NEW-KEYNESIAN ECONOMICS TODAY: THE EMPIRE STRIKES BACK

by Brian Snowdon* and Howard Vane**

"... there is no single doctrine taken to be a scientific truth without the diametrically opposed view being similarly upheld by authors of high repute ... in other fields of science these conflicts usually come to an end ... It is only in the field of economics that the state of war seems to persist and remain permanent."[1]

Knut Wicksell

Introduction

In a recent 'Symposium on Keynesian Economics Today' (*Journal of Economic Perspectives*, Winter 1993) David Romer, James Tobin, Robert King, Bruce Greenwald and Joseph Stiglitz offered a variety of perspectives with respect to the current resurgence of Keynesian ideas which has characterized the macroeconomics literature during the last decade. In his introduction to the symposium Gregory Mankiw noted that the '... literature that bears the name Keynesian is broad and it does not offer a single vision of how the economy behaves.' However, as a leading new Keynesian he did not present his own views in the symposium. In February 1993 we interviewed Gregory Mankiw at Harvard University[2] and here we present his perspective of the current state of macroeconomics in general and what he has called the 'Reincarnation of Keynesian Economics' (Mankiw, 1992a).

We first briefly review the background to the current debate before presenting Mankiw's assessment of some of the important issues in modern macroeconomics. In conclusion we compare the varieties of Keynesian vision presented by some recent contributors to this debate.

Breakdown of the Consensus

In 1977 James Tobin, the United States' most distinguished 'old' Keynesian economist asked the question 'How dead is Keynes?' (see Tobin, 1977). That Tobin was even asking this question highlights the turmoil which had begun to plague macroeconomics in the early 1970s and has continued ever since. Following the publication of Keynes's *General Theory* macroeconomists have been broadly split between those who believe that the price mechanism, unaided by the visible hand of government, is capable of stabilizing a capitalist market economy which is subject to periodic shocks and those, like Tobin, who doubt the capacity of the system to self-equilibrate at a satisfactory level of employment. The synthesis of Keynesian and neoclassical analysis which formed the basis of a consensus in the 1950s and 60s appeared to have achieved an uneasy reconciliation between these two competing views. The theoretical debate relating to the consistency of macroeconomic equilibrium with an excess supply of labour appeared to have been won by supporters of the invisible hand view, but as a practical matter it was accepted that the self-righting properties of the market were too weak and needed the helping hand of fiscal and monetary policies in order to achieve and maintain the primary stated objective of full employment. Keynesians of all persuasions accepted the possibility of widespread and frequent 'effective' demand failures together with prolonged involuntary unemployment. Nevertheless, apart from a small but highly vocal anti-neoclassical group of heretics centered at Cambridge University, the majority of Keynesians were also adherents, and seminal contributors, to the neoclassical paradigm (Paul Samuelson and Robert Solow are the most obvious examples). This schizophrenia could not last.

During the 1960s the synthesis became increasingly associated with an acceptance of a stable long-run trade-off between inflation and unemployment. With the breakdown of the Phillips curve in the late 1960s/early 1970s it

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became apparent that the microeconomic underpinnings of the supply side of Keynesian models were fundamentally flawed. The impact of the first OPEC oil shock in 1973 made this even more apparent. As a result Keynesianism was rejected by a growing number of academic economists during the 1970s, especially in the USA, who were increasingly attracted to the work of the emerging new classical school led and inspired by Robert Lucas who for many is '. . . the leading macro mountaineer of our generation' (see Parkin, 1992). Lucas's incorporation of John Muth's rational expectations hypothesis into a market clearing setting acted like a siren song to the younger generation of graduate economists (see Lucas, 1972, 1973). By 1978 Lucas and Sargent were contemplating life 'After Keynesian Macroeconomics'. Soon after Lucas went so far as to claim that '. . . people even take offense if referred to as Keynesians. At research seminars, people don't take Keynesian theorizing seriously anymore; the audience starts to whisper and giggle to one another.'[3] In a similar vein, a leading 'younger generation' Keynesian, Alan Blinder, confirmed that by 1980 '. . . it was hard to find an American academic macroeconomist under the age of 40 who professed to be a Keynesian' (see Blinder, 1988). Lucas's obituary of Keynesian economics can now be seen to have been premature. However his critiques highlighted the tensions which existed within economics between a flexiprice neoclassical micro world dominated by the fundamental theories of Adam Smith and Leon Walras, and a Keynesian superstructure where arbitrary assumptions relating to nominal price and wage rigidities were the norm. This conflict was in need of resolution as the conventional practice of separating micro from macro analysis was no longer tenable. The new classical solution to this 'crisis' was to adapt macroeconomic theory to neoclassical microeconomics. As Kevin Hoover (1992) has noted the new classical research programme '. . . seeks not only to revivify classical modes of equilibrium analysis, but also to secure the euthanasia of macroeconomics'. In contrast the new Keynesian approach has been to set about building new microfoundations for Keynesian macroeconomics which nevertheless remain faithful to the axioms of utility and profit maximization by individual agents. These ground rules relating to optimizing behaviour have been set by new classical economists who insist that no self-respecting model should contain agents who fail to '. . . exhaust trades that are to the perceived mutual advantage of exchanging parties' (Barro, 1979). In the language of Robert Lucas, any acceptable theory must not allow $100 bills to be left lying on the pavement. Incorporating acceptable microfoundations into macro models has been and remains the principle task facing new Keynesian economists.

The 'Reincarnation' of Keynesian Economics

New Keynesian economics, conceived in the late 1970s, sprang to life in the 1980s. Since the essential feature of Keynesian macroeconomics is the absence of continuous market clearing, the new Keynesian developments during the past decade have been primarily concerned with the '. . . search for rigorous and convincing models of wage and/or price stickiness based on maximizing behaviour and rational expectations' (see Gordon, 1990). In contrast to the new classical monetary surprise and real business cycle models where price taking rational individuals make voluntary choices with respect to quantities, new Keynesian models contain price making, demand taking, risk-averse firms who operate in an imperfectly competitive, uncertain world riddled with imperfect information, transaction costs and asymmetric information (see Mankiw and Romer, 1991). New Keynesian economics seeks to understand and explain the causes of the imperfections in product, labour and capital markets and to show how these imperfections have macroeconomic consequences. In short 'New Keynesianism throws bucket fulls of grit into the smooth-running neoclassical paradigms' (Leslie, 1993). This agenda has led to research into the causes and consequences of:

1. Nominal wage stickiness (see Fischer, 1977; Taylor, 1979; Laing, 1993);
2. Nominal price stickiness (see Mankiw, 1985; Akerlof and Yellen, 1985; Romer, 1993);
3. Real rigidities (see Yellen, 1984; Shapiro and Stiglitz, 1984; Lindbeck and Snower, 1986; Phelps, 1994);

4. Co-ordination failures (see Diamond, 1982; Cooper and John, 1988; Ball and Romer, 1991).

Although new Keynesian theory is still at a rudimentary stage and there are various strands to this diverse school, one of the leading advocates, Gregory Mankiw (1992a) has recently claimed that '. . . the new classical challenge has been met. Keynesian economics has been reincarnated into a body with firm microeconomic muscle.' Mankiw also argues that this reincarnation was necessary because '. . . the new classical revolution seriously wounded the once prevailing Keynesian consensus.' In the development of Mankiw's brand of new Keynesianism it is evident that dissatisfaction with older style Keynesian models emphasizing nominal wage rigidity played a crucial role. A combination of price-taking firms, neoclassical production technology and sticky nominal wages imply that aggregate demand contractions will be associated with a rise in real wages during a recession i.e. real wages will move *countercyclically*. By 1980 Mankiw had concluded that such models made little sense even if modified to allow for rational expectations (e.g. Fischer, 1977) since they imply '. . . that recessions must be quite popular. Sure, a few people get laid off. But most people get to enjoy the higher real wages that result when prices fall and their nominal wages do not. . . If high real wages accompanied low employment, as the *General Theory* and my professors had taught me, then most households should welcome economic downturns' (Mankiw, 1991, pp. 129–130). Since the weight of evidence suggests that real wages do not move countercyclically over the business cycle and the assumption of nominal wage rigidity seems to imply substantial departures from rationality many economists sympathetic to the old neoclassical synthesis view shifted their attention from the labour market to the goods market in their search for *nominal* rigidities. As Mankiw notes 'In fact, it was thinking about the real-wage puzzle that originally got me interested in thinking about imperfections in goods markets and, eventually, about monopolistically competitive firms facing menu costs' (Mankiw, 1991, p. 132). When aggregate supply is derived from inflexible goods prices, rather than from inflexible nominal wages, then real wages can move procyclically or acyclically. Furthermore, new Keynesians argue that price rigidities do not imply gross departures from rationality given the existence of 'near rational behaviour' and 'menu costs' (see Akerlof and Yellen, 1985; and Mankiw, 1985). In new Keynesian models the reason why firms lay off workers during a recession is not because labour costs are too high but for the intuitively appealing reason that sales are too low (Mankiw, 1991, p. 106). Accordingly the new breed of Keynesian models share the spirit of the old Keynesian economics in viewing the business cycle as evidence of economy-wide market failure. This also implies accepting the existence of involuntary unemployment, the non-neutrality of money, sticky prices and wages, and non-clearing markets. However, it is important not to assume that new Keynesians are protagonists in the monetarist-Keynesian debate because new Keynesians do not hold a unified view with respect to the relative potency of fiscal and monetary policy nor do they '. . . necessarily believe that active government policy is desirable' (Mankiw and Romer, 1991). The recent work of Edmund Phelps has also inspired the emergence of a 'structuralist' branch to the new Keynesian school where non-monetary models are given emphasis (see Phelps, 1991 Vol. III and Phelps, 1994).

What progress has been made? What are the likely directions of further research? How does new Keynesian analysis differ from old Keynesian and new classical varieties? We sought answers to these and other questions from Gregory Mankiw. In what follows, where appropriate, we have provided references where the substance of Gregory Mankiw's answers are developed more fully or where some of the ideas discussed have been applied.

General Issues

Why do you think we have so much controversy in macroeconomics compared to microeconomics?

That is a hard question. It is certainly true that there is more agreement among microeconomists as to how they approach things. That is, most microeconomists start off with utility and profit maximization as the underlying motives and go from there. Macroeconomics is in some ways harder since you are dealing with the whole economy; the field therefore requires more simplifying assumptions to make anything manageable, to make the problem simpler than it really is in the world. I think there is disagreement as to which simplifying assumptions are the most natural or the most useful.

How important do you think it is for macroeconomics to have neoclassical choice theoretic foundations?

Well it is certainly true that all macro phenomena are the aggregate of many micro phenomena; in that sense macroeconomics is inevitably founded on microeconomics. Yet I am not sure that all macroeconomics necessarily has to start off with microeconomic building blocks. To give an analogy, all of biology is in some sense the aggregate of particle physics, because all biological creatures are made up of particles. That doesn't mean that the natural place to start in building biology is to start with particle physics and aggregate up. Instead I would probably start with theory at the level of the organism or the cell, not the level of the sub-atomic particle. We have a lot of models like the IS-LM model in macroeconomics that are very useful for studying the macroeconomy, even though those models don't start off with the individual unit and build up from there.

Which papers or books do you feel have had the biggest impact on the development of macroeconomics over the last 25 years?

The biggest impact has undoubtedly come from Lucas. He put the cracks into the Keynesian consensus that existed in the 60s. He really pulled macroeconomics apart by proposing new and intriguing ideas. The disagreements today among macro economists have largely arisen from the critiques of Lucas and of his followers. As you know, I don't agree with Lucas's solutions, but I take the problems that he pointed out very seriously. A lot of the work that I and other new Keynesians have done are a response to the problems that he pointed out in the old Keynesian ideas.
(See Mankiw, 1990).

To some extent you've answered our next question. Where did you draw inspiration for your own work?

It's been a combination of influences. Part comes from the older generation of macroeconomists. I view a lot of the work I do as building on the work of Tobin, Modigliani and Friedman. I see a lot of truth in the views they were pushing. I also take the problems that Lucas pointed out very seriously. A lot of new Keynesian work is trying to reformulate the 1960s Friedman-Tobin view of the world. What is now called the neoclassical synthesis had a large element of truth in it. On the other hand, it had problems, and Lucas pointed those problems out very forcefully. We need to fix those problems and address the concerns that Lucas had while still maintaining the element of truth in the neoclassical synthesis.
(See Mankiw, 1992a).

On Keynes and the General Theory

One interpretation of the neoclassical synthesis which emerged at the end of the 50s suggested that the General Theory *was a special case of a more general classical model. Would you agree with that interpretation?*

I would say that the classical model and the Keynesian model make different assumptions about adjustment of prices. I think of the classical model as being the model that assumes complete price flexibility, and therefore describes a horizon over which it is plausible to make such an assumption. Probably a period of years, rather than a period of months. The Keynesian model applies over a horizon where wages and prices are relatively inflexible or sluggish. Both models are special cases of a more general model which allows a varying degree of flexibility and sluggishness in prices depending on the horizon we want to study. When we study the effect of policies over a quarter or a decade, we want to make a different assumption about the degree of flexibility of prices.

Why do you think there are so many conflicting interpretations of the General Theory*?*

There are a lot of conflicting interpretations because Keynes had a lot of different ideas. The ideas don't necessarily have to be packaged all together, so some people grab on to one set of ideas and say that this is really what is central to what Keynes was saying and other people grab onto other sets of ideas. The question is, when we look at the market imperfection that we call the business cycle, which set of general ideas from the *General Theory* are the most important? There is so much in the *General Theory* that it is hard to comprehend it all at once. Some is very important, but some is not particularly important. Disagreements come by choosing different pieces of Keynes's world view and emphasizing those.
(See Mankiw, 1992a).

Do you think that if Keynes had still been living in 1969 he would have received the first Nobel Prize in Economics?

Oh undoubtedly. I think there are a few very very important economists of the century, and there is no question that Keynes has got to be on anybody's shortlist.

New Classical Macroeconomics

Do you regard new classical macroeconomics as a separate school of thought from monetarism?

I think so. My impression is that monetarism is a school of thought that says fluctuations in the money supply are the primary cause of fluctuations in aggregate demand and income, whereas new classicism is a particular theory as to why fluctuations in aggregate demand might matter through an unanticipated price surprise. This price surprise view proposed by Lucas is, I think, the next step after monetarism. More recently, new classical economists have turned their attention to real business cycle theory, which is the antithesis of monetarism.

Do you think that overall the new classical contributions have had a beneficial effect on the development of macroeconomics?

Debate is healthy, and the new Keynesian school arose largely in response to the new classical school. In that sense it is a debate leading to greater truths, and it has been helpful. A lot of the specific contributions, especially real business cycle theory, are probably not going to survive the test of time. The literature on the time inconsistency of policy is a contribution that will survive and has probably been one of the most important contributions to policy analysis in the past two decades.
(See Mankiw, 1988a, pp. 441–443; Mankiw, 1992a, pp. 563–564).

How important is the rational expectations hypothesis?

It is important in the sense that it has now become the working hypothesis of all practicing macroeconomists. Economists routinely assume that people are rational when they make decisions: they maximize utility, they rationally maximize profits, and so on. It would be peculiar for us to assume that people are rational except when they come to form expectations and then they act irrationally. I don't think the rational expectations hypothesis is important in the sense of having all the sweeping implications as was at first believed. At first people thought that it had all sorts of properties about policy being ineffective.
(See Mankiw, Miron and Weil, 1987; Mankiw, 1990).

Isn't that more to do with the market clearing assumption?

Exactly. People have come to realize that it is other assumptions, like the market clearing assumption, that are really important and that rational expectations in itself doesn't have implications as sweeping as was once thought.

You have questioned the argument that the disinflation experience of the early 80s both here and in Britain has provided decisive evidence against the new classical claim of painless disinflation. Is this because the deflation was unanticipated?

There are two new classical views. The first is the price surprise theory of Lucas. The second is real business cycle theory. This second view says that money anticipated or unanticipated doesn't matter. My view of that is that it is completely at variance with the evidence. Larry Ball has a paper that shows systematically for a large number of countries that whenever you have a major disinflation it is associated with a period of low output and high unemployment (see Ball, 1994). So I think that the evidence is completely clear on that. The evidence is more favourable to early new classical theory. You're right that to a large extent the disinflation was unanticipated even in the United States where Volcker said he was going to disinflate. I don't think people believed he was going to disinflate as fast as he did. 'Most measures of expectations of inflation did not come down until after the recession was well under way. I am sympathetic to the view that credibility is one determinant of how costly a disinflation will be.
(See Mankiw, 1986a, pp. 218–220).

On Keynesianism and the New Keynesians

Do you regard yourself as a Keynesian?

I do but I'm always nervous about the term because the term Keynesian can mean different things to different people, just as different people will read the *General Theory* and pull out different elements as being important. People use the word Keynesian in so many different ways that recently I have actually tried to avoid using the term at all on the grounds that it is more confusing than illuminating. I think of myself as a Keynesian in the sense of believing that the business cycle represents some sort of market imperfection on a grand scale. In that sense I think of myself as a Keynesian. Milton Friedman was also a Keynesian in that sense. My own views emerged as much from Milton Friedman as they have from John Maynard Keynes. Some people take the word Keynesian as meaning a belief in fine tuning the economy so that the government controls every wiggle of ups and downs. Other people take it as a belief that deficit spending is not a bad thing. I don't subscribe to either of those views. I think that the broad theme of the *General Theory* is that the business cycle is something that we really need to worry about because it is a sign of a market imperfection. In that way I am a Keynesian, but as I said so is Milton Friedman.
(See Mankiw, 1987; 1992a).

Was the break-down of the Phillips curve fatal for Orthodox Keynesianism?

It highlighted the absence of a good theory of aggregate supply. What orthodox Keynesians had was a pretty good theory of aggregate demand. The IS-LM model has held up pretty well as a general structure for thinking about how aggregate demand is determined. The problem is once you've got aggregate demand—a downward sloping curve in P-Y space—you still need a good story for the aggregate supply curve. The Phillips curve came out of nowhere. It is really just an empirical description of what was true in the data without any particularly good theories as to why it should look that way, how it would change in response to policy, and what might make it unstable. So we never had a good theory of that, and the breakdown of the Phillips curve made that very apparent and provided room for the more general critique that Lucas put forward. The deficiency on the supply side was always a weakness, but it wasn't given attention until the Phillips curve broke down.
(See Mankiw, 1990, pp. 1647–1648).

What would you summarize as being the central propositions of new Keynesian macroeconomics?

The central propositions are largely theoretical rather than policy oriented. New Keynesians accept the view of the world summarized by the neoclassical synthesis: the economy can deviate in the short term from its equilibrium level, and monetary and fiscal policy have important influences on real economic activity. New Keynesians are saying that the neoclassical synthesis is not as flawed as Lucas and others have argued. The purpose of the new Keynesian school has been largely to try to fix those theoretical problems raised by Lucas and also accept Lucas's argument that we need models supported by better microeconomic foundations. (See Ball, Mankiw and Romer, 1988, pp. 149–161; Mankiw and Romer, 1991, Vol. 1, pp. 1–26).

So you wouldn't subscribe to arguments in favour of incomes policies advocated by Post-Keynesians?

No, not at all. When the government gets in the business of setting wages and prices it is not very good at it. The setting of wages and prices should be left to free markets.

So you are no Galbraithian?

Absolutely not (*laughter*).

How important is the theory of imperfect competition to new Keynesian macroeconomics?

A large part of new Keynesian economics is trying to explain why firms set and adjust prices over time in the way they do. Firms in a perfectly competitive environment don't have any choice over what their prices are going to be. Competitive firms are price takers. If you want to even talk about firms setting prices you have to talk about firms that have some ability to do so, and those are firms that have some market power: they are imperfectly competitive. So I think imperfect competition is central to thinking about price setting and therefore central to new Keynesian economics.
(See Mankiw, 1985; 1988b; Ball, Mankiw and Romer 1988, pp. 156–158).

This is strange, because if you think of the 1930s, you had Keynes and Joan Robinson at Cambridge. Joan Robinson developed the theory of imperfect competition and Keynes developed his General Theory. *Why did it take so long to bring these two ideas together?*

I don't think that Keynes was as worried about building his model based on micro foundations as we are today. Joan Robinson was building the microeconomics that would later prove to be very useful for addressing the macroeconomics of Keynes, but Keynes, not having read Robert Lucas yet, wasn't worried about building the microeconomics of aggregate supply (*laughter*).

In a sense haven't the Post-Keynesians been ahead of you here? People like Paul Davidson have for years taken imperfect competition as their micro foundation. So are the new Keynesians simply catching up on what the Post-Keynesians did quite a while ago?

They have a broad theme of imperfect competition, but the details are not very similar. My impression is that the new Keynesian economics is much more in line with the neoclassical synthesis than with the Post-Keynesians.

You will obviously be very familiar with Alan Blinder's recent surveys. Are they supporting the new Keynesian views? (See Blinder, 1991).

Alan is providing a way of judging a variety of different new Keynesian views. There are a lot of new theories about wage and price rigidities. He is trying to sort out which is right and wrong using a fairly novel perspective of asking firms how they set wages and prices. This is terrific work, but what we are going to learn in the end is still unclear. He is still producing the papers and we haven't seen all the results yet. The goal is to provide one way of deciding which theories we like and which we don't. It's a very exciting project.

An important distinction seems to be made by new Keynesians between real rigidities and nominal rigidities. Why is it important to make this distinction?

The reason is that a real rigidity, which is a rigidity in a relative price, is not a reason for monetary non-neutrality. Unions, for example, could set rigid real wages away from equilibrium. A rigid real wage is not going to provide any reason to believe that money is not neutral,

since it does not create any nominal lever for money to work on. It would cause unemployment but not monetary non-neutrality. To get monetary non-neutrality, which is a central challenge for macro theorists, you need some nominal rigidity such as sticky prices. Having said that, there do seem to be a variety of real rigidities in the world; unions setting wages way above equilibrium levels for example. The question is whether nominal and real rigidities interact. One of the big themes of this literature, mainly due to Larry Ball and David Romer, is that real and nominal rigidities seem to reinforce each other. The real rigidity is actually going to make the nominal rigidity a lot more important than it would be otherwise.
(See Ball, Mankiw and Romer, 1988, pp. 153–156; Ball and Romer, 1990; Mankiw and Romer, 1991, Vol. 2, p. 2).

Critics of the menu cost literature, Robert Barro for example, have suggested that this is a small peg on which to hang an explanation of the business cycle. How can small menu costs have such large real effects on the macro economy? (See Barro, 1989).

It is clear that menu costs are quite small. Firms don't bear huge costs when they change their prices. Yet it also clear that recessions are very costly events. The question is whether these relatively small menu costs can be a key part of understanding this relatively costly business cycle. This literature shows that price adjustment by firms have external effects. When a firm decides to keep prices sticky, this could well be costly for the economy in a way that is not costly for the firm who is making the decision.
(See Mankiw, 1985; Ball, Mankiw and Romer, 1988).

How do efficiency wage and insider/outsider theories fit into new Keynesian thinking?

Both of those theories provide a particular explanation for real rigidities, such as why real wages don't move to the equilibrium level in labour markets. As I said before, real rigidities and nominal rigidities can complement each other. That is, the insider/outsider and efficiency wage explanations for rigid real wages in some senses complement the menu cost story of rigid prices.
(See Mankiw, 1990, p. 1658).

Is the idea of hysteresis crucial to new Keynesian macroeconomics?

Actually I don't think of it as being crucial. It is an interesting idea, that a recession can have long-lived effects on the economy and leave permanent scars after the initial cause of the recession has gone. For example, the high unemployment in Europe in the 1980s persisted far longer than anyone could explain with standard models. But if this idea turned out to be wrong it would not bring down the rest of our theories. This has been an interesting, but relatively separate question.

Do you see the concept of NAIRU, and Friedman's natural rate, as being the same idea or are they different?

I have always thought of them as being basically the same. Most new Keynesian models involve some sort of natural rate; in that sense Milton Friedman has won the debate. Most new Keynesians believe in the natural rate hypothesis except for a small group of people working with hysteresis. The natural rate hypothesis is pretty well entrenched.
(See Mankiw, 1992a, p. 563; 1992b, pp. 483–484).

What about the concept of full employment? It was difficult to think of doing macroeconomics 15–20 years ago without the concept of full employment being central. What do we do about issues like involuntary unemployment? Lucas suggests that we should abandon this concept, what are your views on this? (See Lucas, 1978).

I think there is involuntary unemployment. Part of the new Keynesian literature has come up with models of the labour market to explain why involuntary unemployment exists, why real wages don't adjust to equilibrate labour markets. There is a lot of truth to the efficiency wage theories and the insider/outsider theories, for example.

Do new Keynesians think of full employment as the natural rate?

I avoid the term full employment because it suggests that the natural rate is in some sense desirable. I think there is some natural rate which is the long-run unemployment rate that the economy tends to, that can't be influenced by monetary policy in the long run. That doesn't mean that it is immutable in response to any policy intervention. There are things that have been done to the labour market that either increase or decrease the natural rate, things like the minimum wage, unemployment insurance laws, labour training policies. There are all sorts of things that the government can do to change the natural rate. I don't like calling it full employment because good labour market policies might well raise employment beyond that level.
(See Mankiw, 1992b, pp. 118–139).

How important do you think it is to take into account fairness when looking at the labour market? We are thinking here of the work of George Akerlof, Janet Yellen and Robert Solow who have stressed the idea of fairness. Doesn't this work suggest that perhaps new Keynesians should start looking more closely at the psychology and sociology literature? (See Akerlof and Yellen, 1990; Solow, 1990).

Some of the papers that they have written have been extremely interesting. I don't think there is a lot of compelling evidence yet that we need to abandon neoclassical assumptions. I'm not doing so yet in my work, but I'm certainly happy to read the work of others who are doing so (*laughter*).

In your recent edited volumes of collected papers on new Keynesian economics you say that 'new Keynesian macroeconomics could just as easily be labelled new monetarist economics'. What exactly did you mean? (See Mankiw and Romer, 1991).

The challenge raised by the real business cycle school is the question of whether money is neutral and, if not, why not? Twenty years ago, when Friedman and Tobin were debating, there were some things they agreed on. They agreed on the proposition that the Federal Reserve was an important player in the economy, that what it did really mattered. The real business cycle school has challenged that by writing down models without any real effects of monetary policy. What the new Keynesian models have tried to do is establish why money is not neutral, what microeconomic imperfections are necessary to explain monetary non-neutrality at the macro level. In this sense, these models are trying to support both traditional Keynesian and monetarist views.

Would you agree with Stanley Fischer that the views of Friedman, Brunner and Meltzer are closer to those of Keynesians than they are to equilibrium business cycle theorists? (See Fischer, 1988).

Oh yes absolutely. The essence of real business cycle models is the absence of any role for the Federal Reserve, whereas I think Brunner, Meltzer and Friedman would agree with Tobin that the Fed is very important. None of them would ever argue that money is neutral in the way that real business cycle theorists have.
(See Mankiw, 1986b).

James Tobin has suggested that good papers in economics contain surprises. What surprises have new Keynesian papers uncovered? (See Tobin, 1988).

One of the big surprises is that one can go a lot further with menu cost models than people once thought. A lot of people used to see these models as a silly way of thinking about price rigidity. What the new literature is trying to do is to say no, maybe we should take menu cost models seriously. I think the complementarity between real and nominal rigidities is a surprise. As I mentioned earlier one of the disappointing features so far of the new Keynesian literature is that it hasn't been as empirical as I would have liked. That is a problem being remedied right now in some research. Ultimately that is where the literature should go. More empirical work is needed. (See Mankiw, 1987; Mankiw, Ball and Romer, 1988, pp. 161–201; Ball and Mankiw, 1992a).

Peter Howitt has talked about a Keynesian recovery, Alan Blinder about a Keynesian restoration, you seem to prefer the term reincarnation. Is there something important in the different terms used? (See Howitt, 1990; Blinder, 1992a and Mankiw, 1992a).

I chose the term reincarnation because it means rebirth into another body. While there are many similarities between new and old Keynesian economics, there are also a lot of differences as well, and I wanted to emphasize that. In some senses the spirit of Keynes has been brought back, but it doesn't look like the old Keynes. In fact Keynes might not recognize the new Keynesians as Keynesians at all. In general, people might not recognize themselves after they have been reincarnated. So that is why I used the term reincarnation. (*laughter*).

Would you say that your work is, with respect to Keynes, faithful in spirit, but critical in detail?

I think that is fair. It tries to go beyond Keynes in a sense of taking micro-foundations more seriously. Alan Blinder wrote a paper 'Keynes after Lucas' and I think that title pretty much describes new Keynesians. It takes some of Keynes's ideas seriously, and it also takes some of the critiques of Lucas seriously as well. (See Blinder, 1986).

Do you think Keynes would have been a new Keynesian?

I don't know, I think Keynes was a very unpredictable fellow. I guess he would see some things in it he would like, and some things in it he wouldn't.

Real Business Cycle Theory

You've recently argued that real business cycle theory has served an important function in stimulating and provoking scientific debate, but you predict that the approach will eventually be discarded. What are your main objections to real business cycle theory? What are the weaknesses, theoretical, empirical or both?

My objectives are mainly empirical. Theoretically they are very elegant models and that is a large part of their appeal. They are very parsimonious models. But when I look at the real world I see the same things that Milton Friedman and James Tobin do, which is a very powerful Federal Reserve board in the United States or the Bank of England in the UK. There is a lot of evidence across countries that periods of disinflation are periods of low output and high unemployment. Those effects are completely absent in real business cycle models. I think the central driving forces for the business cycle that those models highlight—technology shocks—aren't very important.
(See Mankiw, 1989; 1994; Campbell and Mankiw, 1989).

Isn't the pro-cyclical behaviour of the real wage a strong feature of these theories? How do new Keynesians explain the movement of real wages over the business cycle?

The theories do predict pro-cyclical wages. Although I've not looked at the models carefully on this question, my understanding is that they predict very pro-cyclical, real wages. While it is true that real wages are pro-cyclical, my reading of the evidence is that they are only mildly procyclical. Therefore, the fact that these theories predict very pro-cyclical real wages, and the data shows that they are only mildly pro-cyclical, makes it hard to reconcile this model with the evidence. I think the real wage evidence is not that hard to explain. If you believe in a world where wages and prices are sluggish over time, the cyclical behaviour of the real wage is really a question of whether wages or prices are more sluggish. The fact that real wages are roughly a-cyclical, maybe slightly pro-cyclical, is some indication to me that wages and prices are simply equally sticky. This is consistent with Alan Blinder's evidence which says that prices change on average once a year, and we know a lot of wages change on average once a year. So I think that explanation is consistent with a lot of the evidence.
(See Blinder and Mankiw, 1984; Mankiw, Rotemberg and Summers, 1985; Mankiw, 1991).

How do we explain pro-cyclical productivity? Some Keynesians seem to suggest that it is due to labour hoarding.

The pro-cyclical behaviour of productivity is a puzzle for people who don't believe in technology shocks. The traditional explanation for why productivity is pro-cyclical is labour hoarding. In recessions firms keep on workers they don't really need so that they can have the workers still available when the next boom comes, and that tends to give the appearance of pro-cyclical productivity. These theories make a lot of sense to me. I know I work my secretary harder when I have more work to be done; therefore her productivity is pro-cyclical. I know I work harder when there is more work to be done (*laughter*). I think there is a lot of causal evidence that labour hoarding and pro-cyclical effort are important.
(See Mankiw, 1989, pp. 83–85).

On Macroeconomic Policy

One of the central ideas of Keynesian economics is that an increase in aggregate demand will stimulate the economy. Under what circumstances do you think a government should actually stimulate demand?

There are a couple of questions. First, when should it act? Second, how should it act? That is, should it use monetary or fiscal policy? On the first question, one should stimulate aggregate demand when it is too low to maintain full employment—that is when you observe very high unemployment or when there is reason to believe that unemployment is going to rise. The policy implications of a lot of new Keynesian theories really go back to a lot of the policy implications of the neoclassical synthesis of the 1960s. Some of the limitations on policy that were then debated are still relevant today. Even if you accept everything that new Keynesians say about prices being sluggish and so on, there is still the question of how good the government is at responding in a timely fashion to the shocks? In that debate, I side to a large extent with Milton Friedman. The government is very bad at recognizing shocks in a timely fashion, and when they do respond to shocks they often do so quite late and often counter-productively. So while I see the business cycle as a sign of market failure I also think it is a kind of market failure that a government is very limited in its ability to fix. If we have a very deep persistent recession, certainly something on the lines of the Great Depression, there is room for the government to do something. For the relatively minor wiggles that we have experienced in the post-war economy, it is not clear that the government can do a lot better than it has.
(See Mankiw, 1992b, pp. 322–341).

Do you think Keynes was politically naive in thinking that politicians would be advised by technocrats and take the correct action? We are thinking here of the public choice literature and the political business cycle literature. Can we actually trust politicians once they have their hands on the fiscal and monetary levers to use them in the right way?

I think that is a serious concern but there are a lot of ways of fixing that problem. For example, there is a large literature showing that countries with more independent central banks have a lower inflation on average. With less independence in the central bank, there is more political pressure and therefore a greater possibility of following a policy of inflating too much. There are ways around the political problem, like making independent central banks, which to some extent are staffed by technocrats. For that reason an independent central bank would be better at fine-tuning the economy, to the extent we fine tune it at all, compared to fiscal policy which is always run by politicians.
(See Mankiw, 1992b, pp. 331–333; Mankiw, 1994).

You've said that the literature on time inconsistency has provided a persuasive case for a commitment to some sort of rule for monetary policy, do you also support fiscal rules?

Fiscal rules have to be well crafted. A balanced budget amendment that is too strict could be a disaster. At certain times, like recessions and wars, it is appropriate to run budget deficits. So any fiscal rule has to take into account those special situations where budget deficits are the appropriate policy response. A fiscal rule by itself wouldn't be a bad idea, but it has to be well crafted and so far I haven't seen one that is.

Isn't one of the problems with devising rules that if the economy is hit by an unforeseen shock then the government really has to renege on that rule and take some discretionary action? It is difficult to think of a rule which really would be binding.

There are two parts to the question. First, how might you make the rule binding? Second, do you want to make the rule binding? One way to make the rule binding is reputational. Many rules are rules just because long tradition has established them as rules and people don't want to break tradition. Another more legalistic way of imposing rules is by writing them into the constitution. I think the harder question you raise is do you want to make rules binding? The question is whether you can write a rule that works well even in response to unforeseen events. If it becomes too costly to be tied by the rule people will stop abiding by it. What we want to do is write down a rule that will be good in response to normal kinds of shocks. That is, you don't know what the shocks are going to be, but you know what kind of shocks are possible. You've got oil shocks, monetary demand shocks and so on. You write down a rule that is good in response to the kinds of shocks you expect the economy to experience, based on the shocks experienced in the past. Therefore, unless something completely unforeseeable happens, you stick by the rule.

Leijonhufvud once argued that the economy can be thought of as travelling along a corridor, as long as it stays in the corridor leave it alone, but if it gets out of this corridor into a severe recession that is the time for intervention. Is that what you are saying? (See Leijonhufvud, 1981).

Well no, because recessions are reasonably foreseeable. Although you don't necessarily know when a recession is going to occur, you know that one will occur eventually. A recession is one of the contingencies that you want your rule to deal with. So I don't think a recession *per se* is one of those extraordinary events that make you want to break the rule. A recession is something you can plan for in advance. I'm talking about an event that not only can you not predict when it is going to happen, but you have never even thought that it might happen. For example, before 1973 people never imagined an OPEC supply shock. The whole idea of OPEC never even crossed anybody's mind. That is the type of situation where you might want to rethink the rule. Now that we know what OPEC is capable of, we can write down a rule that takes oil shocks into account.

What is the role of fiscal policy in new Keynesian macroeconomics?

To a large extent new Keynesian economics has been about the theory of aggregate supply and why it is that prices adjust slowly. It has been relatively neutral on the question of what determines aggregate demand, in particular whether monetary or fiscal policy levers are most useful. As I mentioned a moment ago, I am skeptical personally about the usefulness of fiscal policy in fine tuning the economy because, at least in the United States, the Congress acts very slowly. Even as we are doing this interview (18th February 1993) the Congress is debating a fiscal stimulus, even though the recovery has been going on for about a year now. By the time this fiscal stimulus actually has an effect on the economy, my guess is that we will be pretty close to the natural rate again. This is the perfect example of how the lags can be very long in fiscal policy. Monetary policy is a more useful tool for stabilizing aggregate demand.
(See Mankiw and Summers, 1986).

Do budget deficits matter?

I think they matter a lot. The main way they matter is not for short-run macroeconomic reasons but for long-run reasons—reasons that are best described not by Keynesian models but by growth models. The evidence as I see it is that large budget deficits reduce national saving. And the lesson from growth theory and growth experience across countries is that low saving leads to low growth. This is a big problem for

the United States today.
(See Mankiw, 1992 b, pp. 423–435).

If you were advising President Clinton about macroeconomic policy for the next three or four years what would be the kinds of policies you feel are necessary?

My reaction to President Clinton's speech (17th February 1993) is that I don't think we need the fiscal stimulus that he is proposing. Recovery is already on its way. It wasn't a very deep recession to start off with, so I'm not terribly shocked that there is a mild recovery. It will take the fiscal stimulus a while to get people employed. I am happy that he is worried about the budget deficit, as low national saving is an important macro problem in the long term in the United States. Yet I am disappointed that he is putting so much emphasis on tax increases rather than spending cuts. That is really a view not so much about macroeconomics as about the size of government. I am also disappointed that he is giving no attention to the low rate of private saving in the United States. I would recommend tax reforms to remove the present disincentives toward saving. So I give him a mixed review.

Current and Future Progress in Macroeconomics

Much research in the 1980s, your own included, was directed at providing more rigorous microeconomic foundations for the central elements of Keynesian economics. Taking an overview of the last decade how successful do you think that research has been in providing a more substantial micro foundation for Keynesian economics?

It has been successful at the theoretical level in the sense that one can now say that Keynesian economics, the economics of wage and price rigidities, is well founded on microeconomic models. There are now several microeconomic models that people can pull off the shelf. The theoretical challenge of Lucas and his followers has been met. It is less clear whether this line of research is going to be successful as an empirical matter. That is, to what extent does it yield new insights to help us understand actual economic fluctuations? Does it give us new ways to look at data and policies? The jury is still out on that one. There is a small empirical literature, but I can probably count the number of empirical papers on the fingers of two hands. I hope it is a growth area, but so far the literature has not been as empirically oriented as I would like.
(See Ball, Mankiw and Romer, 1988; Ball and Mankiw, 1992a and b; Mankiw, 1985).

Do you think there is some truth to the view that at the moment we have too many theories?

Yes, I have a lot of sympathy with that view. There is too big a premium for coming up with clever new theories in the profession. Yet I don't know of any way to solve this problem. Obviously I believe the things I believe, and I can't tell people that they should believe what I believe, just because there are too many theories (*laughter*). It would be nice if macroeconomists reached a consensus and they could do more work on details and less work on creating brand new theories of the business cycle. Until we do naturally reach a consensus, there is no way to enforce that by fiat.

Do you see any signs of an emerging consensus in macroeconomics?

That is a good question. I change my mind on that a lot depending on what conference I go to (*laughter*). I think there are certainly groups within the profession that are agreeing with each other. There is much agreement among new Keynesian people like Olivier Blanchard, Larry Ball, David Romer, George Akerlof, Alan Blinder and so on. Whether we as a group are coming to agreement with some of the real business cycle group is hard to say. I'm delighted that some of the people who previously worked closely with the real business cycle models are now trying to incorporate monetary effects into those models. That provides a hope that somewhere down the line the new Keynesian models and the real business cycle models are going to merge to some grand synthesis that incorporates the strengths of both approaches. That hasn't happened yet; that is

just a hope.
(See Mankiw, 1989, pp. 88–89).

Mankiw's Reincarnated Keynesianism

Gregory Mankiw provides a relatively optimistic vision of the future of Keynesian macroeconomics which in his view has been 're-incarnated' during the last decade rather than 'resurrected' in its old form. His Keynesian vision shares the spirit of Keynes in seeing economy-wide market failures caused by the inability of the invisible hand to maintain full employment. However, following the new classical critiques Mankiw, like many other Keynesians, accepts that the fatal defect of the neo-Keynesian synthesis model was the lack of an adequate theory of aggregate supply. To Mankiw and other new Keynesians the wage and price rigidities characteristic of Keynesian models could no longer remain as an assumption but required theoretically rigorous foundations. The vitality of the new classical revolution in the 1970s was attributable as much, if not more, to theoretical flaws in the supply side of the Keynesian model as it was to empirical dissatisfaction (see Mankiw, 1988a). To remedy those theoretical flaws by building a Keynesian theory of aggregate supply which can rationally account for wage and price rigidities and hence the non-neutrality of money is for Mankiw the paramount job facing Keynesian theorists today. Indeed Mankiw goes further and argues that the reconstruction appears to be well on the way and that Lucas's criticisms relating to the microfoundations of Keynesian models have been met.

Old v New Keynesians

Although they share the spirit of Keynes, and Mankiw's work has in part been inspired by older Keynesian views, the difference between James Tobin's 'unreconstructed old Keynesian' views and Mankiw's reincarnation are striking. For Mankiw (and Romer) nominal aggregate demand disturbances have real effects because wages and prices are rigid (see Romer, 1993). In sharp contrast Tobin argues that '. . . Keynesian macroeconomics neither asserts nor requires nominal wage and/or price rigidity' (see Tobin, 1993a). Indeed that Keynesian economics is defined by price rigidities is especially misleading if it suggests that such an assumption is necessary to generate Keynesian results. For Tobin the empirical fact that markets do not clear *instantaneously* leaves room, for '. . . flexibility in any common sense meaning of the word' and the resulting excess supply regimes allow quantities to determine quantities with output and employment constrained by deficient effective demand. Tobin also argues that the classical equilibrating mechanisms are 'weak' or 'possibly non existent or perverse' and certainly require help from activist government fiscal and monetary policy. According to Tobin (1993b) the suggestion that '. . . Keynesian economics is doomed without new theories to explain price and wage rigidities is to misunderstand Keynes himself and old Keynesian economics'. It is evident from Mankiw's answers to our questions that his brand of new Keynesianism has been heavily influenced by the theoretical and empirical contributions of Friedman (1968), Lucas (1972) and Kydland and Prescott (1977). In particular he questions the desirability of activist discretionary fiscal policy as a stabilizing weapon and following the new classical work on the dynamic inconsistency of monetary policy he has also been persuaded by the arguments in favour of a monetary rule, something old (and some new) Keynesians would never subscribe to.

Bruce Greenwald and Joseph Stiglitz, both leading new Keynesians appear to occupy an intermediate position somewhere between that of Tobin and Mankiw. Greenwald and Stiglitz support Tobin's old Keynesian position that increasing wage and price flexibility might well exacerbate a recession. This alternative new Keynesian view suggests '. . . that natural economic forces can magnify economic shocks that may seem small and that existing price rigidities may *reduce* the magnitude of the fluctuations as Keynes argued' (see Greenwald and Stiglitz, 1993a). Hence for Greenwald and Stiglitz the single minded focus by Mankiw and others on wage and price rigidities would appear to be somewhat misguided. With respect to the policy implications of new Keynesian economics Stiglitz supports a more interventionist stance than Mankiw. Most recently Stiglitz has noted that new Keynesians '. . . disagree with virtually every one of the presumptions underlying non-interventionist theories' (see Stiglitz, 1993 chapter 39). He argues that the government on balance has done more to stabilize than destabilize the economy and should certainly not bind itself to fixed rules of the kind advocated by Milton Friedman (1968), Robert Barro (1986), Finn Kydland and Edward Prescott (1977). Like Tobin, Stiglitz favours discretionary policies because '. . . changing economic circumstances require changes in economic policy, and it is impossible to prescribe ahead of time what policies would be appropriate'. Indeed Stiglitz questions whether it would ever be possible for a government to stick by a rule because '. . . if the unemployment rate becomes high, government must and will do something regardless of what is said' (see Stiglitz, 1993). However Stiglitz, like Mankiw, is not as optimistic as old Keynesians on the ability of government to *fine tune* the economy. Here the monetarist and new classical arguments have modified all new Keynesian views (and no doubt some old ones also). Stiglitz, like Mankiw, accepts that '. . . by attempting to do too much the government may do worse than it would if it were less ambitious' (Stiglitz, 1993).

Old and new Keynesians alike are united in their view that the traditional IS-LM model remains the best way to think about the demand side of the macro models although Tobin gives emphasis to real rather than nominal demand shocks (see Tobin, 1993a). Their more unified position here differs considerably from that of equilibrium business cycle theorists. For example in Robert King's contribution to the Symposium on Keynesian Economics Today he criticizes new Keynesians like Mankiw for maintaining their faith in the textbook IS-LM model (see King, 1993; Mankiw, 1990). This is because '. . . of its treatment of expectations the IS-LM model, as traditionally constructed and currently used, is a hazardous base on which to build positive theories of business fluctuations and to undertake policy analysis' (King, 1993). However, even if Keynesians old and new agree on the IS-LM interpretation of aggregate demand a further complication arises in connection with the recent work of Greenwald and Stiglitz on financial market imperfections and business cycles (see Greenwald and Stiglitz, 1993a and 1993b, and Stiglitz, 1993). The Greenwald and Stiglitz model shows how a negative aggregate demand shock could translate itself into a leftward shift of the aggregate supply schedule due to firms' increased perception of risk during an economic downturn. Due to financial market imperfections generated by asymmetric information equity rationed firms can only partially diversify out the risks they face. Their resultant dependence on debt rather than new equity issues to finance investment makes firms more vulnerable to bankruptcy the higher the level of their output. Hence any changes in a firm's net worth position or in their perception of the risks they face will have a negative effect on their willingness to produce. Risk-averse firms will be less willing to supply at every price when the environment becomes less favourable and increasingly uncertain. When in an economic downturn firms observe a shift in their demand curve they must either reduce their output or their price. Risk-averse firms prefer to adjust their output because the '. . . uncertainties associated with changing prices may be much greater' (Stiglitz, 1993). In such a world the Greenwald-Stiglitz model suggests that wage and price flexibility may well be destabilizing and exacerbate any economic downturn. The important implication is that the resultant *risk-based aggregate supply curve* will shift leftwards following an economic downturn initiated by an aggregate demand shock. This results in the non neutrality of money even if prices are perfectly flexible.

A New Keynesian-Monetarist Synthesis?

Not all Keynesian economists are as convinced as Gregory Mankiw and other new Keynesians that real progress has been made during the past two decades. Alan Blinder has questioned whether the '. . . prodigious amounts of labour and capital devoted to macroeconomic research since 1972 have been allocated correctly' and Olivier Blanchard has criticized the readiness of macroeconomists to adopt the new classical 'quasi-religious insistence on microfoundations' which has led to the construction of '. . . too many monsters with few interesting results' (see Blinder, 1986; Blanchard, 1992). Certainly the new Keynesian developments have been criticized for their lack of attention to empirical research, a criticism Mankiw accepts. However, given that the new classical critique was launched mainly from a theoretical rather than an empirical base it is perhaps understandable that younger Keynesians, at least initially, have concentrated their efforts on providing 'fort Keynes' with more solid microfoundations rather than giving continuing emphasis to empirical work (see Snowdon, Vane and Wynarczyk, 1994 chapter 7).

So where does macroeconomics go from here? Recent papers by David Laidler, Olivier Blanchard and Alan Blinder make it abundantly clear that in their view a monetarist augmented mainstream macroeconomics, circa 1972, although not perfect, 'had solid foundations and was basically right' (see Blanchard, 1992; Laidler, 1992; Blinder, 1992b). By the mid 1970s the impact of supply shocks had been successfully incorporated into the mainstream model and as a result it has, in their view, proved capable of withstanding the new classical challenge. Thus the current debate is now mainly between a small but very influential group of equilibrium business cycle theorists (Robert Barro's 'good guys') and a larger group of mainstream macroeconomists who adhere to what could justifiably be called a new Keynesian-monetarist synthesis (Robert Barro's 'bad guys', see Barro, 1989). However, as we have noted above, an important implication of the recent work of Greenwald and Stiglitz is that the traditional distinction between aggregate demand and aggregate supply disturbances, although useful as an organizing principle, may be misleading. In a similar vein Benjamin Friedman has recently argued that '. . . Many occurrences that initially seem to represent disturbances to aggregate supply likewise cause disturbances to aggregate demand and vice versa' (Friedman, 1992). Indeed Greenwald and Stiglitz even suggest that their theory of risk-averse firms if combined with market clearing flexible wages and prices can be viewed as a special case of real business cycle theory. This requires that the financial disorganization and risk associated with recessions can be thought of as representing a form of negative shock to technology and capital. Perhaps here there is some hope that this line of research could lead to some future collaboration between the 'good guys' and the 'bad guys'.

Concluding Remarks

The remarkable versatility of Keynesian economics guarantees that it will continue to serve as a relevant research programme which will influence both theoretical developments and policy proposals. It remains to be seen if Gregory Mankiw's optimism with respect to the usefulness of the burgeoning new Keynesian microfoundations literature significantly improves our understanding of macroeconomic phenomena. We share Mankiw's optimism. Keynesian economics, in resurrected or reincarnated form, is alive and well. Recent controversies surely confirm the observation made by Sir Denis Robertson (1954) many years ago when he noted that:

> 'Highbrow opinion is like a hunted hare; if you stand in the same place, or nearly the same place, it can be relied upon to come round to you in a circle'.

So it's back to the future!

Notes

1. Taken from 'Ends and Means in Economics,' in *Selected Papers on Economic Theory,* ed. E. Hindahl, London: Allen and Unwin, 1958.
2. This interview was one in a series held in connection with the preparation of a new book published by Edward Elgar (see Snowdon, Vane and Wynarczyk, 1994).
3. Cited in Mankiw, 1992a.

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