

WHATEVER HAPPENED TO KEYNESIAN ECONOMICS?

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### Introduction

For some thirty years, macroeconomics has been embroiled in the controversy between Keynesian and Monetarist theory. That controversy is not over yet. Surprisingly little has been definitively settled. But it must be confessed that over the last decade or more the Keynesian "team" has done so badly that the fans hardly care to come out to watch the game any longer. More recently, things have not been going so well for the Monetarists either -- which does not help in maintaining public interest in the proceedings. But, tiresome as such inconclusive controversies do become, we cannot simply walk away from this one. So many issues of great public importance depend for their resolution on coming to an adequate understanding of the macroeconomy -- and Keynesianism and Monetarism are still the main rivals in this field.

The question I want to address is this: Why did Keynesian economics lose influence to the degree that is the case today? The reasons that I will give may be divided into internal and external ones.

In a book that is now almost twenty years old,<sup>1</sup> I maintained that Keynesian economic theory was beset by serious internal problems. There were conceptual and theoretical problems that the Keynesians did not bother to take care of as long as standard

Keynesian models seemed to do all right for all practical purposes. The neglect of these internal conceptual issues, I argued, did not augur well for the future health of Keynesian economics. Needless to say, I have not had reason to change my mind.

It seems appropriate, however, to shift the emphasis in telling the story of "Whatever Happened to Keynesian Economics" to external factors. Simplistically put, the world did not stand still during the decades of the controversy -- and that, rather trite, observation turns out to be germane to the story.

There is a famous (and perhaps too often quoted) saying by Albert Einstein: "Der Herr Gott is raffiniert, aber boshaft ist er nicht" -- "The Good Lord is subtle, but he is not mean." The Lord, meant Einstein, may have made his laws of nature difficult to discover but once you have found them out, he will not change them on you. This is the comforting creed of physicists. They know that if they repair to the ivory tower to settle some quarrel amongst themselves, and even if it takes some time, they will come back out into the same real world that they left. Economists have no such assurance. Our subject matter changes even as we quarrel about it -- and, when the world does not stand still, it may change in ways that can favor one or the other side in an economic controversy.

The aspect of the changing world that is important to the story of the declining influence of Keynesian theory is the postwar evolution of the monetary system. As this system evolved,

Keynesian economics adapted only slowly and incompletely -- and with many creaks and groans. Thus Keynesians were caught defending positions that were no longer defensible -- although roughly right for a bygone era. This gave some easy victories to their Monetarist critics.

The economics profession at large has, however, drawn conclusions from the Keynesian defeats in the controversy that I believe to be in some important respects quite wrong -- even dangerously wrong. There was a healthy baby in that Keynesian bathwater, you see. Nice kid, really. Perhaps he could be resuscitated?

#### External Factors: Changing Monetary Regimes

The expectations of the public play an important role in macroeconomic theory. The reason is very simple. The same disturbance to the economy or the same policy action will have very different immediate results depending upon what the state of expectations happens to be. With one state of expectations, a decision to increase the supply of money, for instance, may cause a rapid rise in the price-level with little change in output and employment. With a different state of expectations, it may be followed by rising output and employment with very little inflation.

In order to predict the consequences of policy actions, therefore, we would need to know what the state of expectations is at the time. The trouble is, of course, that these expectations are on the whole unobservable. This is one of the

main reasons why macroeconomists do not compare favorably with natural scientists when it comes to predictions. The state of expectations, we sometimes say, is in a 'black box'. The term was originally used, I believe, in connection with bomb disposal during World War II. If you can infer what the state of a bomb is on the inside, a perfectly safe way to handle it can often be found. If you cannot infer what it is -- if the bomb is a black box -- it may blow up in your face without forewarning. Just like macroeconomic policy!

One very important lesson that we have learned from the New Classical economists is that this black box problem can sometimes be handled adequately by assuming that people generally understand the monetary regime that they have to live with and form their expectations accordingly. This analytical procedure we refer to as the rational expectations approach. Today it is very widely used also by people who do not otherwise have much in common with New Classical economics.<sup>2</sup>

The concept of a 'monetary regime' may thus be given the following two-part definition: It is, on the one hand, a system of expectations that governs the behavior of the public and, on the other, a consistent pattern of behavior on the part of the policy-making authorities that will sustain these expectations. A society may, in effect, choose a monetary regime for itself by adopting a set of rules for the monetary authorities to follow and by letting people's expectations adapt to these rules. The so-called 'rules of the gold standard' would be an example -- except that these rules changed quite a bit over time.

Nonetheless, it should be clear that people's expectations about the future behavior of the price level, for instance, are bound to be different under all variations of the gold standard from what they would be under inflationary regimes such as have characterized Argentina, Brazil or Israel until recently.

In choosing or constructing a monetary regime, providing for the predictability and stability of the nominal price level will normally be a fundamental objective. We have two basic, inherited ideas of how a society may achieve this. One, I call "quantity control" and the other "convertibility control." The macroeconomic theory appropriate to a regime relying for its nominal stability on convertibility is in several important respects very different from the one appropriate to a regime relying on government control of the stock of money.

The main points are the following. Under quantity control, the monetary authorities fix the quantity of money and allow the markets to determine the corresponding equilibrium level of nominal prices. Under convertibility, the government fixes the nominal price of gold (for example) and leaves it to the banks and their customers to determine the corresponding equilibrium stocks of money and other liquid assets. From the standpoint of the government, the first is a "quantity-fixing, price-taking" and the second a "price-fixing, quantity-taking" strategy.

The short-run macroeconomic theory or monetary policy doctrine appropriate to the one pure case is very different from the one fitting the other. Under convertibility, the commitment

to redeem money in gold (or foreign currency) on demand means foregoing the option of controlling the money stock. Roughly speaking, the money stock is determined by demand rather than by supply. In a system where the money stock adjusts to the price-level rather than the other way around, however, the central bank can worry about the price and availability of credit and their effects on real activity in the economy. If convertibility were generally to be seen to guarantee the price level, the public will have "inelastic expectations", that is, whenever the price-level departs a bit from the longer term trend set by the supply and demand for gold, people expect it to return to trend. In such a system, changes in the central bank discount rate changes a real price and changes in bank reserves change the real volume of credit supplied. The leverage over output and employment that the monetary authorities can gain in this way is rather limited, however.

Much of the monetary policy theory appropriate to the convertible regime has a distinctly Keynesian ring to it. (Note, however, that Keynesians have in general not regarded the validity of their theory as in any way restricted to convertible regimes). By the same token, this monetary policy doctrine has been the target of persistent monetarist criticism. Monetarist theory is itself best suited to the pure quantity control case. When the economy is on a pure fiat standard, control over some nominal stock becomes a necessity in order to provide the system with a nominal anchor. If the authorities try to govern real credit, and do not keep track of the money stock, they are likely

to fail at their primary task of providing nominal stability. Interest-rate targeting of monetary policy, which is a natural tactic when convertibility takes care of the price-level, is positively dangerous under these conditions -- it threatens total loss of control over the price-level.

Monetary policy is clearly "effective" in such a system in the sense that it can bring about large changes in money income (by changing the money stock and letting the price-level adjust). What is not so clear is whether it can have a reliable, predictable effect on real activity. The New Classicals among the Monetarists maintain that the authorities can only control the nominal scale of the economy and have no effect on real activity except for such transitory disturbances as may arise because people sometimes misunderstand what current monetary policy is.

The last 150 years or so have obviously taken us by stages -- and with quite a bit of backing and filling -- from a monetary regime approximating the pure convertibility case to one of quantity control (with its purity only a little bit soiled by some 'dirty floating' of exchange rates). What may not be so obvious is whether the tail end of this long process is really relevant first to the long dominance of Keynesian economics and then to its dramatic loss of influence.

If it were the case that the relative price of gold in terms of all other goods were determined by 'real' factors and were thus independent of the volume of 'Paper Credit', then it would suffice to fix the nominal price at which paper should be



convertible into gold in order to also fix the price level. This sort of model was hardly an adequate guide to realities even 150 years ago. Over time, convertibility became an increasingly loose constraint on the price level as well as on the policy discretion of central banks. What remained of convertibility in the post-World War II Bretton Woods monetary system was terribly attenuated. (Recall that , in the U.S., the public's privilege to redeem in gold had been abolished already in 1933). Most monetary economists over the last 40 years have written on the apparent presumption that the vestigial remains of convertibility were of no theoretical significance. The literature on pure monetary theory, in particular, from Don Patinkin to Frank Hahn and Robert Lucas has concentrated almost exclusively on the fiat-standard, quantity control case.

Nonetheless, under Bretton Woods, it is on the whole only for the United States that this question of interpretation arises. The countries that maintained fixed dollar exchange rates became more -- not less -- constrained by convertibility with time as both goods markets and capital markets became increasingly integrated. Consequently, their willingness to continue with the system depended upon how the dollar was managed. There can hardly be any doubt that the United States could have spread inflation across the world twenty or so years before we actually did so. In that sense, the United States was not significantly constrained by the Bretton Woods system. But the 'implicit contract' with the other members required more responsible behavior. When, in the end, the U.S. refused to let

its external deficits pull it back from its inflationary policies, Bretton Woods was done for. My own interpretation of the period up until the system crumbled, therefore, is that the monetary policies of the United States in effect 'mimicked' behavior under a convertible system and sufficiently so as to sustain in the public a system of nominal expectations appropriate to such a system.

This was the world to which Keynesian economics was adapted and not only (or even mainly) because Keynes was one of the main architects of Bretton Woods and made corresponding assumptions in his theoretical work but because macroeconomics after Keynes evolved in that setting and absorbed features of it into its theoretical structures -- often without explicit recognition. When the system collapsed, Keynesian economics turned out to be ill-prepared to deal with an era of inflationary mismanagement of a fiat standard.

A specific illustration may help at this point. When I began studying economics thirty years ago, I was taught two propositions that were considered lessons of Keynesianism -- indeed, almost 'discoveries' due to the 'Keynesian Revolution':

[a] "money wages are 'rigid' in the modern world," and

[b] "monetary policy is 'ineffective'."

Are these two True or False? In the 1950's, they were taught as True in almost all schools and students who did not think so had low survival probability and seldom joined the profession. For

the last 10 years or so, we have taught that they are False and students who do not think so .... etc.

My point is, of course, that they are not True or False in the abstract but -- as is the case with a great many disputed propositions in monetary theory -- their status depends on what monetary regime we are talking about. If by 'effectiveness' we mean the capability of bringing about large changes in national income in money terms, then a central bank constrained by convertibility does not have that capability. Consequently, rational agents will expect the nominal scale of real wages to be more or less constant. Workers know that a money wage concession means lower real wages. Under such a regime, therefore, money wages are only as flexible as one would expect real wages to be.

Under quantity control, on the other hand, monetary policy is in principle 'effective' and capable of bringing about large changes in the nominal scale of the economy. If this capability is used vigorously, the inelastic nominal expectations that the public may have had originally are bound to give way and with their disappearance the 'rigidity' of the nominal wage will also disappear. Any relationship between nominal and real variables that may previously have appeared stable and reliable will then break down. The so-called Phillips-curve relationship between nominal wage changes and the unemployment percentage is the most pertinent example. Originally charted by A.W. Phillips on British data for the period 1862-1957,<sup>3</sup> it completely disappeared in the mismanaged money period of the 1970's. Keynesianism lost influence in the economic profession in large part because many

Keynesians had put considerable faith in the Phillips curve, while the Monetarists had been vociferous in their mistrust of it.

The progress of the Keynesian-Monetarist controversy is itself relevant to the change in monetary regime. The early Monetarist attacks on Keynesian orthodoxy concentrated heavily on the proposition that monetary policy was 'ineffective'. The 'effectiveness' of money stock policy for the key currency country could not long be disputed and the profession was also gradually won over to this view. The result, however, was that a great many people who still believed wages to be inflexible now believed money stock policy to be effective. This is a dangerous combination of beliefs for it leads to a further belief that is not true, namely, that money stock policy can be used as an effective regulator of employment. This false belief lent an inflationary bias to policy discussions in the 'sixties and 'seventies. Thus, the Monetarist critique on the effectiveness issue (and also on the fixed exchange rate issue) undermined the monetary discipline of the Bretton Woods regime. The Monetarists failed, however, in imposing their own brand of discipline -- the Friedman rule.

### Internal Factors: Changing Theoretical Positions

The course of macroeconomic controversy over the last 30 years is difficult to follow not only because the world about which people were talking kept changing but also because their theoretical positions kept changing. "Things ain't what they used to be" within either Keynesianism or Monetarism. To keep track of these changing positions it is convenient to make use of a figure which should be familiar to this audience, namely, the Swedish Flag.

Business cycle theories may be distinguished according to the hypotheses they make about the impulses that initiate fluctuations and about the propagation mechanisms that turn the impulses into persistent movements in output and employment.

|                |                     | <u>Propagation</u> |       |                         |
|----------------|---------------------|--------------------|-------|-------------------------|
|                |                     | Nominal            | mixed | Real<br>(intertemporal) |
| <u>Impulse</u> | Nominal             |                    |       |                         |
|                | mixed               |                    |       |                         |
|                | Real<br>(intertemp) |                    |       |                         |

"SWEDISH FLAG"

A purely nominal impulse (N) is a disturbance to the system such that the reequilibration of the economy requires (only) a change in nominal scale, i.e., an adjustment of the money price level. A real impulse (R), on the other hand, requires some reallocation of resources between industries or occupations and, correspondingly, a change in relative prices. If it is a 'pure' case of a real disturbance, it will not require a general deflation or inflation in order to restore equilibrium. Close approximations to the pure cases may be relatively rare, so we must recognize 'mixed' categories both on the impulse and the propagation side. (These are complicated matters, however, that I don't dare go into, so you may color them yellow and forget about them!)

How propagation comes in is best explained by going directly to the relevant cases. The nominal-nominal (N/N) combination in the upper left-hand corner is the Monetarism of Milton Friedman<sup>4</sup> -- Monetarism before New Classical Economics. The typical disturbance is an exogenous change in the fiat money supply; the failure of the money wage to adjust immediately propagates the shock to real magnitudes so that real income and employment (and not only nominal prices) co-vary with the money stock.

The intertemporal real-real (R/R) combination at the lower right is Keynes' original case. Various events may cause firms to change their views about the profitability of investment, i.e., of employing present productive resources for the purpose of augmenting future output. If this shift of the "marginal

efficiency of capital", as Keynes called it, were in a pessimistic direction, for example, they will plan to reduce investment thereby creating an excess supply of present resources and, implicitly, an excess demand for future goods. This disequilibrium, Keynes liked to describe in terms of quantities as "saving exceeds investment" while the great Swede Knut Wicksell preferred to describe it in terms of prices as "the market rate exceeds the natural rate." The appropriate system response should be that real rates of interest fall (so as to raise the price of future goods in terms of present goods). The Keynesian propagation hypothesis is that real rates of interest do not move sufficiently with the result that the excess supply of present resources produces a decline in output and employment.

The theoretical cores of these two are easily grasped. One (N/N) argues that, typically, macroeconomic troubles stem from nominal shocks and that the appropriate nominal adjustments are not forthcoming promptly. The other (R/R) maintains that, typically, the trouble starts with a disturbance to real expectations and that the appropriate (intertemporal) relative price adjustments do not occur immediately. The Monetarist-Keynesian controversy was not played out as a contest between these two alternatives, however. Things would have been much easier to understand -- would have made much more sense -- if (N/N) and (R/R) had been the contending positions. But that was never the case.

Before the Monetarists came on the scene, internal developments within the Keynesian camp had already shifted the

Keynesian position into the bottom left (R/N) corner of the Flag. The Keynesian economics that filled the textbooks for three decades retained the hypothesis that shifts in the marginal efficiency of capital was the typical cause of changes in income but stressed money wage 'rigidity' as the cause of unemployment. This latter hypothesis was one that Keynes had explicitly denied and it is at least doubtful that the resulting mix of a real disturbance hypothesis with a nominal inflexibility hypothesis makes a coherent theory. In any case, this shift of the Keynesian position switched the theoretical focus from the role of intertemporal relative prices in the coordination of saving and investment to the relationship between aggregate money expenditure and money wages.<sup>5</sup>

The Keynesian-Monetarist controversy started therefore with the Keynesians in this muddled R/N-position being attacked by Monetarists from a N/N-position. The Keynesians found the defense difficult. Two examples may suffice:

First, consider the Keynesian response to Friedman's so-called 'natural rate of unemployment' hypothesis. This hypothesis was not one of the original issues in the debate but was added rather late in the game. For present purposes, it may be stated as follows:

Employment has a strong tendency to converge rapidly on equilibrium employment. What ensures this result is simply the ordinary supply-and-demand mechanism operating on the price in the relevant market. Unemployment will be found to diverge from its 'natural rate' only when and in so far as the money wage rate temporarily lags behind its equilibrium value.



Now, from the standpoint of Keynes' R/R-theory, the response to this is simply: "True, if and only if intertemporal equilibrium is already assured -- but False, whenever saving does not equal investment at full employment income." But by the time that Friedman added the natural rate of unemployment hypothesis to the structure of Monetarist beliefs about the world, the intertemporal coordination problem was out of sight and out of mind among his Keynesian opponents. No one brought it up! It was not even mentioned.

So what retort was left to them? The answer given was, in effect, that Friedman was right; only lagging wage adjustment stands in the way of full employment; -- but that money wages are more inflexible than he or any other Monetarist would like to believe! On the basis of this, the Keynesians have subsequently built a thriving cottage-industry devoted to the fabrication of a multitude of reasons for the inflexibility of money wages. In the nature of the case, it has become a sort of skeet-shooting sport in the Monetarist camp to take potshots at these reasons as they pop up in print. The irony of all this is of course palpable: money wage inflexibility is down-played by the side that necessarily needs the hypothesis in the context of its own (N/N) theory; it is insistently played up by the side who has stumbled into this hypothesis only by mistake!

Second, consider the Keynesian response to Robert Barro's<sup>6</sup> so-called Ricardian Equivalence theorem. This theorem asserts that the present value of future taxes has the same effect on aggregate behavior as an equivalent amount of current taxes;

consequently, there is no reason to delay taxation, and the Keynesian proclivity for bond-financed deficit spending, rather than simply balanced budget spending, has no rational basis.

The natural Keynesian retort to this should have been to insist that the discussion keep to the original context for these characteristic Keynesian fiscal policy recommendations. That context was, of course, one of unemployment due to intertemporal disequilibrium. With real interest rates at a level that will not allow saving-investment coordination at full employment, the result will be an excess supply of present factor services and an implicit excess demand for future goods. Spending now will reduce this excess supply; taxing later will reduce the excess demand. The temporal structure of the Keynesian policy fits the temporal maldistribution of excess demands left uncorrected by intertemporal price adjustments.

Barro's Ricardian theorem presupposes intertemporal general equilibrium. This is not a state of affairs that needs to be 'stabilized'. Nor has anyone ever suggested that activist fiscal policies should be used in such circumstances. Moreover, the only thing of much interest that can be said about the calculation of wealth at a market rate different from Wicksell's natural rate is that everyone will get the wrong result.

But, again, no one brought it up. Having lost track of Keynes' saving-investment problem, the critical replies to Barro accepted his intertemporal equilibrium assumption but argued that his aggregative conclusions might still be invalidated by

distribution effects. Admittedly, intergenerational distribution effects are somewhat more interesting and slightly more amenable to empirical study than the run-of-the-mill distribution effects that always surround any macrotheoretical proposition with a penumbra of doubts. (We know, for instance, that having one generation enrich itself by borrowing abroad and leaving it to the children to pay the bill is not just an unrealistic figment of the theoretical imagination). But what could be more patently obvious than that Keynesian fiscal policy doctrine can not be restored on this ramshackle foundation?

This Keynesian shift has not been the only one. The New Classical group shifted the position of the new generation of Monetarists to the upper right hand (N/R) corner of the Flag. In trying to construct a micro-theoretically founded model of Friedman's theory, Robert Lucas noted that, in Friedman's story, movements in employment could only be generated by assuming either that the labor market did not clear or else that it cleared but on the basis of asymmetric expectations between the two sides of the market. For methodological reasons that we need not go into, he did not want to make either assumption. Instead, he produced a model where nominal impulses led to changes in the intertemporal prices as perceived by transactors, who would respond by reallocating their supply of labor and their consumption of leisure between the present and the future.

This may be explained as follows. In Lucas' world there is no problem of coordinating either the expectations or the activities of a multitude of people so he uses a 'representative

agent' model. It is traditional in economics classrooms to call this agent Robinson Crusoe. We might well imagine that on Robinson's island there are certain planting seasons when the future return to present effort is particularly high. We would expect Robinson, like farmers through the ages, to work long hours in the planting season and to take his leisure at some other time of the year. This, however, is a real theory of the seasonal variations in Robinson's work effort -- the planting season brings a rise in the marginal efficiency of capital. What is peculiar about Lucas' theory is that, supposedly, the powers that be rain fiat money on Robinson from time to time and that, whenever he sees this irredeemable paper littering the ground, he draws the (unwarranted) conclusion that planting season has arrived. (One of the merits claimed for this model, by the way, is that it is more meticulously based on rational behavior than the competition).

Believe it or not, but for some ten years beginning in the early seventies this theory was the hottest thing around! The New Classical economics upped the ante very considerably with regard to the mathematical modelling skills required for anyone who wanted seriously to participate in the theoretical discussion. This did much to attract the best and the brightest graduate students. From my perspective at distant UCLA (in the general neighborhood of Hollywood) macroeconomics seemed to go the same way as the movies: the plots became strangely simple-minded, but the new special effects were truly mind-boggling!

This decade, then, offered the strange spectacle of a Keynesian-Monetarist controversy on the 'wrong' diagonal (on the Flag). In my own opinion, not much of substance was learned from this prolonged clash between the two mismatched hypotheses -- the supposedly 'Keynesian' R/N theory and the Lucasian N/R theory.

There may be worse to come for we are not finished with the shifting positions. Over the years, a number of erstwhile Keynesians have been won over by the work of Friedman and by Brunner & Meltzer to the belief that the typical impulse is monetary rather than real. This group includes some quite prestigious economists -- it is my impression that one is justified in including, for example, Herschel Grossman, Robert Hall and John Taylor among them. Since they carry on the good fight against the Monetarists of their own generation, who have moved over to the New Classical position (at N/R), and since they tend to insist that nominal wages are more inflexible certainly than the New Classicals would like to believe, some segments of the profession have come to look to them as the last best hope for regaining a theoretical rationale for Keynesian policy activism. But, even if they manage to impart a curiously 'Keynesian' air to the place, the fact is, of course, that (at N/N) they occupy what used to be Milton Friedman's old quarters.

Meanwhile, all was not quiet on the New Classical front. In Minnesota (just up the road a bit),<sup>7</sup> Christopher Sims and Tom Sargent concluded that the nominal impulse hypothesis was not right after all. Hence they deserted Monetarism. At Rochester and elsewhere a number of people have also started to work on real

impulses and intertemporal coordination problems. There are by now enough straws in the wind to amount to a small haystack suggesting that the young vanguard of the profession is moving the frontier of theoretical research into the R/R quadrant of the Flag. If you have followed me attentively all the way, you will recall that this is where we had John Maynard Keynes in the beginning.

The macroeconomic controversy will not cease, of course. What the interested spectator can expect in the next few years is this: 'Keynesian' hopes ride on a group of economists who seek to reestablish the case for useful stabilization policy on the basis of sticky money wages; to them, the 'central question' in macrotheory is why nominal wages do not respond appropriately to nominal shocks; they base themselves on Milton Friedman's old position: nominal impulses propagated by nominal inflexibilities. They are opposed by the vanguard which insists that Keynesian fiscal policy is useless (or worse) and that the best monetary policy is Friedman's; I don't think this latter group is anti-Keynesian exactly. To them, I think, "Keynesian economics" is a term of opprobrium that their elders used to use but that, like "poppycock" or "balderdash", has gone out of fashion. Nonetheless, these 'policy passivists' seem to be moving into Keynes' old theoretical position: real impulses, real propagation.

Will such a debate, on the 'right' diagonal with the positions reversed, be fruitful? I doubt it.

## Conclusion

'Tis a mournful tale. Can we draw any moral from what happened to Keynesian economics?

To the philosopher Karl Popper, science was a process of successive conjectures and refutations. The empirical refutation of a theoretical conjecture should produce an improved theory guiding the next round of empirical research. Through each round, the theory should come to approximate reality better. I do not think that this Popperian process operates satisfactorily in macroeconomics. It is not at all obvious that today's macroeconomics is a better guide to today's realities than the macroeconomics of (say) thirty years ago was to the realities of that day. The empirical methods on which economists now rely to keep them in touch with reality failed to alert the Keynesians to the changing "external" monetary environment and to point out to them the direction in which the theory should be amended. A less exclusive emphasis on econometrics and more reliance on old-fashioned economic historical and institutional knowledge might have helped.

Popper's younger colleague, Imre Lakatos, maintained that cognitive appraisal should focus not on theories but on "research programmes" as the appropriate unit for analysis. A research programme is a temporal sequence of theories which may progress through conjectures and refutations to encompass more and more confirmed empirical content. But it may also degenerate. To keep a healthy, progressive research programme going requires the

ability to keep track of what Lakatos called the programme's "hard core", i.e., the basic ideas that give it continuity. The "internal" dance around the Swedish Flag that I have described has not been impelled step-by-step by empirical refutations; the Keynesian story, in particular, is basically one of loosing track of the hard core. The result is not so much a programme that degenerates but, rather, a succession of theoretical revisions going off in several directions, which cannot be appraised as a progressive or degenerative programme, because the requisite continuity and coherence over time is not present.

For the last few decades, economists have on the whole looked to mathematical economics to maintain a structured order and coherence in the theoretical realm. Today, those younger economists who share the activist proclivity for thinking that something should be done about unemployment, for instance, tend to the hope that Keynesian economics can be revived by use of the modern modelling techniques pioneered by the rational expectations group. They find the older Keynesian literature less than fastidious in its modelling. And the precision that mathematics can provide is of course desirable. But precision of utterance is of little help if we cannot keep track of what we are talking about.

Mathematical reasoning by itself will not guarantee coherence. Admittedly, the IS-LM model looks primitive today, but it was considered the 'mathematical' macromodel in its heyday. Reliance on it did not prevent but produced, as a seemingly



inescapable conclusion, the 'rigid wages' interpretation of Keynes' theory of unemployment -- from which much other nonsense has since followed. A less exclusive emphasis on mathematical economic theory and some increased reliance on recent history of economic thought might help. But, if so, some standards of such scholarship have to be respected, as the standards of mathematical reasoning or statistical inference are respected. And that seems hopeless. The fiction, for example, that Keynes himself based his theory of unemployment on 'rigid wages' is now so firmly entrenched in textbooks and journals, and is reprinted with such frequency, that apparently nothing can dislodge it. Today's economics profession, taken as a whole, simply does not care enough about the truth or falsehood of statements of this doctrine-historical kind to enforce reasonable scholarly standards.<sup>8</sup> The Keynesian tradition, in particular, is the worse for it.

On a more optimistic note, finally, the Keynesian tradition deserves to be , and surely can be, resuscitated. If Keynesianism can only be freed from its unfortunate identification with nominal 'stickiness', there is no reason why it could not be extended to encompass also the analysis of those conditions of monetary instability which, in the inflationary 1970's, served to undermine the until then prevailing Keynesian orthodoxy. The original Keynesian focus on problems of intertemporal (saving-investment) coordination remains valuable and must, in my view, be incorporated in any future theoretical synthesis if it is to be viable. Keynesian theory of this (R/R) brand is needed

even where Keynesian theory of the standard (R/N) brand has been lambasted the most by Monetarists and New Classicals, for these latter theories have not really helped us understand, for instance, why investment often suffers so badly under inflationary conditions.

## Footnotes

1. On Keynesian Economics and the Economics of Keynes: A Study in Monetary Theory, New York: Oxford Univ. Press, 1968.
2. The rational expectations approach was pioneered by John F. Muth in his "Rational Expectations and the Theory of Price Movements," Econometrica, 1961. For a non-technical account of its implications for macroeconomics in general by its most influential proponent, see Robert E. Lucas, Jr., "Understanding Business Cycles," as reprinted in his Studies in Business-Cycle Theory, Cambridge, Mass.: MIT Press, 1981. For applications of the 'monetary regime' concept, see Thomas J. Sargent, Rational Expectations and Inflation, New York: Harper & Row, 1986. For the relationship of New Classical economics to the economics of Keynes, see A. Leijonhufvud, "What Would Keynes Have Thought of Rational Expectations?" in David Worswick and James Trevithick, eds., Keynes and the Modern World, Cambridge: Cambridge Univ. Press, 1983.
3. A.W. Phillips, "The Relation between Unemployment and the Rate of Change of Money Wage Rates in the United Kingdom, 1862-1957," Economica, Nov. 1958.
4. See Robert J. Gordon, ed., Milton Friedman's Monetary Framework: A Debate with His Critics, Chicago: Univ. of Chicago Press, 1974.
5. For the many variations on the idea of intertemporal coordination problem, and the many confusions surrounding its role in Keynesian economics, see A. Leijonhufvud, "The Wicksell Connection: Variations on a Theme," in Information and Coordination, New York: Oxford Univ. Press, 1981.
6. Robert J. Barro, "Are Government Bonds Net Wealth?" Journal of Political Economy, Nov.-Dec. 1974.
7. See esp., Christopher A. Sims, "Is There a Monetary Business Cycle?" American Economic Review, May 1983.
8. See, for example, Paul Samuelson's "Comment" in Worswick and Trevithick, op.cit., p. 216: "The fiction that Keynes assumed rigid wages was found to be a useful fiction."