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# Monetarism - The Unfinished Business

David Laidler

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# PAPERS IN POLITICAL ECONOMY

**Paper No. 29**

**“Monetarism – The Unfinished  
Business”**

**David Laidler**

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**MONETARISM - THE UNFINISHED BUSINESS**

by

**DAVID LAIDLER**

**The 1992 Cyprus Economic Society Annual Lecture to be delivered on September 30, 1992.**

## I.

When someone is described as a "monetarist" these days, it is seldom meant as a compliment. To the lay critic, monetarism is an amalgam of economic policy positions, associated with conservative governments, whose alleged legacy includes high unemployment and the decay of social programmes. To the academic, monetarism is a set of obsolete economic doctrines which, though they may have served a purpose in the past, have now been discredited by empirical evidence and overtaken by new theoretical developments. But we do not generally pin labels on our own lapels, and those of us who have been classified as "monetarists" for the last two decades have no alternative but to try to make that label mean what we, rather than our critics, would like it to mean. And that is what this lecture is about. In it, I shall first of all describe what traditional academic monetarism was, and defend it against charges of error and obsolescence. I shall also have something to say about its "guilt by association" with conservative politics.

But most important of all, I shall draw attention to elements in the monetarist literature which, to my mind, not only render it a "progressive research agenda" to use the currently fashionable Lakatosian phrase, but also suggest that it is in fact nothing more nor less than the latest manifestation of that same ongoing tradition in monetary economics to which Thornton, Ricardo, Marshall, Wicksell, and Keynes, among many others have contributed. The "unfinished business" of my title, indeed, is to draw attention to this interpretation of the doctrine, get it debated, and if it is correct, get it widely accepted too.<sup>1</sup>

## II.

One cannot simultaneously characterise monetarism as a progressive research agenda, and then offer a definition of it which will fit at all times and places. A progressive agenda

does make progress. It will nevertheless be helpful to begin by describing what the doctrine looked like a little over two decades ago when it was named, almost simultaneously, by Karl Brunner (1968) and Nicholas Kaldor (1970). Such a description may be organised in terms of the then popular IS-LM model of the macroeconomy's short run (i.e. abstracting from growth) behaviour, and may be cast in terms of four propositions, three positive and one normative.

First and foremost among these was Friedman's (1956) contention that the demand for money function in the economy was a stable function of a few arguments. Second was the argument, most thoroughly developed by Brunner and Meltzer (eg. 1976) that the supply of money was controllable by the central bank. Third was the contention that the IS-LM model's inability to allocate aggregate fluctuations between real output and employment on the one hand, and the general price level on the other, could be made good by supplementing it with an expectations augmented Phillips curve whose distinguishing characteristic was the absence of any long run trade-off between these variables.<sup>2</sup> Finally came the normative proposition that, because the dynamics of the interaction of money and the macroeconomy were subject to long and variable time lags, and because the dominant shocks disturbing the economy's equilibrium were in any event monetary, it was desirable to govern the behaviour of the money supply by a constant growth rule, chosen in the light of the economy's real growth rate and the real income elasticity of demand for money so as to ensure negligibly low inflation.<sup>3</sup>

Now when we put it this way, monetarism appears to be a largely value neutral body of doctrine. Its positive elements were open to empirical test, and if they failed such a test, it is hard to see how even the most hardened ideologue could have persisted in defending its normative element. This is, and remains, my own position on the matter. Nevertheless, from the outset there was a strong ideological element about the monetarist controversy, and if one

looks a little more closely at the positive content of monetarism, one can see why. An economy characterised by a controllable (and steadily growing) nominal money supply, along with an expectations augmented Phillips curve, will have its only equilibrium at the "natural" unemployment rate, and will, absent perverse dynamics, converge upon that equilibrium, in the wake of disturbances.

It is possible to mount a critique of market mechanisms based upon private property while conceding that they do not lead to economic chaos, but it is far easier to do so, and the critique is far more compelling, if it can be established that those mechanisms are inherently unstable; and the actual experience of the 1920s and 30s had persuaded many people that they were. What came to be called "Keynesian economics" provided both a non-marxist analysis of that instability and a set of policy doctrines designed to cope with it. To its adherents, Kaldor (1970) is a prime example, monetarism, which attributed the experience of the interwar years to monetary mismanagement, looked like a throwback to the Austrian analysis of Hayek (1931) and Robbins (1934) who had argued for policy inaction as the best medicine for the Great Depression. Small wonder that anyone brought up on Keynesian economics and its diagnosis of the Depression found monetarism profoundly disturbing. And on the other side of the same coin, anyone prone to support market mechanisms for ideological reasons, could not fail to find attractive a doctrine which predicted that such mechanisms would in the past have produced full employment and low inflation, and were capable of doing so in the future as well, if governments would only deliver stable monetary policy and otherwise leave well alone.

Thus, deeply held and politically important beliefs, which had of course been matters of controversy long before the 1930s - consider the Ricardo-Malthus debates about Say's Law - were at stake in the monetarist controversy, and it is not surprising that market oriented

politicians of the stripe of Mrs. Thatcher and Mr. Reagan should adopt, and sometimes ostentatiously so, variations on monetarist themes in their macroeconomic policies. But we must not over-simplify. It was the Callaghan, not the Thatcher government, that introduced money-growth targets to Britain, and before, not after an IMF visit; and Paul Volker was appointed by President Carter, not Reagan. Moreover, Mrs. Thatcher was still prime minister during the Lawson boom, while Reagan's macroeconomic policy seems to have had far more to do with "supply side" pop-economics than with the serious analysis of Friedman or Brunner and Meltzer. If monetarism's reputation has suffered from the policy experience of the 1980s, that is more the result of guilt by association than of the failure of any carefully executed policy experiment; and indeed, over that decade, where money growth was kept down, or reduced, so was inflation - a result quite in keeping with monetarist doctrine.

Monetarism, in short, could have survived its association with the policy experience of the 1980s rather easily had it not simultaneously lost much of its academic credibility. It is to this matter that I now turn.

### III.

Monetarism, I have argued, was at heart a body of positive economic doctrine, and as such it was a fair target for criticism with respect both to its empirical content and its logical coherence. These are the normal criteria employed in scientific debate, and it is no disgrace if a set of propositions fails to measure up to them. Indeed it would have been a miracle, never before seen in the history of economic thought, had the monetarism of 1970 vintage withstood all attempts to refute or modify it. Nor would it necessarily have been a welcome miracle. A research agenda is an agenda for creating new knowledge, and the discovery of flaws in existing



doctrine is a necessary part of that process. Though current work on hysteresis phenomena in labour markets may yet undermine it, the monetarist hypothesis about the absence of a long run inflation-unemployment trade off proved remarkably durable, and became a central tenet of the New-Classical economics which succeeded monetarism in the 1980s and 1990s. Friedman's hypothesis about the simplicity and stability of the demand for money function proved less robust, and gave rise to much controversy, as we shall now see.

This hypothesis was surely the cornerstone of early monetarism. Even so, Friedman's (1959) suggestion that the demand for real money balances depended stably on real permanent income and nothing else did not long survive empirical scrutiny. By 1970 it was clearly apparent that some measure of the opportunity cost of holding money belonged in the relationship, thus reopening the possibility of IS curve shocks being a source of economic fluctuations, and of fiscal policy having a stabilisation role to play as well, and hence doing much to blur the distinction between monetarism and the macroeconomic orthodoxy it sought to replace. It was also by then apparent that the lag patterns in the data, which the permanent income hypothesis explained in terms of error learning, could equally well be explained by the presence of adjustment costs in an otherwise rather conventional "Keynesian" demand for money function. In short, this key component of monetarism had generated much fruitful work by 1970, so much so that it was ready to be absorbed into mainstream IS-LM macroeconomics, which it duly was in the next decade.<sup>4</sup> This was not to the ultimate benefit of monetarism, and for two reasons.

First, at the hands of mainstream macroeconomists, working with large scale econometric models largely designed with a view to generating forecasts useful to stabilisation policy, considerable emphasis came to be placed upon the short-run stability of the demand for money

function. Friedman, on the other hand, had emphasised its long run properties, and indeed had used cycle average data (he would later use cycle phase average data) to establish them. And this mattered, because a demand for money function stable on a quarter by quarter basis seemed to provide a sound basis for the design of an activist approach to monetary policy of which no monetarist could, or indeed did, approve.

Second, as Brunner and Meltzer frequently, and correctly, complained, the IS-LM model is inadequate as a device for analysing monetary policy.<sup>5</sup> It leads to the view that, if only the monetary authorities use some representative interest rate as their policy instrument, the quantity of money becomes not merely an endogenous variable, but a passively demand determined variable. The IS-LM framework appeared to suggest that, given a stable demand for money function, monetary policy was appropriately conducted by: setting a target for the quantity of money in circulation; estimating the current values of all arguments of the function, say real income and the price level, (along with appropriately chosen lagged values too, to allow for adjustment phenomena); and then setting a current value for the rate of interest to ensure that the target value of the money supply would be demanded by the public. To put it charitably, policies so conducted did not work very well, leading mainstream macro-economists to reject the monetarist hypothesis of a stable aggregate demand for money function at least as quickly as they had adopted it. This was, in my view, a mistake; for as I shall suggest below, the problem in all likelihood arose from paying insufficient attention modelling the money supply generating mechanism, rather than from instability in the demand function - though there were some problems here, to be sure.

The results which recent econometric literature on the demand for money function has generated are relevant to this judgement. There, the application of co-integration techniques to

time series data for a number of countries has permitted us to test hypotheses about the steady state properties of the function - the very properties which monetarist doctrine stressed from the outset - without simultaneously having to take a position about the mechanisms driving the short run dynamics of relevant variables about that steady state, and hence without their results being subject to distortion by errors in modelling those short run dynamics. Overwhelmingly, the application of these techniques confirms the stability of long-run demand for money functions, though it also reveals that they are, particularly those for narrowly defined aggregates, subject to occasional shifts stemming from institutional changes in the financial system, sometimes associated with regulatory changes and sometimes with technical developments, such as those associated with the adoption of computer technology.<sup>6</sup>

These results make it difficult to argue for rigid money growth rate rules, and hence provide the prime example of how monetarist doctrine of an earlier vintage has had to be modified in the light of subsequent debate. However they leave untouched, indeed they strengthen, the presumption that medium term money growth targets (open to revision as and when institutional developments require it) are a highly desirable basis for monetary policy. Moreover, and crucially in the current context, they tell us that the instability problems which created so much skepticism about the demand for money function in the 1980s, and did so much to prompt a premature abandonment of monetarist hypotheses among macroeconomists, stemmed in large measure from an inadequate handling of the adjustment dynamics of the short-run relationship.

Though I would like to argue that these results also imply beyond reasonable doubt that my earlier suggestion that the major source of difficulties with the demand for money function arose not from troubles with that relationship, but from neglect of the mechanisms determining

the supply of money, I can not defend so strong a position. What I can do, however, is offer arguments in support of it which are firmly rooted in traditional monetary theory. I can also explain why this line of argument is disturbing both to adherents of new-classical economics and to orthodox Keynesians, and therefore why it is proving so difficult to get it taken seriously. As I shall now argue, getting attention paid to these matters constitutes an important part of the "unfinished business" of monetarism to which the title of this lecture refers.

#### IV.

To treat the quantity of money in circulation as measuring the quantity of money demanded, as the vast majority of studies of the demand for money do, is to assume that the economy is "on" its demand function for money. This assumption is probably adequate when dealing with long-run relationships but not in the short run. That is why I believe that our inability to model the short run in a satisfactory fashion stems from our inadequate understanding of the mechanisms whereby the supply and demand for money are brought into equilibrium with one another in the wake of disturbances. To say this is, of course, also to say that the conventional treatment of the supply of money as responding passively to changes in its demand, a treatment which seems so natural when IS-LM analysis is applied to a policy regime in which the authorities treat an interest rate as their policy instrument, is erroneous.

The issue at stake here is not simply a matter of the endogeneity or exogeneity of the quantity of money, much though this matter has been debated. It is true enough that a great deal of monetarist theorising has begun from the assumption of an exogenously given quantity of money, and I readily concede that such an assumption can be descriptively accurate only of an imaginary world. In the world we live in, money is created by a banking system, and its

quantity does respond to impulses stemming from the economy, as well as to those imparted by the authorities. The quantity of money is, then, beyond doubt an endogenous variable. But an argument by analogy with a simple Marshallian partial equilibrium supply and demand apparatus will soon show that to concede this point settles next to nothing.

In a Marshallian market, taking it for granted that quantity demanded depends inversely on price, quantity supplied is only an exogenous variable if the supply curve is vertical. If it is not, then quantity supplied is certainly an endogenous variable; but an upward sloping supply function may still be subject to shifts which occur independently of factors affecting the demand curve, and reasonable predictions about the consequences of those shifts for market price might nevertheless be made while neglecting the upward slope of the supply curve and treating it "as if" vertical. The monetarist position, then, is not that the quantity of money is exogenous, but rather that its supply curve is an independent relationship which can be shifted by factors under the control of the authorities, and that the simplifying hypothesis that the quantity of money behaves "as if" an exogenous variable might be worth maintaining for some purposes and in some circumstances (but not when trying to estimate a short run demand for money function, as my earlier remarks should make clear).

But suppose we carry the Marshallian market analogy a step further; suppose we picture the limiting case of such a market in which suppliers simply set the price and then supply any quantity demanded at that price. Then indeed quantity is not just an endogenous, but a completely passive, variable, and the only independent behaviour relationship in the market is the demand curve. This is surely the kind of thing that exponents of orthodox interpretations of the consequences of central banks treating the rate of interest as a policy instrument have in mind when they deny the existence of an independent supply of money function. To make use

of Basil Moore's (1988) helpful labels, they are "horizontalists" just as monetarists are "verticalists" - the qualifier "as if" is understood to apply in each case. There are good reasons, however, for thinking that our analogy becomes misleading at just this point, because partial equilibrium analysis does not, when all is said and done, quite fit the case of money. There is another market involved here, which cannot be ignored, and consideration of which changes the picture. I refer, as readers of Brunner and Meltzer will have already guessed, to the market for debt, or as they call it, credit.<sup>7</sup>

The "horizontalist" view of the money supply process would have it that, by changing the price at which it stands ready to buy and sell debt, the banking system changes the value of the interest rate argument in the public's demand for money function, and the public exchange debt with the banks in order to restore their cash balances to equilibrium. It also has it that, to the extent that the changed interest rate has effects on other variables, such as output and prices, the changes in the demand for money induced by these will be satisfied by similar means. It also has it that, in the limiting case of a zero interest elasticity of demand for money, open market operations become quite impossible, so that traditional pre-Keynesian stories about the transmission of monetary policy, and some simplified monetarist stories too, told in terms of such a demand function, are seriously flawed. Such views can be found in the writings of many distinguished monetary economists - Hahn, Kaldor, Hicks, and indeed Keynes himself - so it is no light matter to disagree with them. But, as any monetarist must, disagree I do.

When the banking system changes the price at which it stands ready to buy and sell debt - raises it, say, for the sake of concreteness - it disturbs the margin between debt and physical capital (and indeed current consumption too) as well as that between money and debt. That is what Brunner and Meltzer mean when they say that such a step affects the market for

credit as well as the market for money. Specifically, the non-bank public may now want to hold more cash, and may be willing to offer debt to the banks in order to obtain it, but they will also want to acquire more goods in exchange for a further reduction in their holdings of debt. In a barter economy, in which "money" was nothing more than a pure non-interest-bearing store of value, there would, presumably, be a reshuffling of the balance sheets of firms and households to accommodate this change, but, in an economy characterised by monetary exchange, matters are radically different.

In order to acquire the money to buy the extra goods they now demand, agents must be net sellers of debt, and since the banking system is the residual buyer of debt this operation will involve the creation of money over and above that required to satisfy the increased demand for money. An excess supply of money is thus created, which, according to monetarist analysis will have its own impact on the demand for goods and services over and above the direct first round effects of the public's initial substitution of goods for debt. It is also worth pointing out explicitly that, in this view of the matter, the banking system's capacity to create money does not depend upon the existence of an interest elastic demand for money function. The latter phenomenon is simply a complicating factor in the mechanics of open market operations, not their *sine qua non*.

It should be emphasised that none of this is to deny that the initial substitution between debt and goods can have a significant impact upon aggregate demand. It is however to claim that these are first round effects, and to insist that the disequilibrium between the supply and demand for money that arises as the by-product of these first round transactions is also, and additionally important. It is not, however, claimed that the quantity of money created in these transactions must all remain in circulation ever afterwards. Some agents with excess cash

holdings might well find it desirable to reduce indebtedness to the banks and thereby extinguish the money thus utilised. It is, however, reasonable to point out that, even for agents thus indebted, this is by no means the only option available for reducing excess cash holdings, while agents who are not indebted to banks are also likely to be among those finding themselves with excess cash.

Any attempt to adjust cash balances in other ways will of course affect the demand for goods and services, and will continue to do so until such variables as output and the price level have changed enough to lead to the money which remains in circulation being willingly held. Thus, I am claiming here that traditional cash balance mechanics, of a type implicit in the analysis of Thornton and Ricardo, and explicitly analysed in varying degrees of detail by Mill, Marshall and Irving Fisher, among others still has a role to play in helping us understand the interaction of money and economic activity even in the presence of a banking system in which the rate of interest is the policy instrument.<sup>9</sup> Moreover, I would also claim that these second and subsequent round effects are of more empirical significance than those that arise from the first round substitution between debt and goods.

Let me draw explicit attention to the presence of the adjective "empirical" in the last sentence. *A priori* argument of the type presented here can establish at best the qualitative presence of effects, but not their quantitative significance. It could be that the propensity of the private sector to use excess cash to reduce indebtedness to the banks is always and everywhere so high that first round effects dominate the consequences of monetary policy, and that fluctuations in the observed quantity of money in circulation are indeed dominated by variations in the factors affecting the demand for money. If these things were so, though, we would have to explain how, during the 1980s in a number of countries, when it has been uncontroversial



that tight monetary policy has precipitated two recessions, the behaviour of monetary aggregates, particularly narrow ones, has led that of real economic activity and prices. Such timing is hard to reconcile with the idea that the quantity of money in circulation responds passively to arguments in its demand function.

And this brings us back to the empirical problem from which this section of my lecture began. If the dynamics driving the variations about their steady state in the relationships among money, interest rates, output and prices are the outcome of a complex transmission mechanism involving the interaction of the arguments of independent supply and demand for money functions, it is hardly surprising that attempts to model them as if they reflected only the properties of a demand function have led us nowhere. It is surely an important piece of unfinished business to investigate this question. But before we can expect much progress here, an important roadblock needs to be removed, namely the strongly held preferences of some of our discipline's most distinguished practitioners for a certain type of equilibrium modelling. The next and final substantive section of this lecture is devoted to exploring and explaining the relevance of this issue.

## V.

The word equilibrium has more than one meaning in economics, and that is why I referred above to *a certain type* of equilibrium modelling. The phrase "equilibrium behaviour" applied to the individual agent usually refers to the execution by that agent of plans drawn up in order to maximise some utility function subject to constraints imposed by endowments, available technology, not to mention market opportunities. I find it hard to see how economics can do without modelling equilibrium behaviour thus conceived. If individual agents cannot be treated

as forming plans and then carrying them out, predictions about their behaviour can not be made, and economics must forfeit any claim to positive content.

But agents do not usually act in isolation. The market opportunities of one agent are the result of, among other things, the attempts of others to execute their plans. The interdependence of agents must be of the essence in any social science, and the means whereby independently formed plans are co-ordinated is therefore an important element of economics' subject matter. At this point we meet another equilibrium concept: namely, market equilibrium, a state of affairs in which the plans of individual agents are compatible with one another. Defined sufficiently generally, there can be no objection to claims about the universal relevance of this concept either. Individual agents can hardly be expected to execute their own plans if those plans are not mutually compatible. But, as we all know economics deals with some very particular co-ordination mechanisms involving prices, and has shown that a system of competitive markets with flexible prices is capable of producing a harmonious social outcome in which the maximising plans of individual agents are reconciled with one another.

It has also shown that one can feed informational asymmetries into such a system without destroying its capacity to equilibrate plans drawn up on the basis of information which *ex post* will be revealed to be false; though not surprisingly, in this case, agents might well be unhappy with the outcome of their market activities in a way in which they are not when those activities are the outcome of choices made in the light of correct information. And, into the bargain, there is a meaningful sense in which economic activities co-ordinated by such competitive markets maximise social welfare; while, even in the presence of mis-information, agents are doing the best they can in the circumstances. It follows that economic analysis based on such a framework leaves little room for policy. It implies that, if economic policy does not

actually contribute to agents' errors, it is achieving all that can be expected from it.

Now, uncomfortable though it may make us, we should not reject competitive-general-equilibrium modelling on *a priori* grounds. It is logically coherent, and it *has* been shown to yield empirical predictions. In that sense, it is good science and ought to be treated with respect. This is, however, not to say that it is also correct science. That is a matter to be settled by reference to empirical evidence; and my concern about the current status of competitive-general-equilibrium theory is not that it is taken seriously, but that it is too often defended, not with reference to its compatibility with the facts, but rather as embodying first principles which provide the only scientifically respectable way of approaching the subject.

But such an *a priori* defence is no more satisfactory than an attack would be. Because we cannot help but model individual agents as maximising subject to the constraints they face, it does not follow that we must also model their interaction in the context of a social framework that permits them to maximise all potential gains from trade among themselves. It is interesting to know that we can conceive of such a framework, but it is not hard to conceive of others in which such a happy state of affairs does not rule, even though agents' plans, being subjected to constraints that would not be encountered in a competitive market, are nevertheless co-ordinated once those constraint are allowed for. Such alternatives are equally worthy of serious consideration with respect to their empirical content.

Indeed, I would go further than this. Because the development of economic analysis is an historical phenomenon, it is not surprising that, at any particular moment, some hypotheses have been derived with greater logical coherence from individual maximising premises than others. The fact that a framework needs patching up with "free parameters" does not automatically render it empirically vacuous, and if it does not, then its predictions are worth

testing. Should they turn out to have more empirical content than those of a more logically rigorous framework, then so much the worse for the latter. We certainly should be prepared to take seriously the hypothesis that the world behaves as if agents' plans were co-ordinated in continuously clearing competitive markets. Economists have been arguing about this matter in one guise or another for over two hundred years, and so should we. But we should not pretend that so fundamental a dispute can be settled by converting a controversial hypothesis into a non-debatable axiom.<sup>10</sup>

The relation of these considerations to the subject matter of this lecture is this: the traditional analysis of credit creation and cash balance mechanics, whose continued relevance I argued in its preceding section, distinguishes between the supply and demand for money and relies on the persistence over time of discrepancies between these two magnitudes; it is therefore incompatible with continuous Walrasian equilibrium, and an approach to monetary economics which insists on Walrasian foundations can have no room for such analysis. To insist on such foundations, therefore, is to deny the validity of monetarism. That is the nature of the "roadblock" to which I referred earlier, and that is why its removal is the crucial element in monetarism's unfinished business.

There has always been a non-Walrasian streak to monetarist analysis, even though it has seldom been at the centre of attention in debates about the approach, and has often been ignored or overlooked. I refer here to the idea that the institution of monetary exchange should be viewed as a means of coping with the information and co-ordination problems faced by a market economy which was first introduced into the monetarist literature by Brunner and Meltzer in (1963) and further developed by them in (1971). This way of looking at things treats money as an *alternative* to the Walrasian market and its mythical auctioneer, not as a

supplement to these devices, and it has a long history in economics. It can be found in various guises and states of development in the *Wealth of Nations*, in the writings of Thornton, Mill, Jevons, Marshall, not to mention the *General Theory*; and the fact it is also underpins monetarism goes a long way to explaining why monetarists and economists working in the tradition of Clower (1984), Goodhart (1975) and Leijonhufvud (1981), not to mention such post-Keynesians as Chick (1992) and Davidson (1972) often seem to find it easier to talk to one another than to mainstream macroeconomists. Though these groups disagree about many things they all start from the position that the economy they are trying to understand is one to whose organisation money is essential.

There is, of course, nothing incompatible between this view of the world and the postulate of equilibrium behaviour at the level of the individual agent, nor does it deny the importance of taking account of the general interdependence of individual plans when dealing with market activity.<sup>11</sup> What it does do, however, is point to an alternative set of mechanisms to those of continuously clearing flexible price markets as a means of coping with the problems implicit in that interdependence. Specifically, as I have argued elsewhere, it leads one to postulate that precautionary balances of money are held by agents as a means of enabling themselves to carry through individual plans made on the basis of incomplete information and in response to price signals which may turn out to be, relative to those which would be given by a Walrasian auctioneer, "false". Moreover, precisely because money acts as a buffer against the consequences of mistakes that stem from these sources, its very existence reduces the incentives of agents to collect and process information and, in the case of price setters, to get the prices in question "right".

In particular, when information collection and processing is subject to rising marginal

costs, the availability of buffer stocks of money will ensure that, contrary to the dogma of New-classical economics, agents will habitually use less than "all available" information, and where price changing is costly, it will also ensure that prices are habitually slow to move to the levels that would clear a Walrasian market in the wake of changed conditions. In such a world, excess demands and supplies for goods will occur not merely as notional entities during a tatonnement process, but as observable market phenomena, with the incompatibilities between consumption and production plans implicit in them being absorbed by changes in buffer stocks of money (and in inventories of other financial assets not to mention goods as well) pending the revision of those plans.<sup>12</sup>

To return to the topic of the preceding section of this lecture, the excess supply (or demand) for money that arises as a by-product of debt market transactions between banks and their customers, finds a natural role to play in the workings of such an economy. And a moment's reflection will also make it apparent why this line of analysis also leads to the view that monetary shocks are a serious matter: they have a potential for disrupting the economy's co-ordination mechanisms which is kept completely hidden from view by conventional Walrasian analysis.

## VI.

I suggested earlier that it is an important piece of unfinished business for monetarism to get its analysis of the money supply process back onto the research agenda of monetary economics. It should now be clear that this is but one aspect of a more general piece of unfinished business, namely to get the central importance for the functioning of a market economy of the institutions of money and monetary exchange recognised. The currently

dominant intellectual traditions in macro-economics have trivialised this issue, the "Keynesian" tradition by treating the quantity of money as an uninteresting passively adjusting variable, and the "New-classical" tradition by insisting on a type of micro foundation for macroeconomics which leaves no role for monetary exchange. I put quotation marks around both of these labels, because, as I hope is apparent from the foregoing arguments, I believe that this trivialisation of monetary economics is in the spirit neither of Classical economics, nor indeed of the economics of Keynes, and has been an aberration in the development of economics.

I have tried to show that monetarism has remained closer to the traditional concerns of monetary theory than other contemporary and more popular "isms" in macroeconomics, and that, indeed, this closeness has been one of its strongly distinguishing features. If this is so, then when, as I hope will soon be the case, the unfinished business I have dealt with in this lecture is brought to completion, monetarism will lose its distinctive identity. But when monetary economists turn to deepening our understanding of those traditional concerns, that will not, I am confident, spell the end of debate. The very fact that I have cited such a diverse set of writers as Chick, Clower, Davidson, Goodhart and Leijonhufvud, none of whom would ever be taken for a monetarist, as also addressing questions which arise out of those same traditional concerns, ensures that there will still be plenty to argue about. Though one might be able foresee a time when monetarism's business is complete, therefore, that is unlikely to be the case for monetary economics. This, however, is hardly a matter for regret.

## FOOTNOTES

1. Let it be clear that I claim no originality for interpreting monetarism's place in the history of economic thought along these lines. Brunner (1989) has also made this suggestion.
2. The expectations augmented Phillips curve, due to Friedman (1968) and Phelps (1967) was first formally incorporated in simple macro-models of the IS-LM variety (albeit with a vertical LM curve) by this author in a University of Manchester working paper of 1972 a version of which was finally published (1974).
3. The money growth rule was, of course, proposed by Friedman, (1960).
4. My own 1969 book on *The Demand for Money*. . . surveyed much of the evidence referred to here.
5. See, for example, Brunner and Meltzer's two (1976) contributions to the Stein volume on *Monetarism*.
6. These matters are discussed in the latest (1992) edition of my *Demand for Money*. . .
7. On this matter, see again Brunner and Meltzer (1976).
8. I discussed this matter in greater detail in my paper on "The Buffer-stock Notion in Monetary Economics" which has been reprinted as Chapter 2 of Laidler (1990).
9. I have discussed the development of the analysis of cash balance mechanics in the 1870-1914 period in Laidler (1991).
10. The arguments presented here have been developed further in an essay reprinted as chapter 4 of Laidler (1990).



11. The extremely useful distinction between general economic *equilibrium* on the one hand, and *interdependence* on the other, seems to have first been made explicitly by Arthur Marget (1942).
12. The argument presented here is developed at greater length in the title essay, (Chapter 1), of Laidler (1990).

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