The Fall and Rise of Keynesian Economics*

ALAN S. BLINDER
Princeton University

Keynesian economics came under much criticism in the 1970s. This paper argues that the decline in Keynesian economics and the rise in, notably, new classical economics in this period related to their respective theoretical appeal rather than their ability to explain developments in the macroeconomy. As this has become increasingly recognized, and with the development of sound microeconomic foundations, Keynesian economics has again been on the rise.

I Introduction

According to T.S. Kuhn's *The Structure of Scientific Revolutions* (1962), progress in 'normal science' requires an agreed-upon theoretical framework or 'paradigm' within which researchers work to solve puzzles. The stage is set for a paradigm change when anomalies are discovered and documented. After a period of turmoil and extensive questioning of old assumptions, a new theory may emerge which explains not only the anomalies but also the phenomena encompassed by the old theory. If so, the scientific revolution succeeds; although the new theory may itself be subsequently supplanted by a still newer one. Implicitly, a progressive science rarely, if ever, goes back to a previously discarded theory, for that theory was rejected for good reasons.

For a period of roughly 35 years, Keynesian theory provided a central paradigm for macroeconomists, and considerable progress was made on several empirical fronts. It was widely recognized that some of the ingredients of Keynesian economics (e.g. money illusion and/or nominal wage rigidity) rested on slender to non-existent microtheoretic foundations; and there were always dissenters. But, thought of as a collection of empirical regularities that fit together into a coherent whole, the theory worked tolerably well.

In the 1970s, however, the Keynesian paradigm was rejected by a great many academic economists, especially in the United States, in favour of what we now call new classical economics. 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.

A scientist from another discipline might naturally surmise that the data of the 1970s had delivered a stunning and unequivocal rejection of the Keynesian paradigm. He would look for some decisive observation or experiment that did to Keynes what the orbit of Mercury did to Newton. But he would look in vain.[1] I argue in Section III that there was no anomaly, that the ascendancy of new classicism in academia was instead a triumph of *a priori* theorizing over empiricism, of intellectual aesthetics over observation and, in some measure, of conservative ideology over liberalism. It was not, in a word, a Kuhnian scientific revolution.

If this is so, it helps explain a phenomenon that a Kuhnian would find puzzling: macroeconomics is already in the midst of another revolution which amounts to a return to Keynesianism—but with a much more rigorous theoretical flavour. The first stages of the Keynesian counter-revolution—which is still in progress—are summarized and evaluated in Section IV. But before doing this, I must define precisely what I mean by Keynesianism. This I do in Section II.

II What it Means to be a Keynesian

The word 'Keynesian' means many things to

* I am grateful to Ben Bernanke, John Campbell, Stephen Goldfeld, John Seater, Steven Sheffrin and Robert Trevor for helpful comments on an earlier draft.

[1] See Blinder (1979) and Blinder (1987a), Chapter 3.

many people. Decades ago, it was a carelessly applied label for economic liberals and interventionists in general. For a while in the late 1970s and early 1980s it became a pejorative term more or less synonymous with old-fashioned. No two people have precisely the same definition of Keynesian economics. But, as one of the few American economists of my generation who never shunned the label, I feel entitled to my own definition. To me, the heart of Keynesianism consists of six principal tenets.

First and foremost, Keynesian economics is a theory of aggregate demand and of the effects of aggregate demand on real output and inflation. The first three tenets follow from this.

1 A Keynesian believes that aggregate demand is influenced by a host of economic decisions, both private and public, and sometimes behaves erratically. Some decades ago, there were active, impassioned debates over the propositions that (a) monetary policy is powerless because money demand is infinitely elastic, or (b) fiscal policy is powerless because money demand is totally inelastic. But both of these are dead issues now. *Essentially all Keynesians and most monetarists now believe that both fiscal and monetary policy affect aggregate demand.*[2] Many new classicals, however, believe in debt neutrality—the doctrine that substitutions of debt for taxes have no effects on total demand.

2 *According to Keynesian theory, changes in aggregate demand, whether anticipated or unanticipated, have their greatest short-run impact on real output and employment, not on prices,* and the short run lasts long enough to worry about.[3] In textbook expositions, this idea is conveyed by a short-run aggregate supply curve that is upward sloping, and probably quite flat except at high levels of capacity utilization, so that changes in aggregate demand are normally not dissipated in higher prices. In macroeconometric models, the same idea is captured by treating output and employment as demand-determined in the short run and letting an inertial Phillips curve determine inflation.

For a theoretical model to produce real effects from anticipated monetary policy, it is usually necessary to have some sort of nominal rigidity in the model; otherwise, an injection of money is like a currency reform which changes all prices equiproportionately.[4] So Keynesian models generally either assume or try to rationalize nominal rigidities. Because supply and demand curves derived from standard neoclassical maximizing principles are always homogeneous of degree zero in nominal quantities, this is not an easy task. Real effects of government purchases, however, are readily explained on strictly neoclassical grounds.[5]

Since prices do not absorb all shocks to demand, fluctuations in any component of spending will cause sympathetic movements in output. In most Keynesian models, the latter are larger than the former because of the multiplier; but a multiplier greater than one is not central to Keynesian analysis. A positive real multiplier is.

Although real effects from demand fluctuations are often called 'Keynesian effects', most monetarists accept the idea as well—at least as it pertains to monetary policy. So this tenet does not really divide those two schools of thought. However, at least some new classicals insist that changes in money affect real output only if they are unanticipated.

3 *Keynesians believe that goods markets and, especially, labour markets respond only sluggishly to shocks, i.e. that prices and wages do not move quickly to clear markets.* This issue, once again, divides Keynesians more from new classicals than from monetarists—although monetarists probably place more faith in the economy's natural servo-mechanism than Keynesians do. Milton Friedman (1968, p. 13), for example, has written that 'Under any conceivable institutional arrangements, and certainly those that now prevail in the United States, there is only a limited amount of flexibility in prices and wages.' In current parlance, that would certainly be called a 'Keynesian' position.

The next three tenets have to do directly with policy; and here Friedman and other monetarists part company with most Keynesians.

4 To a Keynesian, the actual levels of

[2] That does not, however, preclude the possibility that, for example, monetary policy might cancel the macro effects of fiscal policy by controlling nominal GNP.

[3] That belief need not preclude the possibility that, for example, the division of any given change in nominal GNP into real effects and price effects might depend on whether or not the change is anticipated.

[4] By monetary policy I mean, for example, an increase in base money paid out in lump-sum transfers. Open-market swaps of money for bonds are often (not always) non-neutral because they change interest rates. Monetary non-neutrality can also be rationalized by distribution effects; but these are typically considered unimportant and are rarely the focus of the debate.

[5] See, for example, Barro (1981b).

employment and unemployment have no special claim to optimality—partly because unemployment is subject to the caprice of aggregate demand, and partly because they believe that markets clear only gradually. In fact, *Keynesians typically see unemployment as both too high on average and too variable*, although they know that rigorous theoretical justification for these positions is hard to come by. Keynesians also feel certain that periods of recession or depression are economic maladies, not Pareto-optimal responses to unattractive technological opportunities. All this is summarized in the term 'involuntary unemployment', which Keynesians deplore even though it has proven notoriously difficult to define.[6] On this tenet, new classicals differ sharply from Keynesians, with monetarists somewhere in between.

5 *Many, but not all, Keynesians advocate activist stabilization policy to reduce the amplitude of business cycles*, which they rank among the most important of all economic problems. Here monetarists generally join new classicals, as well as some conservative Keynesians, in doubting both the efficacy of stabilization policy and the wisdom of attempting it. Some new classicals go even further and question whether business cycles are a serious problem at all.[7]

The argument that economic knowledge is not secure enough to support what used to be called fine tuning is by now widely accepted, even by most Keynesians. Yet many Keynesians believe that more modest goals for stabilization policy—coarse tuning, if you will—are not only defensible but sensible. For example, an economist need not have detailed quantitative knowledge of lag structures to prescribe a dose of expansionary monetary policy when the unemployment rate is 10 per cent or more—as it has been in many countries in this decade. Furthermore, and this may be the most important point, the nature of government seems to abhor a vacuum of economic advice. If economists with admittedly limited knowledge refuse to offer their expert (if uncertain) counsel, assorted quacks with no knowledge at all will surely rush in to fill the void.

6 Finally, and even less unanimously, *many Keynesians are more concerned about combatting unemployment than about conquering inflation*.[8] However, there are plenty of anti-inflation Keynesians; most of the world's current and past central bankers, for example, merit this title whether they like it or not. Needless to say, relative attitudes toward unemployment and inflation heavily influence the policy advice that economists give and that policy makers accept. As a broad generalization, I think it safe to say that Keynesians are typically more aggressive about expanding aggregate demand than are non-Keynesians.

My six tenets divide naturally into two equal groups: the first three are clearly assertions about *positive* economics while the last three are mostly *normative*. The division of Keynesian economics into positive and normative components is central to understanding both the academic debate and its relevance to policy.

Positive Keynesianism is a matter of scientific judgement. A positive Keynesian believes that both monetary and fiscal policy can change aggregate demand, that fluctuations in aggregate demand have real effects, and that prices and wages do not move rapidly to clear markets. No policy prescriptions follow from these beliefs alone. And, as I have indicated, many economists who do not call themselves Keynesian would nevertheless accept the entire list.

Normative Keynesians add both value judgements and political judgements to the preceding list. A normative Keynesian believes that government should use its leverage over aggregate demand to reduce the amplitude of business cycles. He or she is probably also far more interested in filling in cyclical troughs than in shaving off peaks. These normative propositions are based on judgements that (a) macroeconomic fluctuations significantly reduce social welfare, (b) the government is knowledgeable and capable enough to improve upon free-market outcomes, and (c) unemployment is a more important problem than inflation.[9]

The long, and to some extent continuing, battle between Keynesians and monetarists, you will note,

[6] For this reason, I recently proposed that we rename involuntary unemployment as 'pornographic unemployment'. See Blinder (1988).

[7] See Lucas (1987) for an example. For a rebuttal, see Blinder (1987b).

[8] I include myself here. See Blinder (1987a, Chapter 2), (1988).

[9] I would not want to place the last of these beyond the realm of positive economics. There is a huge literature on the social costs of unemployment and inflation, and many Keynesians like myself have concluded from this evidence that the costs of low inflation are both small and readily avoidable. Nonetheless, value judgements are still involved in the trade-off between unemployment and inflation.

has been primarily fought over the *normative issues*—particularly (b) and (c).[10] Thus, by my definitions, most monetarists are positive Keynesians but not normative Keynesians. So are other conservatives who shun the label Keynesian. Protagonists in these debates agree on most positive issues but make different value judgements and seat-of-the-pants political judgements, and so reach different conclusions about policy. Their disagreements in many ways mirror disagreements among policy-makers.

The briefer, but more intense, debate between Keynesians and new classicals had, by contrast, been fought primarily over the tenets of *positive* Keynesianism. New classicals argue that anticipated changes in money do not affect real output; that markets, including the labour market, clear quickly by price[11]; and that business cycles may be Pareto optimal. Here 'objective' scientific evidence can be brought to bear and, in my judgement, the evidence on all three issues points strongly in the Keynesian direction.[12] But rather than try to summarize that evidence now, I only want to make one point: that arguments over the positive aspects of Keynesian economics are potentially resolvable by the accumulation of scientific evidence in a way that disputes over normative issues are not.

Before leaving the realm of definition, let me underscore several glaring and intentional omissions.

First, I have said nothing about *rational expectations*. Many Keynesians are doubtful about the validity of rational expectations as a behavioral hypothesis, as was Keynes himself.[13] Others are willing to accept it. But, when it comes to the large issues with which I have concerned myself so far, nothing much rides on whether or not expectations are rational. In particular, rational expectations models with sticky prices—like those of Fischer (1977) and Taylor (1980), for example—are thoroughly Keynesian by my definition. Details of model construction and quantitative answers to specific questions do, of course, depend on how expectations are modelled. And, for some issues, the expectational mechanism is crucial.[14] But, for the most part, these are not central to the debate between new classical and Keynesian economists.[15]

The second omission is the natural rate hypothesis. Pre-1970 Keynesianism included a Phillips curve that was negatively sloped even in the long run. This idea was rejected theoretically by Milton Friedman (1968), a monetarist, and Edmund Phelps (1968), a Keynesian, and shortly thereafter was also rejected in econometric studies by Keynesians like Robert Gordon (1972). Since about 1972, a Phillips curve that is vertical in the long run has been an integral part of Keynesian economics. So the natural rate hypothesis played essentially no role in the intellectual ferment of the 1972-1985 period. Ironically, however, questions about its validity are now playing a role in the Keynesian renaissance. Specifically, models with hysteresis have reopened the theoretical and empirical debate over the natural rate hypothesis, especially for Europe. (More on this below.)

Third, I have ignored the choice between monetary and fiscal policy as the preferred instrument of stabilization policy. People differ along this dimension and occasionally change sides. By my definition, however, it is perfectly possible to be a Keynesian and still believe either that responsibility for stabilization policy should in principle be ceded to the monetary authority or that it is in practice so ceded.

III The Fall of Keynesian Economics

To start with, let me first dispose of the view, promoted in some quarters, that the demise of Keynesian economics was due to the doctrine's poor empirical predictions. Seven years ago, Robert Lucas (1981, page 559) wrote that 'Keynesian orthodoxy is in deep trouble, the deepest kind of trouble in which an applied body of theory can find itself: It appears to be giving seriously wrong answers to the most basic questions of macroeconomic policy . . .' He was talking about the collapse of the Phillips curve in the US during the 1970s, which he and Thomas Sargent (1978, page 57) had characterized as 'econometric failure on a grand scale'.

[10] The one prominent exception was mentioned above: the old debate over whether or not the LM curve is vertical. This has long been a dead issue.

[11] The 'price' may be multi-faceted. Complicated contractual agreements are allowed within the new classical approach.

[12] I do not intend to join the philosophical debate over whether there is such a thing as objective scientific evidence.

[13] See, for example, Blinder (1987b) and Lovell (1986).

[14] One example is the effects of anticipated future changes in policy.

[15] It should be noted that some new classicals disagree and see rational expectations as much more fundamental to the debate.

It is, of course, true that pre-1972 Phillips curves were ill-equipped to handle the food and energy shocks that dominated the period from 1972 to 1981 and, in consequence, badly underestimated inflation. But it is also true that Keynesians quickly added supply-side variables (like oil or import prices) to what had up to then been an entirely demand-oriented theory.[16] Soon thereafter supply shocks were also appended to empirical Phillips curves.[17] By the early 1980s, numerous studies had documented the fact that a conventional Phillips curve equation with a supply-shock variable (any one of several will do) fits the US data of the 1970s and 1980s extremely well.[18] The charge that empirical Keynesian models were, in Lucas and Sargent's (1978) words, 'wildly incorrect' is, well, wildly incorrect.

One objection frequently raised by supporters of new classical economics is that saving the Phillips curve after the fact by adding supply variables is like saving Ptolemaic astronomy by adding a new epicycle. I disagree. Any economic model is fundamentally a set of statements about the behaviour underlying supply and demand and the nature of the shocks impinging on each. For example, an empirical model of the market for wheat consists of a negatively sloped demand curve, a positively sloped supply curve, and some assumptions about the shocks hitting each. Analogously, a macroeconomic model must specify not only aggregate demand and supply behaviour but also the nature of the shocks that buffet the economy.

The empirical correlations implied by either sort of model depend on both the model's structure and the shocks that predominate during a particular historic period. For example, the same structural model of the wheat market will predict that price and quantity are negatively correlated if most of the shocks emanate from the supply side but positively correlated if most of the shocks come from the demand side. Analogously, pre-1973 Keynesian theory produced a negatively sloped statistical Phillips curve because of an unstated assumption that macroeconomic shocks come solely from the demand side—an assumption proven wrong by the events of the 1970s and 1980s. The very same model generates a positively sloped Phillips curve if the shocks come from the supply side, which is just what the econometric evidence says happened in the 1973-1981 period.

If you don't trust econometrics, the following back-of-the envelope calculation should help drive home the point. Keynesian economists in the US in the 1960s and early 1970s developed what I used to call the Brookings Rule of Thumb: that each point-year of unemployment above the natural rate reduces the rate of inflation by 0.4-0.5 of a percentage point. Using a 5.6 per cent natural rate, the US experienced about 15 point-years of extra unemployment between 1980 and 1985 and, during those years, the inflation rate declined about 6-7 percentage points. Once you see how well the rule of thumb worked, you understand why conventional Phillips curves fit data from the 1980s so well.

Why, then, was the alleged demise of the Phillips curve trumpeted so loudly and so widely? I think the reason was the conjunction of two events—one historical, the other intellectual.

First, when supply shocks came to dominate the data in the 1970s, the familiar negative correlation between inflation and unemployment—which is clearly visible on a scatter diagram of data for the 1950s and 1960s—disappeared. The Phillips curve could no longer be depicted in two dimensions. To those too unsophisticated to distinguish between a simple correlation and a multivariate relationship, that seemed equivalent to the death of the Phillips curve.

Second, Lucas' (1976) insightful critiques of econometric policy evaluation provided an elegant *a priori* argument for why an empirical Phillips curve might collapse under the weight of a more inflationary policy.[19] Briefly, the argument went like this. A prototypical empirical Phillips curve explains inflation by lagged inflation and unemployment:

$$\dot{p}_t = a(L)\dot{p}_{t-1} + f(U_t) + e_t, \tag{1}$$

[16] For the rudimentary theory, see Phelps (1978) and Gordon (1975). Phelps' ideas on the subject were first presented at an American Enterprise Institute meeting in April 1974; Gordon's were first offered at a meeting of the Savings and Loan Association that same month. Already in January 1974, Princeton graduate students were being asked (by me!) to analyze supply shocks in Keynesian models in examinations.

[17] See Gordon (1977).

[18] Some examples are Ando and Kennickell (1983), B. Friedman (1983), Gordon (1985), and Perry (1983). There are others.

[19] Though Lucas's paper was published only in 1976, it had been given at a Carnegie-Rochester conference in April 1973 and was well known in academic circles years before it was published.

but is meant to signify a theory in which inflation really depends on expected inflation and unemployment:

$$\dot{p}_t = E_{t-1}(\dot{p}_t) + f(U_t) + e_t. \quad (2)$$

It thus embodies an auxiliary hypothesis that the distributed lag $a(L)\dot{p}_{t-1}$ is a good statistical proxy for expected inflation. Lucas pointed out, correctly, that (1) will continue to fit the data well only as long as $a(L)\dot{p}_{t-1}$ remains the best predictor (i.e. the rational expectation) of inflation. If policy changes, the best forecasts of future inflation might also change, making (1) break down even if (2) is stable.

Academic readers of Lucas put two and two together and jumped like lemmings to the wrong conclusion. The facts were (a) that inflation rose and (b) that the correlation between inflation and unemployment changed. The (untested) assertion was that the Lucas critique explained why (b) followed from (a): the government had adopted a more inflationary policy, which in turn had changed a(L).

It was remarkable how uncritically the Lucas critique was accepted. Had governments really decided to 'ride up' the Phillips curve toward higher inflation, as Lucas claimed, or had they simply encountered bad luck from the supply side? The former was assumed even though the latter seems clearly to have been the dominant factor quantitatively.[20] Did the more inflationary environment shift the distributed lag $a(L)$? Rather than seek evidence on this point, partisans of the Lucas critique became econometric nihilists. Theory, not data, was supposed to answer such questions; and theory allegedly said yes.

But, in fact, a rise in inflation need not mean that the univariate autoregressive representation of inflation must change (other than its constant). Whether or not the lag coefficients $a(L)$ actually shifted in the early 1970s is an empirical question. To investigate whether or not such a shift took place, I estimated simple autoregressions for US inflation over the period 1955:2 to 1987:4 subperiods. As a way of guarding against the danger of choosing among competing regressions on the basis of prior beliefs, the lag length (four quarters) and the price index (the GNP deflator) were specified *a priori* and never changed. I tested for statistically significant breaks in the autoregression at the ends of 1970, 1971, 1972, and 1973. The resulting F statistics were as follows:

break period	F statistic
1970:4/1971:1	0.92
1971:4/1972:1	0.85
1972:4/1973:1	0.77
1973:4/1974:1	0.21

None of these F statistics is remotely close to conventional significance levels. Thus, there is no evidence for a shift in the lag coefficients $a(L)$. And that, in turn, suggests that the breakdown of the old-fashioned Phillips curve cannot be attributed to the reason emphasized by Lucas. The strongest evidence for a break emerges if the sample is split 1955:2-1970:4 vs. 1971:1-1987:4. In that case, the $a(L)$ coefficients sum to 0.73 in the first period and 0.88 in the second, which is an increase, though not a dramatic one.

I have already noted that, once supply variables are added, contemporary Phillips curves look much like their ancestors of 15 years ago. Supply shocks not only provide a more parsimonious explanation for both the rise of inflation and the fall of the Phillips curve, but one that can be substantiated empirically. Yet academic economists, at least American academic economists, opted *en masse* for Lucas's explanation, deserting Keynesianism in the process. Why? The rest of this section gives my personal answers. They are rooted in the sociology of science, in attachment to theory, and in ideology—not in empiricism. I take up the three factors in turn.

The Sociology of Economics

Many people have observed that economics has become a highly technical subject in recent decades, more so in the US than elsewhere. And technicians, of whatever discipline, prize technique; it's how the young cut their teeth. The rational expectations revolution was a godsend for aspiring young technicians. It not only pushed macroeconomic theory in more abstract and mathematical directions, but brought in its wake a new style of econometrics that was far more technically demanding than the old methods it sought to replace.[21]

The tools needed to carry out the new brands of theory and econometrics could not be found

[20] Blinder (1979) and (1982) traces the relevant history for the US and supports the statement, which holds even though the world-wide boom of 1972-1973 was surely demand-induced.

[21] Thomas Sargent and Lars Hansen led in developing the new econometric methods. Sargent always referred to it as 'a technology'.

in the kit bags of the older economists, which gave the young a heavy competitive edge. Not only were they better trained mathematically and, being younger, more flexible of mind, but they were also less distracted by other pursuits and hence more willing and able to absorb the new techniques. As an extra bonanza, the Lucas critique provided a reason to shun the previously accumulated stock of econometric results as unreliable. Thus freed of any need to absorb the knowledge of the past, newly-minted Ph.D. economists could concentrate on developing what they saw as the wave of the future.

It was a recipe for generational conflict within the discipline and, sure enough, the young were recruited disproportionately into the new classical ranks while few older economists converted.[22] Traditional Keynesian tools like IS/LM and large-scale macroeconometric models came to be viewed as relics of the past and, in a strange kind of guilt by association, Keynesian ideas like those discussed in Section 2 also came to be seen as outmoded. By 1980 or so, the adage 'there are no Keynesians under the age of 40' was part of the folklore of the (American) economics profession.

The saying, of course, was meant to encompass only *academic* economists and, indeed, only those in the elite institutions. In fact, virtually no non-academic economists converted to new classicism. Why the sharp bifurcation between professors on the one hand and business and government economists on the other? Part of the answer is that scholars are naturally the producers of new ideas while practitioners are the consumers. Fundamental debates over theory and statistical method belong in the academy, where the protagonists are better equipped to deal with them and have the luxury of a long time horizon. That, I suppose, is what ivory towers are for.

But another part of the explanation lies in the different market tests the two groups must meet. In academia, as in fashion, it is more important to be fresh and creative than to be correct. Cute models, after all, make snappy papers; the real world can be left to less original minds. I have heard it said that the surest route to academic success is to devise a clever proof of an absurd proposition. And dazzling displays of technical fireworks, perhaps accompanied by some impenetrable prose, regularly impress referees and editors of scholarly journals.

Incentives are quite different in business or government, where the important thing is to produce the right answer—or, rather, to *appear* to produce the right answer. Methodological innovation and purity count for little, cuteness for nothing, and technical virtuosity is unappreciated. A professional forecaster seeks accuracy, not scholarly kudos. A policy analyst wants to communicate with policy makers, not to dazzle them with technique.

That new classical ideas failed to migrate from the academy to the worlds of business and government—as Keynesian ideas had done 40 years earlier—suggests that they failed to meet the nonacademic market test: they did not produce useful results. But that is getting ahead of my story.

The Nature of Economic Theory

The triumph of new classical ideas in academia was also rooted in the nature of economic theory and in economists' fierce loyalty to it. We economists proudly distinguish ourselves from the lower social sciences by pointing to our illustrious theoretical heritage. In the economist's world, rational and self-interested people optimize subject to constraints. The resulting decision rules equating 'marginal this' to 'marginal that' lead to supplies and demands, which interact in markets to determine prices. These prices, in turn, guide the allocation of resources and the distribution of income. If not interfered with, markets tend to be highly competitive and have a strong tendency to clear by price. (Here the consensus begins to fray a bit.)

These are the canons of our faith. They are what gives economics the unity and cohesion that, say, sociology lacks. Rightly or wrongly, they also imbue economists with an imperialistic attitude toward the other social sciences—rather like Kipling's attitude toward India. We have a tight theory; they don't. We should treat the heathen kindly, if condescendingly, while we firmly propagate the faith.

Notice, however, that the central economic paradigm is entirely *microeconomic*. Keynesian macroeconomics coexists with it uneasily at best. In at least some Keynesian models, workers are less than rational. (For example, they may harbour money illusions.) Relative wages and notions of fairness probably matter in labour markets. Decision makers frequently bump into corners, so

[22] Regretfully, I have no data to support this quantitative assertion. Lucas, Sargent, Barro, and Wallace are, of course, 'older economists' in this context. But they were the founders of the new school of thought.

that optimal decisions are no longer described by neoclassical marginal conditions. Markets may not clear, and in fact may display surpluses for long periods of time; so trading takes place at 'false prices'. In all these respects and others, Keynesians have long been infidels in the neoclassical temple.

The strength of neoclassical fundamentalism has ebbed and flowed over the decades. The worldwide depression of the 1920s and 1930s undermined it severely, thus paving the way for the Keynesian revolution. The prosperity of the 1960s and early 1970s probably helped restore it. New classical economics was quite explicitly a revival of neoclassical orthodoxy, a return to what Lucas (1987) echoing Marshall, called 'the only engine for the discovery of truth'.

Keynesians had long felt an agonizing tension between the macroeconomics they taught on Mondays and Wednesdays and the microeconomics they taught on Tuesdays and Thursdays. New classicals explicitly sought to end this tension by making macroeconomics more like microeconomics. All supply and demand decisions were to be derived rigorously from neoclassical 'first principles'. Aggregate demand and supply schedules were to be viewed as blow-ups of interior solutions to individual optimization problems. Markets were to be viewed as perfectly competitive and clearing. If necessary, bothersome empirical phenomena like involuntary unemployment were to be ignored, defined out of existence, or ingeniously rationalized by convoluted theoretical arguments.

Methodological purity has a seductive attraction to mathematically minded technicians—which helps explain why rational expectations came to be so intimately tied up in the debate. Modelling expectations as rational—that is, as optimal subject to informational constraints—is the analogue of modeling consumers as maximizing utility and producers as maximizing profits. Rational expectations was therefore a natural accompaniment—and, indeed, a major impetus—to the 'back to basics' movement. It was no accident, then, that those who favoured frictionless, optimizing, market-clearing models were immediately attracted to rational expectations as a behavioural hypothesis without bothering to look for evidence. Linking rational expectations to new classicism (thus leaving 'irrational' expectations to the Keynesians) helped the new theory win converts in the same way that celebrity endorsements help sell products. Theoretically minded economists were predisposed to believe in rational expectations and, at first, took the new classical baggage along with it.

The Role of Conservative Ideology

There were also ideological overtones in the neoclassical revival which I have yet to mention, but which played an important role.

The basic neoclassical paradigm is profoundly conservative, as other social scientists—and, sometimes, our own students—remind us. Those who take it seriously as a description of the economy tend toward the Panglossian view of private economic transactions and look askance at government intervention. When this world view is transported from microeconomics to macroeconomics, it leads to theoretical models in which business cycles are benign, unimportant, or inevitable—perhaps all three. And it leads, as usual, to laissez-faire policy recommendations. For example, Edward Prescott (1986) asserts that 'costly efforts at stabilization are likely to be counterproductive' because the free-market business cycle is Pareto optimal.

Keynesians, as I indicated in Section II, do not buy any of this. They argue that the very existence of macroeconomics as a subdiscipline owes to the massive market failures that we observe during recessions but which the neoclassical paradigm rules out. They believe that recessions are important, malign, and ameliorable, and so are ready to support government interventions designed to stabilize aggregate economic activity. As James Tobin once remarked, they worry more about Okun gaps than Harberger triangles.

The relative strengths of conservative and liberal ideology obviously vary both over time and through space. My argument is that new classical theory could have attracted a large following only in a country and at a time when right-wing ideology was on the ascendancy, as was true in the United States in the 1970s and 1980s.[23] Though we academics live in ivory towers, the social winds blow there, too.

Many observers have noticed that the new classical revolution was mainly restricted to the United States; it never really caught on in Europe. That was no coincidence, I think, for right-wing

[23] Symmetrically, a conservative might argue that Keynesian ideas could only have caught on in a milieu (like that of the Great Depression) in which left-wing ideology was ascendant. Neither statement says anything about the validity of either doctrine.

ideology has long found more adherents in the US than in Europe. The timing was also no accident; new classicism took root just when the political balance in America was shifting toward the right. I don't believe such ideas would have sold in American academia during the 1960s.

What I have just said about the theoretical and ideological roots of new classical economics could equally well have been said about old classical economics. But the 1970s did not witness a revival of Pigou, or even of Friedman. It saw, instead, a movement towards the high-tech economic theory of Lucas and the high-tech econometrics of Sargent. The secret to the success of the new classical economics is that it managed to be at once ideologically backward looking and technologically forward looking. Given the temper of the times, that was a winning formula.

Or, rather, it was a winning formula in academia. Outside the academy, the emphasis on theoretical purity (at the possible expense of empirical validity) and technical wizardry were liabilities, not assets. In addition, the leaders of the new school, particularly Lucas and Sargent, were disinclined to press their views on policy makers because they deemed macroeconomic science insufficiently developed to support such advice. Finally, as we shall see in the next section, the empirical implications of new classical theory were wide of the mark. For all these reasons, the theory that swept academia made hardly a ripple in the world of policy.

IV The Rise of Keynesian Economics

I have argued that empirical evidence played little or no role in the fall of Keynesian economics in academia, which I have attributed instead to the theory's weak microeconomic underpinnings, to the curious sociology of our discipline, and to the rise of right-wing ideology.[24] The story behind the recent resurgence of Keynesianism is quite different, for here the empirical failures of new classical economics are central. In addition, however, new strains of theory are beginning to resolve the tension between microeconomics and macroeconomics in a fascinating way. Whereas new classical economists sought to remake macroeconomics in the image of neoclassical microeconomics, recent developments in economic theory may eventually lead to a reformulation of micro theory that resembles Keynesian economics. I will discuss each of these in turn, beginning with empirics.

Empirical Evidence Against the New Classical Paradigm

In view of the normally strong interplay between events and ideas, it is somewhat astounding that new classical economics caught on during the second half of the 1970s—a time when most of the world's industrial economies were struggling to emerge, often unsuccessfully, from deep and long recessions.

True to its classical roots, new classical theory emphasized the ability of a competitive price-auction economy to cure recessions by wage-price deflation. Its early forms attributed downturns to misperceptions about relative prices (such as real wages) that arise when people do not know the current price level, and implied that unemployment should vibrate randomly around its natural rate. But such misperceptions surely cannot be large in societies in which price indexes are published monthly and the typical monthly inflation rate is under 1 per cent; and they cannot be persistent if expectations are rational. Yet economic fluctuations in the late 1970s and 1980s were both large and persistent.

Later versions of new classical theory replaced monetary misperceptions with changes in perceived intertemporal terms of trade and added several features which produced persistent movements in employment and output.[25] But empirical research has never been able to find large intertemporal substitution effects. And theories that generate employment fluctuations from the supply side of the labour market stumble over the facts that labour supply looks to be quite inelastic (at least in the US) and real wages are nearly constant over the business cycle. They also have a hard time making the jump from persistent changes in *employment* to persistent—not to mention involuntary—*unemployment*. In stark contrast, the Keynesian model may be theoretically untidy; but it is certainly a model of persistent, involuntary unemployment.

So the events of the late 1970s seemed to support the incumbent theory and undermine the

[24] That does not mean that Keynesianism encountered no empirical problems. The most prominent one was probably the collapse of the money-demand equation in the US, Canada, and many other countries. While this is commonly, and correctly, considered a disaster for monetarism, it also poses a serious problem for the Keynesian LM curve.

[25] For a review, see Barro (1981a).

challenger. Yet the challenger prevailed. Curious.

Next came the 1980s, which were ushered in by another oil shock but were dominated by the Reagan-Volcker fiscal and monetary policy shocks and the European depression. I think it fair to say that new classical economics shed little light on any of these events. The events, however, cast deep shadows across the theory.

1 According to new classical theory, a correctly perceived deceleration of money growth affects real output only via its effects on anticipated inflation and real interest rates. Virtually no one thinks real interest rate effects are very large, which is why simple models often ignore them. Yet when the Federal Reserve and the Bank of England announced that monetary policy would be tightened to fight inflation, and then made good on their promises, severe recessions followed in each country.[26] Could it have been that the tightening was unanticipated? Perhaps in part. The Fed did seem to get carried away, and perhaps both central banks lacked credibility at first. But surely the broad contours of the restrictive policies were anticipated, or at least correctly perceived as they unfolded.[27]

Old-fashioned Keynesian theory, which says that *any* monetary restricion is contractionary because firms and individuals are locked into nominal contracts, seems more consistent with actual events, even though it doesn't explain why nominal contracts exist. Strike one against the new theory.

2 An offshoot of new classical theory due to Barro (1974) argued that debt-financed tax reductions should have neither real nor nominal effects because rational agents, correctly perceiving their future tax liabilities or those of their heirs, would act to offset them. The only observable consequence of such a policy, on this view, should be a rise in private saving to offset the government dissaving.

Naive Keynesian analysis, by contrast, sees the same event as an outward shift of the IS curve. If the LM curve is unchanged, real interest rates, real output, and the price level should all rise. If, as happened in the US, the stimulus to demand is snuffed out by contractionary monetary policy, real interest rates should rise even more. There is no reason to expect the private saving rate to rise.

Econometric studies of the Barro hypothesis have yielded highly inconclusive results. The answer seems to depend on who asks the question.[28] Observation of the real world seems to deliver a stronger verdict, however. Taxes were cut massively in the US between 1981 and 1984. Given the thin economic rationale for the policy, the Reagan tax cuts come as close to a truly exogenous fiscal experiment as we are ever likely to get—just the sort of thing that helps scholars discriminate among competing theories. What happened? The private saving rate did not rise. Real interest rates soared, even though a surprisingly large part of the shock was absorbed in exchange rates rather than in interest rates (so that net exports were crowded out rather than domestic investment). Real GNP growth seems not to have been affected; it grew at about the same rate as it had in the recent past.

It would be unfair to say that neoclassical theory offers no explanation for these events. A sudden rise in the productivity of capital in the US would be expected to raise domestic interest rates (and rates of return), draw in capital from abroad (thus causing a current account deficit), and appreciate the currency The only trouble with this explanation is that the alleged jump in the productivity of capital is unobservable and unexplained. Why, for example, did it not also happen in other countries?[29] Why did measured productivity growth not accelerate? Furthermore, neither private saving nor investment really rose much as a share of US GNP. The neoclassical explanation does successfully explain the puzzling rise in the US stock market. But, if the productivity of capital soared only in the US, why did stock markets boom all over the world? And if the rise in capital's productivity was global, why did capital come pouring into the

[26] The story is even worse than this because money growth did not actually decelerate, except fleetingly, in either country. But that has to do with financial innovation and the collapse of the money-demand equation, which is as much a problem for Keynesian theory as for new classical theory.

[27] In the case of the 1973-1975 recession, Blinder (1981) points out that 'unanticipated money', as defined empirically by Barro and Rush (1980), does not come close to explaining the recession. I know of no similar calculation for the 1980s, but it would also not come close since it was declining velocity growth, not declining money growth, that made money tight in 1981-1982.

[28] For a comprehensive review of the evidence, see Brunner (1986).

[29] The US corporate tax cuts enacted in 1981 have been suggested as an explanation. But there is controversy about this. See Blanchard and Summers (1984), Niskanen (1988) and Bosworth (1985).

United States? Strike two against new classical theory.

3 Then we have the nasty matter of the European depression which, in some countries, has been as long and as deep as the depression of the 1920s and 1930s and which, at this writing, is still in progress. The Keynesian explanation is straightforward. Governments, led by the British and German central banks, decided to fight inflation by highly restrictive monetary and fiscal policies. The anti-inflationary crusade was facilitated by the European Monetary System which, in effect, spread the stern German monetary policy all over Europe. If Keynesian theory has any trouble explaining these events, it is because modern versions which incorporate the natural rate hypothesis are not Keynesian enough. (More on this below.)

The new classical explanation of the European depression is . . . well, frankly, I am not sure there is one. Proponents of new classicism, and conservative economists in general, point to microeconomic interferences in labour markets. But most of these policies (like generous unemployment insurance) were in place in 1973 when unemployment was extremely low. In my country, three strikes and you are out. It is therefore not surprising that new classical economics began to lose supporters.

Even this recent history might not have been decisive, given the insular attitudes of academic economists. But there was more scholarly evidence as well.

1 New classical economists had made the Phillips curve a test case and interpreted it in their favour. But, as I have already related, a succession of econometric studies in the 1980s all concluded that the empirical Phillips curve was alive and well once you allowed for supply shocks, at least in the US. Gordon (1987) argues much the same for Europe.

2 The newly developed technology for estimating models with rational expectations began to be applied; and the results were almost uniformly unfavourable to the new classical view.[30] Normally, the trio of hypotheses that (a) expectations are rational, (b) decision rules are first-order conditions to well-defined optimization problems, and (c) markets clear had to be tested jointly. And almost always the joint hypothesis was resoundingly rejected. Which was the weak leg of the tripod? Most economists, instinctively attracted by rational expectations, thought it was market clearing. But it really didn't matter, for the new classical edifice required the entire lot.

3 Finally, the validity of the rational expectations hypothesis itself was called into question. Directly observed expectational data were used to test for rationality. Mostly, these were tests of the *weak* forms of rationality: unbiasedness and/or efficiency. They did not, and could not, test for the much stronger form of rational expectations required by new classical theory: that people's subjective expectations match the mathematical expectations implied by the model. Nonetheless, most of these tests rejected rational expectations.[31]

So by 1983 or 1984, academic macroeconomics was in the following somewhat embarrassing position. Keynesian economics had been maligned on the grounds that its theoretical foundations were prosaic at best, non-existent at worst, and certainly inelegant. Its heir apparent, new classical economics, boasted an elegant and technically sweet theory which passed internal consistency checks with flying colours, but which failed miserably when it came to consistency with observation. In the shorthand that was used both then and now, Keynesian economics was 'bad theory' which nonetheless seemed consistent with the facts while new classical economics was 'good theory' which, unfortunately, did not describe the way the world works.

This is, it seems to me, a curious usage of the terms 'good' and 'bad'—one which reflects the academic economist's preoccupation with elegance and mathematical structure over relevance and empirical accuracy. By these criteria, the 'good theory' is not the one that explains the data best, but rather the one that is truest to neoclassical orthodoxy—which sees people as self-interested and maximizing individuals, who calculate well, have no money illusion, and don't leave unexploited profit opportunities. That is the attitude of a mathematician who deals in logical constructs, not of a scientist who deals in facts. If real people are social beings who care not just about their own well-being but also about their relative position in society, who are not very good at doing calculations or deflating by a price index, and who have other things to do besides maximizing all the time, then Keynesian theory may be the 'good' theory after all—even if it is contaminated by ideas from other social sciences.

Of course, a theory can only be judged good

[30] See, for example, Rotemberg (1984).

[31] Lovell (1986) offers a convenient summary of many studies, one of them by Muth (1985)!

or bad relative to some competitor. There are several senses in which Keynesian theory was and is not good enough. One is that empirical problems continue to beset macroeconometric models built in the Keynesian tradition. The collapse of the LM curve is just the most obvious of these empirical failures, not the only one. Another problem is that the Keynesian model has such a weak microtheoretic structure that it is hard—some would say impossible—to do welfare economics with it. While most Keynesians believe that successful stabilization policies improve social welfare, the theory itself does not really justify that belief.

In any case, the view in academia was then (and in some circles still is) that economists had to choose between a tight theory with severe empirical problems and a sloppy theory that nonetheless worked better empirically. There were two ways to proceed. Either efforts could be made to make Keynesian economics more theoretically respectable, or energy could be devoted to bringing new classical economics into closer contact with reality. Research is proceeding in both directions. In my judgement, the work that is being done along the first route is much the more interesting and promising, so I will dwell on that.

New Theoretical Foundations for Keynesian Economics

Four new developments in economic theory, all of them still in progress, seem to me not only to shore up the theoretical foundations of Keynesianism, but actually to push micro theory in a Keynesian direction. None of them puts sticky nominal wages at center stage.

(i) *Monopolistic Competition* The first idea is to build a macro structure on the foundations of monopolistic, rather than perfect, competition. This helps produce a Keynesian environment in two respects. First, it leads to theoretical models in which firms always want to sell more at current prices because price exceeds marginal cost. Second, output levels in monopolistic equilibria are generally below the social optima, which echoes the Keynesian idea that employment is typically too low. The knotty intellectual problem was always that monopolistic competition theory pertains strictly to *relative* prices while nominal magnitudes matter in Keynesian macroeconomics.

Mankiw (1985) and Akerlof and Yellen (1985) solved this problem at more or less the same time by adding fixed costs of changing nominal prices to the model.[32] Suppose the money supply (M) falls by a small amount. Fixed costs of changing prices will deter some firms from cutting their nominal prices even though their first-bet nominal prices in a frictionless world would be proportional to M. In consequence, the price level will fall less than proportionately to M and real balances, and hence aggregate demand, will decline. More than likely, so will social welfare. On the up side, a small enough rise in M will induce only some firms to pay the fixed costs of raising their prices. So real balances, aggregate demand, and social welfare will all rise.

Mankiw and Akerlof-Yellen pointed to a kind of externality later made more precise by Blanchard and Kiyotaki (1987). This idea is important because economists like to rest the case for government intervention on externalities.

The argument goes as follows. Since each firm sits at the top of its profit hill, a small deviation from its first-best relative price has only a small effect on its profits. But because the pre-existing (monopoly) distortion causes output to be below the socially optimal level, the loss in social welfare is greater than the drop in profits. Although the firm loses little from its deviation from optimality, society loses much.

The so-called aggregate demand externality arises in the following way—which should sound familiar to Keynesians. In equilibrium, individual firms do not find it profitable to reduce their prices. Yet, if all firms would cut their prices simultaneously, real balances would rise, aggregate demand would expand, and all firms' profits (and social welfare) would rise. In a decentralized economy, there is no way to achieve such coordinated price cutting. But a sufficiently large rise in the money stock can accomplish the same thing—just as Keynes suggested more than 50 years ago.

This new strain of theorizing is appealing because it relies on just three seemingly realistic assumptions: (a) that demand curves for individual firms slope down: (b) that firms maximize profits; and (c) that lumpy transactions costs are incurred whenever a nominal price is changed. However, it does not provide a complete theoretical

[32] Actually, Akerlof and Yellen (1985) appealed to 'near rationality' rather than to fixed costs. But the two amount to essentially the same thing. Also, the costs of changing prices do not have to be exclusively fixed to make the theory work.

justification for Keynesian economics for several reasons.

The first is a technical point to which I will return. In a dynamic economy, fixed costs of price adjustment should lead firms to allow their relative prices to drift away from their first-best profit-maximizing levels most of the time. In that case, firms are not atop their profit hills, so a small change in a relative price (caused, say, by inflation) may have a large effect on profits—not the small effect envisioned by the theory.

Furthermore, while the theoretical results of monopolistic competition models are consistent with Keynesian insights, they lack certain important Keynesian features. For one thing, the main finding is that output is normally *too low*, not that it is *too variable*. Hence the obvious policy intervention is an output subsidy, not macro stabilization policy. Using this class of model to justify the Keynesian belief that output is too variable turns out to be quite tricky.[33] For another, the models do not produce any natural notion of involuntary unemployment which, as I noted earlier, plays a central role in the Keynesian tradition.

(ii) Efficiency Wages The next group of theories I will consider addresses itself directly to the involuntary unemployment question. Several microeconomic theories of the labour market based on imperfect, and usually asymmetric, information show that the market can be in equilibrium—in the sense that there are no unexploited profit opportunities—with supply unequal to demand. The simplest, and to me the most appealing, of these is the efficiency wage model. It also seems to accord best with common sense.

Here is a simple example that makes the point starkly.[34] Suppose output, $f(eL)$, depends on labour input in efficiency units, where L is physical labour input and e indicates effort. Suppose further, and this is the efficiency wage hypothesis, that e rises when the real wage, w, rises. Then profits:

$$f(e(w)L)-wL,$$

will be maximized when two conditions hold. First, the marginal product of labour must equal the wage per efficiency unit, $w/e(w)$. Second, the wage must be set at the point (if there is one!) at which the function $e(w)$ has unit elasticity.[35] The second condition fixes the equilibrium wage, call it w^*, on purely technological grounds. Given w^*, the first condition then determines optimal employment, L^*, as long as labour supply at w^* is at least as great as L^*.

An equilibrium with unemployment arises if L^* happens to fall below labour supply at w^*. It is a true equilibrium, not just a long-lasting disequilibrium, because profit-maximizing firms have no interest in reducing wages. It has involuntary unemployment that persists for an indefinite period of time because, at wage w^*, labour supply exceeds labour demand. All this sounds very Keynesian. But there is a hitch. Like the monopolistic competition models, efficiency wage theories are fundamentally models of *relative* prices and *real* wages. They have nothing to say about nominal magnitudes, and hence allow no role for nominal money, until they are altered to include fixed costs of changing nominal wages or prices.[36] Nor, in their current state of development, do they have much to say about *fluctuations* in employment.

Efficiency wage models do, however, have at least one more Keynesian aspect that I think important: they focus attention on relative wages.[37] Ask yourself why higher wages enhance productivity. Theorists have provided many possible answers, but the most plausible for advanced, industrial nations (where malnutrition is not the issue) is that workers who are paid well are inclined to perform better for their employers. Such behaviour can be rationalized if workers care about relative wages, as Keynes believed.

(iii) Fixed Costs and Inertia A third important recent development in micro theory is the revision of the standard theory of optimization to include fixed costs of changing a decision variable. This idea was mentioned earlier in the context of pricing decisions, where it helps impart inertia to the price level. But it also has obvious applications to inventory behaviour (where the idea originated), to the demands for both consumer and producer

[33] See Ball and Romer (1987). However, DeLong and Summers (1988) argue that Keynesians ought to be more concerned with output being too low and less concerned with it being too variable.

[34] For much greater detail, including applications to markets other than that for labour, see Greenwald and Stiglitz (1987a, 1987b).

[35] The proof is straightforward. Maximizing profits with respect to w gives the first-order condition $f'(eL)e'(w) = 1$, while maximizing with respect to L gives $f'(eL) = w/e(w)$. Putting the two together implies $we'(w)/e = 1$.

[36] That is what Akerlof and Yellen (1985) do.

[37] I have elaborated on this theme at greater length in Blinder (1988).

durables, to the demand for money, and to portfolio choice more generally.[38] Though the mathematics can get complicated quite quickly, the basic idea is completely intuitive and easy to grasp. I illustrate it in the case of a consumer durable, but the same idea applies in other contexts.

Suppose a consumer must pay a fixed transactions cost whenever she purchases a durable good, such as a car. Then, each time one of the basic determinants of demand for automobiles (such as income, interest rates, or relative price) changes, she is faced with the following choice. If she switches to the new first-best optimal car, she must pay a fixed cost. If she does not, she suffers an implicit utility cost from having a suboptimal car. Obviously, it does not pay her to adjust her car purchases continuously, for that would entail exhorbitant transactions costs. Rather, optimal behaviour leads to a decision rule something like the so-called (S,s) rule of inventory theory: when the quality of the car deteriorates to some lower bound, s, purchase a new car of quality S; otherwise, do nothing. The parameters S and s are chosen optimally in view of transactions costs, income, and other pertinent information.

Once you think about continuous reoptimization and (S,s) as alternative models of behaviour, even for a little while, two things become clear. First, (S,s) is almost certainly more descriptive of the way people and businesses actually behave; they do nothing for long periods of time and then make large changes in their behaviour. Second, (S,s) behaviour is almost certainly a more sensible theoretical model on *a priori* grounds than continuous reoptimization.

Obviously, (S,s)-type reasoning provides different microfoundations for the traditional aggregative behavioural equations (consumption, investment, money demand, etc.) that constitute macroeconomic models. But what does it have to do with Keynesian versus new classical economics? Three things, principally. First, in a world with important fixed costs, optimizing agents are typically *not* at neoclassical tangencies where marginal this equals marginal that. Instead, behaviour displays a substantial amount of inertia. In the case of price setting, that is a characteristic Keynesian position, as I noted earlier.[39] Second, it leads us to expect to see occasional large adjustments to what appear to be small economy-wide shocks as a substantial number of decision makers trip their (S,s) boundaries simultaneously. Third, the (S,s) view of the world suggests that a more volatile economic environment imposes real costs on individuals and businesses by making them trip their (S,s) barriers more frequently.[40] Though I have not seen the argument worked out formally, this would seem to support the traditional Keynesian advocacy of stabilization policy.

(iv) Hysteresis Finally, it is important to mention the development of modern models of 'hysteresis', that is, models in which the economy's equilibrium state depends on the path we follow to get there, for these bring Keynesian economics back with a vengeance.

Old-fashioned Keynesian models assumed such hysteresis, without thinking much about it, and without using the fancy name. For example, the simplest Keynesian cross model, if taken literally, asserts that equilibrium can occur at *any* level of output, if aggregate demand is high enough. Similarly, the original Phillips curve implied that the economy could achieve equilibrium at a wide variety of (permanent) unemployment rates, each with its own unique (permanent) inflation rate. Both of these ideas were swept away by the natural rate revolution in the late 1960s and early 1970s and came to be thought of as muddled thinking. Economists, Keynesian or otherwise, came to believe that the long-run Phillips curve was vertical; no matter what happened to the economy in the interim, it could come to rest only at a unique 'natural' rate of unemployment determined by microeconomic factors.

Spurred on, I think, by observing what has happened in Europe, modern theorists are now constructing models that do not have the natural rate property. In these super-Keynesian models, expansionary demand management policies can raise employment permanently. In a neat reversal of Say's Law, demand here creates its own supply. Why might this be so? One simple and obvious mechanism is based on human capital. Suppose workers who are more experienced are also more

[38] On inventories, see Blinder (1981). On consumer durables, see Bar-Ilan and Blinder (1988). On investment, see Dixit (1988). On the demand for money, see Bar-Ilan (1987). On portfolio choice, see Grossman and Laroque (1987).

[39] This need not always be true. Caplin and Spulber (1987) show that price-level inertia may be absent in certain cases. But these are very special steady state cases.

[40] However, the (S,s) range can (and presumabaly will) be widened if volatility increases.

productive, perhaps due to learning by doing on the job, and that, conversely, human capital deteriorates when not in use. Then a demand-induced boom will build human capital and hence raise potential GNP for the future; so output can be permanently raised. Conversely, a recession which idles workers will deplete the human capital stock and hence lead to lower potential GNP in the future There is, then, no 'natural' level of employment. The equilibrium level depends on what came before.

The most popular and best developed hysteresis models nowadays are based not on human capital, but rather on the conflict between insiders and outsiders.[41] Suppose unions decide on wages, mindful of the fact that higher wages lead to lower employment but caring only about its members (the insiders). Suppose further that only employed workers are union members; outsiders have no voice in the union's decisions. Now let a recession lead to lay-offs. The union's membership shrinks and, hence, the union begins to give more weight to higher wages and less weight to higher employment. That is, its optimal wage rises and its optimal employment level falls. The outsiders who lack jobs object to this decision, but have no way to change it; they are disenfranchized. The lower employment level brought about by the recession therefore becomes permanent. In the other direction, of course, this vicious circle becomes a virtuous circle. If a demand-induced boom leads to new hiring, some outsiders are transformed into insiders and the protected level of employment rises.[42]

V *In Conclusion*

The empirical evidence does not yet dictate that we adopt these four theoretical innovations. Most industries seem monopolistically rather than perfectly competitive; but no one has yet established that the major costs of price adjustments (and other adjustments) are fixed rather than variable. Nor is the evidence on the efficiency wage hypothesis overwhelming. And hysteresis seems to characterize some economies some of the time, not all economies all the time. But at least these hypotheses have not been refuted by the data.

If we put all four of these theoretical features together—an act of extreme *chutzpah*, to be sure—a thoroughly Keynesian world emerges. Decision variables, including nominal prices and wages, are inertial. Markets often equilibrate with excess supply. So, in particular, involuntary unemployment is common and firms have chronic excess capacity. At least within some range, the economy's equilibrium can be changed by demand management policies because there is no natural rate. Again within some range, welfare can be improved by expanding aggregate demand and by reducing the amplitude of cyclical fluctuations.

This world is different in every particular from the world envisioned by the new classical economics. But its theoretical foundations are no less strong, and perhaps stronger, which is why Keynesian economics now seems to be on the ascendancy in academia. More importantly, it sounds more like the world we live in, which is why some of us find new Keynesian theorizing so hopeful.

[41] See Lindbeck and Snower (1986), Blanchard and Summers (1986), and Drazen and Gottfries (1987).

[42] Acceptance of this model does not necessarily lead to advocacy of expansionary demand-management policies. It might lead, instead, to policies that weaken the power of insiders.

REFERENCES

Akerlof, George and Yellen, Janet L. (1985), 'Can Small Deviations from Rationality Make Significant Differences to Economic Equilibria?', *American Economic Review*, Vol 75, No. 4, September, 708-20.

Ando, Albert F and Kennickell, Arthur (1985), '"Failure" of Keynesian Economics and "Direct" Effects of Money Supply: A Fact or a Fiction', University of Pennsylvania, mimeo

Ball, Laurence and Romer, David (1987), 'Are Prices Too Sticky?' National Bureau of Economic Research Working Paper No. 2171, February.

Bar-Ilan, Avner (1987), 'Stochastic Analysis of Money Demand Using Impulse Control', Dartmouth College, mimeo

Bar-Ilan, Avner and Blinder, Alan (1988), 'Consumer Durables and the Optimality of Usually Doing Nothing', January, mimeo.

Barro, Robert J. (1981a), 'The Equilibrium Approach to Business Cycles', in his *Money, Expectations, and Business Cycles*, Academic Press, New York, 41-78.

—— (1981b), 'Output Effects of Government Purchases', *Journal of Political Economy*, Vol. 89, No. 6, December, 1086-1121.

—— (1974), 'Are Government Bonds Net Wealth?', *Journal of Political Economy* **82**, November/December, 1095-1117

—— and Mark Rush (1980), 'Unanticipated Money and Economic Activity', in Stanley Fischer (ed.), *Rational Expectations and Economic Policy*, University of Chicago Press, Chicago, 23-48.

Blanchard, Olivier and Kiyotaki, Nobuhiro (1987), 'Monopolistic Competition and the Effects of Aggregate Demand', *American Economic Review*, Vol. 77, No. 4, September, 647-66.

Blanchard, Olivier and Summers, Lawrence (1984), 'Perspectives on High World Real Interest Rates', *Brookings Papers on Economic Activity* **2**, 273-324.

(1986), 'Hysteresis and the European Unemployment Problem', *NBER Macroeconomics Annual 1986*, 15-78.

Blinder, Alan S (1988), 'The Challenge of High Unemployment', *American Economic Review* **78**, May, pp. 1-15.

—— (1987a), *Hard Heads, Soft Hearts: Tough-Minded Economics for a Just Society*, Addison-Wesley.

—— (1987b), 'Keynes, Lucas and Scientific Progress', *American Economic Review* **78**, May, 130-36.

—— (1982), 'The Anatomy of Double Digit Inflation in the 1970s', in R.E. Hall (ed.), *Inflation: Causes and Effects*, University of Chicago Press, Chicago. pp. 261-82.

—— (1981a), 'Retail Inventory Behaviour and Business Fluctuation', *Brookings Papers on Economic Activity* **2**, pp. 443-505.

—— (1981b), 'Supply-Shock Inflation. Money, Expectations, and Accommodation', in M.J Flanders and A. Razin (eds), *Development in an Inflationary World*, Academic Press, 61-101.

—— (1979), *Economic Policy and the Great Stagflation*, Academic Press.

Bosworth, Barry P. (1985), 'Taxes and the Investment Recovery', *Brookings Papers on Economic Activity* **1**, pp. 1-45

Brunner, Karl (1986), 'Fiscal Policy in Macro Theory: A Survey and Evaluation', in R.W. Hafer (ed.), *The Monetary versus Fiscal Policy Debate*, Rowman & Allanheld, 33-116.

Caplin, Andrew and Spulber, D. (1987), 'Menu Costs and the Neutrality of Money', *Quarterly Journal of Economics*, November.

DeLong, Bradford and Summers, Lawrence (1988), paper in preparation for Brookings Panel, September.

Dixit, Avinash (1988), 'Entry and Exit Decisions Under Uncertainty', Princeton University, May, mimeo.

Drazen, Allan and Gottfries, Nils (1987), 'Seniority Rules and the Persistence of Unemployment in a Dynamic Optimizing Model', mimeo.

Fischer, Stanley (1977), 'Long-Term Contracts, Rational Expectations, and the Optimal Money Supply rule', *Journal of Political Economy* **85**, 191-205.

Friedman, Benjamin (1983), 'Recent Perspectives In and On Macroeconomics', National Bureau of Economic Research, Working Paper 1208.

Friedman, Milton (1968), 'The Role of Monetary Policy', *American Economic Review* **58**, 1-17.

Gordon, Robert J. (1987), 'Wage Gaps vs. Output Gaps: Is There A Common Story for All of Europe?', National Bureau of Economic Research Working Paper No. 2454, December

—— (1985), 'Understanding Inflation in the 1980s', *Brookings Papers on Economic Activity*, 263-99.

—— (1977), 'Can the Inflation of the 1970s Be Explained?', *Brookings Papers on Economic Activity*, 253-77.

—— (1975), 'Alternative Responses of Policy to External Supply Shocks', *Brookings Papers on Economic Activity* **1**, 183-206

—— (1972), 'Wage-Price Controls and the Shifting Phillips Curve', *Brookings Papers on Economic Activity*, 385-421.

Greenwald, Bruce and Joseph E. Stiglitz (1987a), 'Keynesian, New Keynesian and New Classical Economics', *Oxford Economic Papers* **39**, 119-32.

—— (1978b), 'Imperfect Information, Credit Markets and Unemployment', *European Economic Review* **31**, 444-56.

Grossman, Sanford and Laroque, Guy (1987), 'The Demand for Durables and Portfolio Choice under Uncertainty', Princeton University, June, mimeo.

Kuhn, Thomas S (1962), *The Structure of Scientific Revolutions*, University of Chicago Press, Chicago.

Lindbeck, Assar and Snower, Dennis (1986), 'Wage Setting, Unemployment, and Insider-Outsider Relations', *American Economic Review* **76**, May, 235-39.

Lovell, Michael E. (1986), 'Tests of the Rational Expectations Hypothesis', *American Economic Review*, Vol, 76, No. 1, March, 110-24.

Lucas, Robert E. Jr. (1987), *Models of Business Cycles*, Basil Blackwell

—— (1981), 'Tobin and Monetarism: A Review Article', *Journal of Economic Literature* **XIX**, June, 558-67

—— (1976), 'Econometric Policy Evaluation. A Critique', in Karl Brunner and Allan H Meltzer (eds), *The Phillips Curve and Labor Markets*, Carnegie-Rochester Conferences on Public Policy, Vol. 1, North-Holland Publishing Company, Amsterdam

—— and Thomas Sargent (1978), 'After Keynesian Macroeconomics', in *After the Phillips Curve; Persistence of High Inflation and High Unemployment*, Conference Series 19, Federal Reserve Bank of Boston.

Mankiw, N. Gregory (1985), 'Small Menu Costs and Large Business Cycles: A Macroeconomic Model of Monopoly', *Quarterly Journal of Economics* **100**, May, 529-39

Muth, John F. (1985), 'Short Run Forecasts of Business Activity', Indiana University, March, mimeo.

Niskanen, William A. (1988), *Reaganomics: An Insider's Account of the Policies and the People*, Oxford University Press, Oxford.

Perry, George L. (1983), 'What Have We Learned About Disinflation?', *Brookings Papers on Economic Activity*, 587-602.

Phelps, Edmund S (1978), 'Commodity-Supply Shock and Full-Employment Monetary Policy', *Journal of Money, Credit and Banking*, Vol. X, No. 2, May, 206-21.

—— (1968), 'Money-Wage Dynamics and Labor Market Equilibrium', *Journal of Political Economy* **78**, 678-711.

Prescott, Edward (1986), 'Theory Ahead of Business Cycle Measurement', Federal Reserve Bank of Minneapolis Research Department Staff Report 102, February.

Rotemberg, Julio J. (1984), 'Interpreting Some Statistical Failures of Some Rational Expectations Macroeconomic Models', *American Economic Review* **74**, 188–93.

Taylor, John B. (1980), 'Aggregate Dynamics and Staggered Contracts', *Journal of Political Economy* **88**, 1–23.