public sector borrowing requirement (PSBR) #m (CYSA)	63 1 0 0
Seasonally adjusted	
<hr/>	
TABLE FSF 1 - Central govt : current surplus or deficit	
AACM AACM CG : liabs: flows: Other government overseas financing	63 2 0 0
AIPA AIPA Central govt finance: official reserves #m	46 1 0 0

EQUATIONS USED IN THE ECONOMETRIC MODEL

TABLE FSC 1 - Central govt : current surplus or deficit

AAAA AAAA Central govt : current surplus or deficit 55 1 0 0

TABLE ETAS 1.3 - General govt : taxes on expenditure

AAXP AAXP General govt : taxes on expenditure - CYSA 55 1 0 0
Seasonally adjusted

TABLE MD 1.2 - General govt : taxes on expenditure

DJDJ DJDJ Goods and services: total imports (debits), CONSTANT PRI 55 1 90 0
Seasonally adjusted

TABLE MD 1.5 - Personal sector: savings # m

AIIW AIIW Personal disposable income #m 55 1 0 0
Seasonally adjusted

TABLE MD 3.1 - Employees in employment (UK) - thousands (EG table 1.1)

BCAJ BCAJ Employees in employment (UK) - thousands (EG table 1.1) 59 2 0 0
Seasonally adjusted

TABLE FSF 1 - Central govt : current surplus or deficit

AJIB AJIB Interest on US dollar deposits in London (3 month) 63 1 0 0

TABLE FSF 1 - Central govt : current surplus or deficit

AACM AACM CG : liabs: flows: Other government overseas financing 63 2 0 0
AIPA AIPA Central govt finance: official reserves #m 46 1 0 0

TABLE MD 15.1 - Food beverages and tobacco: OTS : Exports by commodity

BOCD BOCD Fuels : OTS : Exports by commodity 70 1 0 0
Seasonally adjusted

TABLE QA A02 - General govt : taxes on expenditure

DJAZ DJAZ Goods and services: total exports (credits), CURRENT PRI 55 1 0 0
Seasonally adjusted

TABLE QA A09 - Personal sector: total personal income #m

APPENDIX A

<hr/> AIIU AIIU Personal sector: UK taxes on income #m Seasonally adjusted	55 1 0 0
TABLE ETAS 1.6 - Personal sector: total personal income #m	
<hr/> AIIQ AIIQ Personal sector: total personal income #m Seasonally adjusted	55 1 0 0
TABLE MD 1.2 - General govt : taxes on expenditure	
<hr/> AAXW AAXW General govt : subsidies - CYSA Seasonally adjusted	55 1 0 0
TABLE ETAS 1.3 - General govt : taxes on expenditure	
<hr/> AAXP AAXP General govt : taxes on expenditure - CYSA Seasonally adjusted	55 1 0 0
TABLE ETAS 1.2 - Invisibles (balance) : interest, profits & dividends #m	
<hr/> DIAS DIAS General govt : adjustment to factor cost @ 1990 prices - Seasonally adjusted	55 1 85 0
TABLE BB 14.6 - Personal sector: NDFCF: dwellings #m (Annual)	
<hr/> EXGB EXGB Personal sector: NDFCF: all fixed assets #m	48 1 0 0
EXGC EXGC I&C companies: NDFCF: all fixed assets #m	48 1 0 0
EXGD EXGD Financial companies: NDFCF: other fixed assets #m	48 1 0 0

NOTES

1 SSK AND ECONOMIC FORECASTING

- 1 For example, in September 1993 central banks across the world were forced to find US\$3.5 billion in order to prevent the collapse of hedge fund Long Term Capital Management and avert what President Clinton called 'the biggest threat to the global financial system in 50 years'. The crisis was caused when Long Term Capital Management's forecasts for European exchange rates went catastrophically wrong. Ironically, Long Term Capital Management was founded by the economists Myron Scholes and Robert Merton, who were awarded the 1997 Nobel prize for their work on developing the formula that laid the foundations for financial markets in derivatives. Sources: Atkinson 1998, Elliot 1998.
- 2 There are some exceptions, however. For research that adopts a similar theoretical perspective to that used in this book see Ashmore, Mulkay and Pinch 1989 and Yonay 1997. Other relevant literature, written by economists, is Mirowski 1989, McCloskey 1986, Feigenbaum and Levy 1993.
- 3 For example see Holden, Peel and Thompson 1990 and Whitley 1994. The ESRC Macromodelling Bureau books provide lots of interesting detail on models and their comparative properties: see for example Wallis (ed.), Andrews, Fisher, Longbottom and Whitley 1987; Wallis (ed.), Andrews, Fisher and Whitley 1984. Finally, the most important exception to the rule is Giles Keating's *Production and Use of Economic Forecasts* (1985), which provides a detailed account of the way in which a forecast is made.
- 4 Andrew Britton, Wynne Godley and Patrick Minford had all previously worked for the Treasury. David Currie and Gavyn Davies had (at different times) close links with the Labour Party. Tim Congdon and, to a lesser extent, Patrick Minford had connections with the Conservative Party in the early 1980s. Andrew Sentance was Chief Economist for the Confederation of British Industry (CBI).
- 5 It is also worth noting that the forecasts they produced for these meetings were not prepared on the basis of common assumptions about economic policy.
- 6 Examples of this interaction are to be found Morgan and den Butter (eds) 1999.
- 7 In these cases, the models are being used to perform what Ken Wallis refers to as 'if only' analysis.
- 8 For example, the recent criticisms made of the Monetary Policy Committee of the Bank of England by both the CBI and the TUC have all been of this form,

- and perhaps inevitably so given its composition, which includes only one person with industrial or commercial experience.
- 9 I occupy a somewhat ambivalent position, in that I am neither a lay person nor a fully-fledged economic forecaster. The methodology behind the research was participant comprehension: that is, the researcher aims to develop something like the understanding of a insider. To do this I attended economics lectures and estimated a simple economic model of my own. I also talked to economists at length and presented papers to them. These interactions, which involved not only collecting research data, but also learning how to behave like an economist, were also a test of my own learning and understanding. To the extent that these interactions were successful, I know what I am writing about because I have learned to participate in the professional life of academic economics. The rationale for this participant comprehension methodology is found in Collins 1984.
 - 10 There is also a large literature on economic methodology written by economists. Some good starting points are: Gilbert and De Marchi (eds) 1989; Caldwell, 1994; Blaug 1980; De Marchi 1992; Mayer 1995; Blaug 1990; Caldwell 1993; Dow 1994; Stewart 1993.
 - 11 For some key SSK studies see: Shapin and Schaffer 1985; Garfinkel, Lynch and Livingstone 1981; Lynch, Livingstone and Garfinkel, H 1983; Callon 1986a, 1986b; Collins 1985. For a sociologically informed reading of Wittgenstein see e.g. Bloor 1983.
 - 12 A recent textbook giving the 'strong programme' view of SSK is Barnes, Bloor and Henry 1996.
 - 13 Similar problems arise in economics for example when attempting to estimate an econometric relationship. Does a significant coefficient reflect a real association or is it a result of the data collection process?
 - 14 This section owes much to Ashmore 1993.
 - 15 This is the basis of the 'underdog' debate started by Scott, Richards and Martin 1990. For the initial reply and response see Collins 1991 and Scott, Richards and Martin 1991 The latest instalments are in Ashmore and Richards (eds) 1996.
 - 16 This formulation also raises the possibility that if the SSK deconstruction is unusually effective, the orthodoxy will lose credibility to such an extent that it will become the underdog, thus causing the sociologist to have to change sides. For an excellent summary of these and other consequences of the 'underdog' debate see Ashmore 1996.
 - 17 See Collins, H. M. and Pinch, T. J. 'Unwanted Children: Essays in the sociology of fringe science', unpublished manuscript.
 - 18 It is in this sense that the critique is an analytic one: it champions not an outcome but a process.
 - 19 This discussion draws heavily on chapters 1 and 7 of Keating 1985. I am greatly indebted to Andrew Britton for alerting me to this source. For more on Tinbergen's model see chapter 4 of Morgan 1990. Tinbergen's own account is given in Tinbergen 1937.
 - 20 See also Tinbergen's 'Reply' and Keynes's 'Comment', *Economic Journal*, vol. 50: 141-56.
 - 21 Source: Morgan 1990.

- 22 These changes are discussed more fully in Evans 1997b.
- 23 The monthly survey published by the Treasury typically includes forecasts from over thirty forecasting groups and this is by no means exhaustive. It is available on the Treasury web site (<http://www.hm-treasury.gov.uk>).
- 24 Minutes of the meetings at which these decisions are taken are available on the Bank of England web site (<http://www.bankofengland.co.uk/index.htm>).
- 25 The Reports of the Treasury Select Committee are available from the Committee's home page on the House of Commons web site (<http://www.parliament.uk/commons/selcom/treahome.htm>).
- 26 See Britton (ed.) 1983. For comparison, an outline of the NIESR 'model' circa 1959 is given in Surrey (ed.) 1971. A summary of this informal income expenditure framework is also given in Keating 1985: 136-7.
- 27 An overview of the properties of the Cambridge Economic Policy Groups model, and its implications for policy making, can be found in Cripps, Fetherston and Godley 1980.
- 28 Interview, 5 April 1993.
- 29 A collection of Congdon's articles and an overview of his modelling methodology can be found in Congdon 1992.
- 30 For an overview of the Lombard Street Research forecasting approach see Congdon 1993a.
- 31 These changes reflect changes in the staff employed to maintain the model as well as changes in economics more generally and are, at least in part, one of the consequences of being an institutional forecasting organisation.
- 32 The theory of international monetarism assumes that a higher UK money supply will lead to higher UK prices and wages. A secondary assumption of the theory is that 'long-run purchasing power parity' holds. Because of this secondary assumption, which means that the real exchange rate will eventually return to some long-run level, a rise in the UK money supply causes the foreign exchange markets to mark sterling down straight away, rather than take an anticipated capital loss some time in the future. In the LBS model, this meant that new wage and price equations were needed, in order to ensure that the falls in the exchange rate (brought about by the growth in the money supply) were matched by proportionate changes in the domestic price level. The implication of this would seem to be that, by changing three key equations, those determining the exchange rate, prices and wages, the whole character of the model was changed from a conventional income-expenditure model to a international monetarist model. A corollary of this is that by overriding a few equations the international monetarism would be lost.
- 33 In the US things apparently work the other way and consumers may actually save less in order to buy more at current prices in order to avoid the higher ones later on. I am grateful to one of the anonymous referees alerting me to this.
- 34 A 'reduced form' is a version of the macro-econometric model in which only exogenous variables appear on the right hand side of equations. See the recent review by the ESRC Macroeconomic Modelling Bureau: Church, Mitchell, Smith and Wallis 1993, esp. Box A, p. 89.
- 35 This means that any variables that represent the expected value of something in the future are forced by the model to take values that are consistent with the computed outturn values. The main difference between the Liverpool model and

- the others that used rational expectations, for example the LBS model discussed earlier, was that the Liverpool model included rational expectations in all markets.
- 36 Although many people agree that the supply side effects identified by the Liverpool group exist, their magnitude is frequently disputed. The differences between economists on these issues are more usefully considered to be ones of degree rather than principle.
- 37 Most modern macro-econometric models allow for some wealth effect on consumption. Again the differences are of degree rather than principle.
- 38 Forecasts for monetary aggregates are produced but appear only as dead ends in the flow chart: they do not (at least in version 6 of the model) feed back to influence other variables.
- 39 The covered interest rate differential term was intended to proxy expectations of the future exchange rate. The abolition of exchange controls in 1979 however meant that the differential then reflected only arbitrage costs, which were small. Consequently the NIESR had to find other ways of modelling exchange rate expectations. Although both LBS and NIESR models exhibited long-run purchasing power parity they did so in different ways. In the LBS model, longrun PPP was imposed indirectly through the price equation, in the NIESR model, long-run PPP is a property of the exchange rate equation itself.
- 40 See the *National Institute Review*, November 1989 for full details.
- 41 There are many excellent articles and papers comparing the forecasting performance of different organisations. For the models in the UK funded by the ESRC, the Macromodelling Bureau at Warwick has produced a series of comparative studies that are probably unequalled in terms of their depth and sophistication. In the US, Stephen McNees has published a range of papers comparing different forecasts and forecasting techniques. References to a selection of these and other papers are as follows: Wallis (ed.), Fisher, Longbottom Turner and Whitley 1987; Wallis (ed.), Andrews, Fisher, Longbottom and Whitley 1986; Wallis (ed.), Andrews, Bell, Fisher and Whitley 1985; Wallis (ed.), Andrews, Bell, Fisher and Whitley 1984; Church, Mitchell, Sault and Wallis 1997; Wallis 1993; Wallis and Whitley 1991; McNees 1979; McNees and Ries 1983; McNees 1988; Henry and Holden 1990.
- 42 In the budget of June 1979, the new administration introduced a series of reforms which included:
- increasing VAT from 8 to 15%
 - reducing the standard rate of income tax from 33p in the £ to 30p
 - rejecting short-term demand policies
 - enforcing tighter controls on public spending
 - the exclusive use of monetary policy to control inflation
 - allowing the pound to float freely in the foreign exchange markets.
- 43 The fact that Liverpool produced the most accurate forecast for the base year was undoubtedly aided by the fact that their forecast was published three months after the others. In addition, it should be noted that the output and expenditure measures of GDP growth for 1984 were quite different. In 1984, the output measure of GDP growth was 3.1 per cent while the expenditure measure recorded it as 1.7 per cent. In 1985, the figures were 3.4 per cent and 3.3 per cent respectively.

- 44 It is of course extremely difficult to judge forecasts without reference to some sort of big picture. The NIESR may have got the rise in unemployment (almost) right but they only did so because they were forecasting below trend growth. Given that the period saw above trend growth of over 3 per cent, it is possible to argue that they were basically wrong, but managed to get unemployment right by chance. The same of course applies to all the other forecasting groups.
- 45 See Wallis and Whitley 1991. The period 1984–8 was characterized by vigorous economic expansion, with GDP growing by about 4 per cent per annum, far in excess of any comparable period in the previous twenty years. Unemployment rose from just under 10.5 per cent in 1984 to 11.25 per cent in 1986 before falling back to just under 8 per cent in 1988. Inflation was relatively low throughout the period, declining until 1986 and picking up somewhat thereafter. This variation was not great however, with the annual estimate remaining within the range 3.5 per cent to 5.1 per cent.
- 46 When it is remembered that the average figure for GDP growth is around 2.5 per cent, it is apparent that errors of 1.5 per cent can be quite serious. For example, 1.5 per cent is the difference between trend growth of about 2.8 per cent per annum and an economic boom with growth rates of over 4 per cent per annum.
- 47 Source: ESRC *Social Science Newsletter*, April 1995: 2.

2 HOW ECONOMIC MODELS ARE ESTIMATED

- 1 There is no inconsistency between this claim and my own position as a sociologist. To carry out this research I did participate in the life of economics. For example, I attended undergraduate lectures in economic theory and macroeconomic modelling. I also presented papers based on this work to economists. I interviewed economic forecasters at regular intervals and learned a great deal from this. Indeed, as a methodological aside, it is worth noting that, from the sociological perspective, the purpose of the interviews was not to get ‘quotes on tape’ but to learn and thereby approximate socialization into the economic forecasting community.
- 2 According to his entry in *Who’s Who in Economics*, Keating studied economics at Oxford and the London School of Economics and was a Research Fellow at the London Business School. His areas of expertise are macro-econometric modelling and forecasting, and domestic and international financial systems. In 1990 he was Chief Economist and Director of Research for Credit Suisse First Boston Ltd. (Source: Sturges and Sturges (eds) 1990.)
- 3 It should be noted that mainstream macroeconomists, particularly in the US, have moved away from this sort of work. Nevertheless, as the rest of the book shows, these ideas have remained important for policy makers and their advisers.
- 4 For a historical overview of the development of national income accounts, and the influence of the Keynesian revolution in shaping their form, see Patinkin 1976.
- 5 This is how they are explained in economics textbooks. See e.g. Parkin and King 1992 and Begg, Fisher and Dornbusch 1991).
- 6 To complete the story we can think of payments to the households being mediated through the ‘factor market’ (the market for the factors of production such as labour, land and capital) and payments to the firms as taking place in the ‘product market’ (the market for finished products).

- 7 The IS-LM model was developed by the English economist Sir John Hicks. See for example Helm (ed.) 1984.
- 8 Although the IS and LM curves have been drawn as straight lines there is no reason why they must be.
- 9 Keating's book was recommended to me by Andrew Britton, director of the NIESR. Keating is remarkable for the detailed references he gives. This is unusual among economists, and replication of econometric work usually founders because would-be replicators are unable to reconstruct the data set. In a 1986 paper Dewald, Thursby and Anderson (1986) found that data sets and programs submitted with journal articles were often so badly documented that they were unable to identify which variables had been used in calculating the published results. In many cases they found that replication was only possible after extensive consultation with, and active assistance from, the economist who had submitted the paper. In some cases even this was not enough to reproduce the results.
- 10 This is the experimenters' regress. See Collins 1992.
- 11 In this case, an approximation to the type of socialization needed to make these judgements was achieved through repeated interviews with leading economic forecasters. For more on the justification for these sorts of participatory research methods see Collins, 1984.
- 12 The econometric procedures followed adopt what can be thought of as the Hendry style of econometric practice.
- 13 'Lagged' data refers to data from a previous time period. Thus the first lag of consumers expenditure (C) is its value in the preceding time period and is represented by (C_{t-1}) . The data series used were as follows (the four letter code in parentheses indicates the CSO codes for that series): C = Consumer Expenditure in 1990 prices (CAAB); PC = Consumer Price Index, calculated by dividing Consumer Expenditure in current prices (AIIX) by Consumer Expenditure in 1990 prices (CAAB) and multiplying the result by 100; RPDI = Real Personal Disposable Income, calculated by dividing Personal Disposable Income in current prices (AIIW) by $(PC/100)$; RLB = Average Discount Rate on 91 Day Bill (AJNB). In Keating's model, RLB is the UK banks' base rates. However, it was not possible to locate a sufficiently long run of figures, so the average discount rate was used instead.
- 14 In order to replicate Keating's econometrics it was first necessary to obtain the appropriate economic data. As mentioned, Keating provides unusually detailed references and it was possible to download most of the data directly from the CSO databank held at the ESRC Archive. The remaining data series, listed in the *National Institute Economic Review*, the London Business School *Economic Outlook* and the OECD *Main Economic Indicators* were entered manually. Finally several transformations of certain series were necessary in order to bring the data in line with that used by Keating. For example, the CSO databank does not include non-oil imports and exports at constant prices, and so this series had to be constructed by transforming the series in current prices that was available.
- 15 The full details of the regression output for my coefficients are reproduced in Evans 1997b. For further details of Keating's coefficients see Chapter 4 in Keating 1985.
- 16 In his Ph.D. thesis Bernard Walters examines the sensitivity of Layard and Nickel's labour market econometrics to the vintage of the data set and finds that an

- initially supported hypothesis is rejected by later versions of the data. See Walters 1993.
- 17 The 'correct' presentation of these results is itself a matter of convention, as is the degree of similarity required for results to be considered the 'same'. In some scientific fields, accuracy is measured to six or more decimal places, in others an order of magnitude will suffice. See: Kuhn 1961.
 - 18 Subject to the caveat that data revisions have not changed the economic history to such an extent that completely new relationships throughout the entire period are now implied.
 - 19 For full details of the regression analysis in this chapter, see Evans 1997b.
 - 20 The classic case of the experimenter's regress probably concerns the existence or otherwise of high fluxes of gravity waves and is described in detail in Collins 1985.
 - 21 As all the data used is seasonally adjusted this is what one would expect to find, although Keating does not use this as a reason.
 - 22 The same is also true of the impact of interest rates on investment where economic theory expects a clear connection which econometrics has yet to find; see Evans 1993b.
 - 23 This was indeed the case for Keating's equation. ($\rho = 0.00295, t = 0.72$).
 - 24 Source: Davies 1993b: 49, para 17.
 - 25 The retrenchment that took place during the early 1990s, as evidenced by the exceptionally high savings ratio during this period, suggests that consumers are now taking steps to restore their balance sheets. Thus it is possible that in a few years the data might once again support the hypothesis.
 - 26 This question was addressed directly by the NIESR in a report that analyses forecasting performance in the UK during the 1980s (Britton and Pain 1992). In a section entitled 'Tests for a Structural Break', the authors, Andrew Britton and Nigel Pain write that:

The test statistics are best interpreted as asking whether any inefficiencies in the forecasts from 1983Q4 onwards differ from those in earlier forecasts. Somewhat surprisingly, there only appears to be evidence of structural breaks in the relationships for investment and disposable income when the latest outturns are used, although the statistics for GDP, domestic demand and inflation are significant at the 10 per cent level. The most likely explanation is that many coefficients are poorly determined in the final subsample and are therefore consistent with a number of different hypotheses. If the initial outturns are used, the null is rejected for four variables, GDP, inflation, employment and disposable income.

(Britton and Pain 1992: 15)
 - 27 This is not necessarily a bad thing as all theories take time to establish and thus a certain tenacity may be an admirable quality in a scientist.
 - 28 The rule used was the following: starting from the general consumption function (Equation 2.1), exclude the variable with the lowest t statistic and reestimate the equation, repeating this process until all remaining variables are significant at the 5 per cent level.
 - 29 Formulae for calculating RMSE are as follows, where X_f = forecast and X_t = actual:

$$\text{Mean Square Error (MSE)} = \frac{\sum(\mathbf{X}_t - \hat{\mathbf{X}}_t)^2}{(t - 2)}; \text{RMSE} = \sqrt{\text{MSE}}$$

Source: Ramanathan 1992: 115.

3 HOW ECONOMIC FORECASTS ARE MADE

- 1 I interviewed each of the Panel members several times during 1993 about the forecasts, so am reasonably well informed about economic events in that particular year.
- 2 See e.g. the recent review by the ESRC Macroeconomic Modelling Bureau: Church, Mitchell, Smith and Wallis 1993: 87-100, esp. Box A, p. 89.
- 3 Interested readers might like to know that Keating traces his model to: Buiter and Miller 1981.
- 4 A more complex alternative, not used in my model, would be to use a multivariate autoregression model to produce the exogenous projections.
- 5 I had to rely on the distilled wisdom of others because I am not an economic forecaster and have neither the knowledge nor the connections to justify strong and informed opinions. It is this knowledge gap which separates economic forecasters from the general public and explains why a 'ready-to-run' economic model would be of little use to anyone.
- 6 For examples of the sorts of adjustments which professional forecasters make see Wallis and Whitley 1991). The London Business School publication *Economic Outlook* provides a list of the residual adjustments used in its forecasts, although with no description of why they were made.
- 7 For example, interest rates were cut from 15 to 10.5 per cent between 1990 and 1991, and rates had fallen again, from 10 to 6 per cent, in the previous few months. Source: Britton 1993a: 18 para. 6.
- 8 Stockbuilding refers to the accumulation or running down of inventories.
- 9 Only two of the Seven Wise Men (Britton and Currie) forecast an increase in investment for 1993. The median forecast was -0.5 per cent. In addition, Minford's forecast, which is not included in these calculations because he forecast a different measure of fixed investment, was also negative (-2.7 per cent). Overall it seems that the 'consensus' was negative.

It is interesting to note that the uncertainty surrounding the forecast for fixed investment is very large compared to the sort of change in GDP that is important in economic forecasting. For example, if my forecast for fixed investment was adjusted so as to show a growth of 1.5 per cent, which is within the range forecast by the Panel of Forecasters, this would be sufficient to increase the forecast for GDP from its current value of -1.0 per cent to a growth forecast of 0.6 per cent.

- 10 See Chapter Two, Figure 2.1.
- 11 Note that adopting the average of the Panel of Forecasters thus functions as a way of making decisions that I otherwise do not have the experience or expertise to make in 'real time'. For example, even if I did believe that the interest rate would fall, I would still have to forecast its value: would it fall to 6 per cent, to 5 per cent or even 4 per cent? As was pointed out in Chapter Two, however, the aim of these expositions is to emphasis what sorts of expertise is needed to make economic forecasts; it is not to provide that expertise.

- 12 According to a standard economic textbook:

Consumer goods and services are final products which firms sell to households and which households buy because they are useful or pleasurable. They include items such as bread and ice-cream, shoes and ornaments, haircuts and rides at the fair. They also include accommodation in owner-occupied homes which are held to be let by their owners to themselves.

(Parkin and King 1992: 561)

- 13 Stockbuilding is usually measured in terms of its contribution to GDP growth
 14 For example, interest rates often rise in response to increases in inflation, so assuming an increase in interest rates implies an assumption that there will be inflation in future.
 15 The problem is neatly captured in the following limerick:

A trend is a trend is a trend
 But the question is, will it bend?
 Will it alter its course
 Through some unforeseen force
 And come to a premature end?
 (Cairncross 1969)

4 CHOOSING BETWEEN ECONOMIC MODELS

- 1 Economists have a large literature on these issues. The focus here is on sociological understanding. For economists' own perspectives on the methodology of their discipline, see Blaug 1980, 1990, de Marchi 1992, Caldwell 1993, 1994.
- 2 The phrase 'cherished beliefs' is Kip Thorne's. See Collins 1992
- 3 The regression software actually calculated the coefficients to nine decimal places.
- 4 This set of error terms has a normal distribution, with zero mean and a standard deviation which can be calculated from the observed differences between the estimated and actual values of the dependent variable.
- 5 However some modelling teams are attempting to re-estimate their models using full information techniques.
- 6 See e.g. Wallis and Whitley 1991 and Turner 1990. For a criticism of the routine use of judgmental adjustments see Wren-Lewis 1992.
- 7 In addition, the relative weight assigned to the model and modeller are unclear. This poses problems when a person leaves one forecasting group and joins another: should his or her record be transferred to the new group?
- 8 Interestingly, during the 1980s the NIESR, in its *Economic Review*, headed each forecast table with the disclaimer: 'the forecast figures are not intended to be any more precise than the general statements in the text' (see e.g. *National Institute Economic Review* no. 123, February 1988, p. 7, Table 1).
- 9 Note that here macro-economic forecasting and modelling are being positioned as part of academic economics. In this respect the UK situation is perhaps unusual. In other countries, particularly the US, although economic models are

- used by banks and policy makers, they have much weaker connections with academic economics.
- 10 Arguably the publicly/privately funded divide mirrors the science–technology division in the natural sciences.
 - 11 There are however strong similarities between macro-econometric and climate modelling and forecasting. The ‘flux adjustments’ used by modellers of climate change seem to function similarly to the ‘residual adjustments’ used by economists. For an interesting account of the ways in which climate scientists manage the tension between ‘uncertainty’ and ‘authority’ see: Shackley and Wynne 1996.
 - 12 When estimating equations for an econometric model, the modeller usually transforms the variables into logarithms before the regression analysis begins. This means that the coefficients correspond to the elasticities on the variables, which are important analytic concepts in economics. An elasticity of 1 or more means that the dependent variable is sensitive to changes in the independent variable. If the coefficient is less than 1 this implies that the relationship is inelastic, that is, that the dependent variable is relatively insensitive to changes in the independent. Thus the difference of 0.4 is not simply a matter of magnitude but of meaning.
 - 13 If the error terms of a regression equation are correlated and the estimation process does not take this into account then the coefficients cannot be the most efficient estimators as not all the available information will have been used. In particular, no account will have been taken of the fact that the error terms are not random but systematically related.
 - 14 In time series, econometric data from periods before the one in question is called lagged data, and changing the lag structure in an equation means changing the time allowed for a change in the independent variable to affect the dependent variable. For example, instead of saying that investment is a function of interest rates in the last quarter (i.e. interest rates lagged one quarter), we might say that investment is determined by the level of interest rates four quarters ago (i.e. lagged four quarters). Alternatively, both lagged values might be important.
 - 15 Although the Lucas critique relates particularly to endogenous policy change, it is possible to extend the same idea to structural change more generally.
 - 16 This also provides a good example of how econometrics is concerned to develop new statistical techniques to meet the specific needs of economists. In the case of the Lucas critique, techniques such as Kalman filtering have been developed to deal with the problems caused by structural change and changing coefficients.
 - 17 Ashmore, Mulkay and Pinch (1989) also make the distinction between strong and weak economics in their study of health economics.
 - 18 This methodology does not have the unqualified support of all economists. For example, Ormerod rejected regression outright:

Ormerod: It was clear to me some years ago that macro models based on conventional economic theory and then parameterised by econometrics couldn’t do what they claimed to be able to do, which was they claimed to be able to do short-run forecasting.

(Interview 16 March 1993: transcript p. 2)

Godley was critical not so much of regression analysis *per se* but rather of the idea that by expanding the regression equation to include more explanatory variables better forecasts would be produced:

Godley: I think that the conventional aspiration in regard to macroeconomic modelling is wrong. There's a fantasy that you're going to introduce more and more complexity and more and more realistic features . . . and eventually it will work. It's all been a great failure, all of that, and forecasting isn't done better, it makes no progress, as a result

(Interview 5 April 1993: transcript p. 7)

- 19 It can also be argued that the frequency with which forecasts are published and their easy availability make questionable the assertion that the different modelling groups are independent of one another.
- 20 The changes in theoretical orientation may also reflect changes in personnel. Rational expectations were introduced into the LBS model in the early 1980s when Alan Budd was Director of the Centre for Economic Forecasting. David Currie was appointed as Director in 1988 and rational expectations were dropped from the model in 1990.
- 21 For an introduction to the economists' own literature on this see the references in note 1.
- 22 It is interesting to note the idea of science used here. For a sociological analysis of the Michelson Morley experiments see Collins and Pinch 1993.
- 23 It is interesting to note the prominence given to inflation during the 1980s. To the extent that the government, a prime user of forecasts, and the wider society chooses to prioritize inflation, then those models which predict inflation most accurately will come to be seen as the best.
- 24 This (Holden 1989) is the study referred to in Chapter One.
- 25 These issues have been discussed at length elsewhere and I offer only a brief summary of the conventional decomposition here. For more details see e.g. Wallis, Andrews, Fisher and Whitley 1984.
- 26 It should be noted however that if exogenous projections are subsequently replaced by the actual values forecasts are not generally improved. See e.g. Wallis, Andrews, Fisher and Whitley 1984.
- 27 In Evans 1993 I argued that the separation between the exogenous adjustments and judgmental adjustments was not a clear one. In the course of this project I discovered that this concern is also shared by some economists:

Wallis: Right, that was the way we did it. It's arguable whether the actual comparison is a fair one, there will be some interaction between the exogenous variable projection that people are using and the adjustments they are making, because what they are actually adjusting, what they are working with, is a provisional forecast based on those projections

(Interview 21 March 1993: transcript p. 14)

- 28 In the jargon of economics, these commitments are called 'priors'. Strong priors are beliefs which economists would be reluctant to give up.

5 ECONOMIC FORECASTS IN FEBRUARY 1993

- 1 Wealth effects are also to be found in strong Keynesian models. See Arouh 1978.

- 2 Minford reached this conclusion via the following ‘back of the envelope’ calculation:

These figures can also be translated into some estimates of PSBR impact: £30-40 billion of the projected 1993 PSBR is due to the recession. The marginal tax rate paid on the wages of the average worker today (including VAT, all National Insurance and direct tax) is about 47 per cent. On top of this, for every £100 million lost in wage bill, there is probably another £10 million lost in corporation tax (since the Lawson reforms, a highly geared tax), as trading profits fall £50 million: national income basically splits one third trading profits, two thirds wage bill. Finally, there are unemployment benefits, costing the Treasury another £20 million per £100 million loss in wage bill. Hence, every £150 million reduction in national income costs the Exchequer about £77 million, or just over 50 per cent (the Treasury in a recent Bulletin has put it even higher at 70 per cent – this is entirely possible, given the difficulties of assessing precisely how recent tax changes have affected PSBR ‘gearing’ to the economy). National income in 1993 looks like being some 10 per cent lower than its potential, implying an Exchequer loss of £30-40 billion – even more on the Treasury’s estimates of the PSBR’s gearing to national income (Minford, 1993a: 69 para. 13)

- 3 See Congdon 1993a. The same issue of *Economic Affairs* (November/December 1993) contains a piece from which Godley would probably not dissent: Martin, 1993. For Godley’s own analysis see Godley 1993b.
- 4 The relationship between unemployment and GDP is more correctly referred to as Okun’s Law.

6 CONTROVERSIES AND DISPUTES IN JULY 1993

- 1 Economists are aware of this reputation and have some jokes that celebrate it: For example:

Economics is the only field in which two people can share a Nobel Prize for saying opposing things. (Specifically, Myrdahl and Hayek shared one.)

If all the economists in the world were laid end to end, they still wouldn’t reach a conclusion.

For these and other jokes about economists see the ‘economist jokes’ www page (<http://netec.mcc.ac.uk/JokEc.html>).

- 2 For a general definition and discussion of hysteresis, see: Georgescu-Roegen 1971.
- 3 They can be moved off unemployment benefit and on to income support or other benefits, however. When this happens they are no longer included in the claimant count definition of unemployment, but nevertheless remain out of work.
- 4 The actual figures are: 3.1 million at the end of 1993, 3.0 million at end of 1994. Source: HM Treasury 1993b.

- 5 Note that the NIESR does not consider unemployment to be the most useful measure of excess supply in the labour market.
- 6 The majority were forecasting that unemployment would fall to around 2.8 million. Minford's forecast was lower at 2.5 million. Source: HM Treasury 1993c.
- 7 This point was disputed by Congdon. He argued that in practice the deficit as a proportion of national assets was tiny. See Congdon 1993a.
- 8 See Britton 1993b. Andrew Sentance has also written about the 'investment gap': see Sentance 1992.

7 POLICY RECOMMENDATIONS IN OCTOBER 1993

- 1 The unified budget was the first simultaneous announcement of the UK government's revenue and expenditure plans. It replaced a system in which expenditure plans were announced in December and revenue plans in March.
- 2 Wynne Godley was not present at the meeting. Although the Report does contain his individual submission, he cannot be considered a full member of the consensus group. Indeed, as the Panel note in the Introduction to the report, 'there are some areas where Professor Godley would dissent from the text' (HM Treasury 1993c: 3).

8 ECONOMIC MODELS, POLICY AND SCIENCE STUDIES

- 1 This is the main message of much sociological work on the public understanding of science. For an excellent collection of relevant papers see: Irwin and Wynne (eds) 1996.
- 2 This view was expressed by Ken Wallis in a personal communication.
- 3 This point is perfectly illustrated by the Panel of Forecasters: in 1993 it was Andrew Britton's forecast for growth, initially an outlier, which turned out to be the most accurate.

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INTERVIEWS

January to May 1993

John Hudson 15 January 1993
Paul Ormerod 16 March 1993
Ken Wallis 21 March 1993
Patrick Minford 31 March 1993
Wynne Godley 5 April 1993
Andrew Britton 29 April 1993
David Currie 13 May 1993

June 1994

Tim Congdon 2 June 1994
Patrick Minford 3 June 1994
Andrew Britton 6 June 1994
Wynne Godley 9 June 1994

July 1993

Andrew Britton 12 July 1993
Patrick Minford July 1993
Andrew Sentance 13 July 1993
Tim Congdon 13 July 1993
David Currie 13 July 1993

October 1993

Andrew Britton 27 October 1993
Gavyn Davies 27 October 1993
Andrew Sentance 29 October 1993
Tim Congdon 29 October 1993

February to March 1994

David Currie 24 February 1994
Andrew Britton 1 March 1994
Tim Congdon 2 March 1994
Patrick Minford 11 March 1994
Alan Budd 22 March 1994

INDEX

- accounting 24, 79
actions: social 128, 187
adjustments 64, 66, 69-71, 83, 135;
 factor costs 61, 63-4, 73-4;
 judgemental 98, 176; residual 63, 68-
 9, 71, 73-4, 81-2; seasonal 140
advice 127, 165, 182, 186; policy 149;
 scientific 189
agents 3, 19, 189-90; economic 79, 91;
 free 148; private 119-20
aggregation 65, 102
American Economic Association 113,
 132
analyses 94, 100-2, 105, 127-8, 132, 135,
 143, 147, 152-3, 157-8, 172, 177-8; *ex*
 post 81; sociological 172
assessments 189
assets 116, 118, 120, 179; holding 40-42
attitudes: political 153; social 153
availability for work test 139

balance of payments 55, 60, 102, 104-5,
 118-21, 128, 146, 148-50, 169
balance of trade 123, 148
balance sheets 107, 123, 126, 176, 179,
 180
balances 28-9, 61, 149, 179
Bank of England 4, 14-16, 66, 95, 190
Bank of England Quarterly Review 66
banks 107
Barker, Kate 16, 21; *see also*
 Confederation of British Industry;
 see also Panel of Independent
 Forecasters

benefits 131, 135-6, 140, 143;
 administration 142; agencies 140;
 offices 138; payments 25; rates 112;
 reform 112; system 129, 131, 167
Bloor, David 9, 11
boom 46, 10; boom/bust cycle 103, 111;
 credit 123
Bootle, Roger 16 *see also* Hong Kong and
 Shanghai Banking Corporation Bank
borrowing 150, 157-9; government 102,
 104, 116, 121, 150, 165-6; public
 sector 147, 158, 160
Bray Amendment 15
Britton Andrew 15, 80, 82, 85, 87-9, 94-
 6, 98, 106-9, 111, 114, 120-3, 125-7,
 130-1, 138-9, 141, 145, 147, 149, 151,
 155, 159-61, 164, 166-8, 174-5, 177-
 8, 184-5; *see also* National Institute of
 Economic and Social Research; *see*
 also Panel of Independent Forecasters
Brown, Gordon 1, 16; *see also* Chancellor
 of the Exchequer
Budd, Alan 8, 171, 183-6
Budget 117, 119, 144, 146, 149, 153-60,
 163-6; pre-statement 1
Building Societies' Association 113
Business Cycles in the United States of
 America 13
Business Strategies Ltd. 16; *see also*
 Rosewall, Bridget

Cambridge Econometrics 21
Cambridge Economic Policy Group 13-
 15, 17, 21, 97;

- Cambridge Growth Project 14-15
 Cambridge, University of 15; Kings
 College 15, *see also* Godley, Wynne
 capacity 132; plant 125
 capital: fixed 107; in-flows 179;
 working 107
 Cardiff Business School 16; *see also*
 Minford, Patrick
 CBI *see* Confederation of British
 Industry
 CEPG *see* Cambridge Economic Policy
 Group
 CGP *see* Cambridge Growth Project
 Chancellor of the Exchequer 1, 3, 8, 16,
 105, 114-5, 117, 126-7, 144-5, 149,
 153-4, 156-9, 161-2, 164-8, 170-1,
 183, 187; *see also* Brown, Gordon, *see*
also Clarke, Kenneth, *see also* Lamont
 Norman
 City University Business School 15, 23
 claimant count 139-40, 142-3
 Clarke, Kenneth 16, 165-7; *see also*
 Chancellor of the Exchequer
 coefficients 17, 24, 31-6, 39-41, 59, 78,
 84
 Collins, Harry 9
 competition 91, 103, 132
 competitiveness 57, 108-9, 112, 115,
 149-50, 160
 Confederation of British Industry 16,
 108; survey 108, 113, 125, 151;
see also Barker, Kate, *see also* Sentence,
 Andrew;
 Congdon, Tim 15-18, 90, 97, 105, 107,
 110-114, 117-20, 124-6, 132, 145,
 150-1, 156-9, 161, 167-8, 178-80,
 184; *see also* Lombard Street Research
 Ltd; *see also* Panel of Independent
 Forecasters
 Consortium Panel 176; *see also* London
 Business School
 construction industry 107-8
 consumption 18-19, 24, 27-8, 30-2, 36-
 7, 39-41, 44-6, 48-9, 51-3, 57, 61,
 64, 66-8, 74, 77, 95, 102, 105-7, 121,
 123, 127, 144, 146, 149-50, 158-9,
 168, 175, 177, 180, 185; government
 117;
 private 149-50; public 149-50
 Cowles Commission 13
 credit 18; controls 123; net 123
 culture: political 129, 151, 169; social
 129
 current account 61, 105, 118-9, 121, 128,
 148
 Currie, David 15-16, 18, 83, 95, 97, 106-
 9, 111, 114-5, 118, 120-7, 130, 144-5,
 147, 149, 151, 156-9, 161, 167-8, 175-
 9, 184; *see also* London Business
 School; *see also* Panel of Independent
 Forecasters
 cycles 103, 116, 126, 137-8, 142-4, 155,
 174, 181
 data 3, 30, 36, 49, 51-2, 60, 68, 72, 78,
 83-8, 90, 93-7, 99, 102, 104, 110, 112,
 132, 137-8, 140-3, 175-6, 179, 181-2
 Davies, Gavyn 15-16, 67, 106, 111, 118-
 20, 123, 125-6, 130, 140, 144-5, 149-
 51, 155-6, 159, 161, 163-4, 167-8,
 184; *see also* Goldman Sachs
 International Ltd; *see also* Panel of
 Independent Forecasters
 debts 101, 107, 117, 126, 146, 157, 159,
 176, 180; bad 107; consumer 68;
 corporate 176; government 105, 116-
 7, 119, 155, 167-8;
 household 123; levels 123; public 161;
 private 176
December Economic Outlook 124
 deficit 104, 116, 118, 120, 121, 123, 144,
 146-9, 155-7, 168-9, 174;
 budget 104, 113, 117-18, 120, 143-50,
 162, 168-9; current account 118, 120,
 148, 157, 169;
 fiscal 178; government 104;
 public sector 117, 145; reduction 116;
 trade 113, 117-20, 143-50
 deflation 21, 125
 demand 18, 20, 27, 86, 97, 121, 123, 130,
 135, 146, 150; aggregate 30, 91;
 domestic 105-6, 123, 175, 179; money
 53; net export 148
 demographic changes 138;
 explanations 140
 Deutschmark 133

- devaluation 18, 61, 65, 67, 69-70, 72, 75, 106, 108-10, 113-5, 118, 12-4, 126-7, 149, 174-6
- deviations 103, 126, 141, 157;
- standard 78, 80
- dimensions: political 182; social 182
- discounts 178
- dividends 25
- durables: consumer 179; household 178
- Dutch Economic Association 13
- earnings 115; average 133
- Economic and Social Research Council 6, 15, 17, 20, 21, 23, 80-2, 188; *see also* Macromodelling Bureau; *see also* Warwick, University of
- Economic Journal* 13
- Economic Outlook* 81, 83
- economies 98; closed 52, 92; domestic 75, 102, 105; European 109; open 92
- economists 76, 82, 84, 90, 93, 95, 98-103, 110, 113, 115-7, 120-2, 124, 126-8, 131, 134, 137, 140, 142-3, 146, 152-3, 156, 162, 164, 168, 170-1, 176, 182-3, 186-9; classical 124, 126, 129, 178-81 classical group 124-6;
- devaluation/conventional group 120-4, 174-9; monetarist 124;
- new classical 132-6
- education 136, 139, 149-150, 158, 167
- employees 75, 136
- employment 20, 22, 57-8, 69-70, 73, 92, 104, 112, 121, 131-2, 135, 138-40, 143, 179
- Employment, Department of 139
- Employment Gazette* 143
- equations 17-18, 24, 30, 33, 35-7, 41, 44-6, 49-50, 52-3, 58-9, 63, 69-72, 75-6, 78-80, 83-4, 86, 134, 178;
- autoregression 55, 68; behavioural 17, 79; multi 52; price 177; wage 177;
- wage and price 20, 177
- equilibrium 27-30, 110, 119, 124, 126, 132, 135, 138, 141, 157, 174
- equities 179
- ERM *see* European Exchange Rate Mechanism
- errors 46, 53, 61-4, 81, 84, 86, 93-5, 96, 99; root mean square 48;
- standard 44-5, 79-80; term 78
- ESRC *see* Economic and Social Research Council
- estimation 30-43, 49, 75, 83, 120, 176
- EU: *see* European Union
- European Exchange Rate Mechanism 2, 15, 17, 59, 95, 104, 125, 133, 175, 180, 183
- European Single Market 175
- European Union 175
- evaluation 84, 96, 172, 182
- exchange rates 18-20, 29, 55, 57, 59, 64-5, 122, 124, 133, 157, 159, 180-1
- Exeter, University of 15
- expenditure 25, 27, 32, 39, 64, 146, 149, 166-7, 176; -based gross domestic product 53-4, 63; cuts 167; household 25-6, 30; public 116, 167; state 119
- experiments 93-9
- expertise 50-76, 170, 172, 188
- exports 19, 26, 29, 57, 61, 64, 67, 71-2, 101, 105-10, 122, 142, 144, 146, 149, 161, 168, 175, 176, 180, 184; growth 64; net 149; net oil 61, 63, 71
- extrapolation 50-76, 84, 110, 125
- factor payments 25
- February 1993 Report 101-27; *see also* Panel of Independent Forecasters
- February 1994 Report 172-90; *see also* Panel of Independent Forecasters
- finances: government 120, 126; public 131, 144, 147, 150, 155, 159
- Financial Times, The* 190
- forecasters 2, 12, 17, 20, 48, 51, 58-9, 64-5, 70-1, 74-7, 80-3, 90, 101, 103, 116, 120, 122-3, 137, 146, 172, 174, 176, 180-3, 190
- forecasting 1-23, 58-74, 87-8, 102, 105-20, 141, 148, 172, 174, 176, 182, 186; balance of payments and government debt 115-20; inflation 113-15; unemployment/ wages 110-13
- forecasts 1-24, 30, 34-5, 43-8, 50-3, 55, 58, 60-2, 64-76, 78-83, 86-9, 93-9, 101-29, 132, 140, 144, 150, 152-4,

- 159-61, 172-83, 185-6, 189;
distribution of values 79; *ex ante* 44;
ex post 44; mistakes 96-8; short-term
52; tests 6
- foreign exchanges 67
- frameworks 137, 171
- Friedman, Milton 3, 18, 93, 113, 132
- GDP *see* Gross Domestic Product
- GDPE *see* expenditure-based gross
domestic product
- Germany 109-10
- Godley, Wynne 15-18, 67, 72, 85, 89-
90, 93-4, 97, 99, 105, 107-9, 111,
114, 116, 118-20, 122-3, 126-7,
142-3, 146-9, 151, 166-7, 176-7,
180-4; *see also* Cambridge, Kings
College; *see also* Jerome Levy
Institute; *see also* Panel of
Independent Forecasters
- Goldman Sachs International Ltd. 15,
118, 125; *see also* Davies, Gavyn
- goods and services 25, 27, 29, 66, 116
- Government 90, 101, 106, 115, 117, 119-
20, 132, 136, 143, 148-50, 181, 183,
186, 188; Conservative 111, 113, 165;
departments 82, 160; Labour 190;
spending 18-19, 25-6, 29, 53, 61, 105,
150, 165-6
- Gross Domestic Product 19, 22, 26-7,
52-3, 59-64, 67-74, 79, 94, 101-3,
105, 108-10, 113, 117-8, 123, 125,
127, 142, 144-5, 147, 149-50, 156-7,
159, 169, 172-3, 176, 179-80; growth
105-10; non-oil 142
- gross national product 55
- growth 40, 41, 60, 96, 101-5, 108-16,
118-9, 121, 124, 125-7, 131, 133, 135-
6, 148, 152, 155, 157, 160, 165-8, 171,
173-4, 178-81, 189; earnings 65, 68-9;
forecast 67; investment 107; money
179; non-inflationary 125, 148 rate
120; social 190; steady state 41
- Hayek, F. A. 14
- Hendry, David 14, 42-3,
- Henley Centre for Forecasting 6; *see also*
Ormerod, Paul
- HM Treasury 3, 13, 15, 17, 23, 55, 58, 60,
65-7, 70, 81, 105, 137-8, 142, 144-5,
154-6, 164-6, 175, 183-4, 186-7
- Holden, Ken 21, 97
- Hong Kong and Shanghai Banking
Corporation 16; *see also* Bootle,
Roger
- housing: costs 66; market 67, 106-7
- HSBC Bank *see* Hong Kong and
Shanghai Banking Corporation
- Hudson, J. 43
- hypotheses 35, 40-3, 87, 88, 91, 93, 95-6,
99-100
- hysteresis 128-9, 131-7, 147, 151, 157,
169
- ILO: *see* International Labour
Organisation
- imports 19, 26, 29, 54-5, 57, 61-3, 67,
72, 74, 9, 109, 115, 121, 124, 146-8,
175; non-oil 72
- income 26-30, 36-7, 39-42, 46, 52-3, 67,
95, 116, 146, 176-7;
- disposable 25, 31-3, 36-7, 46, 68, 123,
146, 150; national 150
- Independent, The* 165
- index 114; consumer price 65-6; wages
70
- indicators 79; target 114
- inflation 18, 21-3, 31, 36-7, 39-40, 52-3,
65, 67, 69, 82, 95, 97, 101-5, 110-15,
117-8, 120-1, 125-7, 130, 132-4, 136,
147, 157-8, 160, 165, 167, 172-4, 176-
81; headline 115, 133; price 113; wage
113, 124
- infrastructure 149
- institutions 172; social 102
- interest 157
- interest rates 16, 20, 27-9, 30-1, 36-7,
41-2, 46, 52-5, 57, 59, 60-1, 64, 67-8,
95-6, 103, 106-7, 109, 115, 119, 121,
123-6, 145, 147, 157, 159-60, 162,
166, 174, 178-81, 190;
- parity 19, 54
- International Labour Organisation 138-
9
- intervention: government 169
- inventories 57

- investment 20, 25, 27-8, 52, 57, 62, 66, 68-73, 75, 91, 95, 102, 105, 107-8, 117, 123, 144, 149-50, 158-9, 161, 168-9, 174-5; gross fixed 68-70; inward direct 118;
- public 167; strategic decisions 18
- investors 64
- IS-LM models 24-5, 27-30, 91-2, 155
- Jasanoff, Sheila 4, 12
- Jerome Levy Institute 15; *see also* Godley, Wynne
- jobs 132, 138, 165, 172
- judgement 17, 51, 60, 69, 75-7, 89, 101, 153, 168, 176, 181
- July Report 137, 164; *see also* Panel of Independent Forecasters
- Keating, Giles 24, 30-45, 51, 53, 55, 57-8
- Keynes, John Maynard 13, 28, 84, 92
- Keynesianism 3, 17, 19, 155
- Klein, Lawrence 13
- labour 19; costs 138; force 130, 134, 139-40, 143; mobility 129
- Labour Force Survey* 138-9
- labour market 7, 19, 102-3, 112, 120, 129-30, 132-7, 141-3, 147, 157, 169, 174, 178, 188; participation 143; turnover 143; turnover rates 134
- lag 180; structure 84, 90
- Lamont, Norman 165; *see also* Chancellor of the Exchequer
- Layard, Richard 112, 129; *see also* London School of Economics
- LBS *see* London Business School
- Leamer, Ed 14
- liabilities 118
- Liverpool, University of 15, 92, 96-7, 112; Macroeconomic Research Group 17-23, 125, 141, 180; *see also* Minford, Patrick
- Lombard Street Research Ltd. 15, 17-18, 113, 124, 178
- London Business School 13-23, 81-3, 121, 175-6; *see also* Currie, David
- London School of Economics 112; *see also* Layard, Richard
- LSE *see* London School of Economics
- Lucas, Robert 14, 84-6; critique 14, 84, 85
- MacKenzie, Donald 9
- Macromodelling Bureau 6, 15, 21, 80-1, 188; *see also* Economic and Social Research Council; *see also* Warwick, University of
- Manchester, University of 6; *see also* Ormerod, Paul
- manufacturing industry 107-8, 133, 190
- market forces 135-6
- markets 109, 124, 165; European 110; financial 157; flexibility of 181; international 102; world 148
- McCloskey, D. N. 99
- Merton, Robert 10
- methodologies 85-6, 89, 188
- miners strike 22
- Minford, Patrick 15, 16, 19, 68, 74, 80-2, 85-6, 92-7, 99, 110-14, 116-120, 124-6, 132-7, 141-4 146-8. 150-1, 160-2, 166, 168, 180-1, 185; *see also* Cardiff Business School; *see also* Liverpool, University of (Macroeconomic Research Group); *see also* Panel of Independent Forecasters
- models 2-20, 23-4, 30-1, 35, 43-4, 48-61, 64, 67, 69, 77-129, 134, 137, 152, 154, 168, 170, 172-90;
- classical 137; computer 188;
- hysteresis *see* Layard-Nickell;
- multi-equation 79; Layard-Nickell 112, 129, 137; new classical 19; sensitivity 84;
- Treasury 90; vector autoregressive 14, 48, 59-60, 76-7;
- modellers 5, 6, 24, 51, 58, 68, 75-6, 80-4, 87, 89-90, 92, 97-100
- modelling 51-8, 83-4, 99-100, 119, 172-3
- monetarism 3, 18, 97, 113, 115, 119, 132, 157, 180; New Classical 3
- Monetary Policy Committee 4-5, 16, 190
- money: demand 28, 53;

- market 27, 29;
supply 28-9
mortgages 172; interest rates 65-6, 114;
interest relief 160, 166;
payers 190
MPC *see* Monetary Policy Committee
- national income accounts 6, 24-7, 102,
104, 116, 175
National Institute of Economic and
Social Research 6-23, 80-2, 87, 98,
106, 108, 115, 121-2, 139, 151, 159,
175-7; *see also* Britton, Andrew; *see
also* Ormerod, Paul; *see also* Weale,
Martin
neo-Keynesianism 3
New Cambridge economics 104
Nickell, Steve 112, 129, 114; *see also*
Oxford, University of
NIESR *see* National Institute of
Economic and Social Research
- October 1993 Report 153-71 *see also*
Panel of Independent Forecasters
Organisation for Economic Co-
operation and Development 109, 124
Ormerod, Paul 6, 80, 86, 90, 92, 94;
see also Henley Centre for Forecasting; *see
also* Manchester, University of; *see also*
National Institute of Economic and
Social Research
orthodoxy 182-9
output 18, 22-3, 92, 97, 102, 105, 110,
113, 115-6, 119-121, 124-5, 135, 147-
8, 161, 174-5, 177, 190; gap 18, 110,
113-4, 119, 124-7, 178; growth 22
outturn 44, 97, 99, 172-3, 175
Oxford, University of 15, 112; *see also*
Nickell, Steve
- Panel of Independent Forecasters 2-10,
15-18, 50, 53, 55, 58, 60, 62, 64-75,
100-90; *see also* Barker, Kate; *see also*
Britton, Andrew; *see also* Currie,
David; *see also* Congdon, Tim; *see also*
Davies, Gavyn; *see also* Godley,
Wynne; *see also* Minford, Patrick; *see
also* Rosewall, Bridget; *see also*
Sentance, Andrew; *see also* Weale,
Martin
parliament 162
performance 93, 102, 158; export 149;
foreign trade 146; net trade 149
perspectives: sociological 77, 102, 190
Phillips Curve 27, 92, 103
Philosophical Investigation 9; *see*
Wittgenstein planners 187
plans: spending 119
policy 3, 52, 76, 83-5, 88, 90, 94, 100,
103, 116, 124, 127, 131, 133, 135,
143-5, 148, 151-4, 156-8, 161, 163,
165, 167-9, 171-90; advice 100-2,
184;
agenda 171; anti-inflation 66;
choices 182-9; consensus 154, 161-3;
decisions 126, 187; deflationary 111,
115, 117, 147;
fiscal 146-7, 156-7, 159, 162, 168;
framework 188; goals 103;
government 75, 112, 143;
incomes 115; interventionist 4;
labour 131, 135; makers 3, 77, 100, 102,
104-5, 110, 115, 119-20, 124, 127-8,
152-3, 172, 182-9; making 12, 75,
101, 119, 129, 171, 182, 188; mistakes
136;
monetary 59, 61, 65, 122, 126, 133, 135,
155, 157, 159, 166, 181; options 143-
50;
public 119, 169; recommendations 17,
105 152, 154-65, 182, 185-6; social
135; supply-push 131; targets 189; tax
raising 57
Popper, Karl 135
pound *see* sterling
predictions 93; strategic 142
pressures: political 140; social 140, 163-5
prices 18-19, 36, 52, 58, 60, 64-9, 72-3,
76, 102-3, 112-13, 125, 130-2, 159,
175; domestic 53-4, 57; house 64;
indices 60, 64, 66;
world 53-5, 57
production 54, 57
Production and Use of Economic Forecasts
30; *see also* Keating, Giles

- productivity 91, 112, 125, 135, 151;
 industrial 133
 profit margins 109
 profitability 91
 projections 67-8, 75, 98, 143, 159;
 exogenous 82
 property: boom 107; commercial 108
 PSBR *see* Public Sector Borrowing
 Requirement
 public 188; sector 90, 178
 Public Sector Borrowing Requirement 7,
 19, 52, 55, 60-61, 116-9, 121, 128,
 144-7, 149-150, 155-61, 165-70
 purchasing power 19; parity 19
- R&D *see* research and development
 ratios: debt 175
 recession 21, 46, 64-5, 69, 96-7, 103,
 107-11, 113, 116, 120-5, 129, 133,
 136-7, 140, 144, 175, 177, 181, 190
 recommendations 4, 101, 105, 127, 151,
 153-4, 157-8, 168, 170, 182
 recovery 15, 64-5, 67, 71, 106, 108, 111,
 115-7, 119, 121-2, 124, 126-7, 141-2,
 145-7, 150-1, 155, 157, 159-61, 165-
 6, 174-5, 180-1
 recruitment 180
 redundancies 142
 reforms: fiscal 161, 167; labour market
 158; spending 155-6, 160-1, 168; tax
 155-6, 160-1, 168
 regression equations 6, 24, 30-42, 52-3,
 77-8, 83, 88, 93
 relationships 31, 43, 83, 88, 90, 95-6,
 101-5, 115, 134, 184; budget deficit,
 net trade and private sector 104;
 growth and inflation 102-3;
 inflation and employment 103-4
 replication 31-5, 88-9
 research 23, 188
 research and development 149-51, 158,
 167, 169
 reserves: contingency 167
 residuals 177
 resources 148-50; distribution 150
 restart scheme 112
 restraint 144; pay 178
- Retail Price Index 65-6, 133, 177;
 headline figure 177
 revenue 145, 158; tax 160
 Rosewall, Bridget 16; *see also* Business
 Strategies Ltd; *see also* Panel of
 Independent Forecasters
 RPI *see* Retail Price Index
- salaries: *see* wages
 sample periods 84, 95-6
 savings 25, 28, 30, 39, 41, 57, 66, 68, 123,
 146
 science in policy making 3; analytical
 critique 11-12
 science studies 87-9, 96, 99-100, 172- 90;
 sociological analysis of 182-3
 scientific knowledge: sociology of 1-23
 scientists 96, 128, 153, 187, 189
 sectors 18; corporate 106; household 106,
 123; non-bank 179; private 101-2,
 104-5, 115, 117, 146, 176; public 101-
 2 self-employed 143
 Sentance, Andrew 16, 107-111, 116, 119-
 20, 122, 127, 130-1, 134, 145, 149,
 151, 158-9, 161-2, 164, 167-8; *see also*
 Confederation of British Industry; *see*
 also Panel of Independent Forecasters
 services *see* goods and services
 Seven Wise Men *see* Panel of
 Independent Forecasters
 Sheffield, University of 15
 Sims, Christopher 14
 simulation 44; ex post 44, 46
 skills 88; levels 151;
 shortage 136
 social: change 83-93; issues 5;
 practices 77;
 system 35, 83
 Social Science Research Council 13, 15,
 18, 20
 social welfare 190
 sociologists 186, 189; organisational 75
 sociology of scientific knowledge 2, 3, 8,
 10-11, 31, 76, 138
 Southampton, University of 14-15
 spending 103, 151-2, 158, 167, 175;
 business 70; cuts 149, 151;
 domestic 119; government 151;

- limits 165; public 126, 144, 152, 155, 167; public programmes 160; targets 150
- spiral: wage/price 114
- SSK *see* sociology of scientific knowledge
- SSRC *see* Social Science Research Council
- 'stagflation' 14
- stagnation 161
- statistics 13, 25, 30, 37, 42, 77-8, 80-1, 83, 88, 96, 99, 142; aberration 129;
- significance 24, 31-2, 34-43; t-statistics 24-49; technique 84;
- uncertainty 77-83, 110
- sterling 57, 64, 67, 109, 113-4, 118, 121-2, 157, 174; crisis 147
- stochastic: nature 94; relationships 86;
- simulation 79-81
- stock exchange 176
- stockbuilding 20, 61, 70-1, 92, 175
- strategy 3, 5, 164; ad hoc 95; economic 5; marketing 57; political 3;
- social 3;
- supply 19, 86, 90 112, 135, 141, 158, 167, 178, 181; aggregate 91
- surplus 102, 104-5, 115-6, 123, 146, 176
- private sector 117-18
- surveys: business 140; *see also*
- Confederation of British Industry
- sustainability 155; fiscal 162, 166, 168, 182; growth 147
- tax 19, 25, 29-30, 103, 112, 116-7, 126, 136, 144, 146, 150-2, 155-6, 158-9, 165-7, 169; air
- travel 166; direct 166; increases 144-7, 149-50; indirect 57, 159;
- insurance 166; revenue 119-20;
- system 167; value added 160
- technology 3, 8, 75, 88, 187, 189
- testing 6, 30-49, 182
- Thatcher, Margaret 22, 96
- theories 90, 94, 113, 128, 154, 164, 174, 182, 184, 190; moral 189;
- political 189; social 189
- Times, The* 165
- TINA 182-9
- Tinbergen, Jan 13
- TRACY 182-9
- trade 175; balance 20, 52; deficit 7, 102, 118-20, 143-50; gap 118;
- net 71-2, 74, 102, 104, 108-10, 118-19, 147, 174-5; world 74
- trade union 75, 102, 112, 129-30, 136;
- laws 112; membership 131;
- power 135; representatives 153
- training 131, 136, 139, 149-51, 158, 167, 169; agencies 153;
- training programmes 131; government 135
- Treasury: Committee 190; Select Committee 16 *see also* H M Treasury
- trends 59, 67, 75, 107, 124-5, 127, 140; rate 115
- turnover 136
- twin deficit 147-8, 157, 167-70
- unemployment 7, 19-23, 27, 52, 59, 65, 67-70, 97, 102-4, 110-5, 117, 119-21, 124-43, 147-8, 150-1, 174, 176, 178-80, 189;
- benefit 19, 112, 129-30, 139, 143; long-term 130-1, 134-5;
- non-accelerating rate of 103
- United States of America 109-10, 123, 126
- unwaged 189
- upturns 141-3
- US: *see* United States of America
- values 55, 58, 189; added 45, 81; past 52
- variables 17, 34-40, 44-9, 52-3, 55, 57-8, 62, 79-80, 84, 88, 101-2, 120, 132;
- endogenous 44, 55, 57;
- exogenous 54-5, 57, 64, 68, 75, 80-1, 98;
- square root of 44
- variance 89, 94
- VAT: *see* tax, value added
- Vaughan, Lisa 165-6
- wages 25, 27, 57-8, 65, 67-70, 73, 112, 114-5, 129-30, 133, 135, 137, 141, 151; behaviour 177;
- claims 103; demands 114;
- increases 103; pressure 115, 136;
- settlements 115, 133

- Wallis, Ken 6, 22, 46, 80-1, 87, 89, 94;
see also Macromodelling Bureau; see
also Warwick, University of
- Warwick, University of 6, 15, 21; see also
Macromodelling Bureau; see also
Wallis, Ken
- Weale, Martin 16; see also National
Institute of Economic and Social
Research
- wealth 19, 95, 131
- weight 58, 85, 121, 126 177, 186
- welfare state 102
- Whitley, J. 22, 46
- Winch, P. 8
- Wittgenstein 8; see *Philosophical
Investigations*
- worktest 112
- Wynne, Bryan 12