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MACROECONOMIC POLICY DESIGN AND CONTROL THEORY -  
A FAILED PARTNERSHIP?

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ABSTRACT

This paper provides a general review of recent developments in the application of control theory to macroeconomic policy design. It starts by highlighting the crucial difference between the engineering and economic control problem, in that the latter, unlike the former, is concerned with the control of intelligent systems. It examines the problems that this creates for macroeconomic policy formulation, and considers the usefulness of the rational expectations assumption in this context. Policy formulation is then analysed in the context of an interdependent world, and the dangers of single country policy optimisation are highlighted. It is argued that the most pressing issue is that of formulating appropriate rules, supported by penalty strategies, for the orderly conduct of macroeconomic policy in an international setting. The paper concludes by examining the conduct of UK macropolicy over the past five years, arguing that greater attention to issues of macropolicy design would have avoided some serious deficiencies in the conduct of policy.

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## NON-TECHNICAL SUMMARY

In this paper, I seek to provide a broad survey of the current state of the relationship between control theory and macroeconomic policy formulation, paying particular attention to recent work on rational expectations, game theory and control. I start by observing that, until recently, applications of control theory to economics have been based on the analogy between engineering systems and economic systems: this analogy permitted the wholesale transfer of established bodies of control theory to economics.

However, this analogy is now appreciated to be ill-founded: in economics, the systems to be controlled reflect on and react to policy in an intelligent, non-mechanical manner. Because of this, it is not meaningful to analyse in isolation the consequence of a given change in an instrument on the economy without specifying the policy rule from which the change derives. Thus, for example, the announcement of an unexpectedly large money supply increase may cause an exchange rate depreciation if the change is expected to be permanent; but an appreciation if the authorities are seeking to target the money supply, and are expected to react to the money supply increase by raising interest rates.

The assumption of consistent or rational expectations on the part of the private sector provides the most straightforward way of incorporating intelligent behaviour within economic analysis. This assumption may be justified on the grounds that agents have available to them published forecasts of reasonable quality or on the grounds that a general class of adaptive behavioural strategies for learning converge in a stable world on the consistent solution. In the field of policy design, consistent expectations provide an important test-bed for policy evaluation: if policies perform badly when agents understand their consequences, they are likely also to be undesirable where agents are less informed, for they could succeed only through agents'

mistakes.

The early genre of rational expectations models rendered superfluous stabilisation by government, for they gave the private sector effective control. But more recent models emphasise the constraints and coordination problems within the private sector, and give ample scope for stabilisation by government. But the design of policy is greatly complicated by the problem of time inconsistency, whereby government has a short run incentive as time proceeds to renege on previous policy commitments. I argue that this problem may be overstated, particularly for governments that are not too myopic. For the costs of renegeing on optimal time-inconsistent policies may be a loss of credibility and the adoption of a time-consistent policy with a very much worse performance. These ideas are being formalised in terms of the emerging literature on reputations and on the design of threat strategies to support a given policy rule.

Rational expectations introduces a crucial game-theoretic element into macroeconomic policy formulation. But this becomes most crucial in the international sphere where interdependence between countries means that policy formulation must be seen in terms of a game with many players. I emphasise the dangers of problems of the prisoners' dilemma type arising from optimisation for the single country which ignores interdependencies with other countries. In particular, recent work has shown the incentives for countries to rely excessively on exchange rate appreciation to control domestic prices, while using expansionary fiscal policy to sustain output. While such policies may prove advantageous for the single country, they may well prove harmful if implemented by all countries. This highlights the need for substantial work on the appropriate type of cooperative policy rules to be adopted in the international sphere, and the penalty rules required to sustain cooperative behaviour. I discuss the growing literature on repeated games of the prisoners' dilemma type, and suggest the lesson of this literature is that we should not over-emphasise the problems of sustaining cooperative

behaviour if governments are not too myopic.

I conclude by examining the conduct of recent UK macropolicy, and suggest that the Medium Term Financial Strategy (MTFS) contained technical flaws that might have been avoided had greater attention been paid to the literature on policy design. In particular, forward looking behaviour in the foreign exchange market generated a marked sterling appreciation as a result of anticipated high real interest rates; and thereby converted a gradualist strategy of disinflation into a much more sudden process. Moreover, the emphasis on the Public Sector Borrowing Requirement as an intermediate target meant that automatic fiscal stabilisers were turned off, adding to the volatility of the economy in the face of demand disturbances. Moreover, the over-emphasis on monetary targets has led to a consequent neglect of important external considerations, notably the exchange rate. I argue that these dangers were known at the inception of the MTFS, so that these are not criticisms with hindsight. I conclude by calling for greater partnership between the practical and more theoretical arms of policy formulation, but I remain pessimistic about the prospects of realising this aim.

My subject this evening is the applicability of control theory to the pressing questions of macroeconomic policy design that face us, both domestically and in the international arena. This is an issue particularly appropriate for a lecture here at this College; for the Queen Mary College based Programme of Research into Econometric Methods, directed by Maurice Peston and others, was concerned with control and policy formulation in the 1970's; while the current Programme of Research into Small Macromodels (PRISM for short) is making important contributions to this area, and it is on much of that work that I draw this evening. I approach the issue with some trepidation, conscious that I am an amateur in control and policy matters relative to this audience which includes many distinguished theorists and experienced Whitehall practitioners. I am also mindful of the dangers of claiming too much in the field of policy design. Those were nicely stated in a broader context by that eminent biologist, Sir Peter Medawar, when he wrote:

"It is not their wrongness so much as their pretensions to rightness that have brought economic predictions and the theory that underlies them into well-deserved contempt. The dogmatic self-assurance and the asseverative confidence of economists are additional causes of grievance - self-defeating traits among people eager to pass for scientists."<sup>(1)</sup>

I have no wish to pass myself off as a scientist this evening, but I do hope that I shall not be guilty of dogmatism and unreasonable pretensions in arguing my main theme tonight - that we have made important advances in the application of control theory to macroeconomic policy design, which are yielding important insights and should continue to do so.

The control theory that we have mostly applied in economics has been taken over from engineering. This theory tells us how best to control a physical system (a space-ship, for example) in order to achieve certain objectives (to reach and return from the moon) by appropriate adjustment of the instruments of control (the rockets and stabilisers of the space ship), subject to the physical laws constraining the behaviour of the system. These control techniques have been applied with considerable success in a wide range of physical and engineering applications - the control of chemical process

plants, electronic systems, rockets, to name just three. The problem of control in economics is often posed in similar terms. Thus, in macroeconomics, for example, policymakers may wish to ensure that the economy progresses without undue fluctuations in output, unemployment, prices and other key variables, along a course that yields the most prosperous outcome for society, by suitable adjustment of instruments - interest rates, taxes, government spending and so on - subject to the constraints imposed by the structure of the macroeconomy, as expressed in a suitable macroeconomic model. The essential problem - the design of control rules for the instruments that best meet the preferences of the controller given the structure of the system to be controlled - appears to be common with that already solved in the field of engineering. With techniques available from the engineering literature it is not surprising that economists took these over and applied them to the design of macroeconomic policy.

Control methods entered economics quite early on, most notably through the work of Bill Phillips at the London School of Economics in the early 1950s.<sup>(2)</sup> But I think it is fair to say that their influence was only fully felt in the 1970s, when computational techniques caught up and the theory of control entered the tool-kit, albeit the specialised one, of economists. In the UK, the QMC based Programme of Research into Econometric Methods, to which I have referred, was important in hastening this process. By 1976, matters had developed so far that an official committee was established under the chairmanship of Professor Jim Ball, to investigate the usefulness of optimisation techniques for policy formulation.

The report of that Committee gave a cautious welcome to control methods as an additional weapon in the armoury of policy formulation.<sup>(3)</sup> It gave little support to those - and I regret there were some - who naively conveyed the impression that control techniques were the answer to policy-makers' dreams, that all one needed to do was to take the Treasury model, apply control methods, and after a process in which the preferences of policy makers would be established, the budget could be read straight from the computer printout.

Now there are a variety of reasons why the use of control theory in economics is likely to be less straightforward than in engineering applications, and some of these I shall touch on later. But there is one in particular which I wish to focus on this evening. It is distinctive to economics, and social systems more generally. Essentially it arises from the fact that the analogy between engineering and economic systems which inspired much of the early work on control theory in economics is, quite simply, misleading. Economic systems, like all systems of social interaction, are intelligent in a way that engineering systems are not. In economics, we are concerned with the control of intelligent systems that think about what policy-makers do to them and act accordingly.

This distinction is so obviously important once it is stated that it is surprising how little it figures in the report of the Ball Committee, which appeared just six years ago in 1978. That reflects not on the quality of the report, but rather on the state of thinking about these issues within the profession at the time. It is this feature of economic systems - that the components of the economic system that we wish to control reflect upon and react to economic policy in an intelligent, non-mechanical manner - that has made us rethink our approach to macropolicy making. In the process, the subject has become much richer, greatly exciting, and, I believe, more useful. I want to try to give you a flavour of the subject, of the issues with which we have been grappling and our attempts to resolve them, in the rest of this lecture. Unfortunately it can only be a flavour, for it is a subject that can properly be expressed only in rather forbidding mathematics.

Let me give two simple examples of how the control problem is complicated by intelligent private sector behaviour. My examples concern the two main instruments of policy - fiscal and monetary. My first is the use of tax changes - fiscal policy - to control the economy. This was the standard fare of the annual budget, at least until a recent fetish with the Public Sector Borrowing Requirement overthrew all that. The standard view was that, to raise the level of demand and output and reduce unemployment, the Chancellor should cut taxes. Conversely to reduce overheating -



inflationary pressures - he should raise taxes. This policy will indeed work, at least in the short to medium run, in all the current main macromodels; and it operates by regulating the level of aggregate consumption in the economy. Now at the same time, our theories of consumption emphasise the wish of consumers to spread out unevenness in their flow of income so as to achieve a rather smoother pattern of consumption. If we take this permanent income or life cycle view of consumption as it has come to be known, and I think the evidence supports it, we are forced to think with greater care about the consequences of tax changes. In particular, we must think about whether the tax change is permanent or temporary. A cut in income tax that is perceived to be permanent will increase the future stream of income, and lead to a rise in consumption. But a temporary cut has very limited effects on life-time earnings, and consumers are likely to spread out its benefits over a considerable time. Hence a transitory cut in income tax may do very little to affect consumption. But if the object of the tax cut is to offset a temporary fall in aggregate demand, this argument suggests that a cut in income tax, necessarily temporary, will not be effective. To engineer a temporary boost to consumption, the Chancellor needs an alternative: thus a cut in indirect taxes, which lowers prices temporarily, will make it advantageous to buy now rather than later. This example illustrates the point that the effects of a tax cut depend very much on whether consumers see it as temporary or permanent, and this depends in turn on what policy is perceived to lie behind the tax cut.

My second example concerns monetary policy and the exchange rate. Suppose one is asked what effect an increase in the money supply has on the exchange rate - a simple straightforward question to which one might think a simple answer should be possible, if we have our wits about us. But I have at least two contradictory views to offer. On one view, a larger money supply implies higher domestic prices, so that a depreciation of the exchange rate will occur, maintaining the relationship between domestic and foreign prices when expressed in a common currency.<sup>(4)</sup> Indeed the overshooting hypothesis suggests that, if prices adjust slowly to the money supply change, the exchange rate

depreciation may be exaggerated, overshooting its final equilibrium and then appreciating back.<sup>(5)</sup> But on an alternative view, the money supply increase will cause an exchange rate appreciation. This is because the foreign exchange market expects the Bank of England to raise interest rates to dampen monetary growth and bring the money supply back onto target.<sup>(6)</sup>

This is the sort of contradiction that gives economists a bad press. But it is apparent, not real, for the arguments apply to quite different circumstances. The first argument rests on the assumption that changes in the money supply tend not to be reversed - the money supply, for example, follows a random walk. In the second, monetary changes are reversed, and the authorities pursue a policy of monetary targets. Two distinct policies underly the two cases. What my argument illustrates is not that economists can never agree, but rather that to ask how the foreign exchange market reacts to a money supply change is not a well-defined question. The answer to the question depends crucially on how the market expects the authorities to react. Once again the behaviour of the system depends on the perceived policy or control rule of the authorities. In designing policy, we must take account of this dependence.

This is a very general proposition that applies in almost every area of macroeconomics. Its consequences are pervasive. It blurs the usual distinction between technical advice on "how the economy works" on the one hand and policy advice on the other. One cannot discuss how the economy works without first specifying a background of what policy is in force; and it is then a very natural question to lead on to ask how changes in that policy influence the behaviour of the system.

I hope by now that I have persuaded you that there is an extra complication, a new dimension, to the control problem in economics. As in engineering applications, the design of the control rule depends on the structure of the system to be controlled; but unlike engineering, the structure of the system depends on what control rule is perceived to be in force.

To get a grip on this problem, it is clear that we must model the way in which people

form their expectations about the future. We therefore need a theory of the economy in which people's expectations about the future enter as explanatory variables; and we need a theory (or theories) of how people think about economic events and form expectations of the future. The natural way to progress is, of course, to use direct observations of expectations drawn from surveys. Unfortunately such data are rather sparse, available only for relatively short time periods, and usually relate only to the very short term future, not the one, two or five years or longer that are all too often relevant for economic decisions. Cross-sectional panel data on expectations would be an invaluable aid to research in this area. It is therefore regrettable that we are not actively seeking new sources of expectations data - instead Rayner's axing of the general database available to economists offers little encouragement in this.

In the absence of satisfactory expectations data, economists have proceeded in one of two ways. The first is to assume that expectations are formed by some rule of extrapolation, more or less complicated, based on the past. Thus inflation may be forecast from observation of past inflation using some type of adaptive or Box-Jenkins forecasting procedure. Conventional methods of this kind may serve quite well for forecasting a variable such as inflation or output over a one or two year period provided present developments are not very different from the past. But it serves rather badly if circumstances change markedly - for example if the price of oil alters dramatically as in 1973 and 1978, or if policy shifts abruptly as in 1979/80. Moreover, it is most unlikely to perform well in financial or foreign exchange markets, where conventional rules of thumb cannot be expected to produce profits.

The second approach is what has come to be known as "rational or consistent expectations".<sup>(7)</sup> This cuts through the problem of having two distinct models - one for how the economy works and the other for how people form expectations - by assuming that people form their expectations on the basis of knowledge of the true structure of the economy. Thus when testing an economic theory, we also assume that people's expectations are formed consistently with that same theory, and the quantitative

estimates of the model used in forming expectations coincide with those of the model itself. If the theory is correct, this amounts to assuming that expectations are optimal predictors, in the sense of being unbiased and having minimum variance.

Now the assumption that people form their expectations rationally is rather breathtaking in its range, and may remind you of Sir Peter Medawar's dictum that I cited at the start of this lecture. After all, the experiments of psychologists reveal that most of us, present company excepted of course, are ignorant of even the most elementary principle of statistical inference. We find it particularly difficult, so the work of Kahnemann, Tversky and other experimental psychologists indicates, to make rational decisions where the problem is too open-ended.<sup>(8)</sup> In so far as they admit the term "rational", psychologists typically characterise our mode of reasoning as taking the form of bounded rationality. Whether consciously or unconsciously, people limit the range of options to be considered at any particular time. We all, no doubt, can give instances of such behaviour, of course in others, never ourselves. Inevitably, this form of decision-making can be severely sub-optimal, particularly when having to cope with new circumstances that need novel decisions.

Despite this, I hope you will not dismiss the assumption of rational forecasts too easily. One way in which each of us can come to terms with our lack of even elementary skills in forecasting is to draw on outside advisers. A number of groups forecast key macroeconomic variables regularly, and these are available for only the price of a newspaper. Of course, such forecasts have a bad name - I recall, in particular, Sir John Mason's comparison, in his British Association Presidential address last year, between economic forecasting and weather forecasting.<sup>(9)</sup> Sir John spoke of weather forecasting from the informed position of a former Director of the Met Office. But he overstated, I think, the inaccuracy of macroeconomic forecasts, which have indeed a reasonable success rate.<sup>(10)</sup> Provided that their models are well estimated, and provided the models are allowed to influence the forecasts (both of which conditions I emphasise for they

are by no means always met), those who use these forecasts may well behave as though they are making careful forecasts, even though they have no knowledge of economics and statistics. Of course, the extent to which people draw on macro forecasts depends not only on their accuracy, but also on the costs of being wrong, and one might well expect these to be greater in financial markets than other markets. It is no surprise to find, therefore, that the assumption of rational expectations seems to work much better in financial and foreign exchange markets than elsewhere.

My second defence of the assumption of rationality in forecasting, and decision-making more generally, is that it may be a reasonable aggregate approximation. Even if individuals are subject to bounded rationality, divergences from full rationality at the individual level may cancel out in the aggregate, so that aggregate behaviour is such that it is as if individuals can forecast rationally.

This idea has been developed formally in the work of John Cross on adaptive strategies of behaviour.<sup>(11)</sup> He demonstrates that in a stable environment, adaptive behaviour, based on repeating more frequently those successful strategies and shifting away from those unsuccessful ones, leads to a distribution of individuals around the rational outcome in a wide class of problems. Suboptimal strategies persist, though, because, in a large population, some ill-conceived strategies will pay-off and some well-conceived strategies fail in any particular experiment; so that non-rational strategies can survive. This view is helpful since it means that we do not have to accept that each and every one of us forms our expectations rationally in order to apply the theory in the aggregate. It also emphasises the need to understand processes of learning, and the importance of the flourishing literature that is currently integrating the analysis of learning and expectations, as well as relaxing the extreme informational assumptions that underlay the early analysis of behaviour under rational expectations.<sup>(12)</sup>

There is, I think, an even more compelling reason for adopting the assumption of rational expectations in policy appraisal, though in this context the term consistent expectations is probably better. In assessing policies under consistent expectations, one is testing them under conditions where their effects are understood. I submit that a good

performance under these conditions is a necessary condition for a satisfactory policy. For if a policy performs badly under these circumstances, but well under different ones, it can only be because it works through systematic forecasting errors by the private sector. But since there will be an incentive for the private sector (or its forecasting agents) to alter its forecasting method if it generates systematic error, this is a rather weak and vulnerable basis for policy. A policy that performs badly when its effects are understood must be unsatisfactory. To be sure, we must be sensitive to our uncertainty about model structure, but this can be handled within the framework of consistent expectations by methods of robustness testing to check that our policy advice is not too model dependent.

I hope that I have persuaded you that the notion of rational or consistent expectations is useful in the control setting. There is no doubt that it has revolutionised our way of thinking about macroeconomics. This is because, notwithstanding the severe technical problems it poses for analytical and empirical macroeconomics, it provides the simplest possible way of incorporating intelligent behaviour on the part of the private sector, and this is essential for most policy problems in macroeconomics. But initially rational expectations were thought to be destructive for the use of control theory in to economics. This was for two quite separate reasons. The first has a long pedigree stretching back to Adam Smith's invisible hand - the notion that perfect markets and intelligent individuals will lead to an optimal outcome, without any need for government intervention. Essentially the private sector can act as controller, making government control superfluous. This appeared in the early work on rational expectations in the form of a class of models in which government could influence the real economy only by fooling the private sector.<sup>(13)</sup> Since rational expectations imply that such fooling cannot be systematic, government could only add extra unsystematic noise to the system, not assist in stabilisation. To be sure, one might object that government may be better informed than the private sector - the Civil Service would hardly be doing its job otherwise - but then the answer is to make public that better information. Stabilisation policy becomes the province of the Press Office, not the Treasury.

But this argument will not wash. It requires us to believe that the problems of coordination and adjustment within the private sector are negligible, so that each of us can enter into flexible contracts for wages, financial transactions and other dealings, with clauses contingent upon each possible macroeconomic state of the world.<sup>(14)</sup> Just imagine how long the negotiations between Ian McGregor and Arthur Scargill would last in that sort of world. Consider the earnings that this would give to lawyers in drawing up complicated contracts, and I am sure you will agree that such a world would be severely suboptimal. We all of us resent government for some reason or other, just as we curse traffic lights, particularly when they go wrong. But how many of us would really welcome the abolition of traffic lights as the resolution of our traffic problems?

But although consistent expectations do not dispose of the control problem they greatly complicate and enrich it. This is because in order to assess the performance of control rules we must take into account their effect on the way the private sector forecasts and behaves.<sup>(15)</sup> There is, moreover, an extra dimension to the control problem. For different control rules alter the informational content of variables such as interest rates and the exchange rate.<sup>(16)</sup> Intelligent agents make inferences about disturbances to the system from observations of such variables, and their behaviour will depend on these inferences. Policy may operate through these informational channels, as well as through more conventional control mechanisms. For those of a theoretical bent of mind, there is also the possible need for government to intervene to avoid forms of structural instability and chaotic motion that may arise in rational expectations models.<sup>(17)</sup>

The second objection to control with rational expectations was equally erroneous, but highlighted an important problem in the control of intelligent systems which has come to be known as time-inconsistency.<sup>(18)</sup> It is a quite general phenomenon in social decision-making, and will be familiar to those who have decisions imposed upon them, even if decision-makers themselves tend to have a blind spot for it. I shall refer to government in what follows, but you may equally well think of management, head of

department, examiner, dean, governor - whatever you will.

Consider how government should respond to an inflationary disturbance, such as a surge in wages. Before the event, a government concerned with inflation should give the impression that it will not accommodate the wage increase by maintaining aggregate demand and output. For by so threatening that wage rises will result in loss of jobs and unemployment, it may persuade those bargaining over wages to limit their demands. This notion of a threat effect undoubtedly underlies current policy, and is also a plank, though only one of several, in the New-Keynesian policies advocated by James Meade.<sup>(19)</sup>

Before the wage push, therefore, government should be hawkish. After the event, however, the threat effect has served its purpose. Bygones being bygones, the government should consider how best to respond to the wage surge. Under quite general conditions, the optimal response is partially to accommodate the wage surge, so that the effects on unemployment are mitigated, though at the expense of a rise in the price level. The optimal response, it seems then, is to renege on the initial hawkish stance.

It is paradoxical that this problem was first analysed in a two period setting, where, correctly analysed, the problem disappears. For the private sector, if it is bright enough, can figure out that the government will renege, for government has no reason not to do so. The private sector will, therefore, entirely discount the government's hawkish stance. You may be aware of similar chains of reasoning that prove a rational government could not use the UK nuclear deterrent.<sup>(20)</sup> The only way the government can retain any credible threat in this situation is by appearing irrational in its decision making.

With nuclear war, the world as we know it may end if the threat is called; but fortunately the effects of a wage surge are less drastic. It is therefore much more natural to consider the problem as a continuing game, not in the restricted two period setting. This simplifies matters. For it is clear that government cannot go on reneging on its commitments and continue to retain its credibility. Yet it is the credibility of government announcements that gives them their usefulness for control purposes.

There is, therefore, a choice between two types of policy. The first type, called



time consistent policies, are where the government has no credibility. The private sector assumes that the government will succumb to short run temptation, and calculates its expectations of the future on that basis. Government then formulates policy subject to this lack of credibility. Thus in my example the private sector calculates that the government will accommodate wage surges, and ignores all government pronouncements to the contrary. In the stylised history of the postwar period, it is suggested that such was the pre-Thatcher state - before the Flood. Given that no one believes what it says, the best that the government can then do is, indeed, to accommodate.

The second type of policy is time-inconsistent. Government has credibility, because it does not succumb to any short run incentive to renege. Because of this, government policy does influence private sector behaviour through the private sector's expectations of the future, as well as through more conventional control mechanisms. In our example, a policy of not accommodating wage surges, despite the short run temptation so to do, is time-inconsistent.

The view is sometimes expressed that time-inconsistent policies are not feasible, because government cannot credibly commit itself not to succumb to temptation. I find this hard to accept. It is clear that a government with credibility has a wider range of policy options available to it than if it lacked credibility; and it can therefore expect a better policy performance.<sup>(21)</sup> The cost of renegeing on a well-chosen time-inconsistent policy is that you end up without credibility with a time-consistent policy yielding worse results. Some of the QMC PRISM Group's results suggest that this cost can be very high.<sup>(22)</sup> A government that is concerned with more than the very short term has a real incentive to adhere to time-inconsistent policies, and is therefore credible in so doing.

Those of you who feel that the chain of reasoning has now become tortuously theoretical, a feature of much of the game theoretic reasoning of this kind, may be reassured by a more down-to-earth argument. Mrs Thatcher has demonstrated the feasibility of sticking to your guns, with only rather minor wavering in the face of strong temptations to renege. The oft-repeated slogan TINA - there is no alternative - was

recognised, of course, even (or perhaps especially) within government, to have no intellectual basis. But it served to raise the political costs of a U-turn, and hence to make politically credible the time-inconsistent policy that has been pursued. In this sense Mrs Thatcher has done much to restore the credibility of the notion that government can stick to policies through thick and thin. Unfortunately, time inconsistency is not a sufficient condition for a good policy, and Mrs. Thatcher has restored credibility to the policy domain by pursuing what I shall suggest later has been a severely suboptimal time inconsistent policy, so suboptimal that there must be time consistent policies that would have dominated it. But, if such costly strategies can be made credible, a better designed policy should be all the more credible.

Now if you expect me at this point to reveal the nature of that better designed policy, I must disappoint you. I have untried prototypes, and many ideas on what not to do, but I have no streamlined policy to wheel on shining and gleaming, fully tested, with knobs and whistles on. But what we do now have are control methods available to us much better suited to the problems of policy design in economics. We can devise optimal time inconsistent or time consistent policies in rational expectations models, whether in a deterministic or a stochastic setting.<sup>(23)</sup> We can alter the information assumptions in such analysis, permitting us to analyse properly for the first time questions of indicator and intermediate target regimes.<sup>(24)</sup> We can analyse the bargaining, game theoretic aspects of policy that I discuss later. And we have developed experience of the use of these methods that suggests that they are powerful tools for policy appraisal.<sup>(25)</sup>

There is, of course, the worry that our control technology is in danger of outstripping the capabilities of the models available to us, and that is a concern which I share. After all, in engineering applications of control theory, the laws of motion of the system are usually well known; and if they are not, some experimentation can usually reveal the salient features for control purposes. In macroeconomics, our understanding is much poorer, and I say that despite being firmly of the view that the area of common ground between macroeconomists is much larger than is generally appreciated. Moreover,

experimentation is not an attractive option. To make matters worse, the large macroeconomic models (with hundreds or sometimes even thousands of equations) to which control methods have usually been applied do not necessarily incorporate the latest state of understanding. This is not surprising given their sheer size relative to the staff resources available for their support. A frequently recurring analogy in this area, particularly with those like myself who favour smaller models, is with painting the Forth Bridge. But in view of the large number of equations in these models that are just inadequate - whether because they fit very badly, have features that fly in the face of commonsense, have not been updated, or have been imposed in violence to the data - this image does not go far enough. Instead I invite you to think of a Forth Bridge made up in sections of the latest engineering design, while in other places we have sections made of wood, or even, in the case of the monetary sector so crucial to the present government's strategy, submerged stepping stones. Worse still, some would argue, all of this is built across a narrow stretch of river, so that a simpler, smaller design would suffice. Not surprisingly, the heavy technology of control theory reveals all the weaknesses of model construction. Moreover, these models appear more vulnerable in the light of rational expectations, which sheds doubt on the stability of their structure and on the nature of the dynamics that they embody.

However, this is far from being a necessary state of affairs - on the contrary, it is changing fast. Applied econometrics has advanced enormously in the past decade, and good econometric technique is now the norm, rather than the exception. In the past few years, we have had studies which systematically compare the sectors of different models, and which have been most helpful in identifying best, and worst, practice.<sup>(26)</sup> And increasingly expectations are being modelled seriously, with corresponding gains in understanding - to cite just two examples, work at the National Institute has highlighted the importance of forward looking expectations in understanding the stock-led recession of 1980/81, and productivity movements in the recent past.<sup>(27)</sup> I have no doubt that the next generation of models will incorporate the modelling of expectations quite generally.

An important extra element is the creation of the new Macro-Modelling Bureau at the University of Warwick, funded by the Economic and Social Research Council, which is making the large forecasting models available to the academic community. If the Bureau takes the process of model comparison further and more systematically, as it should, in a few years' time we shall, I think, have better models and considerably greater convergence of model properties. My own guess is that in the process models will become smaller, sleeker and fitter.

But policy design cannot, and should not, await these developments. We will never have the perfect model, the state of knowledge in macroeconomics being what it is, and if we did we probably would not agree on it. What we need is an approach to policy design that is ever alert to these uncertainties and differences. This has been by no means so in the past. For example, the control methods that we have applied have often treated the macromodel as a rather precise representation of the economy, instead of being vague and uncertain. In consequence, the resulting control rules may lean far too heavily on quirky and unsatisfactory features of the model. Techniques that avoid these pitfalls are, of course, familiar, but they are computationally expensive, particularly when applied to the very large forecasting models.<sup>(28)</sup> Moreover, they require knowledge of the system properties of the estimates of the model; and this is simply not available for the large models which are generally estimated separately equation by equation, or at best block by block, rather than as a system. This is yet another reason why the future for policy appraisal may well lie in a new generation of small scale macromodels, to which systematic robustness tests can be applied.

Perhaps a more serious problem has been that control techniques have all too often been used to show what policy performs best in each model separately. This is despite the fact that these so-called best policies may perform rather badly in other models. Consider the problem from the policy-maker's point of view. He has available to him a variety of views of the world represented by each of the models and supported by, usually, highly intelligent and persuasive arguments from each of our very articulate and numerate

modelling teams. It is not very helpful for him to know that a certain policy performs well in one model, when it performs badly in another. What instead would be of considerable interest would be to know that a certain policy performs tolerably well in all the models. Such policies might be agreed upon by the different modelling groups despite their important differences in outlook. What the racehorse owners at the Treasury and the Bank of England should be looking for is "a horse for all courses", not a well refined pure-bred capable of performing well only in special circumstances.<sup>(29)</sup> At the moment, the sheer diversity of models means that robust policies of this kind are probably not available. No policy could satisfy simultaneously the Merseyside Monetarism of Patrick Minford and the Cambridge Keynesianism of Wynne Godley. But we can reasonably expect to see a narrowing of these differences under the joint imperatives of the ESRC and Warwick as part of the process of model evaluation and selection that I alluded to before. It would not surprise me then if a robust policy, a horse for all courses, could be found.

This is part of a more general argument that our advocacy of particular policies should be sensitive to the important gaps in our knowledge. This may seem evident enough, but it is not taken seriously in practice. A standard question for all who advocate particular policies should be: What if the world is different, in plausible respects, from that which you assume? And this approach needs to be formally assimilated into our control theory. Control engineers have led the way in this, motivated by similar considerations, and there is a whole body of design theory drawing upon general, rather than specific, features of structure, that could well guide us in this task. There are some remaining technical barriers here - the complications introduced by rational expectations make it hard to take this body of knowledge over wholesale - but there is no reason to suppose that these problems will not be resolved. If we can systematically and routinely examine the robustness of our conclusions to those features of the control problem about which we are most uncertain, I have no doubt that the quality and usefulness of our policy advice will be greatly enhanced.

So far my discussion of the control problem incorporates an important asymmetry.

Government is aware that its actions modify private sector behaviour, and takes this interdependence into account in determining its best course of action; but the private sector does not take into account any dependence of government strategy on its, the private sector's, actions. Were it do so, so that the asymmetry disappears, we would be in a world of bilateral bargaining, in which the private sector thinks strategically.

The usual justification for this asymmetry is that the private sector is made up of many separate agents who cannot act together. For many applications I suppose that assumption will do; though sceptics can point to the coordinating role of the business lunch, the conformity to fashions of opinion in the City that so strikes the outsider, as well as to the more well-worn example of trade union behaviour. But the sphere in which strategic thinking becomes all important is in the international arena, when questions of international policy coordination between governments come under scrutiny.

For much of the postwar era up to the late 1960's, macroeconomic policy was conducted within the international constraints of the system established by Keynes and White at Bretton Woods. But that system showed increasing signs of strain in the late 1960's, as the adjustable peg mechanism for the exchange rate, entailing discrete adjustments to the exchange rate parity, became incompatible with the increasing mobility of financial capital internationally. In the consequent move to generalised floating in the early 1970's, the rules of the game were swept away. Each countries has pursued its own individual macroeconomic objectives with little regard for its international consequences. Although we have seen within Europe the framework for coordination under the auspices of the European Monetary System, the actual degree of coordination has been limited, in part because of the acute pressures from uncoordinated policy in the rest of the world.

Now there is a view that these matters are best left to individual governments. That view is well expressed in the work of Milton Friedman, who emphasises the role of the exchange rate in insulating domestic from foreign developments and conversely, and control of the money supply to stabilise domestic developments.<sup>(30)</sup> Such results, it is true, can be derived from a rather primitive international monetarist model of the early

1970's vintage. But once one takes account of the full interdependencies between countries - through prices, real demands, asset prices and the flow of funds - it becomes very clear that such independence does not hold. We cannot sidestep the question of how best to formulate policy in an interdependent world. These issues are currently high on the agenda of debate and the PRISM group here is much concerned with them as part of a broader programme of research under the auspices of the newly formed Centre for Economic Policy Research.<sup>(31)</sup>

How then should we address this problem? One approach - the hard-nosed realistic one - would be to accept that we are in a world where cooperation is limited, and design the best type of policy that one can for the UK. In other words, we design our policy treating the behaviour of other countries as given. This, it should be noted, is the standard approach to policy design, since almost all our models are of the single open economy, treating the rest of the world as exogenous. If all countries do the same, we arrive (perhaps iteratively) at a noncooperative solution to the policy problem. The second approach - the soft-headed idealist one - is to assume a world of cooperation, and design policy internationally in such a way as to secure the best overall performance, subject to the cooperating members sharing in the resulting benefits. To do this, of course, requires a model of many economies, specifying the interdependencies between them.

The difficulty is that the non-cooperative solution, hard-nosed though it is, may yield pretty disastrous outcomes. Let me give an example of this. Consider a government that wishes to stabilise the trend of prices in the economy (it could equally well be nominal income), in the face of aggregate demand or aggregate supply disturbances. It turns out that in a wide class of models it is not very difficult to do that using monetary policy. What one makes use of is the fairly strong and well established linkage from the exchange rate through to prices, using the fact that import prices figure directly in consumer prices because of the import component of the price index, as well as indirectly through domestic costs and wages. Essentially government responds to rising inflationary pressures

by raising interest rates and inducing an exchange rate appreciation, which acts to dampen the rise in prices. Pursued vigorously enough, such a policy can stabilise variations in prices and other nominal magnitudes fairly effectively. Moreover, some recent PRISM work suggests that one can devise a rule of this kind for targeting the price level that is robust in the multiple sense that it stabilises the system well in the face of a variety of disturbances, wide parameter variation, and model variation.<sup>(32)</sup> From the standard perspective of single country optimisation, it looks a good buy. But, if all countries wish to reduce inflation and all seek to do so by appreciating their exchange rate, they must fail, simply because of the elementary fact that one country's appreciation is another's depreciation. The aggregate consequence of such policies is an interest rate war, of the kind that we have seen in the last five years internationally.<sup>(33)</sup>

Thus single country policy design is prone to generate beggar-my-neighbour policies. This points to the need to examine policy in the inter-dependent, global setting, searching for policies that perform satisfactorily in the aggregate as well as being able to cope with country specific disturbances that generate divergences between one country and the rest of the world. We have some elementary analysis of such problems, and one or two blueprints for a possible international policy design, notably that of James Meade.<sup>(34)</sup> But we have, as yet, no systematic appraisal of policy in this area. Given its importance in the international debate, it is perhaps fortunate that this issue is now the object of intense research and I expect the next year or two to see the flow of important and useable research results.

But the hard-nosed realist will respond by asking what is the point of devising elaborate policy designs that would work well if all countries adhered to them, but will not be implemented because all countries have an incentive to renege, to carry out a different, free-riding policy. The problem is, after all, like the so-called prisoners' dilemma in game theory: there is a cooperative outcome that is best all round, but each player has an incentive to choose a different strategy, since that seems to improve his pay-off irrespective of the strategy chosen by other players. Yet if everyone acts in this



way, all are left worse off. Thus the problem is one of coordination, given the private incentive to renege on cooperative behaviour.

Had Keynes and White taken this sceptical view, and with the background of the 1930s there was every reason for them to do so, we would never have seen Bretton Woods and the postwar settlement. Fortunately the problem is not as intractable as it first appears, for we are concerned with a repeated prisoners' dilemma game, not an isolated one. In this context, the question is whether one can devise a set of threats of penalties to be imposed on those players or countries who renege. That sounds an impossibly complicated task. But recent advances in the theory of non-cooperative game theory suggest that it may be much more amenable than it appears.<sup>(35)</sup> Rather than attempt to describe and motivate the complicated theorems that have been thrown up in the past few years, let me instead describe a revealing experiment that was conducted several years ago.<sup>(36)</sup> Game theorists were invited to submit computer programs to play a game of the repeated prisoners' dilemma kind that I described before, in a league play-off against all other entries. The winner was very simple and familiar - tit for tat - you are nice to people until they are nasty to you, and then you are nasty to them until they are nice to you again. On the second round, competitors were invited to resubmit, having the benefits of a comprehensive analysis of the results of the first round. Once more tit-for-tat came out ahead of the other strategies, some of them enormously complex, devious and Macchiavellian in design.

I find this result, which has been confirmed by subsequent analysis suggested by the experiment as well as other work, most reassuring. The notion that simple, nice strategies of the type that we all commonly use are robust, effective strategies suggests that the cooperation on which all aspects of our social life depend is not as fragile as analysis sometimes suggests. Tit-for-tat's strength lies in its capacity to elicit and reward cooperation in other players - the key to success in games of the repeated prisoners' dilemma type. It does this by offering cooperation, retaliating speedily to non-cooperation, but forgiving equally rapidly; and in all this being transparent, clear and

predictable throughout. Results of this kind being generated by non-cooperative game theory have important implications throughout social science, in particular in the theories of social institutions and social conventions, as well in other areas such as biology.<sup>(37)</sup> In the field of international economic cooperation, it suggests that the design of a threat system to sustain a cooperative policy design need not be as hard as at first it seems, and that the crucial question that faces us is rather whether we can devise a satisfactory cooperative policy that copes with the problems of inter-dependence. The design of a new international order is a formidable task, particularly in the changed balance of forces that now prevails in the world, but it is a challenge to which economists as a profession should, and I believe can, rise.

On the basis of the review that I have given of developments in the field of economics and control, you will perceive that I am optimistic of the future. Unfortunately, and this takes me to my final theme, I am not optimistic that these developments will be translated into a more effective economic policy, at least in the foreseeable future. I base this pessimism on the experience of the past few years, when we have seen a policy implemented that flies in the face of what we know, and knew then, and that seems almost wilfully designed to provide an object lesson in how not to manage our macroeconomic affairs. The points that I wish to argue are not new - they have been argued repeatedly, forcefully and at great length both inside and outside Whitehall. But there has been an absence of interest in taking these arguments seriously, to engage in rigorous policy appraisal in their light.

Lest I be misunderstood, let me emphasise that it is not part of my case tonight to argue that this government was wrong to embark upon a policy of reducing inflation by sole reliance on monetary and fiscal retrenchment. That view can be argued, and has been argued many times, and I sympathise with it. But it was this government's prerogative to take that decision and make it stick; and it is the electorate's prerogative to judge whether it was right. What I wish to argue instead is this: that had it opened its ears to the technical advice available to it, this government might well have managed its

basic strategy much more effectively, with less cost in terms of output foregone, investment and employment.

Since 1979, policy has been conducted in accordance with the principles of the Medium Term Financial Strategy, MTFS for short, formally unveiled in the budget of 1980. This laid down target ranges for the growth of the money supply, initially £ M3, and a supporting stance for fiscal policy in the form of targets for the Public Sector Borrowing Requirement. At the heart of the strategy was the notion, (drawn from Friedman, and developed by Laidler and in the UK context most notably by Ball, Burns and their colleagues at the London Business School) that the best way to reduce inflation was to do so gradually by means of a phased reduction in the money supply.<sup>(38)</sup> Once a low inflation rate is established, continued targeting of the money supply at a low constant rate was hoped to prevent any resurgence of inflation.

Now the conception behind the MTFS - that if one is to embark on a disinflationary programme, it should be gradualist - is a good one. Because of the sluggishness of wage and price inflation in adjusting downwards, for which there is overwhelming evidence, deflation is best administered slowly to smooth the effects on output. This intuition is formally confirmed by the optimal control based analysis of a number of researchers.<sup>(39)</sup> But while the conception may have been right, the delivery went sadly wrong. For while a phased reduction in the money supply may deliver gradualism in the financially closed economy of Friedman's mind, it has a very different effect in the open economy that is the UK. The error was to neglect the consequences of intelligent behaviour in the foreign exchange market. It was clear that the strict implementation of the MTFS gave the prospect of higher UK interest rates for a run of years, which together with lower domestic inflation made sterling a more attractive investment. Funds flowed into sterling and the exchange rate therefore showed a marked appreciation, resulting in an historically unprecedented loss of international competitiveness. So-called gradualism inflicted a sudden sustained contractionary blow to the traded goods sector, notably manufacturing,

with consequences for jobs and output that we observe today. Matters were made still worse by an inauspicious rise in indirect taxes, proclaimed in the Alice-in-Wonderland economics of the time as non-inflationary. This pushed up prices, and exacerbated the upward pressure on interest rates and the fall in output.

This suggests that the MTFS was a poorly designed, high risk strategy. In saying this, I am not speaking with the advantage of hindsight. The essence of the analysis was set out in the work of Rudiger Dornbusch on exchange rate overshooting, published in 1976.<sup>(40)</sup> And there were papers circulating widely in both academic and official circles in 1979 and 1980 that spelt out these points most clearly - notably in the symposium organised by Oxford Economic Papers.<sup>(41)</sup> Moreover, the most obvious resolution of these problems - to engage in the popular pastime of rate-capping, but in the context of the foreign exchange market to stem the rise in sterling, or at the very least to relax the monetary stringency in the face of a rising exchange rate - these policies had a highly respectable pedigree, with the Swiss Central Bank following such a course only the year before.<sup>(42)</sup>

Equally damaging has been the straitjacket in which the MTFS has placed fiscal policy. A long-standing feature of the fiscal system has been the operation of automatic fiscal stabilisers. By this is meant the tendency of the fiscal deficit to fall as demand rises, and conversely to rise as demand falls, because of the link of revenues and expenditures - income tax and unemployment benefit, for example - to demand. By allowing this effect to operate, fluctuations in demand are dampened, helping to stabilise output movements. But over the period in question, the government has been pursuing an absolute target for the deficit, expressed in terms of the Public Sector Borrowing Requirement. Fiscal stabilisers have therefore been switched off, and in consequence the ride has been bumper - the unusual severity of the stock cycle through which we have just been owes much to this feature of policy.

Now it is true that there are problems in operating fiscal stabilisers alongside short run monetary targets. It is a feature of a great many models, sometimes not always appreciated by their originators, that this policy combination generates stochastic instability.<sup>(43)</sup> This is because the need to finance by bond sales residual budget imbalances arising from demand fluctuations generates a rising level of volatility in interest rates, demand and output. The Treasury model, amongst others, shares this feature. But if that is the reason for this bizarre policy of PSBR targeting, it does not stand up. It would not be hard to devise low frequency fiscal adjustments that permit automatic fiscal stabilisers to operate while avoiding such instabilities. One might equally look at the many alternatives to short run monetary targeting that provide assurances about longer run inflation without emasculating fiscal policy.

The consequences of this fiscal conservatism have been severe. With the economy in deep recession, with unemployment well above estimates of the level at which inflation is non-accelerating, the UK is running a substantially contractionary fiscal policy.<sup>(44)</sup> With other EEC countries following the UK example, it is no surprise that eminent economists should be calling for some fiscal relaxation to help the European economy out of the trap of depressed demand and rising unemployment.<sup>(45)</sup> But our policy-makers seem set to disregard the strong case that has been advanced for a supply-side-friendly policy of demand expansion.

But perhaps the most bizarre aspect of the whole strategy is its centrepiece - the targeting of the money supply. It may seem heretical to challenge such ideas - clearly I must be of unsound mind - but let me remind you that we managed without monetary targeting for most of our country's history; and that indeed there has been almost no period in which the money supply has not been free to vary within reasonable limits in response to movements over the business cycle. We are, after all, not interested in the

money supply as such, but rather with prices and output. The attention given to the money supply is usually justified in terms of its being an intermediate target or indicator, containing useful information about the future course of prices and output. But no one has shown that the money supply, however one defines it, is the sole variable that should be used in that way, or that it is very satisfactory in that role: supposed relationships involving money break down as quickly as they are discovered. I am not asserting that the money supply, suitably defined, is unimportant - even though we do not have compelling evidence in its favour, we would be most unwise to disregard money supply movements. But my point is that there are many other variables that are important as well. And one of the damaging features of over-concern with the money supply has been the consequent neglect of international considerations, notably the exchange rate. UK policies have, as a result, been of a severely beggar-my-neighbour character.

But questions of this kind are, I am afraid, of little interest to our current masters. Most recently, we observed the Chancellor of the Exchequer dismissing David Hendry's demolition of Friedman's empirical work for the UK with the lofty words "I am not interested really in the arcane quibbles of econometricians", and then saying in the next moment "the basic concept is very clear".<sup>(46)</sup> There is no empirical regularity involving money that has stood up for more than a brief time that can justify the current naive approach to policy making. The latest in a string of fads is the targeting of M0, cash plus bank reserves. The recent study by Barry Johnston - a fine piece of econometrics incidentally, but widely misinterpreted - has been said to be the econometric basis of the reformulated MTFS.<sup>(47)</sup> Yet we expect in advance that it will not stand up for long, that it will collapse as the latest victim of Goodhart's Law. The study itself made clear just how much financial innovation - the spread of branches credit cards and cash tills - has altered the demand for M0 over the past decade, and this in a period when no one was concerned with M0. How much greater and faster can we expect innovation to be when

government is creating an incentive so to economise by tracking the cash available to the public and banks and with the spread of electronic cash management. If the banks are not busy considering strategies for expanding their business without MO, they are failing as bankers. Can we really expect to control the activities of the financial institutions, amongst the most powerful, innovative and creative of our industries, by devices of this kind?

All of this should be a matter of deep concern to us. For these policies, wished on us by those for whom "the basic concept is very clear", are laid at the door of the economics profession when they fail. And those advances in our understanding of which I spoke earlier will, I think, go untapped so long as this intellectual climate prevails. We must insist that those who would experiment with the British economy and our fellow citizens should first subject their favoured schemes to a process of rigorous scrutiny and appraisal against available alternatives. But I am not hopeful that we will see this happen. My title this evening refers to a failed partnership, but left matters open with a question mark. We must, I fear, take down that question mark: the partnership, rich, rewarding and exciting though its potential may be, is foundering on a failure to listen and to learn.

- (1) Medawar (1984).
- (2) Phillips (1954, 1957).
- (3) Ball (1978).
- (4) This is emphasised by the monetary approach. See, for example, Frenkel and Johnson (1976).
- (5) Dornbusch (1976), Buiter and Miller (1981, 1982).
- (6) Currie (1984a).
- (7) See the papers collected in Lucas and Sargent (1981). For excellent surveys, see Begg (1982) and Sheffrin (1983).
- (8) Kahnemann, Slovic and Tversky (1982). On the notion of bounded rationality in economics, see particularly Simon (1982, 1983).
- (9) Mason (1983).
- (10) See, for example, the National Institute (1983, 1984), Kmenta and Ramsey (1981)
- (11) Cross (1983).
- (12) For models of learning, see Bray (1982), Bray and Savin (1983), and Frydman and Phelps (1983). For analysis of partial information, see Frydman and Phelps (1983), Minford and Peel (1983), Pearlman, Currie and Levine (1983) and Townsend (1983). See also Barro and Gordon (1983a, 1983b), Kreps and Wilson (1982) and Backus and Driffill (1984a, 1984b) for consideration of credibility and reputation.
- (13) See, for example, Lucas (1972), Sargent and Wallace (1976), Barro (1976).
- (14) For the consequences of less flexible contracts, see, for example, Fischer (1977), Phelps and Taylor (1977) and Taylor (1980).
- (15) Lucas (1976).
- (16) See Pearlman, Currie and Levine (1983).
- (17) See Currie (1984), Begg (1984), van der Ploeg (1984).
- (18) See Kydland and Prescott (1977, Prescott (1977), Calvo (1978), Driffill (1980), Buiter (1981), Miller and Salmon (1983)



- (19) Meade (1982), Vines et al (1983).
- (20) McMahon (1981).
- (21) Buitter (1981)
- (22) Levine and Currie (1983). What we here refer to as the time-inconsistent policy may be rendered time consistent if reputations are explicitly analysed (see Backus and Driffill 1984a, 1984b) or if a threat strategy is followed (see Oudiz and Sachs, 1984).
- (23) Buitter and Dunn (1982), Chow (1981), Driffill (1980, 1982), Currie and Levine (1983), Levine and Currie (1984).
- (24) Pearlman, Currie and Levine (1983), Currie and Levine (1984).
- (25) Miller and Salmon (1983, 1984), Currie and Levine (1984, 1985).
- (26) See the set of papers from the National Institute dealing with comparisons of models of exports, bank lending, wages and employment; a useful summary is given in Henry (1983).
- (27) See Hall, Henry and Wren-Lewis (1983), Wren-Lewis (1984).
- (28) Astrom (1970), Holly et al (1979).
- (29) Chow (1980), Currie and Levine (1983).
- (30) Friedman (1953, 1968).
- (31) See Currie and Levine (1985a, 1985b).
- (32) Levine and Currie (1983), Currie and Levine (1983).
- (33) Currie and Levine (1985a, 1985b).
- (34) Meade (1982b, 1984).
- (35) Basar and Olsder (1982).
- (36) Axelrod (1984) (1980a, 1980b, 1981, 1984), Axelrod and Hamilton (1981); for a convenient summary, see Hofstadter (1983).
- (37) Schotter (1981), Axelrod and Hamilton (1981), Maynard Smith (1982), Dawkins (1982).
- (38) Friedman (1975), Ball, Burns and Laury (1977), Ball, Burns and Warburton (1979), Laidler (1982). For a recent assessment of the work of Ball et al from a sympathetic background, see Budd and Longbottom (1984).
- (39) Driffill (1982), Currie and Levine (1983).
- (40) Dornbusch (1976).

- (41) Eltis and Sinclair (1981). See particularly Buiters and Miller (1981), Artis and Currie (1981), and the suggestively titled paper by Scott (1981).
- (42) Artis and Currie (1981), Schiltknecht (1981).
- (43) Blinder and Solow (1973), Currie (1976, 1978), Christ (1979), Currie and Gazioglou (1983), Whittaker and Wren-Lewis (1983), Blackburn and Currie (1984).
- (44) Buiters and Miller (1984), Buiters (1984).
- (45) Layard et al (1984).
- (46) Hendry and Ericsson (1983), Treasury and Civil Service Committee (1984).
- (47) Johnston (1984), Treasury and Civil Service Committee (1984).

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