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OESTERREICHISCHE NATIONALBANK

Stability and Security.

# WORKING PAPER I 28

## THREE LECTURES ON MONETARY THEORY AND POLICY: SPEAKING NOTES AND BACKGROUND PAPERS

DAVID LAIDLER



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## Editorial

In order to promote the exchange of ideas and to support its own research capacity, the Oesterreichische Nationalbank regularly invites internationally renowned economists for short guest professorships. This year, from June 12 -14 2006, David Laidler (Professor Emeritus, University of Western Ontario, Canada) gave three public lectures on topics related to monetary theory and monetary policy. The first lecture was on “*Monetary Policy and the Austrians*”, the second on “*The Rise and Fall of Monetarism*” and the third has dealt with the question “*Is there a Role for Money in Monetary Policy in the 21st Century?*”.

The lectures were based on three background papers, which are contained in this OeNB Working Paper. Two of these background papers have already been circulated as BIS Working Paper No. 136 (September 2003) and as RBC Financial Group, EPRI Working Paper No. 2005-11. The third paper on “Axel Leijonhufvud and the Quest for Micro-foundations – Some Reflections” is made available for a larger audience for the first time. This Working Paper includes also the *speaking notes* that were prepared for the lecture series at the Oesterreichische Nationalbank.

June 19, 2006

## Table of Contents

1. Speaking notes for three lectures on monetary topics .....	5
2. Axel Leijonhufvud and the Quest for Micro-foundations – Some Reflections .....	19
3. Milton Friedman and the Evolution of Macroeconomics .....	43
4. The Price Level, Relative Prices, and Economic Stability. Aspects of the Inter-war Debate .....	77

**Speaking notes for three lectures on monetary topics**

(delivered at the Austrian National Bank June 12<sup>th</sup> -14<sup>th</sup> 2006)

David Laidler



## *Monetary Policy and the Austrians' Cycle Theory*

This group (Mises, Hayek, Robbins, Haberler et al.- not all of whom were Austrians) propounded a specific theory of the cycle during the inter-war years, that for a while looked likely to dominate the then emerging macro-economic sub-discipline. This theory was a component of a broader attempt to revolutionise English language economics, on the basis of GE theory. The micro element here was successful (cf. J. R. Hicks *Value and Capital*) but not the macro. That is an important reason why the standard textbooks of the two areas 1950s and 1960s were so disconnected from each other, and why a search for micro-foundations for macro began at this time.

Austrian cycle theory was a product of the earlier Austrian tradition in economics, that of Carl Menger and Eugen von Boehm-Bawerk among others, and inherited certain distinctive characteristics from this tradition. (a) it was explicitly based - by the standards of its time - on the analysis of maximising individual behaviour in a GE setting (though it was not usually formally mathematical in a Walrasian way) (b) it insisted on the importance of relative prices (as opposed to aggregate concepts such as “the price level”) and (c) it gave a central place to the idea of “roundaboutness” in production.

This last element turned out to be problematic (cf. the later Cambridge controversies in capital theory), but it had the great virtue of fording attention onto the ideas that current saving involves deferring consumption into the future, that current investment provides the economy with the capacity to supply that future consumption when the demand for it materialises, that a relative price - an interest rate - is required to co-ordinate these decisions, and that if it fails to do so, there can be serious consequences for the economy's behaviour.

Wicksell's cumulative process analysis of price inflation was also a crucial inspiration for Austrian cycle theory, but it must be recalled that Wicksell himself was strongly influenced by the earlier Austrian tradition. In particular the analysis set out in *Interest and Prices* Ch. 9, “The pure credit economy” raised the possibility that in such an institutional framework, there might be no endogenous equilibrating mechanism capable of bringing the market rate of interest into line with its “natural” value - the one that would co-ordinate saving and investment decisions, should a disequilibrium occur. Mises attempted to



resolve this problem in 1912, and his insights formed the basis for the subsequent development of Austrian cycle theory

He turned to the idea of “forced saving” (originated by Bentham and Thornton at the very beginning of the 19<sup>th</sup> century) arguing that “too low” a market rate of interest would lead to firms borrowing from banks to undertake investment with a view to providing future consumption goods which consumers did not in fact want. Their expenditure of newly borrowed money would bid up the price of inputs in investment goods industries, so that relative prices would be distorted, and the roundaboutness of production would begin to be lengthened. Simultaneously, an excess demand for current consumption would develop. In this disequilibrium, suggested Mises, lay the seeds of a “crisis” (the upper turning point of the business cycle).

Hayek, a pioneer in extending GE theory to take account of the passage of time (and not at this time anti-mathematical) developed Mises insights, using what was then a “best professional practice” theoretical approach: ie .he began his conceptual experiments from an equilibrium situation, and then tried to show how disequilibrium could develop as a consequence of maximising agents’ responses to distorted relative prices. Specifically he showed, in much more detail than Mises, how a discrepancy between the market and natural rates would lead to forced saving, an excess demand for current consumption goods that was matched by a stock of incomplete capital goods not yet able to provide them.

What would then happen depended upon monetary policy. If the authorities brought about (or permitted) a reversion of the market rate to the natural rate, the mismatch between the demand for consumption goods and the economy’s capacity to supply them would be revealed, and an economic crisis would ensue. If instead, they attempted to prevent a crisis by keeping the market rate artificially low, this could only lead to more forced saving and an even more serious disequilibrium. Furthermore, argued Hayek, the latter path would inevitable be accompanied by a rising inflation rate, so the authorities would be forced to abandon their efforts eventually. So, one way or another a crisis would occur.

Hayek et al. are very vulnerable at this point to charges of having treated logical possibilities that were certainly inherent in their system as logical necessities - cf the contemporary criticisms of Sraffa, Hansen and Tout, the Swedes, etc.. But one should recall that memories of hyperinflation and economic collapse were fresh in their memories.

Note also that they pointed out that forced saving could occur in a growing economy even if the “price level” was stable (quite how this prediction squared with their concerns about the inevitability of forced saving being accompanied by rising inflation was left unclear). After the event, they claimed to have predicted the collapse of the US boom in 1929. This particular claim is debatable in the light of the published evidence, but the simple fact that their system, constructed in the 1920s, seemed to be able to explain the events of the early 1930s gave it considerable credibility. Recall that Friedman’s 1968 accelerationist and natural unemployment rate hypotheses gained similar ex post credibility from the experience of the 1970s.

The Austrian model seemed to its exponents to yield nihilistic policy implications for dealing with the depression. Here we again encounter the logical possibility - logical necessity problem. They argued that, since the crisis had been caused by excessive credit creation, and was marked by an excess demand for consumption and an already too large capital stock, expansionary monetary policies, or fiscal measures that encouraged either greater consumption or investment would only make it worse. The only remedy was to let the passage of time restore equilibrium between the desired time-structures of consumption and production.

This message had considerable resonance in conservative financial market circles where “passive-money” banking school ideas taught that one had to await recovery before allowing credit and money to expand. But it also ran counter to a widespread political desire in more progressive circles, to defend the liberal political order against the idea that only totalitarian regimes could manage the economy - recall the political situation in Europe in the mid-1930s. This is an important reason why the theoretical ideas of *The General Theory* caught on so quickly after 1936, and why Austrian theory was abandoned. But there was another reason - the concept of a “natural” rate of interest determined solely in the real economy was analytically unsustainable except under

extremely special assumptions, so there was a serious logical flaw at the very heart of the theory, and Hayek himself came to realise this (cf *The Pure theory of Capital*)

Even so - Austrian ideas did not totally die out, though the claim that they anticipated Friedman's accelerationist doctrine of 1968 is based on a fundamental mis-reading of their work. To begin with, the idea that micro and macro theory should both start from the same premises was a quintessential Austrian idea and it did in due course reclaim a central place in economic theorising. Also, the idea that some economic crises can result from excessive credit creation has resurfaced in reaction to Japan's "bubble economy", real estate booms in Scandinavia, not to mention the high-tech bubble of the late 1990s. The real symptoms that accompanied these financial crises - half finished office towers, or more recently huge surpluses of computing and fibre-optic equipment, etc. are exactly those to which earlier Austrian analysis had pointed. Recent concerns expressed in certain policy circles - particularly the BIS - about growing financial "imbalances" have strong Austrian overtones, too.

It would be foolhardy to argue that any of the above can be safely ignored. After all, to say that the Austrians over-generalised and claimed too much relevance for their analysis relative to that of others hardly sets them apart from the rest of the profession. We still need to keep their ideas in mind, and Roger Garrison's recent book *Time and Money*, which seeks to reformulate Austrian ideas as contributing to our understanding of inflationary credit-creation driven booms, has been a timely reminder of this.

## *The Rise and Fall of Monetarism*

The word “monetarism” means different things to different people. To some it is a now discredited technical doctrine about how to conduct monetary policy, and to others it is a label for the economic and social policies of that well known politician “Ronald Thatcher” There is truth in both interpretations, but both are over-simplifications. The word was introduced independently by two commentators, Karl Brunner who used it (roughly speaking) in the first sense in 1968, and Nicholas Kaldor who used it very much in the second in 1970 and subsequently. Here, it is worth recalling that there was an important “Structuralist-Monetarist” debate in Latin America in the 1950s and 1960s (in which Kaldor was involved) which was profoundly ideological and highly charged politically too.

Technical monetarism seems to have begun as a critique of the standard “Keynesian” macro economic theory and associated policy doctrine that was built around the IS-LM model, or rather around a particular configuration of that model. A crucial contribution to this critique, whose role in the development of monetarism is not always sufficiently recognised, was Friedman’s permanent income hypothesis of consumption.

To begin with this idea undermined the theoretical basis of a stable marginal propensity to consume out of current income on which Keynesian multiplier analysis depended, and it also provided the theoretical basis for monetarist theories about the stable demand for money function (Friedman 1959, Meltzer 1963). Monetarism, after all, replaced the stable multiplier with the stable velocity function as a fulcrum for macroeconomic policy.

The fact that the PIH itself was a technical achievement that commanded respectful attention throughout the profession gave its specifically monetarist applications a certain intellectual respectability and attractiveness that they might not otherwise have had. Note also that its key ingredients - a micro theory of macro behaviour, forward looking maximisation and the integration of economic with econometric theory - also characterised the late 1960s development of the expectations augmented Phillips curve, which again resonated far beyond the monetarist camp.

Other factors too were important in launching technical monetarism: it seemed to be an application of the then popular “positive economics” agenda - and it came on the intellectual scene just as development in computing technology were making systematic empirical testing feasible. Monetarism also claimed affinity with the old quantity theory of money, which was a theory of price level behaviour. The students who, in the 1960s, were still being offered a macro curriculum that centered on unemployment as a policy problem had not even been born when it had ceased to be one - but in the 1960s inflation was becoming an issue.

The monetary policy implications of monetarism were set out comprehensively in Friedman’s *Program for Monetary Stability* (1961) - the famous money growth rate rule, the case for which rested firmly on the apparent stability of the demand for money function. In the 1970s, after the failure of anti-inflation policies based on wage-price guidelines and controls, which in turn derived from ideas about cost-push inflation not unrelated to Latin American “structuralist” doctrines, the scene was set for the adoption of the monetarist alternative in a number of places.

These experiments, to put it kindly, did not work out very well. With hindsight - for no-one within the monetarist camp said much about these matters at the time - there are a number of obvious reasons for this failure.

First, Friedman’s proposals had been designed to maintain and improve stability in an already low inflation environment, but the policy experiment involved trying to restore stability in volatile high-inflation environments: these are not the same problem, and the right solution to the first was not necessarily the right one for the second. Second the policies were implemented using models in which money was a passively endogenous variable - cf. banking schools ideas mentioned in the previous lecture - and yet they were motivated by the idea that money-growth was the active cause of inflation; the application of monetarist policies was therefore intellectually incoherent. Finally, demand for money functions were simply not stable enough to bear the weight of money growth targets. The longer-run role of institutional change, and the significance of short-run stochastic elements in these relationships should have been appreciated *ex ante*, but they were not.

And yet, though money growth targeting failed, technical monetarism left a profound mark on the policy scene. The idea that inflation control is a problem for monetary policy, which can do nothing to influence real variables such as unemployment in the longer run, is now conventional wisdom among policy makers, but it was considered radical in the 1960s and 1970s. Indeed, modern inflation targeting regimes might well be referred to as “monetarism without money”

But what about political monetarism? Post-world-war 2, there existed a political consensus that the market economy requires activist government to keep it functioning. The intellectual underpinnings of this policy view included the *General Theory*'s claims about a fault inherent in the inter-temporal allocative mechanism in a monetary economy. And these ideas seemed to be obviously true in light of the economic experience of the inter-war years.

Friedman and Schwartz's *Monetary History of the United States* successfully challenged this empirical supposition. They argued that the Great Contraction was the result of incompetent monetary policy, based on the passively endogenous money idea, that had permitted the banking system to collapse in the wake of the stock market crash, and had turned an ordinary cyclical downturn into a major catastrophe. The basis message here, whose political implications took a while to be fully grasped, was that fatal flaw that had caused the depression lay not in the self-regulating mechanisms of the market economy, but in the policies that had been imposed upon it.

Kaldor noted an element of similarity here to Austrian doctrine, but the differences are much more important. The monetarist remedy for the depression would have been as much monetary expansion as it took to stabilise the system - cf the contemporary recommendations of Hawtrey, Currie, Viner etc. - while the Austrians had argued for policy passivity at all costs. However, the broader political-economic implication of the two doctrines - that when not disturbed by activist policies, markets work - is the same, which is why Margaret Thatcher could be comfortable in claiming to have taken inspiration from both Hayek and Friedman

Technical monetarism took its New classical new turn in the 1970s. At first, with its replacement of adaptive learning with rational expectations, and its

adoption of an aggregate supply curve interpretation of the expectations augment Phillips curve, NCE seemed to be doing no more than putting stronger micro-economic foundations under existing doctrines - policy ineffectiveness, the natural rate of unemployment hypothesis etc. - and thus continuing the very process to which Friedman's PIH had made such a major contributions two decades earlier. That is why James Tobin labelled it "monetarism Mark 2"

However, NCE shifted the micro-foundations agenda from the analysis of maximising individuals to the market mechanisms through which their behaviour is co-ordinated. Note that this is necessary if the REH is to be implemented. You can't model expectations "as if" formed on the basis of a "true" model of the economy unless you can solve the model in question. But when you solve a model, you are not just carrying out a technical exercise - you are maintaining the hypothesis that individual behaviour is always co-ordinated. NCE thus settled the age old question that lay at the centre of so much of economics - are markets capable of co-ordinating the choices of individual agents? - by assumption

The trouble here is that, as was already well understood in the 1960s - cf. the work of Don Patinkin, Frank Hahn, Bob Clower, Axel Leijonhufvud - that it is (next to) impossible to find a role for money in an economy where markets always clear. That is why NCE's insistence on clearing markets and sound micro-foundations for everything, including money, produced some very odd economic theory indeed - for example, OLG models, and applications of Clower's cash-in-advance notion that were certainly not what he had in mind when he proposed it. Thus, if NCE really was "monetarism mark 2", it presents a second example of "monetarism without money".

## *The role of Money in Monetary Theory and Monetary Policy*

NCE's fundamental analytic contribution was to show that information problems could be separated from those of co-ordination, and could, in and of themselves, lead to systematic output fluctuations. The approach postulates that an "as if" auctioneer sets prices at the values that will keep all markets cleared (given the structure of agents' information). This postulate has serious consequences for certain traditional problems in monetary economics - eg. why is inflation costly? By what transmission mechanism does monetary policy affect the price level? Monetarism without money, that is to say, evades many issues.

There is a long-standing literature that deals with the origins and role of money. It begins from the recognition that there is no market and no auctioneer, that agents are isolated from one another and subject to serious information problems as and when they interact, and that the system of monetary exchange exists to enable society to cope with these problems. In some versions of this story money evolves spontaneously, and in others through the intervention of the state, but what matters here is the fundamental message that the system of monetary exchange exists to cope with the same information and co-ordination problems that are solved by the auctioneer in standard GE theory.

The "market" is thus an economist's metaphor for the monetary system, and to assume that the market clears is to assume that the monetary system is working well. Small wonder that, if one starts from this assumption, and then tries to add a theory of money, the results are invariably extremely odd. In contemporary economics, search-theoretic models of money avoid this problem, and perhaps will eventually bridge the gap between old insights about the fundamentals of money and our already rich empirical knowledge of how monetary systems actually function. But while we wait for this, we do have to get on with applying such monetary economics as we have to policy problems.

We know that, in the real world, economies tend to settle on a single means of exchange, which serves as the economy's unit of account for current and inter-temporal transactions, and which can also be a store of value. We also know that money prices are not set by "markets" but by endogenous agents (typically firms) and that there are serious problems of maintaining these at their market



clearing values, especially when individual firms are atomistic relative to the economy.

Thus, trade, which is carried on by indirect monetary exchange in a number of separate markets, will often take place at false prices, with outcomes in one market that can impinge in agents' abilities to transact as planned in others. The typical agent's pattern of money receipts and outlays will thus be stochastic, and the traditional "cash balance" approach to the quantity theory of money has long postulated that this fact underlies the holding of an inventory of readily available purchasing power - a stock demand for money. The individual agent's actual money holdings will fluctuate over time around an average value. It is this value that we refer to when we speak of "the demand for money", and there is, as we all know, a vast literature, both theoretical and empirical that deals with its determinants.

Note that the implications of this line of argument for the NCE-RE approach to macroeconomics are subversive. Agents hold money to buffer themselves against the consequences of acting on misinformation, but if information can be improved by devoting more resources to gathering it, there is a potential trade-off between money holding and misinformation. This in turn suggests that, in a money economy, agents will usually act on the basis of systematically less information than is available. Also, if prices are costly to change, and money-holding reduces the losses incurred by failing to do so when conditions require it, then a money economy will also be characterised by price stickiness.

Note also, however, that this way of looking at things enables us to understand why monetary instability can be so damaging. For example, it enables us to see beyond matters of "shoe-leather" when we come to consider the costs of inflation. By reducing the information content of money prices, inflation increases the scope for errors in consumption and production decisions, thus undermining the monetary system's capacity to provide an "as if" functioning market mechanism to co-ordinate activity. Compare this to the typical NCE-OLG model of money, which can go to a "non-monetary equilibrium" under inflation, eliminating inter-generational trade, but leaving spot markets apparently functioning smoothly.

This way of thinking about money requires us to distinguish carefully between the “individual” and “market” experiment. The representative agent short-cut is only of limited use for studying monetary phenomena. Shocks to particular markets that lead to reduced holdings (not demands) of money on the part of some agents will often cause these to be matched by increased holdings elsewhere, so these effects will usually cancel out at the level of the market. But when shocks are economy wide - e.g. a change in the aggregate supply of money, or a change in the economy’s transactions technology or in the costs of gathering information, either of which can cause the majority of agents to adjust their demands for money - the whole economy will be shifted “off” its aggregate demand for money function until one or more arguments in that function adjust.

This argument is the price level in the traditional quantity theory experiment, which deals with the consequences of increases in the supply of money, but the existence of price stickiness suggests that output variation will precede such an adjustment. This is important, because in real world data, this is how these variables seem to move, contrary to the NCE prediction that quantities respond to prices.

In this approach also, an inventory of money balances is one asset in an overall balance sheet, and to the extent that it is subject to diminishing marginal productivity (utility?) a discrepancy between money’s supply and demand will affect agents’ plans for their balance sheets’ overall structure. This idea subverts an important element of current thinking about monetary policy - namely the significance it attaches to the “zero lower bound” problem. Only if there is something unique about the margin between money and short-term financial assets is this a critical issue. Was monetary policy really powerless in the US after 1929? Has the recent recovery of the Japanese economy been a consequence of “quantitative easing” or is its occurrence been just a lucky co-incidence?

This approach also raises a red, or at least an amber, flag about schemes to use monetary policy to deal with asset market bubbles. These tend to arise in particular sectors, and money’s effects are economy wide, rendering it a blunt instrument indeed.

Finally, note that the fact that monetary policy is usually conducted through interest rates does not negate the above arguments. Agents do not borrow from banks in order to acquire money to hold, but money to spend, and newly created money that gets into circulation as a by-product of private borrowing induced by interest rate changes disturbs balance sheets in just the same way as money created as a by-product of government borrowing. The policy significance of the use of the interest rate tool is that its zero lower bound can inhibit the creation of money. But this does not mean that monetary policy becomes powerless when it is reached, but only that open market operations are called for. Hence monetary policy can be effective in the aftermath of burst bubbles.

In short, the interaction of the supply and demand for money is still fundamental to the workings of the economy, and the fact that the demand for money function is insufficiently stable to support a monetary policy regime based solely on money growth targets makes not a jot of difference to this conclusion, which policy makers will ignore, not just at their own peril, but at the peril of the rest of us too..

**Axel Leijonhufvud and the Quest for Micro-foundations –  
Some Reflections\***

**by**

**David Laidler**

\*Draft of a paper to be presented at a conference in honour of Axel Leijonhufvud to be held at UCLA, August 30 - 31, 2006. Please do not quote without the author's permission - Comments welcome

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Key words: Micro-foundations; Keynesian Economics; IS-LM; Disequilibrium; Money; Co-ordination; Information; Expectations; Sticky Prices; Marshallian Economics, Walrasian Economics; Monetarism; New Classical Economics.



I think that I first met Axel Leijonhufvud when he was still a graduate student at Northwestern, and because I had the good fortune to be Bob Clower's junior colleague during my time at Essex, I was kept well aware of the development of his ideas even before *On Keynesian Economics and the Economics of Keynes* appeared. When it was published in 1968, Harry Johnson made sure that I was an early British reader, and I am proud to say that my copy of it bears an inscription from the author, commemorating a visit he made to Manchester in 1974. That was at about the time when his influence was beginning to loosen my up-till-then rather uncritical embrace of monetarism. In short, I have been learning from Axel's work for a long time, and I have sometimes followed it too, usually from a little to the right, but with undiminished admiration over the years.

I have given this essay the sub-title "some reflections" because it is probably as much informed by (no doubt prejudiced) hindsight and (no doubt inaccurate) memory as by a careful weighing of the published record. The history of macroeconomics in the second half of the twentieth century, and of Axel Leijonhufvud's place in it, largely remains to be written, but I hope that this essay will provoke some of those who were not yet professionally active in those years, and can therefore view them dispassionately, to begin that task. When they do so, I also hope that their work will bear out at least some of the judgements offered here.

### *Economic theory in the 1950s*

Half a century ago, those of us starting out on the serious study of economics found a great deal to perplex us. The subject was theory-based, but that theory was divided into two components, the connections between which were, to say the least, obscure. *Microeconomics* dealt with the maximising behaviour of individual households and firms, how the decisions of these individuals were co-ordinated by the price mechanism, and how this mechanism might fail properly to allocate the economy's endowment of productive resources without a few well-placed government subsidies and taxes designed to change the structure of relative prices. *Macroeconomics*, on the other hand, as enshrined in the Hicks-Hansen *IS-LM* model, dealt with aggregate consequences of the behaviour of those same firms and households, arguing that these would often include a failure of that same endowment of productive resources to be fully employed without help from a steady injection of expenditure from the government that was so co-ordinated with the flow of taxes paid by the private sector as to provide an appropriate level of aggregate demand.

Microeconomics and Macroeconomics thus seemed to be telling possibly contradictory stories about how the economy as a whole worked. To be sure, efforts were made to forge a link between them: for example, Paul Samuelson's

*neo-classical synthesis* had it that, though the market economy needed some government help designed with the aid of macroeconomics to bring about full employment, once this was achieved its further allocative functioning could safely be left to those devices which were the subject of microeconomics; and Abba Lerner's *Economics of Control* (1944) had earlier found a role for micro theory in guiding the pricing behaviour of publicly-owned enterprises in a thoroughly socialised economy whose government made maintaining full-employment a priority. But both of these attempted links had to do with the policy applications of received economics; they skirted questions about the logical relations between the theoretical foundations of its two branches and about whether, and if so how, these could be reconciled.

By the late 1950s, IS-LM macroeconomics was beginning to take on the status of an unchallenged orthodoxy, under the label *Keynesian Economics*, and had begun to find its policy feet too.<sup>1</sup> Soon, though belatedly, it would dominate policy making even in the United States. In 1965, at the height of its influence, but in perhaps the worst call made by an eminent economist since John Stuart Mill's (1848) claims about the completeness of the theory of value, Robert Solow (1965) would proclaim that “. . .most economists feel that short-run macroeconomic theory is pretty well in hand . . .All that is left is the trivial job of filling in the empty boxes, and that will not take more than 50 years of concentrated effort at a maximum . . .”

If we remember this (partly tongue in cheek) claim of Solow's nowadays, that is probably because only 3 years after it was made, Axel Leijonhufvud would quote it in his book *On Keynesian Economics and the Economics of Keynes* (Leijonhufvud 1968, p. 4). This work did more than any other single contribution to energise the search for a cure for the discomfort that many economists were feeling in constructing proper *micro-foundations* for macroeconomics, an endeavour that would end up pushing the IS-LM model from the centre of macroeconomics and replacing it with a new approach whose microeconomic basis was thoroughly transparent. But though what Harry Johnson (1971, repr. 1978, p.198) would refer to as Leijonhufvud's “monumental re-interpretation of [Keynes'] thought” was seminal in giving impetus to these developments, the micro-foundations that were eventually established were the very opposite of the ones he had proposed.

In what follows, I shall reflect upon how and why this came about. Specifically, I shall first describe the micro-foundations problem as it appeared about fifty years ago, and how it was then being addressed. Then I shall argue:

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<sup>1</sup>The publication of Alvin Hansen's *A Guide to Keynes* in 1954 was surely a critical step here. The book both symbolised and cemented the dominance of the IS-LM interpretation of Keynes in the standard undergraduate curriculum.

that, even as Leijonhufvud was writing his book, the macro-orthodoxy that so disturbed him was already being undermined by *monetarism*, whose attack was, however, based more on empirical evidence than micro-theoretic considerations; that a by-product of monetarism's success was nevertheless to shift the theoretical concerns of macro-economists away from just those parts of Keynes's legacy upon which Leijonhufvud sought to build; and that, as a consequence, the search for micro-foundations that he helped set in motion was quickly diverted from his chosen path.

### *Marshallians and Walrasians*

The received economic theory whose overall structure seemed so puzzling half a century ago was the product of two intellectual upheavals in the 1930s. Both of these had happened in Britain, but because of the destruction by the Nazis of lively intellectual traditions on the continent of Europe (along with much else), not to mention of the accident that English was also the language of the United States, whose universities would soon come to dominate all of economics (again along with much else), they profoundly influenced the development of the subject as a whole. The first of these was the macroeconomic revolution that surrounded the publication and interpretation of Keynes's (1936) *General Theory of Employment, Interest and Money*, and the second, less noted but just as influential, was an upheaval in microeconomics that saw continental *general equilibrium* theory, whose principal English language text was John Hicks' *Value and Capital*, largely displace Marshallian *partial equilibrium* analysis.<sup>2</sup>

These developments were incompatible with each other. General equilibrium analysis stemmed from the work of Walras and the first generation of Austrians, notably Carl Menger, but the short-run macroeconomic theory that sprang from it - Austrian business cycle theory - had failed to catch on in the 1930s, partly as a result of Keynes's success.<sup>3</sup> Partial equilibrium analysis, on the other hand, was one component of a broader Marshallian approach to economic theory that also permeated the macroeconomics of *General Theory*. Thus, the root cause of economic theory's troubles in the 1950s was that, in the 1930s, competing continental and Marshallian traditions had won one battle each, the former on the micro front and the latter on the macro, and that a third battle remained to be fought, over the micro-foundation of macroeconomics.

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<sup>2</sup>The story of general equilibrium theory's arrival in English language economics is complicated. Walras' *Elements* itself did not appear in translation until 1954 when William Jaffe's edition appeared. (Not coincidentally, Jaffe held an appointment at Northwestern, when Leijonhufvud was a graduate student there) The main source of information about this body of theory available in English in the 1930s was the 1923 translation of Gustav Cassel's *Theory of Social Economy*, though Hicks seems to have read Pareto in the original Italian.

<sup>3</sup>This history too is beyond this paper's scope. I have discussed it at length in Laidler (1999)



The most thorough exposition of the tension between Marshallian and Walrasian approaches to economics written at that time was Milton Friedman's (1953) "The methodology of positive economics". This essay argued that the main point of contrast between the two lay in the Marshallian use of economic theory as "an engine of analysis" that permitted empirically testable hypotheses about real world economic phenomena to be formulated, and the Walrasian quest for an analytic framework general enough to encompass essentially any possibility. Obviously, on this criterion, the economics of Keynes's *General Theory*, with its strong hypotheses about the stability of the consumption function, the volatility of the marginal efficiency of capital, the sensitivity of the demand for money to the rate of interest, and so on, is as thoroughly Marshallian as the general equilibrium theory of *Value and Capital* is Walrasian, and it is hardly surprising that the bodies of literature that followed on from them would prove hard to square with one another.<sup>4</sup>

Even so, by the 1950s, the phrase "Keynesian economics" had come to refer not so much to a system built around Keynes's own specific empirical hypotheses, but around the IS-LM model, a formal framework which could accommodate those hypotheses to be sure, and generate results that bore a reasonable resemblance to what Keynes had claimed them to imply as well. But the IS-LM framework was a general equilibrium model of sorts that could also accommodate other hypotheses which yielded very different predictions.<sup>5</sup> Though IS-LM was certainly not a model in the tradition of Walras in any strict sense, some of its exponents were beginning to deploy it in ways that any follower of Friedman would characterise as Walrasian, and it was hardly surprising that economic theorists working along such lines would begin to explore its logical relationship to traditional general equilibrium theory. That is how the search for the micro-foundations of macroeconomics, to which Leijonhufvud contributed so much, seems to have begun, and two names stand out among those who preceded him, Don Patinkin (see eg. 1956) and Robert Clower (see eg. 1965).

### *The Patinkin-Clower contribution*

The typical general equilibrium model of fifty years ago dealt with an economy with a given endowment of productive resources, inhabited by utility-maximising households and perfectly competitive profit maximising firms, and its analysis showed (among other things) that the resources in question would be

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<sup>5</sup>And the fact that Hicks (1937) had a major role in its creation and popularization makes it tempting to speculate that there was a micro-general-equilibrium influence at work there from the beginning. However, the immediate influence on Hicks's creation of the famous diagram seem to have been Roy Harrod and James Meade. On this See Warren Young (1987)

fully utilised if a set of relative prices ruled in the system that rendered the decisions of each agent compatible with those of all others, this even if the information available to each agent concerned only those prices, (as well as its endowments of resources, its own tastes - if household - or the technology available to it - if a firm).

The typical IS-LM model, on the other hand, was largely devoid of explicit maximising foundations, dealt with a world in which one input, labour (or two if account was taken of an exogenously given capital stock) produced a single good. In that model the nominal wage level was constant, and agents also faced a portfolio decision which was usually reduced to one about holding a stock of nominal money (whose supply was exogenously fixed). Such a model could, and typically did, generate a solution in which some labour remained unemployed. Two salient characteristics in particular differentiated these systems, each of which, be it noted deals with the behaviour of the economy as a whole: the absence of money from the first of them, and the capacity of the second to generate unemployment.

Patinkin's main contribution to their reconciliation was to find a way of introducing nominal money into the general equilibrium system, which he accomplished by including *real* money balances in agents' utility functions, and allowing a "real balance effect" driven by a modicum of price flexibility to ensure that the model generated a stable equilibrium price level. But he also showed that the logical properties of his model implied that, if unemployment was to occur, the labour market in his system must have settled at a point of market disequilibrium, *off and inside* its demand curve for labour.

Clower, on the other hand, emphasised the contrast between the behaviour relations implied by a standard Walrasian general equilibrium model, where quantities responded to prices, and a key relationship of the standard IS-LM model, in which one quantity, consumption, varied with another, income. He then argued that the latter only made sense if agents were trading at *false* prices, prices other than those compatible with general equilibrium. Specifically, he argued that, if households were unable to sell all the labour they intended at the going real wage, they would simultaneously be unable to fulfill their consumption plans, and that their actual consumption would then be constrained to vary with income. The general equilibrium model enabled *notional* demand and supply curves to be generated, but the plans implicit in them could only be accomplished if market clearing prices ruled. If they did not, then actual behaviour would be driven by *effective* demand and supply curves in which quantities figured as arguments.

Patinkin's analysis of the labour market, and Clower's of the goods market were complementary to one another, and implied that micro-economic

foundations for IS-LM macroeconomics were to be found in the hypothesis that trading could indeed take place at non-market clearing prices, and, second, in its implication that an initial shock to the system would then set in motion quantity dynamics, an *income constrained process*, in which deviations from full employment equilibrium were amplified rather than damped.

Such interactions were, of course, amenable to explicit modelling based on maximising premises, and one product of the Patinkin-Clower enterprise was an extensive formal literature whose highlights include Barro and Grossman (1976) and Malinvaud (1977), but whose details need not concern us in this paper. Suffice it to say that the easiest way to build models in which trading takes place at false prices is to hold prices constant, and that more and more elaborate systems built upon this assumption rapidly ran into diminishing returns. The literature in question rigorously established the existence of the linkages between general equilibrium analysis and 1960s style macro-theory that the insights of Patinkin and Clower had postulated, and generalized them as well. To this extent it was important, but its significance was to help bridge an existing gap between two already well established research agendas, rather than to create a foundation for any new work.

#### *Leijonhufvud, Keynes and Marshallian microeconomics*

Leijonhufvud's work should be seen as a search for an alternative and potentially more fruitful way forward from the Patinkin-Clower insights. He assiduously avoided the trap of reducing trading at false prices to trading at fixed prices, so his work had an immediate claim to relevance when it came to analysing the interaction of money prices and quantities over time, a problem that was attracting increasing attention as the great inflation that began the mid-1960s gathered momentum; and crucially, his way of establishing microeconomic foundations for a macroeconomics descended from Keynes's very Marshallian *General Theory* was to seek them, not in contemporary Walrasian microeconomics, but in the equally Marshallian microeconomics that Keynes had worked with, and from which IS-LM analysis had become detached.

This Marshallian microeconomics, though already overshadowed by its Walrasian challenger, had not quite disappeared fifty years ago. Indeed it figured prominently in Friedman's (1953) essay on "Positive Economics" already referred to above, where the main example cited of the advantages of the pragmatic Marshallian approach to economic theory was the theory of perfect competition, whose empirical content Friedman favourably contrasted with monopolistic competition, for him the epitome of Walrasian vacuousness. Nowadays, it seems odd to characterise perfect competition as Marshallian, because we are used to defining it as a state of affairs in which all agents are price takers, who respond to market clearing prices set by an entity known as the

*Walrasian auctioneer*. But Friedman's view made excellent sense at a time when perfect competition's defining characteristic was still regarded as being the absence of any interdependencies among individual firms' roles in the price formation process that would rule out the use of supply and demand analysis at the level of the industry, and when the every-agent-a-price-taker assumption remained to be examined.<sup>6</sup>

Friedman was, that is to say, writing before Kenneth Arrow's (1959) observation that, if every agent was a price taker, then no-one was left to set and change prices, and therefore before the above-mentioned fictitious auctioneer became a central player in microeconomics, whose specific task was to resolve this paradox. Leijonhufvud, on the other hand, was writing in the immediate wake of these developments, and was fully conscious that they seemed to render the Walrasian theory of competitive markets totally unhelpful for analysing real world price adjustment processes. But he was also aware that the older Marshallian conception of competition that had underlain Keynes's macroeconomics left space for prices to be adjusted without the help of an auctioneer; and he saw that modern theories of market search, such as were being developed, among others, by his colleague Armen Alchian were perhaps able to fill this space and in a way that would allow the Patinkin-Clower insights about the consequences of trading at false prices to be placed on a firmer theoretical footing.

Leijonhufvud summarised the point in an *AER* article published shortly before his book, (explicitly citing Arrow 1959 and Alchian and Allen 1964)<sup>7</sup>

Walras' auctioneer is assumed to inform all traders of the prices at which all markets are going to clear. This always trustworthy information is supplied at zero cost. Traders never have to wrestle with situations in which demands and supplies do not mesh; all can plan on facing perfectly elastic demand and supply schedules without fear of ever having their trading plans disappointed. All goods are perfectly 'liquid,' their full market values being at any time instantaneously realizable. Money can be added to such models only by artifice.

Alchian has shown that the emergence of unemployed resources is a predictable consequence of a decline in demand when traders do not have

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<sup>6</sup>The contrast between Marshall and Walras's approach to economics was much discussed in the 1950s and early 1960s, and I am far from sure that everyone who drew a line between the two did so in the same place. It would be interesting to investigate this matter further. It is also worth noting that partial equilibrium microeconomics retained a strong position in introductory textbooks long after intermediate and advanced micro-theory had been taken over by the general equilibrium approach.

<sup>7</sup>Alchian and Allen were, like Leijonhufvud, members of the UCLA economics department. They were probably unwise to publish important and original analysis for the first time in an introductory textbook, if they wanted to maximise its exposure among their professional colleagues..

perfect information on what the new market clearing price would be. The price obtainable for the services of a resource which has become “unemployed” will depend upon the costs expended in searching for the highest bidder. In this sense the resource is “illiquid”. . . Reservation price will be adjusted gradually as search continues. Meanwhile the resource remains unemployed. To this analysis one need only add that the loss of receipts from its services will constrain the owner’s effective demand for other products - a feedback which provides a rationale for the multiplier-analysis of a system of atomistic (“competitive”) markets. (1968b, as repr.1981, p. 6)

The account of the problems associated with finding new equilibrium prices given in the first part of this quotation is more elaborate than those that Keynes frequently offered his readers, but it does not differ in substance from them. Leijonhufvud’s claims that the *Economics of Keynes* was informed by a microeconomic analysis of decentralised markets that did not rely on the auctioneer were thus surely correct, though it is less clear that Keynes was sufficiently aware of the alternative to have self-consciously rejected it.<sup>8</sup> The following passage, taken from the *Treatise on Money*, is typical of several discussions there and in the *General Theory*, of the difficulties faced by agents in such markets when prices must change to keep them cleared.

Under a socialist system the money rate of efficiency earnings of the factors of production might suddenly be altered by *fiat*. Theoretically, I suppose it might change under a system of competitive individualism by an act of collective foresight on the part of entrepreneurs in anticipation of impending monetary changes, or by a *coup de main* on the part of trade unions. . . . In existing circumstances, however, the most usual and important occasion of change will be the action of entrepreneurs . . . in increasing or decreasing the volume of employment which they offer at the existing rates of remuneration . . . and so bring about a raising or a lowering of these rates. (1930, I, p. 141), italics in original)”

### *Inter-temporal co-ordination*

As Leijonhufvud was at pains to argue, what mattered for setting in motion cumulative fluctuations in expenditure and employment was not that prices should be rigid, but only that should move sufficiently slowly to permit trading at non-market-clearing prices to get under way. Indeed, as the passage quoted

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<sup>8</sup>Whether Keynes was actually self-conscious about rejecting the services of an auctioneer, however, seems doubtful. Concern with this entity’s role in the market-place was very much a phenomenon of the 1960s, though he did, as Leijonhufvud (1968a, pp. 68 et seq.) was well aware, appear in the work of Edgeworth though not of Walras.

earlier makes clear, it was an essential characteristic of his analysis that the quantitative consequences of trading at false prices would arise from the very same dynamic processes that would drive variations in those prices. It was partly on this basis that Leijonhufvud argued that fixed price IS-LM exercises not only seriously misrepresented the economics of Keynes but, more generally, were inadequate for analysing the behaviour of any market economy; but only partly. He also strongly criticised the appropriateness of the IS-LM model's treatment of output as consisting of a single good. The distinction between consumption and investment goods was, he suggested, crucial.<sup>9</sup>

Not only did a chronic inability of the price of capital goods to find and maintain its right level relative to that of current consumption lie at the heart of Keynes's explanation of the market economy's inability to maintain full employment, but that explanation was also basically correct. Any shock which required that this relative price should fall to re-equilibrate the system would initially create a shortfall of the nominal demand price of capital goods from their supply price, and set in motion a cumulative contraction of output. However, the required relative price adjustment could not necessarily be accomplished by a fall in money wages (even if these were capable of rapid adjustment) because this would also cause the money price of consumption goods to fall. What was needed was a fall in the rate of interest that would cause the current demand price of capital goods to rise. But, argued Leijonhufvud, "Once the income-constrained process had been allowed to gather momentum . . . expectations would no longer be such as to sustain full employment even in conjunction with a 'metastatically right' interest rate" (1968a, p. 340)

And so, in his view,

Keynes' diagnosis of the conditions leading to a downturn in activity focussed on the relation between the money prices of non-money assets [i.e., investment goods] and the money wage rate. If this relation was out of line, . . . he put the 'blame' on too low asset values as a rule, not on too high wages. The conclusion is that deflation will help *only if* it changes this relative price in the appropriate direction, i.e., *only if it cures the malady that underlies the emergence of excess supply of commodities in the first place* (1968a, pp. 341-342, italics in original)

And to repeat, in Leijonhufvud's interpretation of Keynes, that malady lay in a misaligned relative price of investment and consumption goods, with a concomitant failure of market mechanisms to co-ordinate the allocation of

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<sup>9</sup>Leijonhufvud would later write a seminal study of the earlier history of inter-temporal allocation issues, namely "The Wicksell Connection", (1981b). The relationship between his 1960s views on Keynes's role in developing the analysis of this problem is yet another important topic which lies beyond the scope of this paper.

resources over time; and cumulative output fluctuations, driven by income constrained dynamics, were the market economy's response to this failure. Obviously, a single good IS-LM model could not be used even to formulate this idea, let alone evaluate it.<sup>10</sup>

Now *Keynesian Economics and the Economics of Keynes* presented two challenges for its readers. First, as a work in the history of economic thought, it repudiated IS-LM analysis as an interpretation of Keynes's *General Theory*, and proposed an alternative version of that book's central message; Second, as a contribution to economic theory, it proposed the abandonment of this same IS-LM model in favour of an approach, which, being based on the analysis of trading at non-market-clearing prices, reduced then standard microeconomics to a special and not very interesting case of an altogether broader framework. In short Leijonhufvud argued that macroeconomics had gone off on the wrong track because Keynes's interpreters had failed to understand him, that the perplexing gap between the macro and micro components of then contemporary economic theory referred to earlier this paper had been a direct result of this, and that the gap in question could not be bridged without fundamental revisions to both micro and macro theory.

A full treatment of this extraordinarily ambitious book's significance for the development of economics would have to assess both the validity of its claims about the discipline's past, and the success of its proposals for the subject's future, and there is not space here to do both. The balance of this paper will therefore deal only with the latter topic, and only certain aspects of it into the bargain.

### *Monetarism*

Co-incidentally, the word *monetarism* was introduced into the mainstream vocabulary of economics by Karl Brunner in (1968), the same year in which *On Keynesian Economics and the Economics of Keynes* was published, and ultimately it would be developments springing from this doctrine that would prevent Leijonhufvud's ideas having their intended impact on the future course of economic theory. Monetarism was not new in 1968, of course.<sup>11</sup> On the

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<sup>10</sup>The reader will note that Leijonhufvud's interpretation of the essentially dynamic nature Keynes's central message rests heavily on material that appears in Chapter 19 of the *General Theory* and plays little role elsewhere in the book, and as he himself noted, though that message was about dynamics "Keynes' model was static" (to which this author would add, and was not badly summarised in the IS-LM model, which is why so many readers found versions of it in his book. On this, see Laidler 1999, ch. 12)

<sup>11</sup>Karl Brunner too was a member of the UCLA department in the 1960s, and it is therefore probably no accident that his version of monetarism paid more attention to the information problems that lay at the heart of monetary economics than did Friedman. See Brunner and Meltzer (1971) The history of the UCLA department's contributions to monetary economics during this period would make a fascinating study.

contrary, the appearance in March of that same year of Friedman's AEA presidential address on "The Role of Monetary Policy" (of which more below) put in place the capstone of an intellectual edifice that had been under construction at least since the publication *Studies in the Quantity Theory of Money* in 1956.

As Leijonhufvud himself would later note "By the mid-sixties, . . . macroeconomics was drawing most of its excitement from the challenge posed by . . . the 'monetarist' or 'new-quantity' theory of Friedman, Schwartz, Cagan, Brunner and Meltzer" (1976, repr. 1981, p. 316), and this was an alternative and parallel expression of dissatisfaction with orthodox LS-LM macroeconomics to that represented by his own work. But where Leijonhufvud's research agenda centred on matters of economic theory, monetarism was more concerned with practical policy and the empirical evidence upon which it might be based. Initially too, they emphasised different economic phenomena, income and employment fluctuations and inflation respectively, a factor which Harry Johnson would still argue as late as 1971 made monetarism inherently less interesting to mainstream economics.

But there was more to monetarism than the revival and refinement of the quantity theory of money as an explanation of inflation. Thomas Mayer's organised his still definitive (1975, repr. 1978) survey of the doctrine around twelve defining characteristics, three of which are particularly noteworthy in the current context, namely ". Belief in the inherent stability of the private sector. . . . Irrelevance of allocative detail for the explanation of short-run changes in money income, [and] . . . Focus on the price level as a whole rather than on individual prices" (p. 2). In his contribution to the symposium that Mayer's paper inspired, Benjamin Friedman (1978, p. 96, fn.3) noted in passing that the monetarist debate had not intersected with Clower and Leijonhufvud's work, and this is surely not surprising. Their emphasis on the importance of allocative detail and relative prices for understanding macroeconomic fluctuations, not to mention their insistence on the private sector's vulnerability to income constrained dynamics that tended to amplify shocks, set their work far apart from that doctrine. Nor did it have any point of contact with the characteristic of monetarism that Mayer put at the very top of his list, namely its deployment of "The quantity theory of money in the sense of the predominance of the impact of monetary factors on nominal income"

As we have seen, Leijonhufvud was concerned with the logic of economic theory as it was then expounded, and as it appeared in Keynes's work; but at a time when "positive economics" was popular and "monetarism" seemed to be based on its precepts, many among his readers would be bound to judge his work not so much on the basis of its logical coherence and scholarly accuracy, as on its empirical relevance. His version of the *Economics of Keynes* was,



however, firmly based on the presumption that the experience of the inter-war years in general, and of the United States in the 1930s in particular, had demonstrated that market economies were inherently unstable, and that it was the task of economic theory to discover just where their flaws lay. Hence, monetarism's assertion of the inherent stability of the private sector challenged not only IS-LM orthodoxy, but Leijonhufvud's work too, and it was supported by (among other evidence) a specific and detailed re-interpretation of the causes of the Great Depression in the United States.

The full impact on economics of chapters 7 - 9 of Friedman and Schwartz's (1963) *Monetary History of the United States*, which dealt with the 1930s, was slow to be felt.<sup>12</sup> Their immediate message about economic history was that the downturn with which the Depression had begun in 1929 had very likely been provoked by monetary tightening, and that the economy's subsequent catastrophic contraction had been caused, not as orthodoxy had it, by some exogenous collapse in the marginal efficiency of capital that monetary policy had been powerless to offset, but by colossal ineptitude on the part of the Federal Reserve; and. Leijonhufvud did in fact refer to the *Monetary History's* diagnosis of the role of monetary contraction in bringing on the initial downturn in late 1929.

“Keynes [as author of the *Treatise on Money*] would have concurred with Friedman and Schwartz in all essentials of their critique of Federal Reserve policy in this period [the late 1920s] and in attributing the onset of the Great Depression to the period of tight money preceding the actual downturn in activity, although he would, as usual, have conducted the analysis in terms of interest rates and ‘credit conditions’ rather than the stock of money” (1968a, p. 286)

but did not refer to what Friedman and Schwartz had to say about the Great Contraction itself in that book, nor to the broader implications of their re-interpretation of economic history for macroeconomic theory.<sup>13</sup>

Those implications were nevertheless of profound significance, for if the cause of the Great Contraction had been an avoidable monetary disturbance, did not that perhaps suggest that market economies which were not subject to such

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<sup>12</sup>As Susan Howson has impressed upon me. Note also that we are now more conscious than were readers of the 1960s of the work of some of Friedman and Schwartz's predecessors, and it is hard now to appreciate just how radical it seemed at the time, and strong was its impact. Lauchlin Currie's work, for example, had largely been forgotten, though he had published an article entitled “The failure of monetary policy to prevent the depression of 1929-1932” in the *JPE* in 1934, surely a title that tells its own story. On the reaction of American economists to the Great Depression, See Laidler (1999, ch.9).

<sup>13</sup>Though he did refer briefly to these matters in later lectures given at the Institute for Economic Affairs in London, England in 1969, See (1969, repr. 1981, p.42)

policy disturbances were well capable of coping with the allocation of resources over time and therefore inherently stable after all? If this was indeed the case, then the conventional interpretation of economic history that had motivated Leijonhufvud's work (and much else) was misguided, and though interesting as doctrinal history and economic theory, was it not also empirically irrelevant?

It was not until the early 1970s that these deeper implications of Friedman and Schwartz's work began to sink in among economists in general, and Leijonhufvud addressed them indirectly in his 1973 (repr. 1981) paper on "Effective Demand Failures". There he faced up to what he had by then come to recognize as a weakness of his earlier work, namely that it seemed to make economic instability all too inevitable. He now declared that ". . . the central issue of macroeconomics is - once again - the extent to which the economy, or at least its market sectors, may properly be regarded as a self-regulating system? How well, or badly, do its 'automatic' mechanisms perform?" as a prelude to exploring the properties of the *corridor of stability* within which various mechanisms that he had earlier ignored or downplayed might be at work. These included the capacity of inventories, not least inventories of money and financial assets, to interfere with the mechanics of income constrained processes so as dampen deviations from full employment caused by various shocks.

#### *Newclassical microfoundations and Occam's razor*

This paper provoked little response.<sup>14</sup> By the mid-1970s, a new approach was beginning to take hold of the micro-foundations research agenda..Where Leijonhufvud's *Economics of Keynes* had investigated the non-Walrasian microeconomics of an economy that was presumed to be unstable, and had perhaps explained more instability than the world in fact displayed, *Newclassical* economics went to the opposite extreme. Building upon monetarism - indeed James Tobin (1981) would label it *Monetarism Mark 2* - it investigated the macroeconomic properties of a system in which Walrasian micro-mechanisms were presumed always to work, and which could only be disturbed by arbitrary shocks administered by erratic monetary policy.

The rise to popularity of this approach has a number of explanations. First of all it had an element of empirical plausibility. Not only had Friedman and Schwartz re-interpreted the Great Depression as a consequence of monetary policy, but by the early 1970s, memories of it were fading under the influence of a quarter century of rather stable expansion at more or less full employment; and closely related, inflation, in Harry Johnson's (1971) judgement, the policy

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<sup>14</sup> Peter Howitt's (1978) paper was a notable exception

problem to whose analysis monetarism was in any event best adapted was becoming a serious issue.

But these empirical issues were of secondary importance when compared to the influence of theoretical developments, particularly the discovery of the so-called *expectations augmented Phillips curve*. This was not an exclusively monetarist creation, for Edmund Phelps (1967) was one of its important originators, but Friedman (1968) used it to establish two quintessential monetarist propositions: namely, that the permanent inflation-unemployment trade off with which exponents of IS-LM *Keynesian Economics* were by that time routinely supplementing their analysis, was at best a short-term phenomenon, and that monetary policy's only long term effects were on the inflation rate. In other hands, moreover, though curiously not in Friedman's own, the expectations augmented curve began to fill the role of the "missing equation" that monetarist analysis had long needed to allocate the quantity-theoretic effects of money growth on money income between its real-income and price level components.

The rich literature that in the late 1960s examined potential microeconomic foundations for this relationships still awaits careful attention from historians of economic thought, but it should at least be noted here that Leijonhufvud's deployment of Armen Alchian's search theoretic analysis of non-Walrasian market processes was one candidate, but not the one that ultimately won out.<sup>15</sup> Instead Robert E. Lucas's (1972) thoroughly Walrasian aggregate supply curve interpretation of the curve, coupled with his application to it of John Muth's *rational expectations* concept, found broad acceptance; and this was quickly cemented not just by his own subsequent work (1976) on its application to econometric policy modelling, but also by that of Thomas J. Sargent's (1973) on its implications for Friedman's natural unemployment rate concept, and of Sargent and Neil Wallace (eg. 1975) demonstration that it permitted monetarist scepticism about systematic monetary policy's ability to affect anything other than the inflation rate to be put on firm maximising foundations.

Citing subsequent papers by Lucas, Harry Johnson (1976, repr. 1978)) explained why Leijonhufvud's approach failed to catch on in the following terms ". . . It is virtually impossible to find a simple and comprehensive mathematical device for converting a general equilibrium system of mathematically formulated relationships into a fruitful technique for the study of persistent 'disequilibrium' and 'market failure'." (p. 244), and he elaborated the

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<sup>15</sup>Such a study should begin with the famous "Phelps volume" (Phelps et al. 1970). Phelps (1974) characterised the main purpose of Leijonhufvud's book as being to establish a connection to the *General Theory* for the literature in question, surely too narrow a characterisation of its significance.

point in a footnote “The essential problem is that it is virtually impossible to invent a plausible mechanism that leaves the economy in disequilibrium with unexploited possibilities for profits or increased labour incomes, and at the same time specifies exactly how the economy will respond to a *change* in profit or labour income opportunities” (p. 244, fn) Johnson’s point was a sobering one, for it amounted to saying that to give up Walrasian foundations in order to study macroeconomic phenomena seemed also to require their abandonment when allocative issues were to be discussed, if the analytic consistency of economic theory was to be preserved, and that there was no workable alternative available to permit this shift.

The abandonment of Walrasian general equilibrium theory as a basis for the study of the economics of allocation was too large a sacrifice to contemplate, and perhaps the discipline might have chosen to live a little longer with what was by then an all too obvious inconsistency between its macro and micro branches, had not Lucas’s work seemed to render this unnecessary. Here it was not so much its theoretically compelling treatment of information processing as an exercise in maximising behaviour that mattered as its extremely attractive capacity to reconcile the co-existence of fluctuations in quantities as well as prices with continuously clearing Walrasian markets.

In the conventional *Keynesian economics* that Leijonhufvud had attacked, quantities varied in response to demand shocks because prices did not vary at all, and in his version of the *Economics of Keynes*, they varied because prices did not vary instantaneously. But both approaches simply took it for granted that, if prices were instantaneously flexible, quantities would always remain at their full employment level. In Leijonhufvud’s words “Perfect knowledge and absence of any costs connected with the act of changing price (or rate of output) would enable the traders in an atomistic market to detect and move instantaneously to the new price equilibrium following a disturbance” (1968a, p. 69). Perfect knowledge was to him synonymous with the presence of a Walrasian auctioneer in the market place, and trading at false prices the inevitable consequence of his absence.

Lucas, on the other hand kept the auctioneer in place but limited his activities. Specifically, he still let him set prices that would keep markets cleared but prevented him from informing agents about them. They had to estimate relative prices by applying knowledge of a true model of the economy in which they operated to information about the time series properties of the monetary disturbances to which it was subject (both of which they were assumed to have) and information about particular money prices culled from the markets in which they were sellers. Thus Lucas logically separated the phenomenon of limited information from the mechanics of price formation; and in so doing, he demonstrated that limited information problems that did not imply price

stickiness were nevertheless sufficient to generate quantity variations even in the presence of complete price flexibility.

In short, Lucas showed that neither *Keynesian Economics* nor *the Economics of Keynes* was needed to explain what seemed to be the salient facts of macroeconomic experience, because the addition to a Walrasian general equilibrium model of the right assumptions about agents' limited information was sufficient to do so. Lucas's model, if it was to be taken seriously as an "as if" representation of a real world in which there was no auctioneer, amounted to arguing that markets would be kept cleared, not just by the collective foresight of entrepreneurs acting in anticipation of impending monetary changes as Keynes (1930, p. 141) had suggested when stretching for an example, but also by those entrepreneurs' ability to take account of the errors into which imperfections in that foresight would collectively lead them. But far fetched though it was, even on an "as if" basis, it implied nevertheless that a separate macroeconomics was logically unnecessary for the explanation of output and employment fluctuations, and that Walrasian microeconomics was sufficient as a basis for all economic theory. Economists in large numbers began to reach for Occam's razor, and both IS-LM style *Keynesian Economics* and Leijonhufvud's *Economics of Keynes* were quickly cut adrift. In short, Walrasian economics seemed to have won the third and final battle with the Marshallian alternative.<sup>16</sup>

#### *A postscript on inflation and disequilibrium*

As a matter of logic, to show that it is not necessary to refer to a specific factor when explaining a phenomenon does not also demonstrate that factor's irrelevance, and as Harry Johnson warned in his review of *The Monetary History of the United States*, Occam's razor ought not to be used to "cut the throat of empirical research". Thus, though in the 1970s and 1980s, it was very difficult to get serious attention paid to any analysis of output fluctuations that either relied on price stickiness and/or postulated information problems that did not square with the idea of rational expectations, Marshallian pragmatism turned out still to have some life left in it when it came to coping with the economics of inflation.

As we all know, it is very difficult to find a place for money in an economic model in which markets always clear, which is why work in the Newclassical tradition expended so much energy on *cash-in-advance*

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<sup>16</sup>As with Friedman and Schwartz (1963), it took some years for the full significance of Lucas's contribution to be fully appreciated. As we have seen, even so notable a contributor to the Newclassical literature as Robert J. Barro would later become was still publishing on the economics of fixed price equilibrium models as late as 1976.

constraints, and *overlapping generations* models.<sup>17</sup> For many applications, perhaps this did not matter, but, hardly surprisingly, investigations of the consequences of a falling value of money that began from premises that money had no serious work to do in the first place were hardly likely to find these to be important. The best that they could do to capture the idea that inflation was costly - and everyday experience demonstrated beyond any reasonable doubt that it was, extremely so - seemed to be to follow Friedman (1969) in deploying Patinkinesque formulations of the demand for money that relied on putting real balances in the utility function, or to revert to Baumol-Tobin style models of transactions costs in asset markets; but these rather arbitrary fixes implied that the costs of inflation were merely a matter of “shoe-leather”, barely worth considering when weighted against likely unemployment costs of reducing it, as James Tobin (1972) was quick to point out

The basic trouble here was that, in a Walrasian framework, the costs of inflation at best could be assessed on the assumptions that it was “fully anticipated” and that markets for goods and services continued to clear. Because such an approach trivialised money in the first place, it also trivialised any disorder of the monetary system, inflation included. It is surely no accident that Leijonhufvud (1977a & b, repr. 1981), a recent exponent of a Marshallian approach to microeconomics that left space for market disequilibrium, was quick to recognise these problems, and to propose an alternative line of attack, grounded in an institutionalist vision of the phenomena of monetary exchange that encompassed its essential role in the workings of the market economy. From this viewpoint it was easy to organise ideas about how inflation not only undermined money prices’ ability to transmit information and incentives to agents, but also arbitrarily redistributed the property rights on whose security the very workings of voluntary exchange depend in the first place.

There is not space here to give Leijonhufvud’s work on the costs of inflation the attention it warrants, but let me offer two conjectures. First, I suspect that a careful study of the subsequent literature on these issues, that finally led to policy makers taking them seriously enough to begin to tackle inflation in the 1980s, will show that it had a seminal influence on them; and second, I also suspect that a comparison of Leijonhufvud’s earlier work on the disequilibrium microeconomics of employment fluctuations with his later analysis of inflation’s capacity to disrupt the workings of market mechanisms will reveal close analytic connections between them.

Leijonhufvud himself did not stress these connections; his 1977 paper on inflation started from institutions rather than micro-theory, and when he

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<sup>17</sup>The cash in advance constraint was originated by Clower (1967) The uses to which Newclassical economics put it were not among those that he had in mind for the idea.

developed its ideas further in his much under-appreciated work with Daniel Heymann on *High Inflation* (1995), it was once more these factors, not to mention a great deal of empirical evidence about what actually happens in markets under such conditions that took centre stage. But, recall Harry Johnson's Lucas inspired objection to Leijonhufvud's disequilibrium dynamic reconstruction of the *Economics of Keynes*: "it is virtually impossible to invent a plausible mechanism that leaves the economy in disequilibrium with unexploited possibilities for profits or increased labour incomes, and at the same time specifies how the economy will respond to a *change* in profit or labour opportunities". Does not Leijonhufvud's subsequent work on inflation imply the following pragmatic response to this criticism? "Quite so: that is because once disequilibrium takes hold of a monetary economy, markets stop working. If we want a world to which the special case of Walrasian general equilibrium theory can be applied, we had better have policies that prevent this happening". And dare I conclude this essay by suggesting that this would be a very pragmatic, even Marshallian, comment on the limits to that theory's usefulness, and express the hope that it might also meet the approval of the author of *On Keynesian Economics and the Economics of Keynes*?

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## **Milton Friedman and the Evolution of Macroeconomics\***

**by**

**David Laidler**

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Milton Friedman is a rarity, an economist whose name is widely recognised among the general public but who is also acknowledged within his discipline as having made contributions to it of lasting importance. In the twentieth century, only John Maynard Keynes has a claim - by no means undisputed - to a higher rank than Friedman as a public figure and economic scientist. The reputations of the two are, furthermore, deeply intertwined within the evolution of macro-economics - that branch of the subject that deals with the behaviour of the economy as a whole. In the late 1930s, with the Great Depression still hanging over the world's market economies, Keynes was pivotal in the very creation of macro-economics as a separate sub-discipline, and his work also helped to direct it in a particular policy direction. Beginning in the 1950s, Friedman would play a key part in bringing about a radical re-assessment of macro-economics' central scientific tenets, not least as they appertained to the explanation of the Great Depression, and of their policy implications too. And yet, as I shall argue in due course, Friedman and Keynes belong to the same intellectual tradition in economics - that associated with Alfred Marshall. This tradition has lately fallen into neglect, and ironically so, since this has come about, in some measure, because of Friedman's work.

Friedman's generation of economists came to intellectual maturity during the Great Depression, and it would have been natural for him to have been concerned with macroeconomic questions from the very outset of his career. Anecdotal evidence (see, for example, Friedman and Friedman 1998, p. 81, fn). suggests that, initially, he was a rather uncritical supporter of Franklin Delano Roosevelt's "New Deal" - in its own right, a largely independent source of many of the dirigiste policy ideas that in the post-war years would come to be labelled "Keynesian" - but among his earliest academic interests were pure microeconomic theory and mathematical statistics. As I shall now argue, when Friedman began to help transform macroeconomics in the 1950s, it was by bringing his expertise in these areas to the very centre of the study of consumption behaviour, a topic which had also engaged his attention from the 1930s onwards. His 1957 monograph *A Theory of the Consumption Function* was by no means his first publication on either the economics of consumption in particular, or macroeconomics in general, but it is utterly central, both to his own work and to the evolution of the discipline, so that is where I shall begin this account of his contributions. Only when I have dealt with it shall I pass on to his work on monetary theory, monetary history, and monetary policy.

### *The Consumption Function*

Keynes's 1936 *General Theory* sought to explain the occurrence and persistence of large scale unemployment, and it did so in a way that proved readily amenable to a degree of simplification that, by the 1950s, was making it the stuff of intermediate and even elementary textbooks. The overall level of

unemployment, so it was said, varied with the economy's real output (Y), which, when resources were unemployed, was able to respond more or less passively to satisfy the demand for goods and services. This demand, in turn, came from three sectors of the economy: households (consumption, C), firms (investment, I) and the government (government expenditure, G).

Investment was said to be largely autonomously determined by the “animal spirits” of firms, unstable over time, and sufficiently impervious to influence from monetary policy that even the reasons why this might be so could be safely neglected in elementary expositions. Households were said to divide their incomes between consumption and saving according to a “fundamental psychological law” - Keynes's own phrase (1936, p.96) - that saw a stable fraction (c, called by Keynes the *marginal propensity to consume*) of changes in income spent on goods and services. Since, at the level of the economy as a whole, the real value of output was paid out to households as real income, this simple system could be written down as follows

$$C = a + cY \quad (1)$$

$$Y = C + I + G \quad (2)$$

and then solved to yield the famous proposition that output was a stable multiple of autonomous expenditure.

$$Y = [a + I + G][1 / (1 - c)] \quad (3)$$

This extraordinarily simple system was used to convey particular empirical propositions about the workings of the economy, from which a specific policy message seemed to follow: namely, that as investment fluctuated, so would income and employment; that these fluctuations could be offset by countervailing shifts in government expenditure; and that it was therefore the task of government to take responsibility for creating and sustaining the full employment that the market economy was unable to achieve unaided. Of course, this bare bones model could be, and was, much elaborated in many directions. Taxation could be introduced, as could monetary factors, or open-economy complications, the assumption that investment expenditure was simply autonomous could be softened in many ways, not least by making it a function of the rate of interest, the model could be dynamized by the introduction of time lags, etc. etc. But, so long as monetary factors were downplayed and the resulting systems were anchored by the “fundamental psychological law” that c, and therefore the *multiplier*  $[1 / (1 - c)]$ , was an empirically stable parameter, they conveyed the same messages as did its elementary prototype.

Now, quite independently of Keynes, the 1920s and 1930s had seen a rapid growth of explicitly empirical economics.<sup>18</sup> Data were systematically collected, and a wide variety of statistical techniques began to be developed to analyse them. The National Bureau of Economic Research (NBER), closely associated in the 1930s with Columbia University was at the forefront of such efforts in the US. Friedman took courses from the Bureau's founder Wesley C. Mitchell while a graduate student at Columbia., and his Ph.D thesis, which extended work originally begun by Simon Kuznets and was supervised by Mitchell's collaborator and successor as the Bureau's director, Arthur F. Burns, was published by the Bureau in 1943, with Kuznets as joint author, under the still well-known title, *Income from Independent Professional Practice*. Friedman's abiding respect for data, and his insistence that economic models are there to explain them, mark him as an heir to the NBER tradition, as indeed do some of his specific empirical techniques, which often differed from those that would in due course come to dominate orthodox econometrics.<sup>19</sup>

More to the point under discussion here, so too does his familiarity with the difficulties that empirical economists were having with Keynes's stable psychological law. Consumption did indeed seem to vary as a fraction of income, but the quantitative relationships involved were problematic, and much work on these matters was done under NBER auspices. Over the "long run" of a few decades, the consumption-income relationship seemed to be one of strict proportionality with, to put matters in terms of equation (1),  $c$  being stable and positive and  $a$  being equal to zero. Over shorter periods  $c$  seemed smaller, and  $a$  to be positive but shifting up over time. So already there was a problem, but Keynes's fundamental law was also supposed to apply to households in general, and when cross section data emanating from budget studies were analysed, they yielded a wide variety of estimates for both parameters. There is neither need nor space here to discuss the rich literature that these empirical anomalies generated from the early 1940s onwards, and which Allan Hynes (1998) has carefully discussed<sup>20</sup>. Suffice it to say that many of the ingredients of Friedman's 1957 analysis were to be found in that literature, to which he himself had indeed been a contributor, but that the particular way in which he then put

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<sup>18</sup>In the UK indeed, these developments were hampered by Keynes's own hostility to them  
See Don Patinkin (1976)

<sup>19</sup>On the influence of NBER methods on Friedman's work, see in particular, J. Daniel Hammond (1996). I have already suggested that Friedman's macroeconomics was in a Marshallian methodological tradition. There is no contradiction here. Though, it is often suggested that Mitchell underestimated the importance of economic theory *per se*, he in fact treated it, just as did Marshall, not as in and of itself embodying scientific truth, but as a tool for interrogating empirical evidence with a view to extracting scientific truth from it.

<sup>20</sup>But Franco Modigliani's "life cycle" hypothesis nevertheless merits citation here as an independently arrived at and slightly earlier variation on the same theme that Friedman developed. See for example Modigliani and Brumberg, (1954). What Modigliani's work lacked, relative to Friedman's, was the seamless integration of economic and econometric theory that marked the latter, and on which I comment below.

them together in his *Theory of the Consumption Function* would not only have a direct impact on contemporary macroeconomic orthodoxy, but would also, in the longer run, come to be seen as a fundamental turning point in the way in which macroeconomic theory was done.

To a generation of economists brought up to analyse the economy one market at a time, the idea that the demand for any particular good would, *ceteris paribus*, vary with income was a common-place, and I conjecture that the vast majority of them thought of Keynes's fundamental psychological law as a simple generalisation of this to the level of consumption as a whole. But there is a massive fallacy of composition here. Friedman the microeconomic theorist, with some acknowledged help from Irving Fisher (1907, 1930), understood that the relevant objects of choice in the microeconomics of the consumption function had to be consumption now and in the future, and that the constraint upon that choice was defined by income now and in the future as well as the terms upon which it could be loaned or borrowed. On the assumption of a perfect capital market, the typical consumer could be thought of as able to sell his expected future income stream and purchase an annuity with the proceeds, and it was this hypothetical annuity, the consumer's *permanent* income, that Friedman postulated to be relevant to the choice of today's consumption. Current consumption, the outcome of a forward looking maximising decision about that variable's overall time path, would only vary with *current* income to the extent that variations in the latter affected permanent income. *Transitory* income, the difference between permanent and current income, would have no influence on consumption.<sup>21</sup>

Friedman the statistician then developed the implications of this elementary microeconomic theory of consumption for the statistical theory underlying the least squares estimation of  $c$ . Specifically, if the true model determining consumption was

$$C = a' + c'Y(p) + e \quad (4)$$

but the model estimated by least squares regression was

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<sup>21</sup>The vocabulary of *permanent* and *transitory* components of income, their statistical interpretation as systematic and random components of the variable, and indeed a more general analytic approach based on forward looking maximisation, are already present in Friedman and Kuznets (1943). Given Irving Fisher's emphasis on forward looking maximisation, albeit in a non-stochastic environment, and the fact that Friedman (1957) draws on his work, it is tempting to think of Friedman as self-consciously reviving the important Fisherian tradition in American economics. However, there is no reference to Fisher in Friedman and Kuznets (1943), the capital theory set out in Friedman's (1962) *Price Theory - a Provisional Text* derives from Frank Knight, with no reference to Fisher, while Fisher's name appears in neither the Index nor the Bibliography of Friedman and Friedman (1998). Without discounting it totally, therefore, one should not over-emphasise a direct Fisherian influence on Friedman. I am grateful to Perry Mehrling for discussion of this issue.



$$C = a'' + c''Y + E \quad (5)$$

Where

$$Y = Y(p) + Y(t) \quad (6)$$

then the standard “errors in the variables” model could be applied to the interpretation of the results. Though an unbiased estimate of  $c'$  would be given by dividing the covariance between  $C$  and  $Y(p)$  by the variance of  $Y(p)$ , a downwardly biased one would arise from dividing the covariance between  $C$  and  $Y$ , (identical to that between  $C$  and  $Y(p)$  by assumption) by the variance of  $Y$ , because the latter was the sum of the variances of  $Y(p)$  and  $Y(t)$ .

In aggregate data observed over long periods of time where economic growth dominated their generation, transitory fluctuations in income would tend to average out and become very small relative to those in permanent income, so estimates of  $c''$  yielded by regressing consumption on current income would be rather accurate representations of the true parameter  $c'$ . Over shorter periods, transitory fluctuations in even aggregate income would be relatively more important, and the resulting estimates would be biased downwards (and those of a upwards, the more-so in samples with higher average levels of permanent income). In cross section data there would be no scope for transitory fluctuations in income to be cancelled out at all and estimates of  $c''$  would again be biased downwards relative to the true parameter  $c'$ , the extent varying with the degree to which the incomes of those included in the cross section were subject to transitory fluctuations.

Thus did Friedman offer a seamless blending of microeconomic and statistical theory to resolve the empirical paradoxes that studies of consumption behaviour had revealed, and he proceeded to show that his explanation had exceptionally strong explanatory power over many of the detailed problems that they had uncovered too. The implications of all this for the standard Keynesian model of the 1950s were potentially devastating. Friedman’s permanent income hypothesis implied that Keynes’s marginal propensity to consume and therefore the multiplier were anything but stable, and therefore provided a shaky foundation indeed for any theory that sought to explain the behaviour of the macro-economy or purported to be a reliable guide to policy. The full destructiveness of this analysis was not at first widely understood, however.

To begin with, the particular method that Friedman chose to implement the idea of permanent income for empirical aggregate time series purposes, which were the ones that mattered for macro-economics and hence attracted most of the attention, considerably lessened the impact of his work. In this context he measured it as a geometrically declining weighted average of current

and past aggregate income (multiplied up by an adjustment factor to allow for the fact that such a technique shifted the mean of the series back in time, and hence, given economic growth, would understate its current value in a growing economy.)<sup>22</sup> Specifically, with  $b < 1$ , and ignoring this growth adjustment for simplicity, permanent income became

$$Y(p) = by + b(1-b)Y(-1) + b(1-b)(1-b)Y(-2) \dots \quad (7)$$

This was, at best, a rough empirical approximation to Friedman's basic theoretical concept, but, when used in the consumption function, it had the effect of preserving the stability of both a "long run" marginal propensity to consume -  $c$  - and a "short-run" one -  $bc$  -, and hence of long-run and short-run multipliers too, these being linked by the dynamics inherent in distributed lags. Hence, it distracted attention from the permanent income hypothesis' likely implications for the stability and reliability of the multiplier as a fulcrum for policy. Instead, it emphasised the dynamics with which this process worked out over time, hardly a novel, let alone disturbing, insight to econometricians already working on the quantification of Keynesian macroeconomics, who had by the late 1950s already learned a considerable amount about the usefulness of distributed lags when it came to fitting the data.

### *Money and the Rate of Interest*

A second and more fundamental factor also lessened the immediate impact of Friedman's work on the consumption function. To quote one of his own favourite aphorisms, "it takes a theory to beat a theory" and if systems built around equations 1 - 3 were to lose their dominant position in the mainstream of macroeconomics, something else had to replace them.

In 1936, Keynes had frequently contrasted his new theory with what he called *classical economics*, much to the benefit of the former, it should go without saying. The essential difference between the two systems, he insisted, was that, in his new theory, shifts in the level of investment created shifts in income and employment, so that prolonged depressions could be attributed to a chronic lack of investment opportunities. In what he presented as prevailing classical orthodoxy, on the other hand, such shifts would create variations in the rate of interest sufficient to ensure that investment would always stay at a level high enough to fill the gap between income and consumption - ie. saving - at full

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<sup>22</sup>The "adaptive expectations" idea that underlies this formulation was, as Philip Cagan (2000) has noted, picked up by Friedman in 1952 from conversations with A. W. Phillips. Under Friedman's influence, it had already been successfully deployed to proxy inflation expectations by Phillip Cagan in his (1956) study of hyper-inflations, and was being used by David Meiselman (1962) in a study of the role of interest rate expectations in determining their term structure. It also had the virtue of providing a "good fit" to US time series data on consumption. One can see easily enough why it attracted Friedman in this context.

employment. Keynes argued that this classical interest rate mechanism was flawed.

Classical economics as described by Keynes was a gross caricature. From the 1890s onwards, an increasing number of economists had argued that market economies seemed to have a hard time co-ordinating the allocation of resources over time - keeping saving and investment in equilibrium with one another at full employment - and by 1936 there already existed a large and complex literature that pointed to this failure as the source of real economic fluctuations and to the workings of the monetary system as the source of the trouble. However, that literature had achieved no consensus at all about just how these two factors might be linked. There was nothing original about Keynes' stress on the unreliability of inter-temporal co-ordination mechanisms that were supposed to work through the interest rate in a monetary economy. His specific explanation of why they might fail, however, was highly original, and lies at the heart of the *General Theory's* contribution to economics.

This explanation relied on the theory of *liquidity preference*, the very monetary complication that, by the 1950s, it had become customary to omit from elementary accounts of Keynes's macroeconomics, though it was, of course, included in more advanced expositions. This theory built upon what is nowadays known as the *Cambridge* version of the quantity theory of money, which had initially applied supply and demand analysis to the stock of nominal money in order to determine its purchasing power.<sup>23</sup> The central proposition underlying the Cambridge model was that any representative economic agent would have a well determined demand for a stock of *real* money - ie. money measured in units of constant purchasing power. In the writings of its originators, this demand was said to emanate from money's use as the economy's means of exchange and reflected what we would now call transactions and precautionary motives. They argued that this demand would usually represent a stable fraction of the money-holders' "resources", but they were routinely unclear as to whether this word referred to wealth, a stock, or income, a flow. Also, though they recognised that wealth not held as money could be held in other income-yielding forms - among Marshall's examples were "a horse" and "furniture" - the insight that the demand for money might be systematically related to some measure of the opportunity cost of holding it - a rate of interest on a representative financial asset, for example - eluded them. It was Keynes, in his *Treatise on Money* (1930) who finally brought clarity to these matters in a way that attracted widespread attention - though the priorities

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<sup>23</sup>The pioneers of the Cambridge approach - Alfred Marshall (eg. 1871) and Arthur C. Pigou (eg. 1917) - did not refer to their model *per se* as the quantity theory, but preferred to say that it yielded the same prediction of proportionality between the quantity of money and the price level as did that older model, which was explicitly based on the concept of the velocity of circulation.

of Frederick Lavington (1921) in sketching out the relevant ideas should be acknowledged.<sup>24</sup>

In the *Treatise* Keynes argued that the demand for money should be thought of as the outcome of a portfolio allocation decision, and that the relevant constraint here was wealth. Crucially, he also argued that the rate of interest paid on financial assets such as bonds represented an opportunity cost of holding stocks of money, particularly those whose demand derived not from transactions in markets for goods and services but from keeping options open in the face of the risks posed by financial market activities. He carried these ideas over into the *General Theory*, suggesting there that, in a monetary economy, the principal role of the rate of interest was not only to maintain equilibrium in the inter-temporal allocation of resources, but also, and mainly, to equilibrate the supply and demand for money, particularly that component of the latter which sprang from *speculative* motives associated with uncertainty about the future prices of financial assets, and hence about the future time path of the rate of interest itself. The rate of interest thus had too much work to do in a monetary economy, and could not be relied upon to keep saving and investment in equilibrium. Moreover, and crucially, since holding money always enabled agents to keep their options open, but holding bonds exposed them to the risk of making capital losses if the rate of interest rose, an eventuality whose likelihood increased when rates were at low levels, there would exist a floor below which the rate of interest could not fall. When investors' animal spirits were at a low ebb, this lowest attainable level for the interest rate might nevertheless be too high to induce a full employment level of investment.

This was the state of affairs that, in 1936, Keynes suggested prevailed in Britain and the US, and, because a rate of interest that was "stuck" at a low and more or less constant level could simply be dropped as a determinant of investment, his popularisers were later able to simplify his ideas into the type of system encapsulated in equations (1) - (3) above. The appropriateness of treating the rate of interest as stuck, however, depended in turn on the assumption that the demand for money was indeed so elastic with respect to the rate of interest that the monetary sector of Keynes's more elaborate system could safely be ignored, because that system could yield very different conclusions if this essentially empirical assumption was dropped.

As is well known, if instead of being totally autonomous, investment also depends on the rate of interest, equations (1) - (3) yield an IS relationship defining those combinations of the rate of interest and real income at which investment and saving are equal to one another. If we then follow Keynes

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<sup>24</sup>These developments are discussed in detail by Don Patinkin (1974) and Laidler (2004, ch. 13 [1980]).

himself in characterising the interaction of the supply and demand for real money balances ( $M/P$ ) in the following terms,

$$M_s/P = M_d/P = mY - l(r) \quad (8)$$

we have the LM relationship which defines combinations of these same two variables that equate liquidity preference (the demand for money) to the money supply. And as is also well known, the reduced form of the resulting IS-LM system approaches equation (4) above as the demand for money becomes more and more interest sensitive, but, as that sensitivity disappears, it instead approaches

$$PY = (1/m)M_s \quad (9)$$

which is simply a particular way of writing the traditional income velocity form of the quantity theory of money.

Thus, just what kind of message about the workings of the economy followed from the IS-LM model that economists had extracted from Keynes' work was seen to hinge in an important way on empirical propositions about how the supply and demand for money interacted with one another. That is why, while Friedman's work on the consumption function tended to undermine confidence in the stability of simple multiplier analysis and its policy applications, his essentially contemporaneous work on the demand for money function can be viewed as promoting this relationship as an alternative and more empirically stable fulcrum for the explanation of economic fluctuations and for the design of policies to deal with them.<sup>25</sup>

### *The Revival of the Quantity Theory and the Importance of Money*

By the mid-1950s, Keynes's theory of liquidity preference had already provided the starting point for a number of empirical studies that had seemed to establish that the demand for money was indeed interest-sensitive. Less directly, it also lay behind theoretical work on the transactions demand for money (Baumol 1952, and Tobin 1956), and on the demand for money as a financial asset (Tobin 1958).

Hence, when in (1956) Friedman proposed that the demand for money was fundamentally a demand for real balances, the outcome of a portfolio

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<sup>25</sup>There is no evidence of which I am aware that Friedman self-consciously thought along such lines in the 1950s, though he certainly did later - see (1974). Note also that his work on the demand for money has many other implications beyond the confines of IS - LM analysis, for example with regard to inflation and optimal money growth - see (1969).

allocation decision, and would vary with real income and a number of measures of the opportunity cost of holding it, his formulation of the relationship stood only a little apart from contemporary discussions of the topic on matters of substance, the most important difference here being his explicit claim that the relationship was empirically stable.<sup>26</sup> However, the title of the (1956) essay, “The quantity theory of money, a restatement”, and its publication as the introductory essay to a set of *Studies in the Quantity Theory of Money*, in and of themselves matters of style rather than substance, were calculated to be much more controversial. In the 1950s, if the quantity theory was discussed at all, it was as a meaningless tautology that had been part of the erroneous classical doctrine that Keynes had successfully overthrown.

Three of the four *studies* which the 1956 essay introduced dealt with episodes of high and even hyper-inflation, an unusual topic for the time, and they accorded a central role to the idea that the demand for money varied inversely with the opportunity cost of holding it created by inflation. It was, and remains, a well established stylised fact of high inflation that, as it gathers momentum, the price level tends to accelerate faster than the money stock, and this often was, and still sometime is, presented as evidence against monetary expansion being inflation’s main cause. But when money is non-interest bearing (or where interest rates paid on bank-money are low and rigid), the expected inflation rate represents an opportunity cost of holding it, even if official interference in financial markets prevents this being reflected in recorded interest rate data. In these conditions, provided inflation expectations track experience, the just-mentioned stylised fact can be reconciled with a purely monetary explanation of inflation.

It was a key message of the (1956) *Studies* that this proposition was well supported by empirical evidence - Nazi Germany (John J. Klein) being a counter-example - but it was also shown that the parameter values underlying the relevant dynamics were such that it would have been possible to bring the inflations studied under control by reducing the rate of monetary expansion.<sup>27</sup> Even so, (and with the exception of Richard Selden’s paper on the long-term

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<sup>26</sup>Among other differences, Friedman referred to money as a “temporary abode of purchasing power”, avoiding then usual distinctions among transactions, precautionary and speculative motives for holding it, and he did not discuss the “liquidity trap”, as economists had come to call that region of the by then standard intermediate textbook version of the function, where its interest elasticity approached infinity. In effect he treated real money balances “as if” a consumer durable good, thus forging an unusual link between his Fisherian theory of the consumption function and the Cambridge approach to the quantity theory. In 1956 the latter would have been seen as being only tenuously connected to Fisher’s own (1911) transactions velocity formulation.

<sup>27</sup>The monetary economics of inflation presented in the *Studies* nevertheless went beyond what was implicit in the orthodox macroeconomics of the 1950s, as represented by equation (10) above, which, so long as  $l$  is finite and with  $Y$  held constant, can be re-arranged to show that  $P$  is strictly proportional to  $M$ , but Martin Bailey (1962) and Robert Mundell (1963) would soon bring the Fisher effect into the orthodox model, making the nominal rate of interest respond to expected inflation, and close this gap.

monetary experience of the United States) the *Studies* dealt with rapid inflation in rather far-off times and exotic places, and few readers seemed to have thought that they had much to say about contemporary advanced economies, where the low but persistent inflation that was being experienced was usually attributed to institutionally driven cost-push forces rather than monetary factors.<sup>28</sup>

Be that as it may, the challenge to macro-economic orthodoxy implicit in Friedman's invocation of the quantity theory of money in 1956 was given added substance, and placed firmly in a US context too, with the appearance in 1959 of his "The demand for money: some theoretical and empirical results". This paper's main theoretical innovation was to affirm that the measure of income upon which the demand for money ought to depend was its permanent and not its current value, from which proposition there seemed to follow a startling empirical result, which Friedman developed using not conventional econometric techniques, but statistical methods well grounded in the NBER tradition. Treating each NBER-dated business cycle as a single observation, he ran the regression of real money holdings on real income, which he argued was essentially equal to permanent income when measured over a complete cycle. He then projected annual average money holdings by substituting into this equation estimates of annual permanent income obtained in his study of the consumption function, and showed that there seemed to remain no systematic within-cycle variations in the demand for money that could be attributed to variations in interest rates. Perhaps, he suggested, the strong evidence that others had found of an important interest sensitivity to the demand for money was the result of their having erroneously used current rather than permanent income in their regressions.

This study was followed up in short order by another - Friedman and Meiselman (1963) - which used more orthodox regression analysis to relate variations in nominal consumption (instead of income of which autonomous expenditure was itself a component) to variations in a measure of autonomous expenditure -  $(I + G)$  in equation (3) - and compare the outcome to that obtained when the money supply was used as an independent variable, as well as to estimate equations containing both variables. Its results seemed to show that, except in the 1930s, money dominated autonomous expenditure as an explanatory variable. Taken together, these two studies suggested that a demand

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<sup>28</sup>Though this was the view from the US and Europe, it was a different matter in Latin America, where the quantity theory as a theory of inflation played a critical role in the *Monetarist-Structuralist* debates that began in the mid-1950s. See Baer and Kershenetsky (eds.) (1963) for a representative collection of contributions to this debate. In general, Friedman's revived quantity theory found more immediate applications in less developed economies than at home. A subsequent collection of essays based on Chicago Ph.D. theses edited by David Meiselman (1970) contained studies of Chile (J. V. Deaver) Argentina (A. C. Diz), Post-War Japan (M. W. Keran), South Korea and Brazil (C. D. Campbell), as well as of Canada (G. Macesich) and a cross section of 47 countries, 26 of which were, however, located in either Asia and Latin America (M. Perlman).

for money function in which the rate of interest played no significant role could usefully replace the Keynesian consumption function as the crucially stable empirical relationship around which explanations of macro-economic instability could be constructed and policies to counteract it designed, that a theory which could beat the Keynesian model, already weakened by Friedman's work on the consumption function, had indeed been found.

These papers made a considerable impact in the early 1960s, but with the passage of time, both of them turned out to be somewhat flawed..<sup>29</sup> Their longer-term influence in any event pales in comparison with that of *A Monetary History of the United States*, which, though not published by the NBER until 1963, represented the fruits of a collaboration between Friedman and Anna J. Schwartz that had begun more than a decade earlier and had been influencing Friedman's monetary economics throughout the 1950s. The *Monetary History* was a work of quantitative, though not econometric, history, systematically tracing the causes and effects of variations of the quantity of money on the US economy since 1860, and it drew on an extremely large background literature dealing with specific historical episodes and/or issues, some produced by other NBER affiliates, and some by Friedman's Chicago graduate students. Not surprisingly, furthermore, its analysis revolved around the interaction of the supply of money with a demand function very like that postulated in Friedman's 1956 and 1959 papers, although no explicit model expounding the details of these mechanisms was set out.

The story that the *Monetary History* documented, on a cycle by cycle basis and in considerable detail, was that variations in the rate of growth of the money supply seemed systematically to lead the cycle, and in all probability to play a significant role in causing it too. The evidence was stronger for some cycles than for others to be sure, and often showed strong feed-back effects from economic activity to money, but overall the picture seemed to be clear. This was particularly the case for the so-called the *Great Contraction* of 1929-33, the very episode when, according to the conventional wisdom prevalent in the 1950s, market mechanisms had most clearly failed, and the weakness of monetary policy had most vividly been demonstrated. Quite to the contrary, Friedman and Schwartz claimed that a typical cyclical downswing had started in late summer of 1929, but had been first allowed to get out of hand, and then exacerbated, by Federal Reserve policy. This had permitted the money supply to collapse amid banking crises that a sufficiently vigorous response on its part, by way of lender of last resort activities and large scale open market operations, could have

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<sup>29</sup> Even in cycle average data, it was possible to find a role for an interest rate effect on the demand for money, so Friedman's implicit assumption that, if such a relationship existed, it would be solely a cyclical phenomenon was empirically wrong (Friedman, 1966, Laidler 1966). Friedman and Meiselman's results were in due course shown to be very sensitive to their particular way of distinguishing autonomous components of national income from the rest. (Ando and Modigliani 1965, DePrano and Mayer 1965).



prevented. Rightly or not, and that is not the point here, many more readers were eventually to be convinced by this narrative than by, say, the Friedman-Meiselman study, that Friedman's message about the importance of money had to be taken seriously.

It is important to grasp just how deep that message went in undermining the Keynesian consensus described at the outset of this essay. That it seemed to favour a version of the IS-LM model that downgraded the macroeconomic significance of real shocks and fiscal policy and attached increased importance to money was evident enough from the outset, of course, and this generated considerable controversy in its own right. But Friedman's message also contradicted the view, so much taken for granted in the early 1960s that it was rarely debated, that a modern monetary economy is fundamentally incapable of effectively allocating resources over time - of co-ordinating savings and investment while maintaining full employment - so that active and continuous government intervention is required to ensure its stability. This implication of his work sank in only slowly, though it would eventually come to leave deep marks on both academic economics and economic policy from the 1970s onwards.<sup>30</sup> At considerable risk of oversimplification, it perhaps required Axel Leijonhufvud's (1968) success in making economists once again self-conscious about Keynes' vision of the flaws inherent the monetary economy's co-ordination mechanisms to enable them to appreciate the full extent of Friedman's challenge to received orthodoxy.

As Robert Clower stressed in his (1964) review of the *Monetary History*, Friedman's analytic methods were Marshallian, and in this respect his macroeconomics bore a close affinity to that of Keynes. Each sought to construct a simple macro-economic framework around a single empirically stable relationship, and one within which the economy's responses to shocks would could be analysed as evolving over time as the constraints imposed by various short-run rigidities were relaxed. Nevertheless, Friedman's specific framework seemed to support a vision of the monetary economy's workings essentially identical to that of those Austrian economists who, using a theory of real economic fluctuations based explicitly on Walrasian foundations, had been Keynes's principal rivals in the 1930s in the competition to shape the then emerging sub-discipline of macroeconomics.<sup>31</sup> For Friedman, every bit as much

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<sup>30</sup>I am grateful to Susan Howson for making me pay attention to the relative slowness with which the full implications of Friedman's work on money made themselves felt.

<sup>31</sup>As he made clear in (1953a), Friedman regarded the essential difference between Marshallian and Walrasian methods as lying not in the distinction between partial and general equilibrium analysis, but in that between economic theory used to formulate refutable hypotheses and hence empirically useful, and economic theory constructed so as to encompass all logically possible outcomes, and hence empirically vacuous. His (1949) interpretation of "The Marshallian Demand Curve" made a persuasive case for treating Marshall as a general equilibrium theorist. I am grateful to Allan Hynes for discussion of this point.

as for von Mises or von Hayek, markets were stable and capable of dealing efficiently with allocative challenges. If they failed to meet them, that was not because they were inherently flawed, but because misconceived monetary policies had been visited upon them. Friedman's views on just what constituted misconceived policies, and how they inflicted harm, differed considerably from those of the Austrians, but he agreed with them wholeheartedly that activist policies, far from being needed to stabilize the market economy, were the principal source of its instability. Unlike them, however, he derived these conclusions from careful empirical analysis of competing models, rather than from any set of first analytic principles.<sup>32</sup>

### *Monetary Policy for a Dynamic Economy*

The full extent of Friedman's contribution to macroeconomics cannot be grasped within the confines of IS-LM analysis, and the dominance of this framework in the 1950s and 1960s is one important reason why his ideas came to be appreciated only slowly, and with much debate. In particular, as Backhouse and Laidler (2004) have argued, the IS-LM model, being a comparative static construct, helped to create an intellectual climate in which, for a while, the central fact that economic activity happens in time became obscured.

This had certainly not been Keynes's intention. On the contrary, his stress on animal spirits as determining investment, and hence the level of economic activity, was his response to an acute awareness that investment decisions were inherently forward looking, and to a conviction that expectations about the economic future were subject to fundamental uncertainty that could not be bypassed by resort to the calculus of probabilities. But his solution to the analytic difficulties inherent in this viewpoint had been to treat long-term expectations as exogenous factors that shifted what, in the hands of his successors, became a static IS curve, and in due course, the importance that Keynes himself had attached to time was pushed into the background. Furthermore, when it came to matters of policy, Keynes had shared a blind-spot with many of his contemporaries. Though he stressed that private agents could not be expected to make rational forward-looking decisions, nor markets to coordinate them, he envisaged no such limitations on the wisdom of policy makers. Nor did his popularisers, and in the simple model which they had

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<sup>32</sup>Nicholas Kaldor (1970) was early among Friedman's critics in noticing an affinity between his work and that of the Austrians. However, there were and are important differences here too. For example, Hayek's views on the dangers of fighting depression with "a little inflation" (see, eg, 1936, p. 125) are sometimes read as having anticipated Friedman's (1967) accelerationist doctrine, discussed below. However, Hayek was discussing contemporary policy issues, and he was referring to inflation of the money-supply, not of the price level. Hence his remark stands in flat contradiction to Friedman's later conclusion that monetary expansion was exactly what was needed to come to grips with the contraction of 1929-33.

extracted from the *General Theory*, fiscal stabilisation policy looked to be an easy business.

Perhaps because of his considerable exposure to the NBER tradition of business cycle analysis, Friedman seems from a very early stage to have thought about policy problems in the context of a dynamic world where knowledge of even the near term future course of events was scarce. There was, therefore, much more to his dissent from contemporary policy doctrine than a desire to substitute monetary for fiscal measures. He also insisted that monetary shocks impinged upon the economy with time lags that are (now famously) *long and variable*, and that this fact both required policy to be forward looking and exposed it to serious risk of error into the bargain, for the simple reason that his policy makers were much less well informed than Keynes's.

In an IS-LM model in which the demand for money is more or less independent of the rate of interest, fluctuations in Keynesian animal spirits do not affect income and employment at all, and a constant money supply guarantees real stability, but such analysis does not do justice to Friedman's policy vision. For him, there was ample elasticity in the system to permit real shocks to have consequences, either because the economy could function temporarily with less money in circulation than was demanded, or because that demand depended in any case on permanent rather than current income. In such circumstances offsetting policy measures could in principle improve matters, but in practice they risked making them worse, as he showed, for example, in (1953b)<sup>33</sup> By the time they began to take effect, they could be end up not stabilising, but further destabilising an economy that was already responding to new shocks. For Friedman, then, the principal problem in the implementation of policy was not to create institutions that would facilitate the rapid and continuous discretionary deployment and withdrawal of economic stimulus - *fine tuning* as such measures were called in the 1960s without a trace of irony - but that would impose constraints to prevent policy makers over-reaching themselves. Already in (1948), he had, in this spirit, proposed a system that would exploit the built-in stabilising effects of having the counter-cyclical deficits generated by the interaction of stable government expenditure and progressive income taxation automatically funded by money creation, but as his work progressed and he became convinced of the inherent stability of an economy that was not subject to monetary shocks, his attention shifted to devising an institutional framework that would prevent their occurrence.

The *Program for Monetary Stability* that Friedman laid out in (1960) amounted to no more, and no less, than the imposition on the Federal Reserve

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<sup>33</sup>This paper provides another example of Friedman's pioneering use of statistical theory in the analysis of a problem in economics.

system of a legally binding requirement that the money supply grow at a constant percentage rate on a month by month basis. It thus involved a *rule* for monetary policy, in two distinct senses. First, the behaviour of the money supply was to be *systematic* as opposed to *arbitrary*, and second, such behaviour was to be achieved not by *persuading* the central bank to *choose* it, but by *constraining* it by law to do so, by taking away from it the *discretion* to do otherwise.

In the first, less important sense, Friedman's money growth rule involved the deployment of monetary policy as a built-in stabiliser, and was defended on the quite practical economic grounds that, given the state of macroeconomic knowledge at the time, this would maximise monetary policy's contribution to the stability of the economy. In the second, it invoked the political principal that decisions impinging upon the stability of the monetary system, and hence of the market economy, were too important to be delegated to unaccountable functionaries, and were proper objects for legislated, or even constitutional constraints. Here it will suffice to recall Sir Robert Peel's 1844 Bank Charter Act, Irving Fisher's tireless efforts in the 1920s to persuade Congress to legislate the Federal Reserve system into the single minded pursuit of price level stability, and Henry Simons' celebrated advocacy of similar measures in (1936), to demonstrate how deeply embedded was Friedman's proposal in the economic agenda associated with what used to be called *classic liberalism*.<sup>34</sup> The empirical foundations of this doctrine seemed to have been thoroughly undermined by the interpretation of the experience of the 1930s as clear evidence of a fundamental flaw in the workings of a monetary economy, but by 1960 Friedman's positive work was well on its way to restoring these foundations as we have seen, and hence was in close accord with his policy proposals.

The analysis of the difficulties created for stabilization policy by the simple fact that it must be implemented over time that most closely parallels Friedman's was carried out by A. W. H. (Bill) Phillips, and there seem to have been important intellectual interactions between the two during Friedman's stay in Britain in 1952-53. Phillips name would in due course become closely associated with the idea of a stable inflation-unemployment trade-off that lay at the very heart of 1960s analysis of optimal fine tuning, but this particular application of the Phillips Curve was the work of others. For its originator, the curve's purpose was to forge a smooth analytic link between variations in output and the inflation rate in a dynamic model constructed at a time (1954) when most systems dealt with one variable or the other, but not both simultaneously. But Phillips did investigate the curve's empirical content (1958), discovered that it seemed to have some, and its adoption as a supplement to orthodox IS-LM

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<sup>34</sup>The direct influence here on Friedman was Simons, as he himself acknowledged in (1967)

style macroeconomics duly followed, whether he liked it or not, (and by and large, he did not).

For Friedman, already sceptical about the possibilities of fine tuning, claims about the theoretical and empirical robustness of the policy trade-off implicit in the Phillips curve were problematic. They seemed inconsistent with everything else that he thought he knew about how economies functioned, but it was one thing to be aware of this tension and quite another to explain precisely where the problem lay. It was the latter task that Friedman undertook in his 1967 Presidential address to the American Economic Association (Friedman 1968).

Phillips himself had said next to nothing about the theoretical basis for his hypothesis that the rate of change of money wages would vary systematically with unemployment, but Richard Lipsey's (1960) careful elaboration of the few hints he had given soon became widely accepted. First, frictional and structural factors ensured that the supply and demand for labour would be equal to one another at a positive unemployment rate, and when they were, money wages would be stable. Second, any deviation of unemployment from that same level was a sign of an excess demand (or supply) for labour, and money-wage-change was a response to it. The Phillips curve, in short, was simply the result of applying ordinary supply and demand analysis to a particular market.

Not so, was Friedman's response. The price that cleared the labour market was the *real* wage, not the *money* wage, and excess demand there should generate real, not money, wage changes. Agents bargained over money wages of course, so excess demand would initially cause money wages to rise, but this would soon feed through to the behaviour of prices and engender inflation expectations that would feed back into money wage bargaining. If excess demand was then held constant by policy, the inflation rate would have to rise, setting in motion a never ending upward spiral. Therefore, any efforts on the part of policy-makers to buy, at the price of a little inflation, an unemployment rate permanently lower than the *natural* - structural and frictional - properties of the labour market could deliver, were doomed to collapse in the face of a perpetually accelerating price level. Long before 1968, Friedman had argued that monetary policy should provide a background of macroeconomic stability against which agents could then pursue their various private interests, but now he was able to be much more precise about just what was involved here. The *only* macroeconomic variable that monetary policy could influence on anything other than a purely temporary basis, was the inflation rate, and Friedman had earlier argued, in company with Phillips, be it recalled, that short-run fine

tuning was too difficult to be practicable. “The role of monetary policy”, then, was to stabilise inflation in the medium term, and that was all.<sup>35</sup>

### *The Transmission Mechanism and its Missing Equation*

In the early 1960s, many economists expressed doubts about the transmission mechanism that underlay Friedman’s claims for the importance of money, even though rather elaborate accounts of it were to be found in Friedman and Meiselman (1963) and Friedman and Schwartz (1963b).<sup>36</sup>

These accounts began from the idea that, given that their initial levels were appropriate, a moving equilibrium between the supply and demand for money would be sustained so long as nominal money growth kept pace with any expansion in the demand for money generated by the combined effects of real income growth and inflation. They then noted that any deviation of money growth from this equilibrium path would cause the build up of an excess (or deficient) stock of money in the economy, and that private sector agents would respond to this stock disequilibrium by increasing (decreasing) their flows of expenditures across a wide range of margins - money and consumer goods including durables, money and investment goods, and money and financial assets, where the consequences for interest rates would induce further changes in flows of expenditure on goods and services. Increased expenditure in turn would put upward pressure on both output and prices relative to their initial time paths, and tend to eliminate the initial stock disequilibrium that had set the whole process in motion. However, such adjustments would be drawn out over time, and the movements in money income that they involved could well feed back to the banking system and induce further responses in the time path of the money supply, and therefore of expenditure flows, in a recursive process. The dynamics involved might also result in equilibrium being overshoot, thus inducing a

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<sup>35</sup>In this, Friedman’s views on the policy implications of what came to called *the expectations augmented Phillips curve*, differed sharply from those of Edmund Phelps (1967), who had worked out the same analytics as Friedman at about the same time. Phelps treated this new relationship not as presenting an obstacle to the fine tuning of an inflation-unemployment trade off, but simply as introducing a dynamic element into the optimization problem that policy makers faced. In the late 1960s, when some empirical evidence still seemed to suggest that the practical implication of the Friedman-Phelps curve was that the inflation unemployment trade-off became steeper in the long run than in the short, but did not vanish, Phelps’ views received at least as much attention as Friedman’s, but as the 1970s unfolded, Friedman’s position would become the dominant one.

<sup>36</sup>It should be explicitly noted that Friedman’s was not the only account available of the transmission mechanism of monetary policy at that time. In his highly influential (1962) “Survey of monetary theory and policy” Harry Johnson noted that a number of economists were developing such ideas, and quoted extensively from Karl Brunner (1961) to illustrate their basic nature. Brunner, usually working with his long-time collaborator Allan Meltzer, made important contributions to the subsequent development of monetary economics, often (but not always) working along lines parallel to those followed by Friedman. Brunner and Meltzer (1993) provides a comprehensive account of this contribution, while (1974) highlights the relationship between their work and Friedman’s.

cyclical element into the economy's response to any initial monetary disturbance.

Friedman and Schwartz documented the workings of this mechanism, which bears a strong family resemblance to that postulated even before the Great Depression by Irving Fisher (eg. 1911, ch. 4) and Ralph Hawtrey (eg. 1913), on a cycle by cycle basis in the *Monetary History* (1963a) and showed that its precise operations in any particular episode depended on the extent to which the economy was simultaneously being affected by other shocks, not mention the nature of the monetary policy regime that was in place, which would determine the nature and extent of the money supply's endogenous responses to swings in real income and inflation. Nevertheless their work also seemed to confirm that a hard core of common elements were always present, three of which were of particular importance. First money was substitutable across a wide range of margins, a characteristic that followed from Friedman's conception of it as a "temporary abode of purchasing power" whose demand was determined like that for any other service-yielding durable good. This implied that monetary policy's transmission mechanism was not just a matter of interest rate effects in organised financial markets. Second, the stock-flow interactions that lay at the transmission mechanism's heart were inherently dynamic and drawn out over time. Third, though it thus seemed possible to tell a coherent story about how and why variations in money growth induced changes in the time path of nominal income, the factors determining precisely how those changes were divided up between real income and prices in any particular episode remained elusive beyond a general tendency for variations in output and employment to get under way before price level effects came through.

There thus seemed, as Friedman put it, to be one equation missing from his system; and at this point the development of his macroeconomics presents a major puzzle. Initially, not just Friedman, but all macro-economists had faced a *missing equation* problem, but, as has already been noted, by the early 1960s, the curve, created by Phillips in 1954 to fill just this gap in his own model of stabilization policy, was being adopted by many others for the same purpose. Though it is easy to understand why Friedman, being suspicious of the original Phillips curve's validity, did not initially follow this trend, it is much harder to understand why, when he gave a largely retrospective account of his *Monetary Framework* in (1974), he presented the missing equation as a still current problem. By then, the expectations augmented version of the curve which he himself had developed in (1968) was already being taken up by others to deal with it.

Perhaps the solution to this puzzle is to be found in the fact that two mutually inconsistent sketches of the theoretical underpinnings of the curve in question were in fact present in the (1968) paper, and that Friedman was at least

partially aware of the problems this posed for his work. The first of these, set out as Friedman criticised Phillips for having chosen money wage change instead of real wage change as the dependent variable of his relationship, followed Phillips in treating variations in measured unemployment as a proxy measure of the state of excess demand or supply in the labour market. The second, which came a little later in the paper, treated them as manifestations of supply side responses by members of a labour force who had mistaken local money wage changes, that were in fact part of economy-wide inflationary adjustments, for real wage changes. The first theoretical sketch treated the Phillips curve as a manifestation of a market *adjusting to disequilibrium*, and the second as the consequence of it remaining *in equilibrium* under conditions of less than perfect information. To see precisely what is involved here, it is convenient to consider the Phillips curve in its price-inflation-output rather than its wage-inflation-unemployment form, (though that is not how Friedman himself discussed the matter).

With all variables in logarithms,  $p = P - P(-1)$ ,  $p(e) = P - P(e)(-1)$  and  $y = Y - Y^*$ , where  $Y^*$  is “full-employment output”, or equivalently that associated with Friedman’s “natural unemployment rate”, the original curve, essentially as proposed by Phillips in 1954, may be written

$$p = gy \quad (10)$$

Once inflation expectations come into the picture, this becomes

$$p = gy + p(e) \quad (11)$$

which is equivalent to the first of Friedman’s two formulations of it in wage-change-unemployment space. To get to the second, we start from a conventional aggregate supply curve

$$y = h(P - P_e) \quad (12)$$

which rearranged, and with  $P(-1)$  subtracted from both sides, becomes

$$p = (1/h)y + p_e \quad (11')$$

Equations 11 and 11' are observationally equivalent as written here, but they nevertheless embody fundamentally different visions of the economy’s workings. The individual behaviour underlying 11 is not well defined, but seems to require an ability on the part of some agents, presumably sellers, to adjust prices in response to some kind of quantity signals emanating from the markets in which they operate. Its microeconomic foundations must therefore be sought in the theory of monopolistic competition. 11' on the other hand is transparently grounded in the theory of the price taking perfectly competitive agents operating



in a continuously clearing Walrasian market, and has obvious attractions for anyone who has come to macro-economics by way of micro-theory, let alone someone who, like Friedman (see 1953a), had been a leading champion of the perfectly competitive model in the face of the challenges mounted to it by Edward Chamberlin (1933) and his supporters.<sup>37</sup>

It is perhaps not surprising then, that, when Friedman revisited his (1968) critique of Phillips in (1975), he repeated the charge that Phillips had discussed money wages instead of real wages, but now added that Phillips had also gotten the direction of causation underlying his curve wrong: he had quantities affecting prices - unemployment causing wage changes - instead of prices affecting quantities - wage behaviour causing variations in unemployment. By 1975, that is to say, Friedman seems to have recognised the tensions between the two theories of the expectations augmented Phillips curve that uneasily co-existed in his Presidential address, and to have opted for the second of them. Certainly it is this second rationalisation that is to be found in the *New Palgrave* version (1987) of his major essay on the Quantity Theory of Money. Even so, Friedman seems never to have adopted the rational expectations hypothesis that usually complements the Phillips-curve-as-aggregate-supply-curve in New classical economics. Though respectful of this idea's theoretical usefulness, he did not find it "the open sesame to unravelling the riddle of dynamic changes that some of its more enthusiastic proponents make it out to be." (Friedman and Schwartz 1981, p.630)<sup>38</sup>

But in opting for the aggregate-supply-curve interpretation of his own expectations-augmented Phillips curve even without rational expectations, Friedman nevertheless rendered it incapable of filling the role of the missing equation in his account of monetary policy's transmission mechanism. That account was designed to deal with the stylized facts about the interactions of money and other variables over the course of the cycle, and prominent among these was, and still is, a systematic lead of output and employment over inflation. Quantities cannot simultaneously lead prices and be interpreted as responding to them. Thus, one equation would always remain missing from Friedman's model of the transmission mechanism, as he affirmed as late as (1992, p. 49).

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<sup>37</sup>In 1953, however, Friedman treated perfect competition as a Marshallian tool, useful for empirical analysis, and classified monopolistic competition as empirically vacuous and therefore Walrasian. At that time the awkward question of who sets prices in an economy in which everyone is a price-taker was not on the agenda, and the necessity of postulating an "as if" auctioneer who continuously keeps prices at their market-clearing values as a necessary component of the microeconomic foundations of the perfectly competitive model was not understood.

<sup>38</sup>I am grateful to Allan Hynes for discussion of this point

## *Friedman's Influence on Macro-policy and Macro-theory*

The intended audiences for the two papers on the expectations augmented Phillips curve discussed above were very different. The first (1968) was aimed at Friedman's professional peers, and the second (1975) at an interested lay-audience, in the UK as it happened. This is not accidental, because the years that separate their appearance saw Friedman systematically paying less attention to pure academic research directed at his professional colleagues, and devoting more time to defending and popularising the ideas he had already developed. The dividing line here was not sharp nor was the transition abrupt, but they are nevertheless evident.

The (1969) collection of essays, *The Optimum Quantity of Money*, contained only one new paper - its title essay - albeit a lengthy one, that developed the case for a fiat monetary system's aiming at a stable rate of *deflation*, equal in absolute value to the economy's real rate of interest, so as to maximise the productivity of the economy's stock of real balances. This essay is still much cited, but it was only one of a number of papers on this and related issues published at about the same time, and its survival where others of comparable quality, for example Johnson (1969), have faded from view perhaps owes as much to its author's overall reputation, as to anything unique in its content. Friedman's already cited contributions to Robert Gordon's (1974) edited volume *Milton Friedman's Monetary Framework*, were also aimed at his fellow economists, but their intention was to summarise previously expounded ideas and defend them, not to break new ground. *Monetary Trends in the United States and the United Kingdom* (Friedman and Schwartz 1981) was a substantial work by any measure, but this, the final product of the monetary history project that had started in the early 1950s, had originally been intended to deal with trends *and cycles*, and had been in a more or less complete first draft form as early as 1969.<sup>39</sup>

Meanwhile, Friedman was becoming increasingly visible and respected as a commentator on economic policy, sometimes writing alone - notably as a *Newsweek* columnist - and sometimes with his wife Rose Director Friedman.<sup>40</sup>

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<sup>39</sup>This long delay in publication did much to reduce this book's impact, not least because of the explosive development of econometric techniques in the 1970s made much of its quantitative work seem obsolete. David Hendry and Neil Ericsson (1983, 1991) were particularly uncharitable in their treatment of it, strangely so, since their substantive conclusions, that there did exist a stable long-run demand for money function, which nevertheless had been subject to some unexplained structural shifts during the twentieth century, were essentially identical to those of Friedman and Schwartz.

<sup>40</sup>Their collaboration on popularising the policy implications of their economics had begun as early as 1962 with the initially under-appreciated *Capitalism and Freedom*, and would reach its high point in 1980 with *Free to Choose*, itself the by-product of an immensely successful television series. As the titles of these books indicate, Friedman's engagement in policy debates ranged well beyond the boundaries of macroeconomics. So did his influence, and his reputation among the public at large rests not just on his ideas about monetary policy, but on

It is beyond the scope of this essay to weigh the merits of Friedman's broader policy contributions, but his influence over macroeconomic policy from the 1970s onwards does require attention here. At the very beginning of that decade Harry Johnson (1971) had noted that Keynesian economics was at its strongest in dealing with the problems posed by unemployment, while Friedman's approach, by then known as *monetarism*, had the advantage when it came to inflation.<sup>41</sup> Because unemployment was the more serious policy issue, Johnson went on, Friedman's critique of Keynesian orthodoxy was likely to prove ultimately ineffective. Even as he wrote, however, inflation was fast becoming the main macroeconomic policy problem facing the US and those countries linked to it through the Bretton Woods system, and it would soon generate that ugly phenomenon with the equally ugly name, *stagflation*.

As was noted earlier, the conventional wisdom prevailing at this time was that inflation in advanced economies was not a monetary phenomenon at all, but a matter of institutionally driven cost push forces, and the simultaneous occurrence of rising inflation and rising unemployment gave superficial plausibility to this explanation. That plausibility did not, however, survive the abject and extremely visible failure in the early 1970s of wage-price controls unsupported by monetary measures in such economies as the US and the UK, and it soon became apparent that Friedman's monetary explanation of inflation, supplemented by his expectations-augmented Phillips curve, seemed to fit the stagflationary facts rather well. The scene was thus set to try out a monetarist policy program, and from the mid-1970s until the early 1980s, with the precise dates varying from country to country, the rate of growth of one or more monetary aggregate became the lynchpin of anti-inflation policy in many places. Of course Friedman was not the only advocate of such policies, but it is hard to imagine that they would have become so widely popular so quickly without his specific influence, so it is not surprising that, when they went wrong, he was expected to shoulder a good deal of the blame.

At the serious risk of over-generalising, monetarist policies everywhere foundered on the same factors. To begin with, as implemented from the mid-1970s onwards, these policies were only distantly related to Friedman's (1960) *Program*. This had proposed a legally binding money-growth rule in order to maintain low inflation in an already stable monetary environment, but actual

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his having been, along with Friedrich von Hayek, one of the important brains behind that famous composite politician "Ronald Thatcher".

<sup>41</sup>Johnson was clearly unaware that Friedman's early work on the economics of inflation (eg. Friedman, 1943) was in fact heavily influenced by Keynes (1940), albeit indirectly through the 1941 UK budget. Friedman in turn seems not to have known of Keynes's role in developing the concept of the "inflationary gap" that informed that budget, and hence of Keynes's influence on his own subsequent thinking about the economics of money and inflation.

policy tried to use discretionary control over money growth to restore stability in economies where inflation was uncomfortably high and still rising. Second, where Friedman had advocated that the monetary base be used as the policy instrument whereby money growth was controlled, actual policy relied on the manipulation of an interest rate. Central banks in effect used a model of the generation of the money supply in which the latter variable was posited to *adjust passively* to the behaviour of real income and prices in order to implement a policy derived from a model whose key postulate was that the money supply *actively drove* their behaviour. Friedman is hardly to be blamed for policy failures attributable to these causes. Nor should he be expected to bear all of the responsibility for the frequency with which the quarterly and even monthly demand-for-money equations that underpinned the abovementioned policies displayed instability. In (1959) he had claimed empirical stability for a function fitted to business cycle average data, and in *Monetary Trends* Friedman and Anna Schwartz had dis-aggregated their data only to cycle phases. Short-run demand for money functions are tools of monetary fine-tuning, which Friedman had never advocated.<sup>42</sup>

He (and everyone else writing on the demand for money before the mid-1970s) were, however, more culpable in having failed to grasp the impact that institutional change within the financial sector could have on the economic significance of any specific monetary aggregate, including that (or those) chosen as the fulcrum of monetary policy. Some of this change in the 1970s was the result of regulatory interference with banking practices on the part of authorities who were simultaneously anxious to hit money-growth targets and to avoid some of the consequences of doing so (Goodhart's Law), some was the consequence of the private sector's reaction to the adoption of new policies (the Lucas critique), and some was caused by the application to banking of then recent developments in computing technology; but added together, these factors undermined not just short-run relationships but even the longer term stability of the demand for money functions on which policy was beginning to rely. Such instability in earlier times is evident in the historical results presented in *Monetary Trends* (1981) but there is no trace of Friedman, or anyone else, having recognised this problem early enough for such information to have been useful to the conduct of money growth targeting.

Furthermore, Friedman's very public (1984) prediction that the rapid money growth the US was then experiencing would soon lead to a resurgence of

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<sup>42</sup>If any single economist is to be singled out as having based a case for fine tuning monetary policies on the existence of a stable demand for money function, it is probably Franco Modigliani (1977). However, this attribution of responsibility, along with its accompanying relative absolution of Friedman, is informed by a good deal of hindsight. The distinctions between long and short-run stability of the demand for money function on which it rests were certainly made in the 1970s, but their crucial significance for policy was barely recognised, certainly not by this author.

serious inflation did further harm to the reputation of his policy doctrines. This prediction should not have been made, of course, because the rapid fall in the inflationary opportunity cost of holding money that was in motion in the early 1980s was creating ample room for the demand for money to absorb ongoing money growth, and indeed, in its absence, recovery from the steep recession with which that decade had begun might well have been aborted. The prediction was made, however, and its failure, which in fact provided further strong evidence in favour of Friedman's monetary model of inflation, was, quite wrongly, widely regarded as discrediting it.

In any event, the 1980s would see Friedman quietly abandon his advocacy of a constant money-growth rule, and begin to move towards supporting a version of *free banking*, whereby the monetary base would be more or less fixed, and competitive forces in the banking system would thereafter be relied on to determine the money supply. Whether that base would have to be anchored in commodity such as gold in order to ensure its stable behaviour, or whether the declining attractions to government of inflation as a source of revenue would render a fiat base more stable in the future than it had in the past, was a question he still left open in (1992 Ch. 10).

The story of money-growth targeting and its aftermath seems at first sight to suggest that Friedman's influence on macroeconomic policy was short-lived and even superficial, but nothing could be further from the truth. Even in the mid-1970s, let alone the 1950s when he first began to develop them, his claims that inflation is always and everywhere a monetary phenomenon, that monetary policy was ill-suited for short-term stabilization of real variables, and that its only role could be to control and then stabilise inflation, were very much minority viewpoints. By the 1990s they had become commonplaces. If money growth targeting proved inadequate as a specific means of implementing monetary policy, inflation was nevertheless brought under control by monetary measures beginning in the early 1980s, and when, a decade later, that task had been completed, monetary policy in many places found an anchor in medium term inflation targeting of various degrees of formality, while labour market reforms had become the instruments of choice among mainstream economists for dealing with unemployment. As to fiscal policy, the longer term consequences of deficits and debt, particularly as they threatened to impinge upon monetary stability, pushed its potential as a short term stabilization device far into the back-ground.

In short, and to return to themes raised at the very outset of this essay: policy in most places has now been dominated for a decade or more by the preconception that gross macroeconomic instability is more likely to be the result of policy-induced monetary shocks than of any fundamental flaw in the workings of a monetary economy; monetary policy has displaced fiscal policy at

the centre of things; and a medium term nominal objective, usually a quantitative target for inflation, and sometimes a legally binding one at that, has replaced short-term output and employment goals. There is not much to be seen here of the macro-economic orthodoxy that was so deeply entrenched in the textbooks of the 1950s, but there is a great deal that bears more than a passing resemblance to the alternative, then apparently outlandish, view-point that Friedman began to develop at that time. And, it is worth noting, macroeconomic policy has done well since the early 1990s both in delivering low inflation, but also in maintaining a high degree of real stability too. Friedman deserves a significant share of the credit for all this.

Friedman's influence on macro-economic theory has also been profound. The microeconomics that underlay his (1956) demand for money equation derived from his work on the consumption function, which, as we have seen, he modelled as the outcome of explicitly forward looking maximising behaviour in a stochastic environment. The idea of forward looking maximisation derived, as Friedman acknowledged, from Irving Fisher, but the combined influence of Keynes's self-conscious rejection of this approach in the light of his ideas about fundamental uncertainty, of the subsequent disappearance of virtually all questions posed by the passage of time from the simplified macroeconomic model that others extracted from the *General Theory*, and of the lack of a technical apparatus within which this approach could be exploited, had led to its becoming almost lost by the 1950s. Friedman, not co-incidentally a micro-theorist and mathematical statistician before he was a macro-economist, revived this idea in 1957, placed in the stochastic framework used by econometricians, and redirected macro-economics towards what is nowadays regarded as the only sound way to proceed.

It is not clear that contemporary macroeconomic theorists are conscious of the fact that, before Lucas, Sargent et al. showed them how to make progress with their work, there had been Friedman. They do not often cite *The Theory of the Consumption Function*, but it is arguable that, in the case of forward looking maximisation in a stochastic environment, we have an idea which has become so deeply embedded in the subject, and so taken for granted, that the need to cite even its recent origins has simply disappeared. And in any event, perhaps Friedman might not want to take credit for much of modern macroeconomics. Currently available technology, along with the role played by the rational expectations hypothesis, which, as we have seen, he stopped short of fully accepting, seems to dictate that Walrasian micro-foundations are required fully to exploit the idea of forward looking maximisation when the economy as a whole is analysed.

Friedman's unwillingness to give up his Marshallian vision of how monetary factors impinge upon economic life when, as a matter of simple logic,

his adoption of the aggregate supply curve interpretation of the expectations-augmented Phillips curve seemed to require it, has already been noted, as it has that Keynes too was a thoroughgoing Marshallian. Though any verdict on Friedman's place in the development of macroeconomics must acknowledge his importance in undermining Keynes's influence on policy, it must also note that, with his 1967 development of the Phillips curve, the acknowledged inspiration for Robert E. Lucas's (1972) "money supply surprise" model of the cycle, he also, perhaps inadvertently, set in motion the demise of the Marshallian tradition in macro-economic theory of which both he and Keynes were such distinguished exponents. How important this second factor will ultimately prove to be, must await the further development of the sub-discipline, but viewed from a present-day perspective, the irony implicit in it is striking.

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**THE PRICE LEVEL, RELATIVE PRICES, AND ECONOMIC  
STABILITY:  
ASPECTS OF THE INTER-WAR DEBATE\***

**by**

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**Abstract:** Recent financial instability has called into question the sufficiency of low inflation as a goal for monetary policy. This paper discusses inter-war literature bearing on this question. It begins with theories of the cycle based on the quantity theory, and their policy prescription of price stability supported by lender of last resort activities in the event of crises, arguing that their neglect of fluctuations in investment was a weakness. Other approaches are then taken up, particularly Austrian theory which stressed the banking system's capacity to generate relative price distortions and forced saving. This theory was discredited by its association with nihilistic policy prescriptions during the Great Depression. Nevertheless, its core insights were worthwhile, and also played an important part in Robertson's more eclectic account of the cycle. The latter, however, yielded activist policy prescriptions of a sort that were discredited in the post-war period. Whether these now need re-examination, or whether a low inflation regime, in which the authorities stand ready to resort to vigorous monetary expansion in the aftermath of asset market problems, is adequate to maintain economic stability is still an open question.

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## *Low Inflation and Economic Stability*

The 1990s saw low and stable inflation in many countries. It turned out, however, that, in and of itself, this was not a sufficient condition for financial stability more broadly defined, nor for stability in the real economy. To be sure, the most dramatic problems, those experienced in Asia during 1997-98, or Argentina in 2001, arose in economies where low inflation had been held in place by exchange rate pegs - more or less formal, depending upon the specific economy - that proved unsustainable, and this was hardly a novel occurrence. Furthermore, asset market problems and subsequent slowdowns in real activity have frequently been associated with contractionary monetary policy designed to bring general inflationary pressures, themselves the visible consequences of earlier monetary over-expansion, under control. For an example here, we need look no further than the late 1980s, when general economic expansion and incipient upward pressure on inflation were accompanied in a number of countries by bubbles in real estate markets, whose subsequent collapse was one of the most visible features of the recession with which that decade ended.

## *Policy Issues, Now and Then*

But, even at the end of the 1980s, real estate bubbles occurred in some economies without being accompanied by any obvious general inflationary pressures. The Nordic countries provide a notable example here. Furthermore, the high-tech bubble that shocked North American and European markets in the late 1990s occurred in markets where monetary policy was aimed at domestic goals and inflation remained low. It also had consequences well beyond financial markets, involving as it did considerable over-investment in computing and communications equipment of all sorts. And in its aftermath, there are fears in a number of countries that bubbles are now developing in markets for domestic real estate.

This experience at the very least raises questions about the adequacy of low inflation as a sufficient goal for monetary policy, whether pursued in the context of formal inflation targets, which, beginning with New Zealand and Canada, were adopted by an increasing number of countries in the 1990s, or less formally, as in the case of Japan in the 1980s or the United States in the 1990s. Perhaps economic performance might be better if, instead of focussing only on

the behaviour of the general price level, monetary policy also pays attention to countering rapid increases in asset prices, even those occurring while, overall, inflation remains well under control.

Questions bearing on this issue were much analysed by economists before the Keynesian Revolution, and in this essay I shall discuss some highlights of the literature that their debates generated.<sup>43</sup> In those years, a body of theory dealing with fluctuations that took the influence of monetary factors on the general price level as its starting point vied with others that stressed monetary effects on relative prices, and on asset prices in particular, and yet others that downplayed the role of the monetary system in generating the cycle. These competing views yielded sometimes contradictory positions on the sufficiency of price level stability in particular, and even monetary measures in general, as a means of avoiding fluctuations. There are, nevertheless, certain important differences between the policy problems and approaches of the inter-war years and those which dominate present day debates, and these have influenced my choice of topics to be discussed below.

To begin with, policies aimed at domestic goals, such as low inflation, or economic stability more generally, are nowadays self-consciously implemented against a background of exchange rate flexibility. In the inter-war years, matters were not so clear cut, and international monetary considerations were never too far in the background. But interwar macroeconomic theory nevertheless did have a good deal to say that can be applied to illuminating the policy choices that nowadays face monetary authorities that have already opted to pursue domestic goals. I shall emphasise these aspects of the literature in what follows. As a corollary, I shall have relatively little to say about how it treated international monetary arrangements, notably the gold standard, which were matters of central policy importance in many countries in the 1920s and 1930s, and therefore were extensively discussed.<sup>44</sup>

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<sup>43</sup>In what follows, I draw on my own earlier work on this material, in particular, Laidler (1999)

<sup>44</sup>Bernard and Bisignano (2002) provide an account of interwar debates that is in many respects complimentary to this one, inasmuch as it pays particular attention to international monetary factors as sources of domestic policy problems, both then and more recently.



Furthermore, present day economics largely takes it for granted that activity in the real economy is well co-ordinated by market mechanisms, and as a result, discussions tend to focus on instability within the financial system. The economic theory of the inter-war years, on the other hand, took very seriously the possibility that the real economy was subject to chronic co-ordination failures, particularly as far as the allocation of resources over time was concerned. It therefore paid considerable attention to fluctuations in investment and imbalances in the capital stock, and by and large treated financial instability, not as a problem in and of itself, but as a reflection of these deeper phenomena. In this case, I have chosen to follow the emphasis of these earlier discussions, because I believe that there are perhaps a few valuable insights into modern policy problems that the later literature has been inclined to neglect.

### *An Outline of the Paper*

This paper begins with an account of theories of the cycle and stabilisation policy that started from the quantity theory of money. These are the direct intellectual ancestors of the modern view that monetary policy should be aimed at maintaining low and stable inflation. Their exponents argued that stabilisation of the general price level was not only the right goal for monetary policy, but one whose successful pursuit would, in an of itself, eliminate, or at least markedly reduce, the likelihood of real instability. But some of these exponents, particularly those who favoured discretionary rather than rule-bound monetary policy, were also strong advocates of the monetary authorities making rapid and vigorous use of their lender-of-last-resort powers should things nevertheless go wrong, as of course they did, with a vengeance in 1929.

Accounting for the stylised facts about fluctuations in investment has always been an awkward corner for traditional monetary accounts of the cycle. In what follows, therefore, some brief comments on attempts by Cambridge economists to extend the traditional monetary theory of the cycle to deal with these facts will precede discussion of an alternative real tradition in cycle theory, Marxist in origin, that faces them squarely, and indeed takes their explanation as the being the central goal of cycle theory. Many inter-war cycle theorists were acutely aware of the challenge that this Marxist tradition posed, and a selection of their responses to it will then be examined.

Here, I shall begin with Austrian theory, which self-consciously defended the neo-classical vision of a stable market economy against the Socialist critique. This theory stressed the role of the monetary system in creating, not fluctuations in the general price level, but distortions in relative prices that led directly to destabilising imbalances in the structure of the capital stock. It will be argued that, though this specific theory's logical structure was deeply flawed, the phenomenon to which it drew attention, *forced saving*, has considerable appeal as a means of explaining how asset market imbalances can arise against the background of stable prices. However, as will also be shown, when Dennis Robertson incorporated forced saving into a more eclectic framework than the neo-classical general equilibrium approach adopted by the Austrians, his analysis led him to join other Cambridge economists in recommending exactly the kind of policy regime whose collapse in the 1970s and 1980s provides the background for current pre-occupations with confining monetary policy to the pursuit of low and stable inflation, and denying any significant stabilising role to fiscal policy.

In conclusion, it will be suggested that experience in the aftermath of the recent high-tech bubble will in due course provide evidence that will help us to decide whether we need to re-examine recommendations such as those of Robertson, or whether the quantity theory inspired advocates of discretionary monetary policy were right that a speedy deployment of the central bank's powers to create liquidity is adequate to cope with the consequences of crises that arise under monetary policy regimes that are aimed at low and stable inflation..

### ***The Quantity Theory Tradition in Cycle Theory***

The Quantity Theory of Money had its modern origins in the great gold inflation that marked the 16th and 17th centuries. It was, and remains, in the first instance, a theory about the relationship between the quantity of money and the price level. Since the first half of the 19th century, however, a particular extension of the quantity theory tradition has emphasised the potential for interactions between the supply and demand for money to create systematic fluctuations in the price level, and/or the aggregate demand for goods and services, which in turn seem to be central features of what was once called the *credit cycle*, but which is nowadays more usually referred to as the *business*

*cycle*. Even though this *quantity theoretic* approach to the cycle has evolved over time, it display considerable continuity, from its first appearances in early 19th century debates about the workings of the British monetary system, right down to the *monetarism* of the 1960s and 1970s which provided many of the intellectual foundations for contemporary inflation-targeting regimes.

### *The 19th Century Background*

*Money Credit and Commerce* (Marshall, 1923) was a work of its author's old age, and was by no means the most up-to-date work on monetary issues then available, but it would still have been of more than antiquarian interest to its intended readers. There is no better evidence of the essential continuity of earlier work on the quantity theoretic approach to the cycle than Marshall's exposition of it. He begins by quoting Samuel Jones Loyd's (later Lord Overstone) (1837) characterisation of the cycle: "It has been well said that "the state of trade revolves apparently in an established cycle. First we find it in a state of quiescence---next, improvement---growing confidence---prosperity---excitement---overtrading---convulsion---pressure---stagnation---distress---ending again in quiescence" (p.243) and soon proceeds to an account of "*The ordinary course of a fluctuation in commercial credit*" that relies heavily on a long passage transcribed from Marshall and Marshall (1879, pp. 154-156), which in turn is clearly related to a similar account given by John Stuart Mill in his *Principles of Political Economy* (1848, 1871, pp. 542-543)

This particular account of the cycle is typical of the literature it represents. It is vague about just what kind of disturbance might initially shift the economy from a state of quiescence to improvement, but it emphasises that, once begun, economic expansion will be amplified by the recursive interactions of expanding money and credit, rising prices, and the profits made by borrowers. This expansion in turn will go on until "trade is in a dangerous condition" (1923, p.250) and distrust that creates incentives for lenders to tighten credit begins to set in. Given the precarious state of the market after a long bout of speculation, such tightening is sufficient to persuade some speculators to begin selling their holdings of goods and set in motion a collapse of prices.<sup>45</sup> Thereafter, "As credit

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<sup>45</sup>In 19<sup>th</sup> century accounts of the cycle, the role of an adverse balance of payments in draining gold from the banking system was often accorded an important role in precipitating a downturn. This matter is mentioned by

by growing makes itself grow, so when distrust has taken the place of confidence, failure and panic breed panic and failure”(1923, p.250). The essential features of the upswing and downswing of the cycle according to this monetary approach were, then, rising and falling prices driven by cumulative expansions and contractions of credit and bank money. Furthermore, the development of “overtrading” - what we would now call speculative bubbles - was seen as an integral feature the final stages of the upswing, while its collapse - the bursting of the bubbles in question - was a key characteristic of the onset of the downswing.<sup>46</sup>

This vision led naturally to the view that policies capable of stabilising the cycle would also eliminate speculative bubbles, not to mention their real consequences. From the very beginning, the quantity theoretic literature contained an important strand that advocated legally enforced monetary rules of one sort or another as a means of ensuring stability. Even by the 1830s, when Overstone wrote the description quoted by Marshall, it had become clear from experience that the mere legal requirement that the liabilities of the banking system, or more specifically of the Bank of England, be convertible on demand into gold, was not a sufficient rule to eliminate cycles and their accompanying crises, and the Bank Charter Act of 1844, of which he was a major architect, attempted to solve this problem by tightening the legal restrictions under which the Bank of England operated. The Act established the Bank’s note issuing department as what we would now call a currency board, based on gold, in the expectation that this would eliminate the monetary system’s capacity to generate the fluctuations in money and credit that were necessary for the cycle to persist as a regular feature of economic life. This rule too proved inadequate, however, as its Banking School opponents had predicted, and as subsequent crises in 1847, 1853 and 1865 soon demonstrated, because its framers had paid insufficient attention to the role of bank deposits, including Bank of England deposits, in the monetary system.

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neither Mill nor Marshall in the passages referred to here, though both pay considerable attention to it elsewhere in their writings.

<sup>46</sup>I know of no better evidence of this than a cartoon, probably drawn about 1857, of “the Overstone Cycle”, reproduced in O’Brien (ed.) (1971). “Excitement” is depicted with bustle around a building bearing the sign of the “South Pole Warming Company”, and “Convulsion” with an exploding “Royal Bubble Bank”. I am grateful to Denis O’Brien for advice about the date of this cartoon, and to Walter Eltis for first drawing my attention the significance of its content.

By the 1870s, therefore, it was widely taken for granted that short-run discretionary policy (though always conducted against the background of gold convertibility) was required to cope with problems created by cycles. The policy envisaged, however, was not of the counter-cyclical type that later came to be associated with the exercise of such discretion. Rather, the *British Monetary Orthodoxy*, encapsulated in Walter Bagehot's (1873) precepts for central banking, recommended no more than that the Bank of England should always be ready to act as a lender of last resort to any and all solvent institutions whose survival was threatened by the contagious crises which had marked the upper turning point of cycles from 1825 onwards<sup>47</sup>. As it came to be adopted in the last quarter of the 19th century, this orthodoxy was a considerable success in its own terms. Gold convertibility was never in question, and though financial crises still occurred from time to time, notably that created by the near failure of Baring Brothers in 1890, and fraud aside, bank failures were a thing of the past in Britain from the 1870s onwards. This British experience was well known to observers in the United States, where crises involving bank failures were a regular feature of the financial landscape until 1907, and provided an important impetus to the founding of the Federal Reserve system.<sup>48</sup>

### *Price Stability as a Policy Goal*

As I have argued in greater detail in Laidler (1991, 2002), even as Bagehot's policy orthodoxy was becoming established in central bank practice, developments in economic theory that would ultimately render it obsolete were already getting under way. Marshall himself presented a key refinement of the standard monetary account of the cycle in (1887). He linked the expansion and contraction of money and credit to rising and falling prices by noting that nominal interest rates failed fully to adjust to inflation and deflation, and, like Marshall and Marshall (1879), he attributed variations in output and employment to counter-cyclical movements in real wages that in turn were the result of the interaction of sticky nominal money wages with more flexible output prices. Evidently, both of these sources of trouble could be avoided either

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<sup>47</sup>The italicised phrase is, of course, taken from the title of Frank Fetter's still indispensable 1964 history of British monetary economics over the years 1797-1875.

<sup>48</sup>For a n influential contemporary account of financial crises in the United States, and the effect that the absence of a lender of last resort had on their course, see Sprague (1910)

by ensuring price level stability or by linking debt and wage contracts to a suitable price index. Marshall in fact recommended the latter, but this proposal got nowhere in practice.<sup>49</sup> In the longer run, however, his 1887 work remained important because it provided arguments implying that price level stabilization was the key to achieving more general financial and economic stability.

Even before the outbreak of World War 1, Irving Fisher, who was well aware of Marshall's contribution, began to advocate his "compensated dollar" scheme as a means of automatically stabilising prices, and to campaign for legislation that would subject the newly founded Federal Reserve system to a legislated price stability rule. Since the cycle was, in Fisher's eyes, largely a "dance of the dollar", he confidently expected that the enactment and observance of such a rule would eliminate it.<sup>50</sup> This campaign continued with varying degrees of intensity, though no legislative success, throughout the 1920s, and Henry Simons preserved its basic message in a now classic 1936 paper, which would be much cited in post-war monetarist literature.

In the inter-war years, however, legislated monetary policy rules were a minority taste, as indeed they are now. Even by the mid-1920s, European economies were already facing what seemed to be unprecedented policy problems of apparently secular stagnation and mass unemployment created by botched attempts to restore the Gold Standard after the end of World-War-1, and discussions of counter-cyclical monetary policy seemed barely relevant in this context. Even in the United States, where, before 1929, the debate was dominated by more traditional questions, discretionary stabilization policy, "credit control" as it was usually called, attracted more support, than did legislated rules, while advocates of both found themselves in an ongoing debate with exponents of Banking School ideas inherited from 19<sup>th</sup> century Britain.

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<sup>49</sup>It seems to have been Jevons (1875) who first suggested indexation, but only for credit market contracts. Marshall extended the proposal to the labour market.

<sup>50</sup>The "compensated dollar" involved varying the gold content of the dollar to offset changes in the price of gold relative to a general price index. It seems to have been first discussed as a logical possibility in a footnote in Marshall (1887). Initially, Fisher proposed to have this scheme imposed upon the Federal Reserve system, but it was dropped from his later campaigns for a price stability rule.

The case for discretionary policy aimed at stabilising prices, and hence the cycle, has roots in the late 19<sup>th</sup> century literature, where an early and eloquent case for using discount rate policy for this purpose was made by the Swedish economist Knut Wicksell as early as 1898. As Thomas Humphrey (2002) has recently reminded us, Wicksell's positive analysis of inflation was taken up (without acknowledgement) by Gustav Cassel (eg. 1928). However, Wicksell derived his proposals from a theory of secular inflation, not of the cycle, and Cassel's variation on his theme was deployed in the course of, and probably in support of, one of Fisher's campaigns for subjecting the Federal Reserve system to a price-stability rule.<sup>51</sup> In the 1920s, the best known exponent of the quantity theoretic approach to cycle theory, and of the discretionary stabilisation policy that it seemed to support, was Ralph Hawtrey, an official of the UK Treasury. His works were widely read in the United States, and greatly admired too, not least by Allyn Young of Harvard, probably the most influential teacher of monetary economics in the country at that time.<sup>52</sup>

In Hawtrey's view, the cycle resulted from what he called the "inherent instability of credit" which arose from essentially the same interest rate mechanism that Marshall had identified in 1887. To that mechanism he added an analysis of the endogenous cyclical behaviour of what we would now call the public's currency deposit ratio that ensured that an internal drain (inflow) of cash from (into) the banks' reserves that came late in the upswing (downswing) would curtail (enhance) their willingness to lend, hence precipitating the cycle's turning points. According to Hawtrey, even under an international gold standard, the cycle could in principle be ironed out by the judicious use of discount rate policy on the part of central banks acting in concert, though, to his credit, he doubted the practical feasibility of doing so completely. Nevertheless he believed that stabilisation policy, even imperfectly executed, could improve matters, and, as early as (1913), he entertained the logical possibility of an individual central bank attempting such a policy under a flexible exchange rate.

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<sup>51</sup>For documentation of Cassel's support of price stability legislation in the United States in 1928, see Charles Hardy's discussion of this episode in (1932, pp. 199-206)

<sup>52</sup>I have given an account of Hawtrey's views in Laidler (1999, pp.112-128). For a recent discussion of his influence on Young, and of Young's own views, see Mehrling (1996, 1997).

The Federal Reserve system's cautious attempts during the 1920s at "credit control", when the scale of US gold holdings insulated domestic monetary policy from any international constraints, were very much along the lines that he, as well as Young and others under his influence, advocated.

As we now know, despite the Fed's success in maintaining a non-inflationary environment in the 1920s, an asset market bubble developed whose collapse in October 1929 ushered in the Great Depression. Hawtrey's (1932) essay on *The Art of Central Banking*, written in the aftermath of these events, throws light on the general question of whether price level stability is a sufficient indicator of the success of monetary policy, and discusses in some detail how a central bank should respond to a collapsed asset market bubble.

One important feature of the traditional monetary theory of the cycle was clearly discredited by the events of the 1920s: namely, that theory's presumption that the swelling of a speculative bubble in asset markets required a prior period of general price level inflation driven by the over- expansion of bank credit and money to bring it about. Hawtrey (1932) himself conceded as much when he asserted explicitly that "the speculation which trebled the average market values of shares in the American market between 1923 and 1929 was not due to inflation", and, consistent with this view, he did not blame the market's collapse in October 1929 for the severe reduction in aggregate demand that followed.

"That spending power did shrink is beyond dispute, but the cause was not the Wall Street crisis. The shrinkage of spending power and the Wall Street crisis were both the effects of a common cause, the credit contraction towards which the Federal Reserve System and the Bank of England had been directing their combined efforts. The Wall Street crisis only intensified the depression through its psychological effects." (p. 80)

After the crash, furthermore, the Fed's efforts to counter the economy's contraction had been inadequate. Interest rates did fall, "but the process was deplorably slow" (p.213), while the open market operations undertaken in 1930 were "half-hearted", so much so that, by the middle of 1930, what Hawtrey usually termed a "credit deadlock" had developed. Such a deadlock was a state of affairs in which there was a general reluctance on the part of the business community to borrow from the banking system on any terms offered. "When



that happens, it seems to be the extreme of paradox to say that there is a shortage of money. . .” (p.172) but that was indeed what ailed the economy. For Hawtrey, “The Central Bank is the *lender of last resort*. That is the true source of its responsibility for the currency” (p.116, Hawtrey’s italics), and “The essential duty of the central bank as the lender of last resort is to make good a shortage of cash among the competitive banks.” (p.126) Therefore, what was always required in the face of a credit deadlock was open market purchases of securities on whatever scale was needed to bring about a resumption of spending by the public.<sup>53</sup>

Two point should be made about Hawtrey’s credit deadlock concept. First, it must not be confused with the *liquidity trap* doctrine, which sees a *high*, and in the limit infinite, interest elasticity of demand for *money* as an obstacle to effective monetary policy, rather than a *low*, and in the limit zero, interest elasticity of demand for *credit*.<sup>54</sup> Second it also differs from the modern concept of a *credit crunch*. It locates the principal source of credit market difficulties in the public’s unwillingness to *borrow* from the banking system rather than in the latter’s unwillingness, or inability, to *lend* to the public. Nevertheless, the concepts are not totally unrelated: a deadlock in credit relations between the central bank and commercial banks stemming from their unwillingness to borrow new reserves can co-exist with a general unwillingness on their part to lend to the public at large, and Hawtrey’s insistence on the importance of the central bank providing cash to commercial banks seems partially to recognise this.

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<sup>53</sup>It is worth noting explicitly that there are two elements to the lender of last resort doctrine in 19<sup>th</sup> century discussions, as Humphrey and Keleher (1985) have carefully documented. The first stresses the central bank’s role as a provider of liquidity to the market. The second sees it as providing liquidity to specific institutions, which though solvent, are in difficulties. Hawtrey stressed the former element. Laidler (2002) also discusses this issue. Hawtrey, it should be noted, was willing to countenance government expenditure financed by money creation should open market operations fail, though he did not think this eventuality very likely. See Laidler (1999, pp. 125-126) on this point.

<sup>54</sup>A credit deadlock prevents money being created. In a *liquidity trap*, money lies idle after it has been created. Keynes’ did much to refine the latter concept in the *General Theory*, though it is not clear that he believed in its empirical relevance (See Laidler 1999, pp.258-259). However, a primitive version of the doctrine, that carried with it a warning that surplus balances might suddenly be spent, thus creating inflationary instability, was a commonplace throughout the 1930s. For example, Hardy (1932, p. 92) argued as follows: “The result of open market purchases during a depression might be simply to pile up idle reserves in banks, and idle balances in the hands of individuals, until the load gets so great that confidence in the currency suddenly disappears -with the usual accompaniments-an accentuated decline of business confidence, budget disorganization, gold hoarding, flight to foreign currencies, and finally the complete collapse of the currency system”

Be that as it may, with reference to the specific situation in the United States in 1930, Hawtrey argued explicitly that “demand could have been revived by a *sufficient* surplus of idle money” (p.242) but pointed out that the Fed. had failed to provide such a surplus. Writing as he was in 1932, however, he noted that “Much may be hoped from this policy [open market purchases started in March 1932], if it is persisted in.” (p. 242). In the event, the 1932 initiative also proved inadequate, perhaps because the operations were carried out on too small a scale, and were brought to end prematurely. This was certainly the retrospective opinion of Friedman and Schwartz (1963), and it was also the contemporary view of Hawtrey’s erstwhile assistant, and Young’s student, Lauchlin Currie, who, in (1934) succinctly summed up the lessons of 1929-32 period in the following terms.

“Much of the current belief in the powerlessness of the reserve banks appears to arise from a complete misreading of the monetary history of 1929-32. It is generally held that the reserve administration strove energetically to bring about expansion throughout the depression but that contraction continued despite its efforts. Actually the reserve administration’s policy was one of almost complete passivity and quiescence,” (Currie, 1934, p. 147)

This is not the place to debate the adequacy of an essentially monetary interpretation of the Great Contraction.<sup>55</sup> For purposes of this essay it is sufficient to summarise the following salient characteristics of the body of economic doctrine of which it came to form so important a part. First, and above all, that doctrine treated the cycle as an essentially monetary phenomenon, not in the sense that it always had its origins in an exogenous monetary impulse, but in the sense that its upswing and downswing represented a process in which expanding (contracting) money and credit interacted recursively with rising (falling) prices and demand, and in which turning points were initiated by changes in the willingness of the banking system to lend, and hence create money. Second, and closely related, that doctrine argued that the cycle could be stabilised by measures that would prevent cumulative expansion and

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<sup>55</sup>The idea that a monetary interpretation of the Great Contraction and proposals for a monetary cure for it were the unique product of a *Chicago Tradition* was once widely entertained, but is quite insupportable. Some Chicago economists, for example Jacob Viner and Henry Simons did indeed support such a view-point, but it neither originated at, nor was confined, to Chicago. For a fuller discussion, See Laidler (1999 pp. 231-239).

contractions of money and credit getting under way in the first place. Some of its exponents, for example Fisher, Simons, and, in the post-war years, Friedman, thought that this could be achieved by imposing legally binding rules of various kinds on the central bank.<sup>56</sup> Others, were more cautious, and like Hawtrey, preferred a discretionary approach to stabilisation policy, regarded price level stability as a sign of its success, but also thought of the central bank's lender of last resort powers, and more specifically its powers to inject liquidity into the banking system, as being a vital second line of defence should things nevertheless begin to go wrong, as they did in 1929.<sup>57</sup>

The vision that has dominated monetary policy since the beginning of the 1990s is clearly the latest incarnation of this approach. Its immediate intellectual roots lie in the monetarist doctrines of the 1960s and 1970s which did so much to breathe new life into these older ideas, and the main factor separating it from these roots has been its replacement of the legislated money growth rule favoured by the academic exponents of monetarism with more practically oriented inflation targets as anchors for monetary policy. This shift of emphasis has provided policy makers with what they would defend as a much needed degree of discretion in their choice of policy instruments and in their ability to manipulate of them.

And yet, just as things went wrong in the US in 1929, even though the price level seemed to be behaving well, so they went wrong in Japan at the beginning of the 1990s under similar circumstances, and again in the United States, and to a lesser degree in Europe too, in 2001. In each case an asset market bubble seems to have developed and collapsed without this event being preceded by any general upsurge of inflation. With the passage of time, it has become tempting to treat the boom of the late 1920s that ended with the bursting of the stock market bubble in 1929 as an outlier, but the recent occurrence of two apparently similar episodes suggests that there are forces at work in the

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<sup>56</sup>Fisher always favoured a price stability rule, and Friedman a money growth rule. Simons vacillated between the two, but in 1936, opted for price level stability. On this latter point, see Laidler (1999, pp. 241-242)

<sup>57</sup>It should be noted explicitly that, in the early 1930s, as the Great Contraction gathered momentum, Fisher was an energetic advocate of monetary expansion aimed at restoring the price level to its 1929 level. His well known "debt deflation theory of great depressions" (Fisher 1932, 1933) should be seen as an extension of a quantity theoretic analysis of events that focussed on the consequences of rapidly falling prices. That theory added both strength and, more important, urgency to Fisher's longstanding claims on behalf of the virtues of price-level stability.

economy that are not properly integrated into those explanations of the cycle and doctrines about macroeconomic stabilization that derive from the quantity theory of money. That was precisely the opinion of a large number of economists writing in the interwar period, as we shall now see.

### ***Investment and Real Explanations the Cycle***

The monetary theories of the cycle discussed above have, formally speaking, no special place for investment expenditure. Within them, it is simply one component of aggregate demand that is affected by monetary impulses. And yet no exponent of this approach would have denied that asset market bubbles were financial market manifestations of deeper imbalances that had arisen within the structure of the economy's capital stock, or that their collapses had been accompanied by the recognition of these real imbalances and an associated cessation, or at least a dramatic slowdown, of investment activity.

Even in the early 1860s, when economists first began to recognise that the cycle involved systematic fluctuations in real variables, it was known that investment activity was particularly prone to large swings. Thus, Jevons (1863) conjectured that the fundamental causes of “great commercial fluctuations, completing their course in some ten years” seemed to lie “in the *varying proportion which the capital devoted to permanent and remote investment bears to that which is but temporarily invested soon to reproduce itself.*” (p. 27, Jevons' italics), and he further noted that “It is the peculiarity . . .of great and permanent works . . .to be multiplied at particular periods.” (p. 28). Monetary theorists of the cycle were just as aware of these facts as anyone else. In (1879) Marshall and Marshall had singled out for special mention “. . . companies [that]. . .have borrowed vast sums with which they have begun to build docks and ships and ironworks and factories” and whose projects were not yet complete as the crisis phase of the cycle approaches, and in (1923) Marshall himself still found this passage sufficiently important to quote, as we have already seen..

Hawtrey too was conscious of the empirical importance of fluctuations in fixed investment, and, in his first book, *Good and Bad Trade* (1913, p. 207)) he went so far as to outline a simple accelerator mechanism as an explanation of the phenomenon. But it is one thing to recognise and attempt to explain what we

would now call a *stylized fact*, and another to incorporate that explanation systematically into the logical structure of a model, and this he failed to do. In (1913), the accelerator was tacked on to Hawtrey's analysis, and it disappeared altogether from *Currency and Credit* (1919), the book that would nowadays be regarded as containing the definitive exposition of his theory of the cycle.

### *Marshall's Cambridge Successors*

But Hawtrey, though a Cambridge graduate, and despite the similarities between his cycle theory and Marshall's, is not usually regarded as his intellectual heir, at least within academic economics. Credit for developing Marshall's thought at Cambridge in the 1920s and 1930s is usually allocated, according to the commentator's taste, among Arthur C. Pigou, Frederick W. Lavington, and of course John Maynard Keynes, and in the inter-war years all of these made serious and systematic efforts to incorporate the behaviour of investment into the Cambridge cycle theory which Marshall had built on quantity-theoretic foundations.<sup>58</sup>

Pigou and Lavington overlaid Marshall's account of the cumulative nature of upswings and downswings with what was often characterised as a *psychological* theory, in which *errors of optimism* and of *pessimism* drove business decisions, errors which were correlated across agents and tended to be amplified with the passage of time. Furthermore,

“Inasmuch as optimism or pessimism naturally has greater influence on business judgements the less certain the basis on which those judgements rest, it is only to be expected that this influence should be most marked, and maladjustments of resources consequently most evident, in the output of new capital plant designed to produce goods for very far-ahead and uncertain markets.” (Lavington, 1922, p. 91)

Cambridge theorists thus brought swings in investment into the explanation of the cycle as a supplement to the recursive interactions among money, credit and prices that had dominated Marshall's account. As a corollary, they no longer

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<sup>58</sup>I have discussed the work of Pigou and Lavington in Laidler (1999, pp. 83-90) and Keynes of the *Tract* and *Treatise* in pp. 106-112 and pp.130-154, respectively of the same work.

regarded stabilisation of the price level as sufficient to eliminate the cycle. Pigou (1929) opined as follows: “I hold that, if a policy of price stabilisation were successfully carried through, the amplitude of industrial fluctuations would be substantially reduced - it might be cut down to half of what it is at present - but considerable fluctuations would still remain.” (p. 219) He advocated activist monetary policy, supplemented by public works expenditure if need be, an approach to macroeconomic policy which was widely implemented in the post-war years, only to be discredited by the inflation of the 1970s and 1980s which ushered in the era of inflation targeting.

At one time it was usual to credit (or debit) Keynes (1936) with originating the case for such policies, but it is by now uncontroversial that the major contribution of the *General Theory* to inter-war policy controversy was to provide a more rigorous theoretical rationalisation than had previously existed for an already widely accepted activism. Indeed Keynes was rather a late-comer to that position. The *Tract on Monetary Reform* (1923) starts just as clearly from the quantity theory as anything that Hawtrey produced in the 1920s.<sup>59</sup> As to the *Treatise on Money* (1930), though it located the key to cyclical swings in the influence of the long rate of interest on savings and investment, its formal analysis (as opposed to informal discussion) extended only to cyclical swings in the price level. Its policy advice for dealing with the then just beginning Great Contraction - “open market operations *a outrance*” - was, moreover, no different from Hawtrey’s, though its theoretical basis was different. It was only after the publication of the *Treatise* that Keynes began to move decisively towards the policy positions later associated with his name, and, not entirely coincidentally, to adopt an explanation of fluctuations in investment that, focussing as it did on the role of *animal spirits*, differed from that offered earlier by Lavington and Pigou more in vocabulary than in substance.

We shall return to the matter of generalised macroeconomic activism below, but first we need to look at approaches to explaining the cycle that paid even closer attention to the role of investment in economic fluctuations than did

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<sup>59</sup>Large parts of the *Tract* should be read as constructive comments about the practicalities of the Genoa Resolutions of 1923, whose principle architect was Hawtrey, offered by a critic who completely accepted their analytic basis. See Laidler (1999, pp.106-107, 122-123) Those resolutions sought to re-establish the gold standard as a basis for internationally co-ordinated monetary stabilization policy, and Keynes’s proposals for a heavily managed float between sterling and the US dollar amounted to a modification of this recommendation.

anything developed at Cambridge, and therefore deviated much more radically from the quantity theory tradition.

*The Influence of Marx and Wicksell.*

The quantity theory of money was far from providing the only starting point for thinking about the cycle in the 19th century. Karl Marx's *Capital* (1867) set out an alternative and very different view of the workings of the market economy. Rather than present capitalism as a system which, if left to itself, tended to converge on some sort of full-employment equilibrium in the wake of monetary disturbances, Marx identified a process of inherently unstable cyclical economic growth as its central feature, and he expressed contempt for any purely monetary explanation of fluctuations: "The superficiality of political economy shows itself in the fact that it looks upon the expansion and contraction of credit, which is a mere symptom of the periodic changes of the industrial cycle, as their cause." (1867, p. 633)

For Marx, the cycle was not the outcome of a series of shocks that created fluctuations around an equilibrium growth path. Rather it was the essential characteristic of that growth path, the inevitable consequence of the relentless pursuit of profit by the capitalist class. In the expansion phase, individual capitalists pursued *relative surplus value* by investing in ever more efficient labour saving machinery, but from the viewpoint of their class as a whole, this was self defeating. In a competitive market economy, the value of goods was determined by the labour *socially* necessary to produce them, and the very process of widespread investment in innovative technology reduced this. Thus the economy-wide rate of profit was also driven down, bringing about a crisis and a halt to the economy's expansion. Labour was then released into the reserve army of the unemployed and would provide cannon-fodder for the next expansion. A succession of ever more violent cycles would culminate in a crisis severe enough to provoke revolution.

Now many more economists were influenced by this vision of the cycle than adopted the revolutionary Socialist doctrines that Marx believed it to imply. Indeed, in the period that is the focus of this paper, theories that emphasised the role of innovation as the main impetus driving cyclical growth were probably a majority taste among continental economists. A large literature

noted the challenges posed to the stability of the growth process by the time lags inherent in the construction of capital goods, and their indivisibility and durability once they were in place. It is impossible to do justice to the non-quantity-theoretic, but nevertheless monetary, analyses of the cycle to which the next section of this paper is devoted without understanding that they are the outcome of attempts to reconcile these ideas, which may here conveniently be labelled as representing a *real* tradition in business cycle analysis, to neoclassical doctrines which placed more emphasis on *monetary* factors.<sup>60</sup>

A crucial link between these two traditions lies in the work of Wicksell, who, though apparently little known among economists writing in English until the late 1920s, was influential elsewhere from a much earlier date.<sup>61</sup> His now well known *cumulative process* analysis focussed on the interaction of the *market* rate of interest, set by banks, with the *natural* rate, which in various places, and inconsistently, he defined as the marginal product of capital, the rate of interest at which saving and investment would be equal in a frictionless barter economy, and the rate of interest to which the market rate had to be equated to stabilize the price level.<sup>62</sup> Wicksell himself thought of this analysis as extending the quantity theory of money to a world dominated by banks, and saw it as a tool for explaining secular movements in the price level rather than cyclical fluctuations. It was also his view that, in the 19<sup>th</sup> century at least, such movements had mainly originated on the real side of the economy as productivity shocks drove the natural rate of interest away from the market rate.

Consistent with this, Wicksell's few sketchy writings on the cycle show him to have been close to the real business cycle tradition, more particularly that

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<sup>60</sup>Much of the relevant literature was written in German, and it is one of the many virtues of Harald Hagemann's (2002) four volume collection of key papers in the development of business cycle analysis that several critical contributions are made available in English for the first time.

<sup>61</sup>The question of Wicksell's influence on English language economics is a thorny one. It is usually accepted that, before the considerable attention given to it by Keynes in the *Treatise*, his work was largely unknown, and certainly it was not cited before then in England. In the United States Fisher (1911, pp. 59-60) did acknowledge his work, however, and a key paper summarising his theory of inflation (Wicksell 1907) was presented at the meetings of the British Association for the Advancement of Science and published in the *Economic Journal*.

<sup>62</sup>The inconsistency between defining the natural rate in terms of its capacity to generate price level stability and equilibrium between savings and investment will be discussed below. The very real problems inherent as treating it as the marginal product of capital per unit of capital in a world of heterogeneous capital goods are not quite central to this paper, and will not be discussed. For my own views on this contentious issue, see Laidler (1999, pp. 33-34, 53-57)



branch of it that follows Marx and Joseph Schumpeter in emphasising the role of technical change as the fundamental source of economic fluctuations, and sceptical about attributing any important causative role to the monetary sector. Small wonder then, that Wicksell's successors saw the potential for his cumulative process to become a starting point for cycle theories based on the idea that the monetary sector interacts with the real economy by way of the influence of the interest rate on savings and investment, and used it as such, thus providing an alternative to monetary theories that began from the influence of the quantity of money on prices.

### ***Forced Saving, Relative Prices and the Cycle***

Real cycle theory and Wicksellian monetary economics crucially influenced, on the one hand, the work of the so-called Austrians - Ludwig von Mises, Friedrich von Hayek and Lionel Robbins among others -, and of Dennis Robertson on the other.<sup>63</sup> The first these, exponents of a self-consciously individualist economics that was closely allied to political liberalism, reacted strongly against any presumption that market economies were inherently unstable, and their work should be seen as an attempt to refute this postulate, while reconciling the basic facts of the cycle, including those concerning investment activity, with neo-classical orthodoxy. In contrast, Robertson's earliest book, *A Study of Industrial Fluctuation* (1915) was heavily empirical, located firmly and sympathetically in the tradition of explaining the cycle in terms of waves of fixed investment driven by innovation, and it acknowledged the specific influence of the work of Michael Tugan-Baranowski and Albert Aftalion.<sup>64</sup> It was only in the 1920s, under the influence of Keynes, that Robertson broadened his analysis to accord a significant role to monetary factors in the cycle.

Though they proceeded independently of one another, and initially in ignorance of each others' work too, the Austrians and Robertson developed views of the role of the monetary system in the economy that had many analytic properties in common. Critically for the matters under discussion in this paper, these views led them to be sceptical about the sufficiency of price level stability

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<sup>63</sup>I have discussed the Austrians' work in Laidler (1999, pp. 27-50) and that of Robertson as well (pp. 90-99)

<sup>64</sup>Samples of the work of both of these are available in Volume II of Hagemann's collection.

for the overall stability of the economy, and to propose price deflation at the economy's rate of productivity growth as a defence against the development of imbalances in financial markets and, more fundamentally, in the underlying structure of the real capital stock.<sup>65</sup> But, Robertson also differed sharply from the Austrians on some important matters. In particular his scepticism about the capacity of market mechanisms to function smoothly without help from policy makers led him to be much more activist in his advice than were they.

### *Forced Saving in Austrian Analysis*

As Hagemann (1994) has pointed out, though Hayek's *Monetary Theory and the Trade Cycle* did not appear in English until 1933, its German version was written in 1928, initially as a response to the socialist economist Adolf Loewe (later Adolph Lowe). Loewe (1926) had challenged not just a particular approach to cycle theory, but the entire paradigm of neo-classical theory as it was then represented by general equilibrium analysis. He had argued that the basic claim that the economic system had a tendency always to return towards equilibrium after an exogenous disturbance was inconsistent with the obvious fact that actual economies continuously moved over time in a series of cyclical swings that never showed any sign of coming to rest, let alone in any state that remotely resembled such an equilibrium. This observation was, in his view, more consistent with a vision of an essentially unstable system driven by an open ended process of capital accumulation than anything offered by neo-classical theory.<sup>66</sup>

It is easy to forget nowadays that, perhaps especially in the German speaking world of the late 1920s, a challenge like this was of more than merely academic significance. What was required to meet it in the eyes of committed liberals like the Austrians was, first a model inhabited by maximising agents, initially in general equilibrium, which, when subjected to a certain type of disturbance would move along a time path that for a while led cumulatively

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<sup>65</sup>George Selgin (1997) has recently restated the case for aiming monetary policy at a rate of deflation equal to the economy's rate of productivity growth., arguing that, relative to price level stability, such a goal "(1) is likely to involve lower 'menu' costs of price adjustment; (2) is less likely to invite monetary misperception effects; (3) is more conducive to the achievement of efficient outcomes using fixed nominal contracts; and (4) generally keeps the money stock closer to its 'optimum' level." (p.70)

<sup>66</sup>Translations of two important papers by Loewe (1926-28 and 1926) are now available in Hagemann (ed.) (2002, Vols. III and IV,) respectively, while a translation of Hayek's original (1928) reply appears in Vol. III).

away from the initial equilibrium, and then reversed direction only to overshoot that same equilibrium, and second, some insight into how these tendencies could be curbed. Hayek attempted to rebut Loewe by arguing that a business cycle theory such as that already sketched by Ludwig von Mises in (1912, 1924) would do just this. Many of his contemporaries agreed with him, and Hayek's own elaboration of this theory in his (1931) LSE lectures on *Prices and Production* for a while looked set to create a revolution in macroeconomic thought of a magnitude achieved a little later by Keynes.

Mises original insight had come in response to a puzzle posed by Wicksell's cumulative process analysis. In an economy that used currency as well as bank deposits as money, and in which the banks held reserves, a shortfall of the market from the natural rate of interest could not persist. The inflation that it generated would increase the public's demand for currency, and the resulting drain of reserves from the banks would cause them to raise the market rate so that it equalled the natural rate, thus restoring equilibrium to both the real economy and its monetary sector. But Wicksell had also analysed the theoretically limiting case of a *pure credit* economy, in which there was no currency, and all transactions were carried out using deposits. Here, if a shortfall of the market from the natural rate of interest led only to inflation, there would be no mechanism to bring the market rate back into equilibrium with the natural rate, and disequilibrium could apparently persist for ever. Mises found the conclusion deeply discomfiting, and in order to meet the challenge it presented, he introduced the process now known as *forced saving* into the Wicksellian framework.<sup>67</sup>

This mechanism depends crucially upon three special assumptions, the first two of which Mises, Hayek, and most other exponents of forced saving usually left unstated. The first is that the newly created bank deposits that enter circulation when the natural rate of interest exceeds the market rate do so by way of loans to firms; in modern terminology, for forced saving to occur, there must be *limited participation* in the market for bank credit. The second is that any effects on the general price level that arise from money creation leave the

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<sup>67</sup>It should be noted that The Austrians did not originate the idea of forced saving and that Wicksell was aware of it though he did not explore its implications for his cumulative process. The idea can be traced back to the very beginning of the 19<sup>th</sup> century. Hayek (1932) provides the still standard account of the doctrine's development from its origins in comments by Jeremy Bentham and Henry Thornton.

real consumption plans of the general public unaffected; in modern equilibrium models of inflation, money holders vary their consumption in order to maintain their real balances in equilibrium as the inflation rate varies, thus paying seigniorage to the creators of nominal balances, but for the forced saving mechanism to work along Austrian lines, the effects of nominal money creation must be *unanticipated*. Finally, and Hayek would elevate this assumption to a methodological principle, in Austrian analysis any conceptual experiment involving variations in the rate of money creation had to begin with the economy in full general equilibrium at full employment.<sup>68</sup>

On these three assumptions, money creation following the opening up of a discrepancy between the natural and market rates of interest can have certain effects beyond those on the general price level that Wicksell analysed. This money goes, in the first instance, into the hands of firms for whom the expected return on new investments (the natural rate) is now higher than the rate of interest at which they are borrowing (the market rate). The demand for investment goods thus increases, and the firms that produce them are able to bid resources away from the consumption goods sector. Households, who have anticipated none of this, are, in effect, forced to reduce their consumption, and therefore to increase their saving, in real terms.

In the Austrian way of putting these matters, a voluntary decision to save by households is simultaneously a decision to consume at some time in the future, while a decision to invest by firms is simultaneously a decision to supply consumption goods in the future, and the rate of interest is the crucial relative price that co-ordinates these choices. Only so long as the market rate of interest is equal to the natural rate does it accomplish this, and create a state of what was usually called *monetary neutrality*. Another way of looking at the process of forced saving, then, is to observe that a shortfall of the market rate of interest below its natural value involves firms receiving a signal that the price of future

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<sup>68</sup>The limited participation assumption presumably reflects the characteristics of *universal banking* as practised on the Continent in the 1920s. The distinction between anticipated and unanticipated inflation was simply not part of the infra-structure of macroeconomic theory in the 1920s, even though certain ideas that we now know to depend crucially upon it - the Fisher effect and the inflation tax - were current. As to assuming full employment, it seemed only proper that an explanation of unemployment should begin at full employment and show how unemployment could then develop. As it happened, the task of assuming, without explanation, the existence of unemployment, and then showing how its level could change, proved much more tractable, and its results much more illuminating too, as Brinley Thomas (1936), who built on foundations provided by the Stockholm School would note. See Laidler (1999, pp. 63-64)

consumption goods has risen relative to that of present consumption goods, and that they should switch the production of such goods from the present to the future, without households having transmitted any such signal. The latter reduce current consumption because the operations of the monetary system compel them to do so, and not because they want to raise their future consumption.

In short, forced saving is the outcome of a co-ordination failure with respect to the inter-temporal allocation of resources. It occurs because a relative price is set at a disequilibrium value by the banking system. The longer it continues, the greater is the imbalance created between firms' capacity to provide consumer goods in the future, and households' desires to purchase them, an imbalance that is matched by a shortfall in firms' ability to meet households' current demand for consumer goods. But, continue it will, so long as the crucial inter-temporal relative price remains at the wrong level.

### *Crises as the Inevitable Result of Forced Saving*

Up to this point, given the assumptions underlying the analysis, the conclusions follow. It is indeed the case that the banking system's power to make loans with newly created deposits that have no counterpart in savings decisions by households can be a source of disturbances to investment and saving, that these can persist for a while, and that this can cause trouble. But the conclusions that Mises, and after him, Hayek, Robbins and others went on to draw from this insight were, when considered in the light of the criticism to which they were subsequently subjected, highly speculative, to say the least.

In particular the assumption that it begins at full employment is critical to the forced saving process. If resources are not fully utilised when firms receive a signal to increase investment, then they can clearly do so without forcing a decrease in consumption. Indeed there is room for both investment and consumption to increase together. Since cyclical upswings typically begin from a trough that is characterised by unemployment generated during a previous downswing, forced saving cannot be a feature of the whole cycle, but only, at most, of the later stages of its upswing when resource constraints have begun to bite.

This insight, however, was not available to the Austrians in the late 1920s, and they argued that, once an expansion was under way, whose central feature was always, in their view, a process of forced saving, a crisis was inevitably in the making. It would involve the excess demand for current consumption goods making itself felt before new investment goods had been completed and brought into production. More by a leap of intuition than careful analysis, the Austrians identified the disruption associated with attempts to unwind the imbalances created by forced saving with the onset of the downswing and subsequent depression, and they suggested that analysis along these lines “leads ultimately to a theory of business cycles” (Mises, 1924, p. 365). According to that theory, as elaborated, for example by Hayek (1931), crises could be staved off temporarily by keeping the market rate of interest below the natural rate, but this would be at the cost of making them more severe when they arrived. And arrive they eventually would, for a side effect of prolonging the process of forced saving by ever more credit creation would be an inflation rate that would rise to ultimately intolerable levels. As Lionel Robbins put it in (1934)

“Once costs have begun to rise it would require a continuous increase in the rate of increase of credit to prevent the thing coming to disaster. But that itself, as we have seen in the great post-war inflations, would eventually generate panic. Sooner or later the initial errors are discovered. And then starts a reverse rush for liquidity. The Stock Exchange collapses. There is a stoppage of new issues. Production in the industries producing capital goods slows down. The boom is at an end.” (pp. 41-42)

This is not the place to embark on a detailed critique of Austrian cycle theory. Suffice it to suggest that its exponents took logical *possibilities* implicit in the analysis of forced saving, and treated them as logical *necessities*.<sup>69</sup> As one sympathetic modern commentator has remarked, “the Austrian theory is not a theory of depression *per se*, but rather a theory of the unsustainable boom”

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<sup>69</sup>See Laidler 1999, pp. 45-46, pp. 51-75, and pp. 63-67, for accounts of and references to contemporary treatments of forced saving that suggested that an initial excess of investment over saving might generate an equilibrating response on the part of savers. Among the possibilities that the Austrians did not notice, but were raised by their contemporaries, were that changes in the distribution of income from wages to profits during the upswing might cause an increase in voluntary saving and eliminate disequilibrium between saving and investment, that the creation of an appropriate amount of new bank credit by way of consumer loans (at that time a new phenomenon, largely confined to the US) could enable households to hold their own in competition with firms, and that, as inflation became anticipated agents would increase their saving in order to maintain their holdings of real balances. The last effect will be discussed below, in the context of Robertson’s work.

(Garrison, 2001, p. 240). Given the Austrians' underlying agenda, however, which was to produce a theory of the cycle that was compatible with a general equilibrium theory strongly tinged with methodological individualism, and could be used to counter socialist critiques of a liberal capitalist society, and given that the great hyper-inflations were a recent memory, one can see how they were led into their over-generalizations.

### *Forced Saving without Price Inflation*

For all its weaknesses however, one feature of Austrian analysis remains particularly relevant to the topic of this paper: namely, the insight that forced saving can nevertheless take place and create trouble against a background of stable, or even falling prices. This possibility, which sets Austrian theory apart from a more traditional quantity theoretic approach to the cycle, as Garrison (2001, p. 242) stresses, arises when the effects of economic growth, particularly that which arises from productivity improvements as opposed to mere increases in population, are brought into the picture.<sup>70</sup>

Wicksell had defined the natural rate of interest as that which would equate saving and investment at full employment, hence implying zero credit creation by the banking system, but he had also defined it as the interest rate which, if it ruled in the market place, would generate stable prices. In a stationary economy, where savings and investment are equal to zero, these two definitions are compatible, but not in a growing economy. There, on the assumption that the velocity of circulation is constant, zero credit creation implies deflation at the economy's rate of growth. Given this, and the assumption that its effects on the price level are unanticipated, forced saving is always implicit in positive credit creation in conditions of full employment, and will occur in a growing economy whenever the rate of price level change lies above that rate of deflation.<sup>71</sup> The

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<sup>70</sup>The literature associated with the forced saving concept sometimes encountered extremely heavy weather in dealing with this distinction between the sources of economic growth. Dennis Robertson (1926, 1928a and b) whose variation on the forced saving theme will be discussed below, dealt extensively with this matter, but the space available in this paper does not permit to do full justice to the topic.

<sup>71</sup> Let it be noted explicitly that modern analysis would tell us that any rate of inflation is compatible with monetary equilibrium and therefore the absence of disequilibrium forced saving, just so long as it is properly anticipated in the term structure of nominal interest rates, a result stated by Eric Lindahl in (1930), though its significance would not penetrate the literature beyond that written in Sweden till much later. Deflation at the rate of productivity growth remains of some analytic significance, however, because at that rate, when it is fully anticipated, the revenue accruing to the monetary system from seigniorage - what Robertson (1926) called "induced lacking" is zero. See below for further discussion.

potential significance of this conclusion for the behaviour of the US economy in the 1920s was not lost on the Austrians, and it formed the basis of their interpretation of the Great Contraction and its aftermath.

But to explain 1929 and its aftermath as a crisis created by a prior process of forced saving required them to argue that disequilibria in the inter-temporal allocation of resources sufficient to have provoked it had built up even without the onset of general inflation. On Hayek's treatment of this specific point, Gottfried von Haberler, himself an ardent exponent of Austrian theory in (1932), would later remark "the reasoning is not . . . altogether convincing" (1937, p. 52), and his more general conclusion was

" . . . that the present theory does not prove, as it claims to do, that a credit expansion which does not lead to a rise in prices but only prevents a fall in prices must have the same evil effect as the more violent type which brings about a rise in the absolute price level" (p. 51)

Once again, we see the Austrians criticised for claiming that a logically possible outcome was logically necessary. But we should also note that, as far as the sufficiency of price level stability for general economic stability is concerned, the mere logical possibility that forced saving can cause trouble under such a regime is of some significance.

### *Austrian Policy Prescriptions*

Be that as it may, what seems to have undermined the reputation of Austrian cycle theory as the 1930s progressed was not so much doubts about its coherence as a positive theory of the cycle, but some of the normative implications that were claimed to follow from it. These reversed the assignment of priorities between prevention and cure for financial and real instability implicit in quantity theoretic reasoning. Where Hawtrey, for example, advocated stabilising the price level, and meeting the consequences of any crises that might nevertheless occur with vigorous money creation after the event, the Austrians argued that the central policy problem was to prevent crises happening in the first place. This was to be accomplished by avoiding any net credit creation by the banking system, or, if the velocity of circulation could not be relied upon to remain stable, by varying credit creation in order to stabilise the rate of flow of



money expenditure. Where prevention had failed, as they thought it was very likely to do, the Austrians further, and crucially, argued that expansionary monetary policy, and/or fiscal measures designed to increase investment and consumption expenditures would make matters worse rather than better, and were to be avoided.<sup>72</sup>

That is why, in the early 1930s, the Austrians urged governments to remain passive and allow the passage of time to unwind the imbalances in the capital stock that they thought lay at the root of the depression. In so doing, they lent considerable academic respectability to similar views, based on the so-called *needs of trade* or *real bills doctrine*, that at that time had wide currency in banking circles in general, and at the Federal Reserve Board in particular. Exponents of the latter doctrine argued that, in the late 1920s, credit creation in the United States had gone beyond meeting the needs of firms for working capital and had fuelled an “artificial” investment boom. As a corollary they argued that, after 1929, any monetary expansion should await the revival of economic activity.<sup>73</sup> With the implementation of the New Deal, such views became increasingly unpopular, and Austrian cycle theory began to suffer from guilt by association, as for example, Friedman (1974) would later note.<sup>74</sup>

Lionel Robbins’ (1971) retrospective verdict on Austrian theory is worth quoting at some length. On the policy nihilism which he himself had supported in the early 1930s, he commented as follows:

“On the assumption that the original diagnosis of excessive financial ease and mistaken real investment was correct - which is certainly not a settled matter - to treat what developed subsequently in the way which I then thought valid was as unsuitable as denying blankets and stimulants to a drunk who has fallen into an icy pond, on the grounds that his original

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<sup>72</sup>For a more detailed discussion of Austrian policy proposals, See Laidler (1999, pp 46-49)

<sup>73</sup>And the influence of such views had a great deal to do with the indecisiveness of Fed. policy during the Great Contraction, as both Currie (1934), and Friedman and Schwartz (1963) argued. For a contemporary example of such analysis, see Willis (1932). It is also worth noting that, partly, under the influence of Joseph Schumpeter’s own distinctive brand of Austrian analysis, a number of Harvard economists adopted a confused and sceptical attitude to activist policies intended to counter the contraction. See Brown et al. (1934)

<sup>74</sup>See Friedman (1974, pp.163-165) Unfortunately, in this passage, Friedman describes the Austrian view as an “atrophied and rigid caricature” of the quantity theory, thus displaying an extremely uncertain grasp of its intellectual origins.

problem was overheating. I shall always regard this aspect of my dispute with Keynes as the greatest mistake of my professional career, and the book *The Great Depression*, which I subsequently wrote, partly in justification of this attitude, as something which I would willingly see forgotten ” (p. 154)

But he was somewhat kinder to the positive aspects of the theory :

“Now I still think that there is much in this theory as an explanation of a *possible* generation of boom and crisis. I suspect that there are some episodes in economic history, the railway crisis of 1847 and the American crisis of 1907 for instance, on which it can cast valuable light. I am strengthened in this view that something like it, although with a different terminology and a different derivation, figures largely in one of Dennis Robertson’s models. But, as an explanation of what was going on in the early thirties, I now think it was misleading. Whatever the genetic factors of the pre-1929 boom, their *sequelae*, in the sense of inappropriate investments fostered by wrong expectations, were completely swamped by vast deflationary forces sweeping away all those elements of constancy in the situation which otherwise might have provided a framework for an explanation in my terms. The theory was inadequate to the facts ” (p. 154, Robbins’ italics)

Now Robbins’ mention of Robertson in this context is telling, Forced saving (often under the label “involuntary lacking”) does indeed figure prominently in the latter’s interwar work, but not as an explanation in and of itself of the crisis phase of the cycle. Rather, as we shall now see, it appears as one element in an altogether more eclectic story.

### *Robertson’s Approach*

Just as certain political preconceptions underlay Austrian cycle theory, so was it with Robertson’s work. Nowadays he is mainly remembered as an effective critic of Keynes’s (1936) rejection of what the latter called “classical economics” and hence as something of a scientific conservative, but his work is in fact marked by the same scepticism about the self-regulating nature of the market economy and faith in the abilities of well informed policy makers to

remedy this state of affairs as was Keynes's. If he expressed these attitudes with less self-confidence, that had more to do with his personal style than with the depth of his convictions about these matters.

Robertson's cycle theory had continental, and ultimately Marxist, roots. He stressed waves of innovation as the basic source of fluctuations, and unlike those Cambridge economists more closely wedded to the quantity theory, he entertained the possibility that to this extent they were "appropriate" phenomena, whose stabilization might do more harm than good.

"I do not feel confident that a policy which, in the pursuit of stability prices, output and employment, had nipped in the bud the English railway boom of the forties, or the American railway boom of 1869-71, or the German electrical boom of the nineties, would have been on the balance beneficial to the populations concerned." (1926, p. 22).

But in practice, Robertson thought it all too likely that "inappropriate" fluctuations in output would be overlaid upon the appropriate ones by the malfunctioning of the monetary system, and it was in this context that he deployed his analysis of forced saving. Though he developed this quite independently of Mises and Hayek, and indeed a little before the latter took up the phenomenon, his approach had much in common with theirs, as Robbins noted. Even so, in the context of this paper, it the differences between the two versions of the doctrine that merit particular attention.

As we have seen, forced saving occurs when an economy operating at full employment is disturbed by the opening up of a discrepancy between Wicksellian natural and market rates of interest. Although the Austrians recognised that shocks to the system could arise in principle from either the real or monetary side of the equation, in practice they treated the banking system as the main source of disturbances. Not so Robertson, for whom inappropriate fluctuations in output were usually the result of market interest rates failing to catch up with the effects of innovation on the expected returns to investment. Quite unlike the Austrians, moreover, he was even willing to contemplate the possibility that forced saving, though usually destructive, might in some circumstances have a beneficial effect. Specifically, he lacked confidence in the capacity of the short-end of the market automatically to provide the working

capital needed to bring plant and equipment into production once it had been created, and looked to the banking system to provide firms with the means of acquiring it.<sup>75</sup>

“I am not sure that a little forced saving now and again may not be the necessary price we have to pay for what we call progress, and that a doctrinaire application of the principle of price-stabilisation in all circumstances might not be inimical to the rapid growth of economic welfare.” (1928b, p. 145).

Evidently, then, Robertson did not regard forced saving as an *inevitable* harbinger of crisis in the way that the Austrians did. In part, this open-mindedness stemmed from greater analytic sophistication on Robertson’s part. Unlike Mises, Hayek, Robbins, or anyone else in that camp, he understood that, because rising prices would reduce the real value of the public’s cash holdings, “it is at least possible that some of them will seek to restore this real value towards its old level, and to that end to refrain from consumption . . .” (1928b, p. 134) This effect, which in (1926) he had labelled “induced lacking”, is the very one that lies at the heart of analyses of the extraction of seigniorage in modern models of fully anticipated inflation, and in principle it would ensure that the banking system could continuously transfer resources from the public at large to firms without the process breaking down. The saving needed to finance the extra investment would be voluntary, in the sense that it would arise from the payment of what we now call *the inflation tax*.<sup>76</sup>

Even so, Robertson understood just as well as did the Austrians that forced saving could occur in a growing economy without prices having to rise, and he also believed, as did they, that it had considerable potential to do harm. That is

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<sup>75</sup>Robertson’s particular stress on the provision of short-term capital by way of forced saving reflects the practices of the British and American *commercial banking* systems. Any careful comparison of Austrian and Robertsonian treatments of forced saving needs to pay attention to the different assumptions about the nature of banking institutions that were usually left implicit in their analysis.

<sup>76</sup>In this context, Robertson’s acknowledgement that the analysis of induced lacking was suggested to him by Keynes is worth noting, since the latter had already set out a treatment of the inflation tax in the *Tract* (1923). On this See Laidler (1999, pp.95-96, pp.110-111). Harry Johnson (1974) pointed out the relationship between the analysis of forced saving as developed by Robertson in the 1920s, and that of the inflation tax, seigniorage and the revenue from money creation (the three terms are synonymous). The latter topic was much discussed among monetary theorists for two decades following the work of Martin Bailey (1956) and a key contribution which either avoided or cleared up the many analytic confusions that permeated much earlier work was Alvin Marty (1976)

why, in 1928, he expressed trepidation at the way events were unfolding in the United States: “. . .in so far as the Federal System [sic] has not gone all out for stabilising the price of *labour*, it cannot, I think, be wholly absolved from the charge of having burgled from the public in these years of rapidly advancing productivity” (1928b, p. 144, Robertson’s italics). He particularly feared for the consequences “if that great country *should* ever become even temporarily saturated with 50-storey buildings and motor cars” (p. 146 Robertson’s italics), noting that

“The out-and-out price-stabiliser claims that he can always check a fall in prices and cure unemployment by monetary means . . . I think the difficulties experienced by the Federal Reserve system even in times of raging prosperity should make us pause before admitting such extreme claims.” (1928b, p. 146)

Not that Robertson came anywhere near to Austrian nihilism in his prescriptions for policy in the event of a crisis. On the contrary he was just as much an activist as any of the Cambridge economists discussed earlier in this paper<sup>77</sup>.

“. . .the ideal banking policy might be one which was founded on the principle of price-stabilisation as a norm, but which was ready to see the fruits of a prolonged and general increase in individual productivity shared in the form of lower prices, and perhaps to acquiesce in moderate price-rises in order that advantage might be taken of discontinuous leaps in industrial technique. And it would be a policy that did not claim omnipotence, or feel competent of its ability to cure the evils of uncertainty except in alliance with a much more comprehensive attempt to control and stabilise the desires and activities of the community than most monetary reformers - even I think, the most thorough-going Socialists - have yet visualised” (1928b, p. 146)

Robertson’s avoidance of Austrian policy conclusions about how to deal with crisis and the depression which would inevitable follow it, despite his belief in the capacity of forced saving to generate excessive investment, has a very

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<sup>77</sup>It should be noted that, just as Pigou came to pay increasing attention to forced saving as the 1920s progressed, so did Robertson appreciate the potential significance of error. See Laidler (1999, pp.89-90, 98-99)

simple and down-to-earth explanation, namely that he invariably associated such investment with the over-expansion of particular sectors of the economy, rather than with the creation of an economy-wide imbalance in the capital stock. He therefore believed that there was always scope to fill any void in demand stemming from a private sector collapse, as his following comment on the desirability of deploying counter-cyclical public expenditure policies makes clear.

“What, after all, can be more sensible than that the Central government should organise a collective demand for telephone equipment, or the local governments a collective demand for municipal lavatories, to take the place of an individual demand for ships or steel rails, which has rightly and reasonably fallen temporarily away?” (1928a, p. 178)

### ***Some Tentative Lessons***

To draw conclusions from an earlier literature about current monetary policy is always risky. No matter what some might claim about the universality of the laws of economic science, it has often been observed that monetary economics in particular evolves along with monetary institutions, and the possibility that conclusions drawn at an earlier time, might have lost their validity with the passage of time is ever present.<sup>78</sup> But recent bouts of asset market instability and associated problems in the real economy, do seem to present puzzles that contemporary economic theory has trouble getting to grips with, and they do bear more than a passing resemblance to earlier episodes, so perhaps the literature discussed in this paper might have something to tell us about our present problems.

### ***Continuity and Discontinuity in Economic Ideas***

As we saw above, there is considerable continuity in what has here been called the quantity-theoretic approach to the cycle in particular, and asset market instability in particular. Its current exponents still argue that monetary policy cannot be expected to do more than influence the price level, and the differences

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<sup>78</sup>This was a constant theme in the later writings of Sir John Hicks, right down to his final book (1989), and it permeates the post-Keynesian writings of Victoria Chick: eg (1992)

between their advocacy of targets for low inflation and the preferences of, say Marshall, Hawtrey or the Keynes of the *Tract*, for outright price level stability are surely of minor significance to anyone who believes that the virtues of such policy goals should be analysed on the assumption that the inflation rate is fully anticipated. Furthermore, they are every bit as insistent on the rapid deployment of the central bank's powers of money creation, should things nevertheless go wrong, as their inter-war predecessors.

There is less continuity between the ideas of inter-war and contemporary critics of the quantity theoretic tradition. Like their predecessors, of course, the latter are still concerned with the apparent inability of a monetary policy geared solely to the general price level to forestall instability in asset markets, and they also suggest that monetary policy should try to deal with this phenomenon. But beyond this point, differences begin to appear. Nowadays, the case that monetary policy should concern itself with asset prices is mainly justified by the possibility that banking systems can cease to function efficiently as providers of credit to the private sector once asset market instability turns into outright crisis. Interwar economists did not neglect instability in asset markets or the banking system in particular, or indeed financial markets more generally, but dissenters from quantity theoretic analysis among them paid far more attention to the possibility that such difficulties reflected altogether deeper problems with the structure of the real economy. The Austrians aside, moreover, they adopted a far more activist approach to macroeconomic policy than anything that is to be found among critics of current policy orthodoxy, whose main concern seems to be that monetary policy should look beyond stabilising the inflation rate, and pay special attention to asset prices.

### *Co-ordination Failures*

I conjecture that the reason for this latter difference, and also for our current perplexity about how monetary policy should be configured in the light of the experience of Japan, or more recently the United States, to mention but two examples, stems from the fact that much of economics as it is now widely taught has tried to settle an old debate about a matter of real substance by methodological *fiat*. That old debate, whose importance Axel Leijonhufvud (eg. 1981, 1999) has long stressed, reached a peak in the inter-war years. It was about whether, and, if not, the extent to which, the institutions of a market

economy were up to the task of harmoniously co-ordinating the maximising decisions of self-interested individual agents. Nowadays, this debate is short-circuited by the widely held belief that good scientific practice requires us to assume that markets always clear, unless specific reasons rooted in maximising behaviour conditioned on all relevant and available information can be given for assuming otherwise.<sup>79</sup> This belief has led much economic theory to take it for granted that the interest rate, or the term structure thereof, successfully co-ordinates the allocation of resources over time, even though this assumption makes it very hard to understand such phenomena as canals and railways that lead to nowhere, see-through office buildings, mile upon mile of redundant fibre-optic cable, etc.

In earlier times, when, by and large, economists started with facts to be explained, and worked backwards to more and more general explanations, it was regarded as methodologically legitimate to postulate co-ordination failures to account for observations without first of all having to rationalise these in terms of some set of deeper (and allegedly first) principles. And given the facts of 19<sup>th</sup> century fluctuations, let alone those of the inter-war years, this seemed like a reasonable procedure. The key positive question underlying the literature discussed in this paper was, that is to say, what sort of co-ordination failure was responsible for observed fluctuations, and the key normative question was what policy should do about it. These seem to me to remain interesting questions, even if the current rules of the game make it embarrassing to pose them in respectable academic circles.

Exponents of a quantity-theoretic approach to fluctuations located co-ordination failures in the slowness of interest rates and money wages to respond to swings in the price level, an argument implying that the maintenance of price stability would prevent their occurrence. Some of these exponents, for example Fisher, were sufficiently confident of this way of looking at things that they argued that a price stability rule would be sufficient to guarantee overall

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<sup>79</sup>But not by everyone: I have already mentioned Victoria Chick's contribution to the post-Keynesian literature, and Axel Leijonhufvud's constant and eloquent insistence of the importance of co-ordination issues and to the rich inter-war literature dealing with them, continues to do so). Garrison (2001) to which I have also referred above represents a recent and refreshingly unorthodox attempt to blend insights culled from quantity theoretic and Austrian approaches to the cycle into an account of the phenomenon that places co-ordination failures at the centre of things. But students trained in mainstream modern macroeconomics find it hard to come to grips with such contributions because they are not susceptible to the equilibrium modelling techniques in which they are so well drilled.



economic stability. Others, for example Hawtrey and those whom he influenced, were more cautious and were willing to settle for discretionary policy. Furthermore, after the events of 1928-1930 had confirmed that things could nevertheless go badly wrong against a background of apparently stable inflation, they continued to argue that vigorous monetary expansion would suffice to prevent a cyclical downturn getting out of hand. Austrian theorists, on the other hand, stressed the capacity of any monetary expansion to generate forced saving, urged that the authorities should try to stabilize the level of money expenditure to prevent this happening, and, took a fatalistic attitude to the consequences of failure. Quantity theorists and Austrians nevertheless had an important belief in common, namely that malfunctions of the monetary system, rather than any deeper flaw in the workings of market mechanisms underlay co-ordination failures, and that appropriate (albeit very different) monetary measures could in principle prevent them.

In this these two groups differed strongly from those who argued that the very process of economic growth in a market economy was inherently prone to such failures. These arguments came in many disguises, ranging from the Pigou-Lavington hypothesis that investment behaviour was systematically subject to error, to an altogether more thoroughgoing Marxist belief, as represented, say, by Loewe, in the instability of Capitalism. Commensurate with their playing down the importance of the monetary system itself as a source of instability, these economists cast doubt on the sufficiency of any purely monetary cure for such problems, and many of them, for example the Cambridge economists, including Robertson, and eventually Keynes, advocated an activist stabilisation policy with a strong fiscal component aimed in particular at dealing with the consequences of instability in investment.

### *Implications for Present Day Monetary Policy*

Certain facts of economic history can help us to make tentative choices among these competing views. To begin with, in their light, it seems unlikely that traditional quantity theoretic explanations of the cycle tell the whole story. One episode of serious financial instability occurring without a prior burst of general price inflation, such as that which preceded the Great Depression, might be written off as an anomaly, but, as was noted at the outset of this paper, we have seen too much of this recently not to take seriously the possibility that price

stability alone is not a sufficient condition for more general stability. But it seems even less likely that Austrian analysis tells the whole story either. As was noted earlier, the account of the cycle given by Mises, Hayek, Robbins et al. treated logical possibilities as logical necessities and seriously oversold its central message.

But, that being said, inter-temporal co-ordination failures aided and abetted by the monetary system are both logically possible, and they do seem to happen. Quantity theoretic models of the cycle certainly have long had room for them to occur when nominal interest rates fail to keep up with inflation, and hence permit the real market rate of interest to fall short of the real natural rate. But these failures also sometimes happen in the absence of inflation: how else does one account for all that surplus fibre-optic cable? When all the over-generalizations are stripped away from Austrian theory, there still remains a hard core of insight: namely, that discrepancies between the market and natural rates of interest with a capacity for damaging the real economy can arise even in the absence of inflation. This insight seems to be valid far beyond Austrian theory's own narrow and ideologically drawn boundaries, as Robertson's work clearly showed, as Robbins later acknowledged, and as Garrison has more recently reaffirmed. Nor, incidentally, is it obviously incompatible with a quantity theoretic analysis that is constructed broadly enough to allow productivity shocks and/or what the Cambridge economists called errors of optimism, to happen against a tranquil monetary background and independently of the activities of the banking system.

At first glance, it is hard to resist the conclusion that a monetary regime geared solely towards generating low inflation is not in and of itself sufficient to guarantee economic stability, to allow that monetary policy needs to become more ambitious, and perhaps even to concede that a more generally activist approach to macro-stabilization needs to be re-considered. Some of us, who remember where policies like that led from the 1960s to the 1980s, however, are bound to resist this last step, and perhaps even the one that precedes it too, and for the present, we can take some comfort from certain other facts. After all, vigorous monetary expansion, based on the central bank's lender of last resort powers, was not tried after the stock-market collapsed 1929, nor was such a policy implemented after the Japanese "bubble economy" came to grief in 1991.

Thus, it is not clear from these episodes that a traditional quantity theoretic approach to monetary policy is inadequate.

At the time of writing, moreover, after the collapse of the high-tech bubble of the late 1990s, the Federal Reserve system does seem to be taking the advice of Hawtrey, Currie, Friedman and Schwartz, et al. to heart. Perhaps, then, timely monetary expansion will succeed in staving off stagnation in the U.S., and if it does, then the systematic pursuit of low inflation by central banks which are also ready to be lenders of last resort when things go wrong in asset markets, will be a defensible monetary policy regime after all. If it does not, then we shall indeed have to consider something more ambitious. Here, it is worth recalling that regulatory frameworks for financial markets that we take for granted nowadays are mainly products of the 1930s and after, and would surely have looked like the creations of “thorough-going Socialists” to most of Robertson’s readers in 1928. There may be room to deploy these more actively than has been done in the recent past to stabilise asset markets, when the latter threaten to create dislocations in the real economy, while continuing to rely on low inflation as the centrepiece of monetary policy. This would certainly be worth trying, because, if the more generalized activism, that Robertson and his colleagues so confidently recommended were ever again to re-emerge as the alternative of choice to current policies, we could not be nearly as sure as were they were that it would end up doing more good than harm this time around.

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## **Index of Working Papers:**

January 2, 2002	Sylvia Kaufmann	56	Asymmetries in Bank Lending Behaviour. Austria During the 1990s
January 7, 2002	Martin Summer	57	Banking Regulation and Systemic Risk
January 28, 2002	Maria Valderrama	58	Credit Channel and Investment Behavior in Austria: A Micro-Econometric Approach
February 18, 2002	Gabriela de Raaij and Burkhard Raunig	59	Evaluating Density Forecasts with an Application to Stock Market Returns
February 25, 2002	Ben R. Craig and Joachim G. Keller	60	The Empirical Performance of Option Based Densities of Foreign Exchange
February 28, 2002	Peter Backé, Jarko Fidrmuc, Thomas Reininger and Franz Schardax	61	Price Dynamics in Central and Eastern European EU Accession Countries
April 8, 2002	Jesús Crespo-Cuaresma, Maria Antoinette Dimitz and Doris Ritzberger-Grünwald	62	Growth, Convergence and EU Membership
May 29, 2002	Markus Knell	63	Wage Formation in Open Economies and the Role of Monetary and Wage-Setting Institutions
June 19, 2002	Sylvester C.W. Eijffinger (comments by: José Luis Malo de Molina and by Franz Seitz)	64	The Federal Design of a Central Bank in a Monetary Union: The Case of the European System of Central Banks
July 1, 2002	Sebastian Edwards and I. Igal Magendo (comments by Luis Adalberto Aquino Cardona and by Hans Genberg)	65	Dollarization and Economic Performance: What Do We Really Know?
July 10, 2002	David Begg (comment by Peter Bofinger)	66	Growth, Integration, and Macroeconomic Policy Design: Some Lessons for Latin America



July 15, 2002	Andrew Berg, Eduardo Borensztein, and Paolo Mauro (comment by Sven Arndt)	67	An Evaluation of Monetary Regime Options for Latin America
July 22, 2002	Eduard Hochreiter, Klaus Schmidt-Hebbel and Georg Winckler (comments by Lars Jonung and George Tavlas)	68	Monetary Union: European Lessons, Latin American Prospects
July 29, 2002	Michael J. Artis (comment by David Archer)	69	Reflections on the Optimal Currency Area (OCA) criteria in the light of EMU
August 5, 2002	Jürgen von Hagen, Susanne Mundschenk (comments by Thorsten Polleit, Gernot Doppelhofer and Roland Vaubel)	70	Fiscal and Monetary Policy Coordination in EMU
August 12, 2002	Dimitri Boreiko (comment by Ryszard Kokoszcyński)	71	EMU and Accession Countries: Fuzzy Cluster Analysis of Membership
August 19, 2002	Ansgar Belke and Daniel Gros (comments by Luís de Campos e Cunha, Nuno Alves and Eduardo Levy-Yeyati)	72	Monetary Integration in the Southern Cone: Mercosur Is Not Like the EU?
August 26, 2002	Friedrich Fritzer, Gabriel Moser and Johann Scharler	73	Forecasting Austrian HICP and its Components using VAR and ARIMA Models
September 30, 2002	Sebastian Edwards	74	The Great Exchange Rate Debate after Argentina
October 3, 2002	George Kopits (comments by Zsolt Darvas and Gerhard Illing)	75	Central European EU Accession and Latin American Integration: Mutual Lessons in Macroeconomic Policy Design
October 10, 2002	Eduard Hochreiter, Anton Korinek and Pierre L. Siklos (comments by Jeannine Bailliu and Thorvaldur Gylfason)	76	The Potential Consequences of Alternative Exchange Rate Regimes: A Study of Three Candidate Regions

October 14, 2002	Peter Brandner, Harald Grech	77	Why Did Central Banks Intervene in the EMS? The Post 1993 Experience
October 21, 2002	Alfred Stiglbauer, Florian Stahl, Rudolf Winter-Ebmer, Josef Zweimüller	78	Job Creation and Job Destruction in a Regulated Labor Market: The Case of Austria
October 28, 2002	Elsinger, Alfred Lehar and Martin Summer	79	Risk Assessment for Banking Systems
November 4, 2002	Helmut Stix	80	Does Central Bank Intervention Influence the Probability of a Speculative Attack? Evidence from the EMS
June 30, 2003	Markus Knell, Helmut Stix	81	How Robust are Money Demand Estimations? A Meta-Analytic Approach
July 7, 2003	Helmut Stix	82	How Do Debit Cards Affect Cash Demand? Survey Data Evidence
July 14, 2003	Sylvia Kaufmann	83	The business cycle of European countries. Bayesian clustering of country-individual IP growth series.
July 21, 2003	Jesus Crespo Cuaresma, Ernest Gnan, Doris Ritzberger-Gruenwald	84	Searching for the Natural Rate of Interest: a Euro-Area Perspective
July 28, 2003	Sylvia Frühwirth-Schnatter, Sylvia Kaufmann	85	Investigating asymmetries in the bank lending channel. An analysis using Austrian banks' balance sheet data
September 22, 2003	Burkhard Raunig	86	Testing for Longer Horizon Predictability of Return Volatility with an Application to the German DAX
May 3, 2004	Juergen Eichberger, Martin Summer	87	Bank Capital, Liquidity and Systemic Risk
June 7, 2004	Markus Knell, Helmut Stix	88	Three Decades of Money Demand Studies. Some Differences and Remarkable Similarities
August 27, 2004	Martin Schneider, Martin Spitzer	89	Forecasting Austrian GDP using the generalized dynamic factor model
September 20, 2004	Sylvia Kaufmann, Maria Teresa Valderrama	90	Modeling Credit Aggregates

Oktober 4, 2004	Gabriel Moser, Fabio Rumler, Johann Scharler	91	Forecasting Austrian Inflation
November 3, 2004	Michael D. Bordo, Josef Christl, Harold James, Christian Just	92	Exchange Rate Regimes Past, Present and Future
December 29, 2004	Johann Scharler	93	Understanding the Stock Market's Response to Monetary Policy Shocks
Decembert 31, 2004	Harald Grech	94	What Do German Short-Term Interest Rates Tell Us About Future Inflation?
February 7, 2005	Markus Knell	95	On the Design of Sustainable and Fair PAYG - Pension Systems When Cohort Sizes Change.
March 4, 2005	Stefania P. S. Rossi, Markus Schwaiger, Gerhard Winkler	96	Managerial Behavior and Cost/Profit Efficiency in the Banking Sectors of Central and Eastern European Countries
April 4, 2005	Ester Faia	97	Financial Differences and Business Cycle Co-Movements in A Currency Area
May 12, 2005	Federico Ravenna	98	The European Monetary Union as a Commitment Device for New EU Member States
May 23, 2005	Philipp Engler, Terhi Jokipii, Christian Merkl, Pablo Rovira Kaltwasser, Lúcio Vinhas de Souza	99	The Effect of Capital Requirement Regulation on the Transmission of Monetary Policy: Evidence from Austria
July 11, 2005	Claudia Kwapil, Josef Baumgartner, Johann Scharler	100	The Price-Setting Behavior of Austrian Firms: Some Survey Evidence
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