

# Keeping the Keynesian Faith

## Alan Blinder on the Evolution of Macroeconomics

*An interview with introduction by Brian Snowdon*

There is an appealing philosophy of economic policy that combines hard-headed respect for economic efficiency with soft-hearted concern for society's underdogs... I would like to call this philosophy 'liberal' in both the eighteenth—and twentieth century—senses of the word, for it combines profound respect for the virtues of free markets with profound concern for those the market leaves behind.

(Alan Blinder, 1987)

### Introduction

Professor Alan Blinder is a leading macroeconomist and political liberal who for over thirty years has remained an outspoken champion of Keynesian economics and policies. He is currently Professor of Economics at Princeton University where he has taught and conducted research since 1971. During the 1990s he was engaged in public service, first as a member of President Clinton's Council of Economic Advisors (1993–94) and then as Vice-Chairman of the Board of Governors of the US Federal Reserve System (1994–96). Having been on both sides of the fence he is in a unique position to comment on important theoretical and practical economic policy issues. In the interview that follows, Alan Blinder gives his outspoken views on a number of important contemporary macroeconomic issues. First, I will provide a brief historical framework as background to the issues under discussion.

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Alan Blinder is the Gordon S. Rentschler Memorial Professor of Economics at Princeton University. Brian Snowdon is Principal Lecturer in Economics at the University of Northumbria, Newcastle upon Tyne, UK.

## Historical background

In 1776 Adam Smith's celebrated *An Inquiry into the Nature and Causes of the Wealth of Nations* was published in which he set forth the invisible-hand theorem. The main idea here is that profit and utility maximising behaviour under competitive conditions will translate the activities of millions of economic agents into a social optimum via free market forces. Following Smith, political economy had an underlying bias towards *laissez-faire* and the classical vision of what we now call macroeconomics found its most famous expression in the dictum 'supply creates its own demand'. This view, popularly known as Say's Law, denies the possibility of general overproduction or underproduction. In contrast to this prevailing orthodoxy, the most revolutionary aspect of Keynes's work from the early 1930s onwards was his clear and unambiguous message that with regard to the general level of output and employment there was no invisible hand channelling self interest into some social optimum. The most objectionable feature of capitalism for Keynes was the intolerable levels of unemployment which emerged in the UK economy during the 1920s and on a global scale in the 1930s. The classical theory clearly appeared to be inconsistent with these events and created the need for a new approach which was provided by Keynes (1936) in his *General Theory of Employment, Interest and Money*. For Keynes the traumatic events of the 1930s were symptomatic of a fundamental flaw in the operation of the price mechanism as a co-ordinating device. Capitalism was not terminally ill but highly unstable. Therefore, Keynes's main objective was to inspire a conservative revolution in order to modify the rules of the game within the capitalist system in order to preserve and strengthen it. For Keynes, the ultimate political problem was how to combine economic efficiency, social justice and individual freedom. But questions of equity were always secondary to questions of efficiency, stability and growth. Therefore his solution to the economic malaise which was sweeping the capitalist economies in the early 1930s was to accept as inevitable 'a large extension of the traditional functions of government'. But as Keynes argued in *The End of Laissez-Faire* (1926), if the government is to be effective it should not concern itself with 'those activities which private individuals are already fulfilling', but attend to '... those functions which fall outside the private sphere of the individual, to those decisions which are made by no one if the state does not make them'.

The first twenty five years following the end of the second world war were halcyon days for Keynesian macroeconomics. The new generation of macroeconomists generally accepted Keynes's central message that a *laissez-faire* capitalist economy could possess short run equilibria characterised by excessive involuntary unemployment. The main policy message to come out of the *General Theory* was that active government intervention in order to regulate aggregate demand was necessary, indeed unavoidable, if a satisfactory level of aggregate output and employment were to be maintained. That the *General Theory* was written in the early 1930s should be of no surprise given the cataclysmic events associated with the Great Depression. One lesson from the history of economic thought is that a main driving force behind the evolution of new ideas is the march of events. While theoretical ideas can help us understand historical events, it is also true that the evidence from major historical events often challenges, and may overturn, existing theories, leading to the evolution of new ideas. The Great Depression gave birth to modern macroeconomics as surely as accelerating inflation in the late 1960s and early 1970s facilitated the monetarist and new classical counter-revolutions.

## **A Keynesian crisis?**

During the early 1970s there was a significant renaissance of the belief that a market economy is capable of achieving macroeconomic stability, providing that the visible hand of government is prevented from conducting misguided discretionary fiscal and monetary policies. In particular, the stagflationary experiences of the 1970s provided increasing credibility and influence to those economists who had warned that Keynesian activism was both over-ambitious and, more importantly, predicated on theories which were fundamentally flawed. To the Keynesian critics, the events of the Great Depression, together with Keynes's theoretical contribution, had mistakenly left the world 'deeply sceptical about self-organising market systems' (Sachs, 1999). The orthodox Keynesian insistence that relatively low levels of unemployment are achievable via the use of aggregate demand management was vigorously challenged, first by Milton Friedman, who launched a monetarist counter-revolution against policy activism during the 1950s and 1960s, and later by the new classical critiques (Snowdon and Vane, 1996, 1999a).

It soon became apparent to the Keynesian mainstream that the new classical critique represented a much more powerful and potentially damaging challenge than the one launched by the monetarists which was of longer standing. Although orthodox monetarism presented itself as an alternative to the standard Keynesian model, it did not constitute a radical theoretical challenge to it (see Laidler, 1986; Mayer, 1997; De Long, 2000). In contrast, the new classical view was that the Keynesian model could not be patched up. The problems were much more fundamental and related in particular to (i) inadequate microfoundations which assume non-market clearing; and (ii) the incorporation in both Keynesian and monetarist models of a hypothesis concerning the formation of expectations which was inconsistent with maximising behaviour, i.e. the use of an adaptive rather than rational expectations hypothesis. By 1978 Robert Lucas and Thomas Sargent were contemplating 'Life after Keynesian macroeconomics'. In this brilliant polemical article Lucas and Sargent portray Keynesian economics as an unscientific orthodoxy which is fundamentally flawed and well beyond repair (see Backhouse, 1997). Two years later, in a paper entitled 'The death of Keynesian economics, issues and ideas', Lucas (1980) went so far as to claim that '...people even take offence if referred to as Keynesians. At research seminars people don't take Keynesian theorising seriously anymore; the audience starts to whisper and giggle to one another' (cited in Mankiw, 1992). In a similar vein, Blinder (1988a) has confirmed that:

By about 1980, it was hard to find an American academic macroeconomist under the age of 40 who professed to be a Keynesian. That was an astonishing intellectual turnabout in less than a decade, an intellectual revolution for sure.

By this time the United States' most distinguished 'old' Keynesian economist had already posed the question 'How dead is Keynes?' (Tobin, 1977).

## **A Keynesian resurgence?**

In retrospect, the Lucas and Sargent obituaries of Keynesian economics can now be seen to have been premature. Although Keynesianism in general has been declared defunct by numerous critics on the classical side of the macroeconomics divide, it has steadfastly refused to go away (Shaw,

1997). The poor performance of Keynesian wage and price adjustment equations based on the idea of a stable long-run Phillips curve (Phillips, 1958) made it imperative for Keynesians to modify their models so as to take into account both the influence of inflationary expectations and the impact of the 1970s OPEC-induced supply shocks. This was duly done, and once the Phillips curve was suitably modified it performed ‘remarkably well’ (Blinder, 1986). The important work of Gordon (1972, 1975), Phelps (1968, 1972, 1978), and Blinder (1979) was particularly important in creating the necessary groundwork which has subsequently allowed the Keynesian model to adapt and evolve in a way which enabled monetarist and supply shock influences to be absorbed within the existing framework. Moreover this transition towards a synthesis of ideas did not require any fundamental change in how economists viewed the economic machine.

Indeed during the past fifteen years there has been a renaissance of ‘Keynesian’ thinking in the form of ‘new Keynesian’ theorising (Gordon, 1990; Snowdon *et al*, 1994; Snowdon and Vane, 1997). It also remains the case that mainstream intermediate level macroeconomics textbooks are still predominantly ‘Keynesian’ in their structure, even if substantially modified compared to their 1960s counterparts to take into account both the monetarist and new classical critiques (see Mankiw, 2000). So the ‘bad guys’ appear to have been making a comeback since the mid 1980s, a time when Alan Blinder was writing about ‘Keynes after Lucas’ (1986) and the ‘Fall and Rise of Keynesian Economics’ (1988a). By the early 1990s, he was announcing ‘A Keynesian Restoration’ (1992a). While there are many varieties and definitions of Keynesianism, Alan Blinder, as one who kept the faith during the 1970s and 1980s, has suggested that ‘the heart of Keynesianism consists of six principles’:

- (i) both monetary and fiscal policy affect aggregate demand;
- (ii) because prices and wages are not perfectly flexible, changes in aggregate demand (anticipated or unanticipated) have their main short run impact on real variables such as employment and GDP;
- (iii) the short run is long enough to worry about;
- (iv) Keynesians believe that unemployment is an important problem, is often involuntary, and certainly not the consequence of Pareto optimal responses to supply side disturbances as claimed by real business cycle theorists;

- (v) many (but certainly not all) Keynesians support activist stabilisation policies designed to reduce aggregate instability;
- (vi) although far from unanimous, many Keynesians worry more about combating unemployment than they do about conquering inflation.

Alan Blinder set forth these six ‘principal tenets’ some thirteen years ago (1988a). However, over the thirty year period of his professional career, despite the ‘revolutions and counter-revolutions’, there has undoubtedly been a steady accumulation of knowledge in macroeconomics, much of it critical towards the orthodox conventional Keynesian wisdom of the 1960s. In his recent survey of the development of macroeconomics during the twentieth century, Olivier Blanchard (2000) argues that macroeconomists have ‘learned a lot’ and that ‘progress in macroeconomics may well be the success story of twentieth century economics’. In light of the enormous changes and upheavals that have occurred in macroeconomics since 1971, to what extent has Alan Blinder kept faith with Keynesian ideas? How does he now view the evolution of modern macroeconomics? To find answers to these and other important macroeconomic questions, I interviewed Professor Blinder on 6th January 2000, in his office at the Brookings Institution, Washington DC, where he was on leave from Princeton.<sup>1</sup>

## Background information

*What persuaded you to become an economist and in particular a macroeconomist?*

The latter was mainly by chance, the former was the result of a fairly typical pattern for a young man in those days... I was young in those days (*laughter*). I was mathematically inclined but not good enough to pursue pure math, at least not very far. I was also interested in social issues which were not quite as dry as pure math. So I stayed with pure math as long as I could, which for me was roughly mid way through university, and then I dropped out into something which I found easier. As for becoming a macroeconomist, as I said, that was purely by chance. When I was just finishing my PhD in economics there was a job opening at Princeton late in

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<sup>1</sup> This interview is one in a series of ten with eminent economists to be included in a new book (see Snowdon, 2002).

the year and they needed someone to teach macroeconomics. Richard Quandt, who was then the Chairman, called me and said, “Could you teach macroeconomics?”, and I said, of course I could! I had studied macroeconomics as part of my PhD, but it was not my main interest. My thesis was on income distribution.

## The development of macroeconomics 1936–2000

*If you look back over the period since the publication in 1936 of the General Theory, which contributions are the ones which have really influenced the development and direction of macroeconomics?*

So this is not a short-answer question! (*laughter*).

*Well no, but let us try and identify the really big ideas by breaking the period up. What, for example, were the major developments up until 1970?*

Then first we have to think of a series of contributions that gave theoretical and empirical content to Keynesianism. This would include Lawrence Klein’s models of the US economy [*Klein and Goldberger, 1955*], James Tobin’s [*1958*] work on the demand for money, Albert Ando and Franco Modigliani’s [*1963*] work on the consumption function and Dale Jorgensen’s [*1963*] work on the investment function. In the period from 1945 until the early 1970s there was what seems to me to be a constructive period of normal science in the Kuhnian sense. Indeed, Bob Lucas [*Lucas and Rapping, 1969*] was also a participant in those mainstream developments. A large number of people made contributions, some major, some minor and you could see definite progress, even if it was not quite linear. The Keynesian model was gradually being fleshed out and improved and many of the participants in these developments have earned Nobel prizes for their contributions. The contributions of Milton Friedman [*1968*] and Edmund Phelps [*1967, 1968*] I also regard as part of the mainstream development. Basically they were getting the Phillips curve right.

*After 1970 we then witness an unravelling of these mainstream developments. Samuelson’s neoclassical synthesis comes under attack throughout the 1970s. How do you interpret that period?*

Things really started to change dramatically after 1972–73 when we had the Lucasian revolution. I thought, and still think, this development was largely destructive. It was mainly about what was wrong. I want to emphasise that this is not meant as a criticism of Bob Lucas because the Lucas critique [1976] was a brilliant insight and, moreover, it was correct. So you can hardly criticise anyone for that, and I don't, because it needed to be said. Bob Lucas pulled together a number of strands that were out there. For example, unease about the Phillips curve, unease about the consumption function, unease about the investment function and so on. Lucas had the brilliant insight that made all of these doubts cohere and invited further applications. The problem was that the reaction in the profession to the Lucas critique turned out to be much less constructive than the period of normal science that had preceded it and was in many respects nihilistic. So I really think that by the late 1980s we as a profession were no closer to understanding how the economy works, and having a good model of it, than we were in 1972. Now I emphasise again that I do not blame Bob Lucas for this. But that was the kind of reaction we had within the profession. So, on your list of major influences, I would certainly want to include Bob Lucas for the Lucas critique article and also for his 1973 *American Economic Review* paper. These papers were landmarks and were extremely influential.

After that several things happened, various disparate strands. First, you have the development of what is called new classical economics evolving from the work of Lucas and also the contributions of Tom Sargent and Neil Wallace [1975, 1976]. Robert Barro [1977, 1978] also made some important contributions, especially his empirical work testing the impact of unanticipated money on the real economy. This latter work did not stand the test of time, but was very influential in the 1970s. But this whole line of thinking kind of petered out as it became increasingly clear that the surprising conclusion coming from new classical economics—that only unanticipated money matters for real variables—turned out to be wrong. However, it was not so obvious at the time that this line of inquiry would turn out to be a blind alley.

There is a second stream that came out of the Lucasian revolution, which I also think was a blind alley; but this one is still going on: the real business cycle idea. Here it is not monetary forces which are causing all the real action, rather it is technological changes, with money following



passively. As I said, this idea hasn't quite died yet but it has been changed quite substantially. About the time I was going into the government (January 1993) the heirs to the new classical tradition, the real business cycle theorists, were just discovering sticky prices and putting sticky prices into their models. Of course, this made them behave quite differently—more like Keynesian models. That was just beginning to happen in the early 1990s. Among the influential people in the RBC developments were, of course, Finn Kydland and Ed Prescott [1982].

I attribute a third strand, which I think has had and will continue to have durable impact, to Lars Hansen and Tom Sargent [1980]. This is the development of models from microfoundations which can be estimated in more complicated ways than the old macroeconomic models. These new techniques will last and have durable influence—partly for the better and partly for the worse. The 'partly for the better' aspect is obvious: you get deeper insights. You get estimates of parameters that can at least claim to be deep structural parameters, such as tastes and technology. You can also incorporate rational expectations into the estimation. It's important to note that the hostility that a lot of us felt for some of the developments which followed the Lucasian revolution was not hostility to the notion that expectations were rational. Rather, the hostility was directed at the market clearing assumption. That part of the new paradigm made no sense to me and to many other people. Anyway, Sargent and Hansen showed us how to develop rational expectations econometrics. The 'partly for the worse' aspect of this work is that it had a tendency to turn into a religion and was far removed from reality.

The fourth influence went in a totally different direction, but has also been very, very influential. This is the work of Chris Sims [1980] on vector autoregressions. The Sargent-Hansen work inputs a lot of theory into the econometrics. In complete contrast, Sims's approach says, let's just look at the facts. Let's just run unstructured equations and try and find out what kind of dynamics are actually in the data without imposing any theory at all. If you think about it, that kind of estimation should be the most vulnerable to the Lucas critique. The VAR technique estimates parameters which no one on earth could possibly think are structural parameters in the Lucasian sense. Yet Sims's contribution had a very profound influence on how econometrics is done. Somehow, the nihilism that arose in the 1980s paved the way for the VAR methodology, and it caught on

dramatically and is still with us. And I think VARs will be with us forever because they are a very good way to uncover basic macroeconomic facts. They help us to answer questions such as, What's in the time series? What do they actually show? What are the facts that our theories should account for? And which are the non-facts which shouldn't be in our theories? So it's an extremely useful methodology. So that brings us up to about the mid 1980s.

*About this time Keynesian economics begins to have somewhat of a reincarnation, to use Mankiw's [1992] terminology. In your Challenge article in 1992 you also talk about a 'Keynesian restoration'.*

Yes, the next important development is the emergence of new Keynesian economics. I think of this work as bringing a lot more theoretical rigour in the form of microfoundations to the Keynesian model. I don't think it has helped us understand the economy much better or helped us forecast better. But it did provide a theoretical legitimacy which, in the atmosphere created subsequent to the Lucasian revolution, was necessary. In those days, you couldn't be heard unless you had explicit microfoundations in your models. So some Keynesians said, 'Here are my microfoundations, now let's get on with it' (*laughter*). But these developments undoubtedly played a role in the rehabilitation of Keynesian thinking. I don't believe it changed the way macroeconomics was, and should be, taught to students at the principles or intermediate level. But at the rarefied level of graduate academic economics, it bred a little more tolerance for Keynesianism by legitimising Keynesian thinking. The important people contributing to the new Keynesian research are Greg Mankiw, David Romer, Larry Ball and several others. The two volume set edited by Mankiw and Romer [1991] collects much of this work together.

*During the past fifteen years we have also witnessed a renaissance of research into economic growth. This has involved both theoretical and empirical contributions. What are your thoughts on these developments?*

The research on growth fed naturally into the emphasis on the long run that was implicit in the 'classical' element of new classical analysis. The earlier interest in growth theory had come to an end in the late 1960s.

When I entered MIT in the fall of 1969, if you looked on the shelves at the previous PhD theses, they were almost all growth models. We were definitely in the deep epicycle phase of growth theory at that time—after that it died. Perhaps all the cute theoretical models that could be written on that subject had been written. Later, Paul Romer [1986], Bob Lucas [1988] and others revived the profession's interest in growth. Their work has had a major effect on the discipline, judging by the number of citations. But in terms of actually improving our understanding of the economy, I am not so sure. For example, when I try and explain these developments in growth theory to non economists I usually end up saying, well, these new theories are about how the real driving force behind the growth of the economy is technological change and the accumulation of knowledge. But this causes people to look at me and say, “Did anyone not know that?” (*laughter*). And, of course, we did know that. It wasn't that we learned this in the 1980s from Paul Romer. What Paul did was develop a novel—one might say, elegant—theoretical way of modelling this influence. But if you go back to the initial work of Robert Solow [1956, 1957] and Edward Denison [1967], we see the same message: growth is about knowledge and technology. However, technological change was modelled in a very crude way. In the early growth literature, it was just an exogenous trend; it somehow just happened. But even when I was in graduate school in the late 1960s and early 1970s, there were models of endogenous growth. As you know, economics runs in fads.

In my lengthy fifty year synopsis, I have not mentioned supply side economics because that didn't have any intellectual content at all. But it was an idea, a dumb idea (*laughter*). Unfortunately, this dumb idea had a very dramatic influence on economic policy in Reagan's America during the 1980s.

*It is clear that much has happened to macroeconomic analysis since Keynes published his General Theory in 1936. What is the place of the IS-LM model in the teaching of modern macroeconomics given the enormous changes that have taken place since John Hicks's famous 1937 paper?*

I think IS-LM, with suitable modifications, is still the primary vehicle for teaching basic macroeconomics—and for formulating policy. Among the essential modifications I think of right away are: the Friedman-Phelps

Phillips curve, opening the economy, and perhaps replacing the LM curve by an assumption that the central bank sets the nominal interest rate. There are others.

## The influence of ideas and events on macroeconomics

*I am very interested in the relationship between the influence of events on ideas and ideas on events when it comes to progressing our knowledge and understanding of macroeconomic phenomena. Do events mainly influence the development of macroeconomics or do ideas develop because of an internal dynamic within the discipline as techniques improve and new ideas emerge?*

They are bound up, but I think the influence of events on ideas is more important. In my previous tour of the literature I did not mention monetarism, which was like a fourth of July rocket: it went up then came down (*laughter*). But monetarism's rise is a good example of what you are talking about. Monetarism caught on because its advocates criticised Keynesian theory for leading inevitably to inflationary policies. I don't think that was a correct criticism in principle, but it stuck in practice because of accelerating inflation in the 1970s. Monetarists claimed that they had a better way to control it. They were wrong. There is a great story to be told about the control of inflation in the western democracies starting around 1980. But the control of inflation was not the outcome of monetarist policies. Rather, central banks and governments around the world looked at the high inflation, concluded it had not done anyone any good, and decided to do something about it—using, by the way, very Keynesian tools. In any case, the inflation problem did contribute to the rise of monetarism. The new classical critique started in a similar way. If you look at the Lucas critique article, he provides three examples: the Phillips curve, the investment function, and the consumption function. But it was the Phillips curve that got all the attention. Why? Because inflation was accelerating and the old fashioned Phillips curves were, according to Lucas, missing the boat. So Lucas, Sargent, Barro and others said, "Look we have the answer. This is why inflation is so bad." I didn't agree, but their ideas caught on.

Another example is more recent. To the extent that real business cycle theory has any roots in reality, the supply shocks of the 1970s were surely the biggest supply-side influences that produced business cycles. So these

events had something to do with the initial attractiveness of real business cycle analysis.

Again, monetarism died under the weight of events. You could hear what they said and see what happened, and they were just dead wrong. Similar problems killed new classical economics in the early 1980s. It was abundantly clear to anyone who wanted to look at events crudely, like a historian, or look at them econometrically, like a statistician, that nominal things like the money supply influenced real variables. It was very hard to deny that. Remember when Margaret Thatcher said we are going to stop inflation with tight money. Well, that episode sure looked like it demonstrated that a fully anticipated tightening of money had very real effects on unemployment and GDP in Britain! Paul Volcker did the same in the US, and with the same result on unemployment and GDP. So these events, with a lag, had dramatic effects on the way ideas evolved.

Having said all this, I don't want to deny the other influence, of course—that ideas influence events. There is also another important point to take into consideration. There is no doubt that in many cases the new classical Lucasian ideas were intellectually niftier than the mainstream macroeconomics of the neoclassical synthesis. For example, Keynesians often just assume that wages and prices are sticky. They say, "We don't know why they are sticky but let's just get on with it." To some economists, this is not a satisfactory approach to price stickiness.

## The Great Depression

*We talked earlier about the relationship between events and ideas. Clearly the Great Depression was a massive influence on the development of macroeconomics as a discipline and also gave birth to Keynesianism. Ben Bernanke [1995] has said that to find an explanation of the Great Depression is the 'holy grail' of macroeconomics. There is a fair amount of consensus that it was a fall in aggregate demand which caused this catastrophe. However, there has been a lot of controversy about why and how aggregate demand declined. Did the driving force behind the decline in aggregate demand come from consumption, investment, the money supply, financial fragility, the influence of the gold standard or the 1929 Stock Market Crash? [see Snowdon, 2000]. What are your thoughts on this issue and in particular do you think this experience demonstrated the weakness or the power of monetary factors and policy in both causing the Great Depression and promoting a recovery?*

That's a hard question. I think what this period does show is the impact of incompetent monetary policy. The lack of understanding of how to do monetary policy is clearly evident. I believe, as many economists believe, that as far as the US economy is concerned it was the disastrous conduct of monetary policy which made the Great Depression last so long and become so deep. It wasn't monetary policy that started the whole thing off, but it was a major reason why the US stayed down in a deep hole for so long. So, in that sense, the Great Depression illustrates the power of monetary policy. I don't agree, subject to reading more of Ben's work that might persuade me otherwise, that it was finally the power of monetary policy that got us out of the Great Depression. What finally got us out was the mobilisation for the war.

*In looking at the US recovery from the Great Depression, Christina Romer [1992] has stressed the importance of the switch to a more expansionary monetary policy in the US after 1933, stimulated in part by a large inflow of gold from an increasingly politically uncertain Europe.*

Sure, but it's hard to make a sharp distinction between monetary policy and fiscal policy in those years. The money supply was growing very rapidly because we were running big budget deficits to finance pre-war expenditures and financing a sizeable chunk of that by creating money. So that makes it hard to distinguish between the monetary and fiscal influences.

*Christina Romer [1986a, 1986b] also created a considerable stir in the mid 1980s by presenting new evidence which challenged the consensus view regarding the extent to which stabilisation had been achieved post 1945 compared with pre 1914. She suggests that the instability of the earlier period was largely a 'figment of the data'. This means that the post 1945 improvement is not as dramatic as had been claimed, but also that the Great Depression is an even more dramatic event compared to the pre 1914 period. The Great Depression was not just a very bad depression following on from an earlier period characterised by great instability. It really was unique. Are you convinced by Christina Romer's research and conclusions on this issue?*

I don't think that even Christie, even in her most enthusiastic moments, would claim that the economy was more stable pre-war than post-war. I interpret her claim to be that the margin of improvement was pretty narrow. I think the outcome of that debate, although I have not read all the latest rounds, was that there is some reduction in variance in the post 1945 period compared to the pre 1914 period, but the reduction was not as dramatic as the raw unadjusted data suggested. She admits there was some improvement, I believe.

## Keynes and Keynesianism

*In your interview with Arjo Klamer [1984], conducted in July 1982, you were described by him as 'an outspoken advocate of Keynesian policies' and as a 'younger neo Keynesian economist'. What made you a staunch defender of Keynesian economics and how would you describe yourself today, in January 2000?*

First of all, when I was growing up in macroeconomics just about everybody was a Keynesian. It was the only game in town—except for monetarism. So it was natural to be a Keynesian in those days. I used to joke that I was one of the people who never grew out of it (*laughter*). That was my attitude around 1982. Now I can be less apologetic because Keynesian-style thinking and conclusions are much more in vogue. So becoming a Keynesian was like passing through puberty (*laughter*). It was just the way life went. The only question was whether you stayed there or drifted away. I didn't drift away because I didn't think the other pretenders to the macroeconomic throne held up empirically. The notable exception is the Friedman-Phelps amendment to the Phillips curve, which I do not regard as anti-Keynesian in any case. I think that the ultimate test of a model is whether it holds up empirically, not how beautiful it is. So I was never persuaded that the monetarist, supply side, new classical, or real business cycle schools of thought held out anything very attractive empirically. This was in great contrast to the Friedman-Phelps view of the Phillips curve, which did.

*A lot of economists are not too keen on being put in a specific category even if other economists recognise them as belonging to a particular school of thought. So are you*

*still quite happy to call yourself a Keynesian and wear the label prominently on your lapel?*

Sure. I have never shunned the Keynesian label.

*One of the problems with the Keynesian label is that there are varieties of Keynesianism. The ideas of Post Keynesians, mainstream Keynesians and new Keynesians have some common elements but also differ in many respects in their emphasis. I tend to think of Paul Davidson, yourself and Greg Mankiw as probably the best representative sample of the three groups, although the gap is obviously biggest between Davidson and Blinder/Mankiw. What are the differences?*

I have already talked about new Keynesians. As I said before, I view that development as a way to give modern intellectual justification to old Keynesian ideas—like accepting that if you boost government spending then GDP will go up, not go down or stay the same. There is very little empirical evidence in the new Keynesian research that wasn't already there in the old Keynesian beliefs. They have mainly concentrated on providing microfoundations for Keynesian models.

I have always had trouble with the Post Keynesians. Their work is very historical and descriptive. It's easy to make seemingly profound statements about uncertainty that don't lead you anywhere. Sure, there is pervasive uncertainty out there in the real world. But what are we going to do about it? If we write down models of certainty, they are only meant to be allegorical. Nobody ever thought that the world was free from uncertainty—that somehow we know the true model.

*Post Keynesians also seem to be obsessed with correctly interpreting the General Theory and identifying the true message of the text. And their interpretation paints a much more radical picture of Keynes's message.*

Exactly. That's what I meant by historical. They want to know what is really in the good book. I just don't think that is all that important. It also leads to an endless and somewhat fruitless debate.

*Do you think students should still be encouraged to read the General Theory? In a previous interview with Lucas [1999] he gave a one word answer...No!*



Do you mean undergraduate or postgraduate students?

*I mean advanced undergraduates, seniors in the US, third year students in the UK, and graduate students.*

I am not sure. I used to think yes. It's certainly not a book for elementary students. But I am not so convinced anymore, simply because of the passage of time—that's all. When I started teaching economics in the early 1970s, there were about thirty-five post *General Theory* years. Now there are sixty four and there are more things to know about—not all of which are important, but some are worth knowing. Unlike computers, the human mind has not got faster and faster at processing information and storing it. So you have to jettison some things. I still think it is important for advanced students to have at least some familiarity with the *General Theory*, to have at least read excerpts. But I would not expect students to have studied the great book in depth, line by line. Physics students don't study Isaac Newton's *Principia*. So I agree with that point, it's not important to read Isaac Newton in order to understand modern Physics. But for students who specialise in macroeconomics, they should at least have looked at the book at some stage. What I do think is more important than that is for students to get a feel for the history of the discipline. Graduate students should not be thinking that Bob Lucas invented the neutrality of money in his influential 1972 paper. David Hume was talking about this in a sophisticated way in 1752! Students ought to have some idea that the macroeconomic world did not start in 1985 or some other recent year (*laughter*).

*A very important issue raised by Keynes in the General Theory is the possibility of involuntary unemployment. Many economists I have talked to, not all of them new classical by any means, now seem to share the view expressed by Lucas [1978] that macroeconomists should treat all unemployment as voluntary. Both Solow [1980] and yourself in your Richard Ely lecture [1988b] have defended the idea of involuntary unemployment as a useful concept. Does this concept still have a useful place in macroeconomic analysis? Is it something students ought to know about?*

Yes, although this is more true of lower-level than of higher-level students. I say that because there is a social context here. High unemployment of

humans is not quite the same as high unemployment of ballbearings. The unemployment of people has social consequences, and at some basic level we need to understand that. Now, if you study macroeconomics at graduate level you leave that idea behind in the economics kindergarten. So it's not necessary that graduate students spend a lot of time worrying about definitions. Involuntary unemployment is in fact hard to define. Does it mean that you would not want to work at any wage at all? We need a working definition of how to measure it and also recognise that it can be a slippery concept. I think Lucas once said something like: You can always sell apples at Grand Central Station. So, if you are not doing so, you must be voluntarily unemployed. That's one approach to a definition, but it does not strike me as being a very useful one. I do admit it is a legitimate intellectual question, which I took on in my 1988 Ely lecture. If you lose your job in the steel mill, why don't you go and sell apples? We should explore the reasons for that. I think what is important for doing macroeconomics is to use and understand the concept of 'slack'. You don't hear terminology such as involuntary unemployment so much anymore. You do hear terminology such as slack or simply high unemployment. The notion that resources are being wasted in a recession is to me fundamental to the Keynesian view of the world. Recessions are not the extended vacations implied by equilibrium business cycle theorists.

*Another concept that used to be central in macroeconomics is 'full employment'. Again you rarely if ever see this concept used in modern macroeconomics textbooks. Why has this happened?*

Well, now you get concepts such as NAIRU or the natural rate, and these amount to more or less the same thing in my view.

*In a recent Brookings Paper, Laurence Ball [1999] argued that 'monetary policy and other determinants of aggregate demand have long-run effects on unemployment'. This argument is related to the idea of hysteresis which unlike Friedman's natural rate hypothesis does allow aggregate demand to influence the natural rate or NAIRU. What are your views on the hysteresis explanations of unemployment?*

Until recently, I thought it had validity in Europe, but not in the United States—where our unemployment rate seemed to fluctuate cyclically

around a (roughly) unchanged NAIRU. But recent data have called this view into question. Maybe there is hysteresis even here. But I'm not sure. We need to let a few more years go by.

*Some of your research in recent years has been devoted to identifying the reasons for price stickiness [Blinder et al, 1998]. You have also suggested [1988a] that Keynesians have always been 'infidels in the neo-classical temple', a temple that assumes price flexibility. What has your research using surveys identified as the principal reasons for price rigidity or stickiness?*

A short answer to your question is that, according to these survey results, of the twelve theories tested, many of the ones which come out best have a Keynesian flavour. When you list the twelve theories in the order that the respondents liked and agreed with them, the first is co-ordination failure—which is a very Keynesian idea. The second relates to the simple mark-up pricing model, which I might say is a very British-Keynesian idea. Some of the reasons given for price stickiness are not Keynesian at all. For example, non-price competition during a recession. The Okun [1981] implicit contract idea is also very Keynesian. We also have a crude non-theoretical Keynesian reason, which amounts to saying 'prices are set in nominal terms, period'. So if you look at the top five reasons given by firms as to why prices are sticky, four of them look distinctly Keynesian in character.

If we then look toward the bottom of the list, to the least-favoured ideas, we see some of the modern theories, such as judging quality by price or that the marginal cost curve is flat. These are not well supported by the survey evidence. To the extent that you are prepared to believe survey results, and some people won't, I think this research strikes several blows in favour of Keynesian ideas. By 'Keynesian', I don't mean by that just what came out of the good book itself, but ideas that a modern economist would recognise as Keynesian thinking.

*At the end of the day, empirically, we observe sticky prices outside of commodity and financial markets. Is that observation in itself sufficient or must we have micro-foundations for price stickiness?*

It's good enough to start with. I would like to have better and deeper theoretical explanations that we could believe and had some empirical support. That's what my recent book, *Asking About Prices [1998]*, is all about. The parts of macroeconomics that took off in the 1970s and 1980s were those based on denying sticky prices. The attitude of new classical and real business cycle theorists seemed to be... 'if you don't have a coherent theoretical explanation of sticky prices then it cannot be true that prices are sticky'. Personally I found that approach unscientific.

*The first Nobel Prize for economics was awarded in 1969 just about the time you were starting your graduate studies at MIT. If Keynes had still been living aged about 85 do you think he would have been the first recipient?*

I don't think there is any doubt that he would, at least not in my mind.

*Charles Plosser [1994] thought the success of Keynes was in large part due to the work of John Hicks [1937] and also Irving Fisher, had he been alive, was a better candidate than Keynes. He also mentioned other twentieth century economists such as Pareto and Wicksell who might be better candidates.*

When Will Baumol and I first wrote the first edition of our principles textbook in 1979 we decided to include profiles of four great economists, and we concluded that Keynes was the most influential economist of the 20th century and one of the four most influential of all time.

## **Lessons from the 'Great Inflation'**

*Another fascinating period with respect to the interaction of events and ideas is the period 1967–75. This is the period between the Friedman-Phelps theoretical contribution and the peak of 1970s inflation rates in most of the G7 countries. This was also the period when you were moving from being a student of economics to becoming a professional economist. It is also the period when we see open warfare between Friedman and Tobin, the first new classical contributions of Lucas and the impact of the first OPEC supply shock. During this time we also witness a transition within Keynesian economics towards an acceptance of the Friedman-Phelps critique of the old style Phillips curve. You have argued that by the mid 1970s the Keynesian model had been modified to take on board the impact of supply stocks*

*and the expectations augmented version of the Phillips curve. As a result, you argue that this modified Keynesian model was quite capable of explaining the events of the 1970s [Blinder, 1992b]. So in your view Lucas and Sargent were certainly premature, indeed mistaken, to pronounce the death of Keynesian economics in their 1978 paper. Is this a fair representation of your view of this period?*

Yes, unless you count as Keynesian economics the old fashioned view of the Phillips curve with a negative long-run slope. The Keynesian revolution was, in some sense, the supplanting of concentration on supply-side factors by concentration on demand-side factors. Part and parcel of the Keynesian view of the world is that demand-side factors dominate economic fluctuations. We did go through a period of time, although it seems brief in retrospect, when demand factors were not dominant and supply shocks were important. These factors had to be incorporated into the Friedman-Phelps critique of the Phillips curve.

*There are a variety of explanations of how the ‘Great Inflation’ of the 1970s got started, ranging from policy errors, the influence of the OPEC supply-shocks [Blinder 1979] and the use of the wrong model for policy purposes [Taylor, 1997; Mayer, 1999]. Bradford de Long [1997] sees the ‘Great Inflation’ as the inevitable result of what he calls the ‘Shadow of the Great Depression’. Do you think the idea that the excessive application of the ‘new economics’ in the 1960s created the ‘Great Inflation’?*

I think this idea is mostly wrong. If you go back and look at the history of this period, you will see that by about 1965 the US had more or less reached full employment combined with very low inflation. Then, on top of that, you had Vietnam war spending which, contrary to the advice he was given by extremely Keynesian economists, Lyndon Johnson refused to finance by raising taxes. He preferred to lean on the Fed to monetize the deficit. So we got the beginnings of the inflation from a purely Keynesian mechanism: excess aggregate demand. Now, as you said, Keynesianism has its roots in the Great Depression, when all the emphasis was on inadequate demand. But, if you turn Keynesian theory around the other way, it says that events such as those that happened in the US between 1964 and 1968 should lead to higher inflation—and they did. It is true that in the background there was also an incorrect view of the

Phillips curve, which is what I presume John Taylor and Thomas Mayer are referring to. This incorrect view of the Phillips curve was that you could have a *permanent* trade-off between inflation and unemployment. I think that idea played some part in what happened, but I would not give it the major role. The US had a combination of a relatively small recession, beginning in 1969–70, and the Nixon wage and price controls. As a result, inflation fell quite a bit. But in 1973 we were hit by the first OPEC oil price shock. So I think that the role of bad Keynesian theory in producing the 1970s inflation, while not zero, was rather modest.

*As you mentioned earlier, the rise of monetarism is in large part linked to the acceleration of inflation in the 1970s. In 1981 you did an interview for Challenge where you declared monetarism to be 'obsolete'. Bradford de Long [2000] has a forthcoming paper entitled 'The Triumph of Monetarism'. In that paper he argues that what we now call new Keynesian economics could justifiably be labelled 'new monetarist' economics. The same point is also made by Mankiw and Romer [1991] in their introduction to the edited collection of papers on new Keynesian economics. This is because many of Friedman's ideas have now been absorbed into the mainstream so that we no longer talk about monetarism as a separate idea anymore. Which parts of monetarism have not been digested by the mainstream?*

The part of monetarism which is obsolete is the part which says that the principal macroeconomic relationship on which we should rely is the stable demand for money. Various things follow from that, such as the idea that the best monetary policy you can have is one where the money supply, if you can define it, grows at a constant rate. That's a major policy prescription of monetarism. Another less important strain of monetarism is the denial that fiscal policy actions can influence aggregate demand. That denial fuelled the old Keynesian-monetarist debate for some time. I believe that debate was clearly resolved to show that the monetarists were wrong on fiscal policy. They were also shown to be wrong on the stable demand for money function. So I think what Brad means is that today very few economists denigrate the importance of monetary policy, while there were many who did so back in the 1930s, 40s and 50s. These people were called 'crude Keynesians' and most of them were British (*laughter*). They were never a big group, but there were such people. The end of crude Keynesianism was mostly biological—they grew old, retired, and died.

There were some American crude Keynesians, but most were located in Cambridge, England. I don't know if that's what Brad meant, but most economists these days think first of monetary policy when they think of government policy to influence aggregate demand.

## Macroeconomic policy advice

*Bob Solow [1997] has described economic advisors to government as 'intellectual sanitation cleaners'. In your Hard Heads, Soft Hearts book [1987] you talk about 'Murphy's Law' i.e. that economists' advice is most influential when they know the least and least influential when they are most agreed and know the most. What did you learn about policy advice in the real world when you were a member of the President's Council of Economic Advisors?*

Do you have about four hours for me to answer that (*laughter*)? First of all, I learned that policy advice is unlikely to be taken unless the political winds are blowing in the right direction. It's not impossible, but it's not very likely. What I mean is that it is very easy to be overwhelmed by political forces pushing in the opposite direction. If you are pushing against these forces, it's going to be very difficult to get good economic advice adopted. Second, however, if good economic policy is practised, the effects are likely to be very agreeable for the electorate and hence for the politicians. You don't hear as many economist jokes now as you did in the 1970s and 1980s. Now there are many more jokes about lawyers (*laughter*). A period like the 1960s was a good time to be an economist because things were going very well, at least until the late 1960s. As far as the late 1990s are concerned, things are going very well because Bill Clinton followed some pretty good economic advice. So did Alan Greenspan, although in his case the advice was coming mostly from his own head. Alan Greenspan wouldn't call himself a Keynesian, but he sure as hell is one (*laughter*).

*Let's turn to your time at the Fed. Macroeconomists have written a lot about political distortions which may affect macroeconomic policy both for opportunistic electoral considerations or for partisan or ideological reasons [see Snowden and Vane, 1999b]. I am thinking here of the work of people like William Nordhaus [1975] and Alberto Alesina [1987]. With reference to your own experience at the*

*Fed, do you think that political forces influence monetary policy decisions? In other words, how independent is the Fed?*

Although I cannot speak for other central banks, or for the Fed at other times, in my experience the political influence on monetary policy is trivial, next to zero. It was certainly something that I was wondering about when I took up the position at the Fed. I was very pleased to see how infrequently any political influence was ever even brought up in discussions relating to the conduct of monetary policy.

*But what about the Richard Nixon—Arthur Burns period?*

I think it was not true in the Nixon period (*laughter*). Then you had an extremely political chairman of the Fed. Alan Greenspan is very political, but in a different kind of way. He never seems to be tied to the fortunes of one particular President like Burns was.

*In your recent book containing your Lionel Robbins lectures on your experience working at the Fed, you make a great deal of how useful the Tinbergen [1952]—Theil [1961] framework is for thinking about the conduct of monetary policy. In the American Economic Review symposium on ‘A Core of Practical Macroeconomics’, to which you contributed [Blinder, 1997a], and in contrast to your view, Martin Eichenbaum [1997] argued that during the 1970s a critical change in methodology took place with respect to economists’ approach to the analysis of stabilisation policy. This was the switch to ‘thinking about stabilisation policy as a game-theoretic problem, rather than control theory problem’. Eichenbaum clearly sees this as a constructive change in approach. In your paper ‘What Central Bankers Could Learn from Academics and Vice Versa’ [1997b] you are very critical of the time inconsistency literature suggesting ‘economists have been barking up the wrong tree’. This view is also shared to a certain extent by Charles Goodhart [1994]. Why are you not convinced by this game theoretic monetary policy literature?*

I don’t agree with Marty on this, although his comment does accurately reflect much of the academic literature. But I don’t think policymakers do or should think of monetary policy in a game-theoretic way—at least, not literally. I sometimes put it this way: if I am at the Central Bank making



monetary policy decisions, and I am in a game against an adversary, who is my adversary? There isn't an adversary out there; there is an economy. What is correct in the new approach, which the older approach missed, is that those other actors, who are not adversaries of the Central Bank, are looking at the Central Bank and forming expectations about what it is likely to do. Therefore it is important for any Central Bank to take those expectations into account—which the simplest versions of dynamic programming do not. But I find a lot of the literature that views the Central Bank and the private sector as locked in a game against each other unconvincing.

*One of the things that the Robert Barro and David Gordon [1983] framework suggests is that the Central Bank attempts to target unemployment below the natural rate because the natural rate may be too high due to supply-side distortions. Do you not see this as a real problem in practice? For example, the current [January 2000] rate of unemployment in the US is 4.1 per cent and most economists who try and estimate the natural rate of unemployment come up with numbers in the 5–6 per cent range. Until recently, Robert Gordon's [1997, 1998] work suggested that the natural rate of unemployment for the US economy was about 6 per cent. Why has Alan Greenspan allowed unemployment to fall to levels below what most economists estimated to be the natural rate or NAIRU?*

In the Barro-Gordon framework the natural rate of unemployment is a number known to everybody. The Central Bank, knowing that number, then targets monetary policy to achieve a level of unemployment below the natural rate. This is a description of no central bank that I have ever heard of. What is happening now is that there is pervasive uncertainty about where the natural rate is. The analogous Barro-Gordon question would be 'Does the Central Bank systematically take its gambles on the low side?'. So, if I have a density function of where the natural rate is, should I play only on the low side of that distribution? I guess you could argue that the Fed has done that and taken a lot of chances. But I don't think that is a correct interpretation for a couple of reasons. First, inflation has not in fact been accelerating. So are we so sure we are on the low side? Second, from the Fed's point of view, this very large reduction in the unemployment rate is largely accidental. The Fed's forecasts are published (after a lag) so we know that the economy has consistently grown

faster than the Fed expected. The question for the Fed in this situation is, 'Shall we call a halt to this?' So the Fed did not deliberately push the unemployment rate this low, and inflation, at least so far, has not accelerated. The US Fed, unlike the 'Fed' over in Frankfurt and the 'Fed' over in Tokyo, let the very low unemployment persist rather than getting rid of it. Alan Greenspan had a hunch that we could live with such low unemployment without having to pay the inflationary piper, and so far he has been right.

*You have argued in support of the idea that an independent Central Bank is a good idea as far as instrument independence is concerned, but that democratically elected politicians should determine the goals. In a recent paper by Christina and David Romer [1997], they argue a case for goal independence for central banks because they believe that it is important that advances in economic understanding be rapidly incorporated into decision making. In their institutional set up specialists will have 'discretion about both the ultimate goals of policy and the specifics of policy operations'. Their proposals also allow for frequent evaluation of policymakers' performance. Do you find this proposal attractive?*

No, I do not agree with that. I think that there ought to be expert advisors helping non-experts make judgements about the choice of inflation target and estimates of things like the natural rate of unemployment. But ultimately, since some of these decisions are heavily influenced by value judgements or distributional judgements, they have to be political decisions. To put it concretely, if the electorate really liked six per cent inflation rather than one per cent inflation, who are we as economists to say that is the wrong target? In fact, I think the electorate actually prefers one per cent inflation to six per cent. But if it was the other way around, then I think the central bank should take its marching orders from the politicians and produce six per cent inflation.

*Relating to that point, it seems that the public does not like inflation, as surveys have shown. Robert Shiller's [1997] research also indicates that the general public does not understand the real economic costs of inflation. Economists' understanding of the costs of inflation does not match up with non-economists' lack of understanding.*

Yes I'm with you here. I read Shiller's survey as saying that the public is hopelessly confused about inflation. Paradoxically, that contains the seed of an argument for higher inflation: if people hate inflation, but are wrong about the true costs of inflation, then if they understood it better, they would not hate it so much (*laughter*).

*Economists have not been very successful in producing convincing evidence that moderate inflation is costly. In particular, the arguments in favour of getting inflation down from two per cent to zero are, to say the least, highly controversial. Feldstein [1999] believes in a zero inflation target but economists such as George Akerlof, Ben Bernanke, Bradford De Long, and Paul Krugman and many others favour a positive inflation target, say about two per cent [Akerlof et al, 1996; Bernanke et al, 1999; De Long, 1999; Krugman, 1998, 1999]. Their arguments relate to the threat of deflation and problems which occur with zero inflation due to the floor in the nominal interest rate and labour market inflexibility caused by wage rigidity. What kind of view does the Fed have on the choice of inflation targets?*

I think the Fed is quite happy with the idea of aiming at keeping inflation in the range of two per cent. The revealed preference of Alan Greenspan suggests this is the case. He has in the past said favourable things about zero inflation, but not recently. There is no evidence that Alan Greenspan is shooting either actively or passively for zero inflation. If we were, he would never have allowed the economy to grow so strongly for so long.

*To what extent does the Fed follow a strategy based on something like a Taylor [1993] rule?*

Very considerably, but not consciously. Many people have estimated the reaction functions of the Fed, and they look a lot like Taylor rules.

*Does the Fed follow a policy of implicit inflation targeting as implied by Mishkin [1999], or is it a 'just do it' kind of pragmatic strategy?*

As they say in poker, I'll see you and raise you on that. You are right, but I would go one step further, to the Taylor Rule which says we here at the central bank care about inflation, so we have an inflation target. We also

have an employment target, and lately a very elastic one. When I was at the Fed, the unemployment target would have been more like six per cent. But as it has looked to Alan Greenspan and others that we could sustain lower unemployment they have made their unemployed target more aggressive.

*What are the main determinants of Central Bank credibility?*

First, nothing succeeds like success. Second, a track record of doing what you say you will do. But to accumulate such a record, you have to say what you intend to do.

*A couple of years ago [1997c] you wrote an interesting paper for Foreign Affairs entitled 'Is Government too political?' and recently The Economist (27th November 1999) picked up on it. In that paper you discuss the idea of giving some aspects of fiscal policy to an independent body. I think we all recognise that what often makes good economic policy may in the short run make for bad politics and vice versa. Do you really think that politicians would hand over some of their fiscal responsibilities to a group of experts?*

That paper was really a radical thought experiment. Contrary to what *The Economist* suggested, I did not necessarily advocate this idea but rather raised it, in a deliberately provocative way, as something worth thinking about. In reality, I am somewhat sceptical that politicians would ever hand over some of their fiscal powers, although Australia is trying something along these lines now. The Business Council of Australia actually has a proposal to hand over the level of taxation, but not the structure, to a panel of experts in order to regulate it rather like monetary policy. So we will see what happens in Australia. But I remain sceptical that American politicians would be willing to do that. However, as I point out in that article, there are a number of instances in which they have done something like this, usually where for some reason or another they have thought it politically wise to get rid of the power. One example I give is the closure of defence installations. Nobody wants to close down a defence installation in their own Congressional district. But they all realised that, following the end of the Cold War, this was going to happen. So politicians turned the decision over to a panel of experts. There is also an asymmetry in that politicians

would always prefer to hand over to a panel of experts decisions relating to an increase in taxes and leave decisions on tax cuts to themselves. They want the credit for the tax cuts, but would like to fob off the blame for tax increases to someone else. So when I balance all these things out, I think it is doubtful that the US will adopt this idea.

## **A 'new' US economy?**

*Do you believe, as some commentators do, that the US now has a 'new economy' with a permanently higher growth rate than the past? The US has been viewed as having a 'Goldilocks' economy with low unemployment and inflation together with an underlying surge in productivity growth.*

Yes, but its very easy to exaggerate. I certainly do not come anywhere close to subscribing to the *Wired Magazine* view of the world that we are in a long perpetual boom. But I do think that the natural rate of unemployment has fallen, the econometric evidence points to that conclusion. Some of that fall in unemployment we understand, such as the part due to the ageing population. But some of it we don't understand and some of the micro-economic attempts at explanation do not get you near to a natural rate of 4.2 per cent. It's still an open question as to whether the US has a natural rate so low as that because we have had a lot of supply shocks. These have now dissipated so let's see what happens. If in fact the natural rate is 4.2 per cent then as economists we have a lot of microeconomic explaining to do because at the moment we are not even close to a coherent explanation. On the productivity issue I think there is supportive evidence on this although not of the magnitude suggested and talked about in *Business Week* for the last four years. But in the data of the last few years there is not enough evidence to conclude at the 95 per cent confidence level that we have had a significant acceleration of productivity. But if you are a little less stubborn than that there is now statistical evidence for a rise in the productivity trend which I would peg at 35 to 50 basis points and rising, because as we are getting more data that number is getting bigger. I think a prudent estimate now of the rise in productivity trend, from what it was in the 1973–95 period, is around 35–50 basis points, but I would not be shocked if three years from now I was answering 75–80 basis points. It's moving that way but I don't know if it will happen.

*The current expansion which began in March 1991 is now [January 2000] as long as the previous record breaking expansion of the 1960s and soon will become the longest continuous expansion in US history. Will it all end in tears or do you see the possibility of a soft landing?*

I see a good prospect of a soft landing. When we were trying to soft land the economy when I was on the Fed it became clear to me that when you are trying to achieve this you have to be both lucky and good at what you are doing. The Fed was very skilful in 1994–95 and also had the good fortune of not having any dramatic downdrafts or updrafts as it was trying to land the economy. As any pilot will tell you landing is a tricky business in bad weather. I think the Fed is still going to be skilful and the main question is will its luck hold out and nobody knows about that. So I think that we have a fighting chance of another soft landing. The interesting question now is, ‘where is the runway?’. We were talking about this earlier in relation to changing estimates of the natural rate of unemployment. Could we really be wanting to land the economy at 4.0 per cent permanent unemployment? Boy I hope so but I am not too sure.

## References

Akerlof, G. A., W.T. Dickens and G. L. Perry (1996), ‘The macroeconomics of low inflation’, *Brookings Papers on Economic Activity*, No.1.

Alesina, A. (1987), ‘Macroeconomic policy in a two-party system as a repeated game’, *Quarterly Journal of Economics*, August.

Ando, A. and F. Modigliani (1963), ‘The “Life-Cycle” hypothesis of saving: Aggregate implications and tests’, *American Economic Review*, March.

Backhouse, R.E. (1997a), ‘The rhetoric and methodology of modern macroeconomics’ in B. Snowdon and H.R. Vane (eds), *Reflections on the Development of Modern Macroeconomics*, Cheltenham: Edward Elgar.

Ball, L. (1999), ‘Aggregate demand and long-run unemployment’, *Brookings Papers on Economic Activity*, No. 2.

Barro, R.J. (1977), ‘Unanticipated money growth and unemployment in the United States’, *American Economic Review*, March.

Barro, R.J. (1978), ‘Unanticipated money, output and the price level in the United States’, *Journal of Political Economy*, August.

Barro, R.J. and D.B. Gordon (1983), 'Rules, discretion and reputation in a model of monetary policy', *Journal of Monetary Economics*, July.

Baumol, W. and A.S. Blinder (1979), *Economics: Principles and Policy*, New York: Harcourt Brace Jovanovitch.

Bernanke, B.S. (1995), 'The macroeconomics of the Great Depression: A comparative approach', *Journal of Money, Credit and Banking*, February.

Bernanke, B.S., T. Laubach, F. Mishkin and A. Posen (eds) (1999), *Inflation Targeting: Lessons from the International Experiences*, Princeton: Princeton University Press.

Blanchard, O. (2000), 'What do we know about macroeconomics that Fisher and Wicksell did not?', *Quarterly Journal of Economics*, November.

Blinder, A.S. (1979), *Economic Policy and the Great Stagflation*, Academic Press.

Blinder, A.S. (1981), 'Monetarism is obsolete', *Challenge*, September/October.

Blinder, A.S. (1986), 'Keynes after Lucas', *Eastern Economic Journal*, July–September.

Blinder, A.S. (1987), *Hard Heads, Soft Hearts: Tough-Minded Economics for a Just Society*, New York: Addison Wesley.

Blinder, A.S. (1988a), 'The fall and rise of Keynesian economics', *Economic Record*, December.

Blinder, A.S. (1988b), 'The challenge of high unemployment', *American Economic Review*, May.

Blinder, A.S. (1992a), 'A Keynesian restoration is here', *Challenge*, September/October.

Blinder, A.S. (1992b), 'Deja vu all over again' in M. Belongia and M. Garfinkel (eds), *The Business Cycle: Theories and Evidence*, London: Kluwer Academic Publishers.

Blinder, A.S. (1997a), 'Is there a core of practical macroeconomics that we should all believe?' *American Economic Review*, May.

Blinder, A.S. (1997b), 'What central bankers can learn from academics and vice versa', *Journal of Economic Perspectives*, Spring.

Blinder, A.S. (1997c), 'Is government too political?' *Foreign Affairs*, November/December.

Blinder, A.S. (1998), *Central Banking in Theory and Practice*, MIT Press.

Blinder, A.S., E. Canettio, D. Lebow and J. Rudd (1998), *Asking About Prices: A New Approach to Understanding Price Stickiness*, Russell Sage Foundation.

De Long, J. B. (1997), 'America's peacetime inflation: The 1970s', in C.D. Romer and D. Romer (eds), *Reducing Inflation: Motivation and Strategy*, Chicago: University of Chicago Press.

De Long, J. B. (1999), 'Why we should fear deflation', *Brookings Papers on Economic Activity*, No. 1.

De Long, J.B. (2000), 'The triumph of monetarism', *Journal of Economic Perspectives*, Winter.

Denison, E. F. (1967), *Why Growth Rates Differ: Post-war Experience in Nine Western Countries*, Washington DC: Brookings Institution.

Eichenbaum, M. (1997), 'Some thoughts on practical stabilisation policy', *American Economic Review*, May.

Feldstein, M. (ed.) (1999), *The Costs and Benefits of Price Stability*, Chicago: University of Chicago Press.

Friedman, M. (1968), 'The role of monetary policy', *American Economic Review*, March.

Goodhart, C.A.E. (1994), 'Game theory for central bankers: A report to the Governor of the Bank of England', *Journal of Economic Literature*, March.

Gordon, R.J. (1972), 'Wage and price controls and the shifting Phillips curve', *Brookings Papers on Economic Activity*.

Gordon, R.J. (1975), 'Alternative responses to external supply shocks', *Brookings Papers on Economic Activity*.

Gordon, R.J. (1990), 'What is New-Keynesian Economics?' *Journal of Economic Literature*, September.

Gordon, R.J. (1997), 'The time-varying NAIRU and its implications for economic policy', *Journal of Economic Perspectives*, Winter.

Gordon, R. J. (1998), 'Foundations of the Goldilocks economy: Supply shocks and the time-varying NAIRU', *Brookings Papers on Economic Activity*, No. 2.

Hansen, L. P. and T.J.Sargent (1980), 'Estimating and formulating dynamic linear rational expectations models', *Journal of Economic Dynamics and Control*, Vol.2, No. 1.



- Hicks, J.R. (1937), 'Mr Keynes and the "Classics": A Suggested Interpretation', *Econometrica*, April.
- Hume, D. (1752), 'Of money', reprinted in A.A. Walters (ed, 1973), *Money and Banking*, Harmondsworth: Penguin.
- Jorgenson, D.W. (1963), 'Capital theory and investment behaviour', *American Economic Review*, May.
- Keynes, J.M. (1926), *The End of Laissez-Faire*, London: Hogarth Press.
- Keynes, J.M. (1936), *The General Theory of Employment, Interest and Money*, London: Macmillan.
- Klamer, A. (1984), *The New Classical Macroeconomics: Conversations with New Classical Economists and their Opponents*, Brighton: Harvester Wheatsheaf.
- Klein, L.R. and A.S. Goldberger (1955), *An Econometric Model of the United States*, New York: Wiley.
- Krugman, P. (1998), 'Its Baaack! Japan's slump and the return of the liquidity trap', *Brookings Papers on Economic Activity*, No. 2.
- Krugman, P. (1999), *The Return of Depression Economics*, New York: W.W. Norton.
- Kydland, F.E. and E.C. Prescott (1982), 'Time to build and aggregate fluctuations', *Econometrica*, November.
- Laidler, D. (1986) 'The new classical contribution to macroeconomics', *Banca Nazionale del Lavoro Quarterly Review*, March.
- Lucas, R.E. Jr. (1972), 'Expectations and the neutrality of money', *Journal of Economic Theory*, April.
- Lucas, R.E. Jr. (1973), 'Some international evidence on output-inflation tradeoffs', *American Economic Review*, June.
- Lucas, R.E. Jr. (1976), 'Econometric policy evaluation: A critique', in K. Brunner and A.H. Meltzer (eds), *The Phillips Curve and Labour Markets*, Amsterdam: North Holland.
- Lucas, R.E. Jr. (1978), 'Unemployment policy', *American Economic Review*, May.
- Lucas, R.E. Jr. (1980a), 'The death of Keynesian economics: Issues and ideas', *University of Chicago*, Winter.

Lucas, R.E. Jr. (1988), 'On the mechanics of economic development', *Journal of Monetary Economics*, July.

Lucas, R.E. Jr. (1999), 'Interview with Robert Lucas', in B. Snowdon, H.R. Vane, *Conversations with Leading Economists: Interpreting Modern Macroeconomics*, Cheltenham: Edward Elgar.

Lucas, R.E. Jr. and L. Rapping (1969), 'Real wages, employment and inflation', *Journal of Political Economy*, September/October.

Lucas, R.E. Jr. and T.J. Sargent (1978), 'After Keynesian macroeconomics' in *After the Phillips Curve: Persistence of High Inflation and High Unemployment*, Boston, MA: Federal Reserve Bank of Boston.

Mankiw, N.G. (1992), 'The reincarnation of Keynesian economics', *European Economic Review*, April.

Mankiw, N.G. (2000), *Macroeconomics*, 4th edn, New York:Worth.

Mankiw, N.G. and D. Romer (eds) (1991), *New Keynesian Economics*, Cambridge: MIT Press.

Mayer, T. (1997), 'What remains of the monetarist counter-revolution?', in B. Snowdon and H.R. Vane (eds), *Reflections on the Development of Modern Macroeconomics*, Cheltenham: Edward Elgar.

Mayer, T. (1999), *Monetary Policy and the Great Inflation in the United States*, Cheltenham: Edward Elgar.

Mishkin, F.S. (1999), 'International experiences with different monetary regimes', *Journal of Monetary Economics*, June.

Nordhaus, W.D. (1975), 'The political business cycle', *Review of Economic Studies*, April.

Okun, A. (1981), *Prices and Quantities: A Macroeconomic Analysis*, Washington DC: Brookings Institution.

Phelps, E.S. (1967), 'Phillips curves, expectations of inflation and optimal unemployment over time,' *Economica*, August.

Phelps, E.S. (1968), 'Money wage dynamics and labour market equilibrium', *Journal of Political Economy*, August.

Phelps, E. S. (1972), *Inflation Policy and Unemployment Theory*, New York: W.W. Norton.

- Phelps, E.S. (1978), 'Commodity-supply shock and full employment monetary policy', *Journal of Money, Credit and Banking*, May.
- Phillips, A.W. (1958), 'The relationship between unemployment and the rate of change of money wages rates in the United Kingdom, 1861–1957', *Economica*, November.
- Plosser, C. (1994), 'Interview with Charles Plosser', in B. Snowdon, H. R. Vane and P. Wynarczyk, *A Modern Guide to Macroeconomics: A Guide to Competing Schools of Thought*, Cheltenham: Edward Elgar.
- Romer, C.D. (1986a), 'Spurious volatility in historical unemployment data', *Journal of Political Economy*, February.
- Romer, C.D. (1986b), 'Is the stabilisation of the post-war economy a figment of the data?', *American Economic Review*, June.
- Romer, C.D. (1992), 'What ended the Great Depression?', *Journal of Economic History*, December.
- Romer, C.D. and D. Romer (1997), 'Institutions for monetary stability', in C. D. Romer and D. Romer (eds), *Reducing Inflation: Motivation and Strategy*, Chicago: University of Chicago Press.
- Romer, P. (1986), 'Increasing returns and long-run growth', *Journal of Political Economy*, October.
- Sachs, J. D. (1999), 'Twentieth-century political economy: A brief history of global capitalism', *Oxford Review of Economic Policy*, Vol. 15, No. 4.
- Sargent, T.J. and N. Wallace (1975), 'Rational expectations, the optimal monetary instrument, and the optimal money supply rule', *Journal of Political Economy*, March/April.
- Sargent, T.J. and N. Wallace (1976), 'Rational expectations and the theory of economic policy', *Journal of Monetary Economics*, April.
- Shaw, G.K. (1997), 'How relevant is Keynesian economics today?' in B. Snowdon and H.R. Vane (eds), *Reflections on the Development of Modern Macroeconomics*, Cheltenham: Edward Elgar.
- Shiller, R.J. (1997), 'Why do people dislike inflation?', in C.D. Romer and D. Romer (eds), *Reducing Inflation: Motivation and Strategy*, Chicago: University of Chicago Press.
- Sims, C. (1980), 'Comparisons of inter-war and post-war business cycles: Monetarism reconsidered', *American Economic Review*, May.
- Smith, A. (1776), *An Inquiry into the Nature and Causes of the Wealth of Nations*, edited by R.H. Campbell and A.S. Skinner, 1976, Oxford: Clarendon.

Snowdon, B. (2000), 'The international economic system in the twentieth century: An interview with Barry Eichengreen', *World Economics*, July/September.

Snowdon, B. (2002, forthcoming), *Conversations on Growth, Stability and Trade*, Cheltenham: Edward Elgar.

Snowdon, B. and H.R.Vane (1996), 'The development of modern macroeconomics: Reflections in the light of Johnson's analysis', *Journal of Macroeconomics*, Summer.

Snowdon, B. and H. R. Vane (eds.)(1997), *A Macroeconomics Reader*, London: Routledge.

Snowdon, B. and H.R. Vane (1999a), *Conversations With Leading Economists: Interpreting Modern Macroeconomics*, Cheltenham: Edward Elgar.

Snowdon, B. and H.R. Vane (1999b), 'The new political macroeconomics', *American Economist*, Spring.

Snowdon, B., H.R. Vane and P. Wynarczyk (1994), *A Modern Guide to Macroeconomics: An Introduction to Competing Schools of Thought*, Cheltenham: Edward Elgar.

Solow, R.M. (1956), 'A contribution to the theory of economic growth', *Quarterly Journal of Economics*, February.

Solow, R. M. (1957), 'Technical change and the aggregate production function', *Review of Economics and Statistics*, August.

Solow, R.M. (1980), 'On theories of unemployment', *American Economic Review*, March.

Solow, R. M. (1997), 'It ain't the things you don't know that hurt you, it's the things you know that ain't so', *American Economic Review*, May.

Taylor, J.B. (1993), 'Discretion versus policy rules in practice', *Carnegie Rochester Conference Series on Public Policy*, Amsterdam: North Holland.

Taylor, J. B. (1997), 'Comment', in C.D. Romer and D. Romer (eds), *Reducing Inflation: Motivation and Strategy*, Chicago: University of Chicago Press.

Theil, H. (1961), 'Economic forecasts and policy', in *Contributions to Economic Analysis*, Amsterdam: North Holland.

Tinbergen, J. (1952), 'On the theory of economic policy', in Vol. I of *Contributions to Economic Analysis*, Amsterdam: North Holland.

Tobin, J. (1977), 'How dead is Keynes?', *Economic Inquiry*, October.

Tobin, J. (1958), 'Liquidity preference as behaviour towards risk', *Review of Economic Studies*, February.



