

# Some Aspects of Monetarism Circa 1970

## A View from 1994\*

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### I. Introduction

When the first meeting of the Konstanz Conference took place in 1970, its organisers seemed to be establishing a European outpost of a major intellectual “counter-revolution”. “Monetarism” as *Karl Brunner* (1968) had recently named it, appeared to be fundamentally opposed to the then reigning “Keynesian” orthodoxy in monetary economics. In the next few years an extensive literature seeking to define the nature of Monetarism developed, to which the most comprehensive contribution was surely *Mayer* (1978).<sup>1</sup> Naturally enough, Mayer and his associates focused on the common elements in the work of various monetarists, notably Milton Friedman and Karl Brunner and Allan Meltzer, elements which differentiated it from the Keynesian economics of the time. Now, a quarter century later, the Monetarism and Keynesianism of around 1970 are often presented as nothing more than competing versions of the macroeconomics of the “Neo-classical synthesis” and similarities among the ideas of, say, Brunner and Meltzer, Friedman, Franco Modigliani and James Tobin are portrayed as more important than the differences which they debated with such vigour.<sup>2</sup>

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<sup>1</sup> It should be noted that *Mayer's* own survey paper, which formed the core of this 1978 volume, originally appeared somewhat earlier, in 1975, as a two part article in *Kredit und Kapital*.

<sup>2</sup> In some measure this shift in views stems from a broader movement towards emphasizing theoretical as opposed to empirical issues in macroeconomics. In his contribution to the Mayer volume *Benjamin Friedman* (1978) had already drawn

There is no denying that what seemed, twenty five years ago, to be radical empirical propositions about money and inflation emanating from the Monetarist camp, have turned out to be easily accommodated within a slightly extended version of the same theoretical framework used in the exposition of then conventional Keynesian wisdom – IS-LM analysis supplemented by a Phillips curve, though in its Monetarist version the Phillips curve is expectations augmented. But, pace *Harry Johnson* (1971), though Monetarism was concerned with inflation as a policy problem, it was concerned with other issues too, and its pioneers in fact laid the groundwork for important innovations that took monetary economics well beyond that framework.

In his 1971 De Vries Lectures, (*Johnson* 1972) Harry Johnson suggested that, although Monetarism's first major innovator had been Milton Friedman, by the early 1970s leadership of the school seemed to have passed to Karl Brunner and Allan Meltzer. It is now clear that Friedman's major contributions to the academic literature on Monetarism had virtually all been completed by then, while many of Brunner and Meltzer's had yet to appear. Even so, the latter had, for the previous decade, been making their own distinctive contributions to what had originally been very much a Chicago based enterprise. Moreover, in one or two instances, what seemed to be differences of degree between the views of these writers turned out to be matters of real substance: a fact which becomes particularly apparent when we bring a further twenty-five years of invaluable hindsight to bear on the ideas in question and can see where they have led. Friedman's analysis of *circa* 1970, though by no means in and of itself New-classical, can now be read as paving the way for New-classical macroeconomics. Brunner and Meltzer, on the other hand, were at that time emphasising certain ideas about the nature of a monetary economy which have turned out to be fundamentally incompatible with New-classical doctrine.<sup>3</sup>

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attention to the theoretical similarities between the underlying models used by Keynesians and Monetarists.

<sup>3</sup> Just how incompatible is made clear in *Brunner and Meltzer's* Mattioli Lectures (1993). Because this paper is primarily intended to be an assessment of the literature as it appeared circa 1970, I do not refer to this source with great frequency in what follows, but it is inconceivable that my views have not been influenced by reading it. Moreover, it should be said explicitly that these lectures provide an essential summary of *Brunner and Meltzer's* views, not just on the topics discussed here, but on the current state of macroeconomics in general, and from the point of view of current economics, rather than the history of economic thought, should be the starting point for anyone wishing to assess their contribution.

In this essay, I shall stress the contrast between certain ideas of Friedman on the one hand, and Brunner and Meltzer on the other, and in doing so, I shall seek to correct both the tendency of the 1970s to make Monetarism seem more homogenous than it really was, and more recent tendencies to neglect those of its ideas which made it theoretically distinct. I shall first deal with monetarist analysis of the demand and supply of money, showing how Brunner and Meltzer's relatively elaborate treatment of the money supply process as an integral part of a macroeconomic model, which had no formal parallel in Friedman's work, helped produce a framework in terms of which competing views about the importance of monetary and fiscal influences on aggregate demand could be systematically formulated. Then I shall contrast Brunner and Meltzer's frequent discussions of the nature of money as a social institution with Friedman's remarkable reticence on this same matter. I shall go on to draw attention to *Friedman's* (1975) acceptance of an aggregate supply curve interpretation of the expectations augmented Phillips curve. I shall suggest that this, surely Friedman's closest approach to a New-classical position, was related to his apparent lack of interest in the institutional basis of monetary exchange, and that Brunner and Meltzer's systematic adherence to a price setting equation interpretation of the Phillips relation followed naturally from the extensive discussions of monetary exchange that appeared in their work from 1964 onwards.

This difference in the interpretation of price-output interaction lies at the heart of contemporary divisions among macroeconomists. That, I shall argue, is why the Monetarism of circa 1970 now seems as remarkable for the differences as for the similarities among the views of its leading proponents, and also why it should still be of more than anti-quarian interest.

## II. The Demand and Supply of Money

No single work launched the Monetarist episode in the way that the General Theory set the Keynesian Revolution in motion. A reading of *Friedman* (1952), for example, will reveal that he had by then developed many of the ideas which we now-a-days associate with his later work. Even so, the publication in 1956 of *Studies in the Quantity Theory of Money* focused professional attention on the new doctrine with a new intensity. That volume led off with Friedman's celebrated "Restatement" of the quantity theory as a theory of the demand for money, and it is now uncontroversial that this essay was far more original than its author



claimed: it had very little to do with conveying “the flavor of the oral tradition” (1956, p. 52) which had dominated Chicago monetary economics since the 1930s; rather, it represented the outcome of a search for micro-foundations for the demand for money function in the same Fisherian capital theory which underpinned Friedman’s work on the consumption function.<sup>4</sup> It expounded a theory of the demand for real balances as the demand for a durable good in which, to use Brunner and Meltzer’s words, “... the demand for money is assumed to depend on asset prices or relative returns and wealth or income”. (*Brunner and Meltzer* (1974, pp. 66). Friedman argued explicitly that

“The quantity theory is ... not a theory of output, or of money income, or of the price level. Any statement about these variables requires combining the quantity theory with some specifications about the conditions of supply of money and perhaps about other variables as well” (*Friedman* 1956, p. 52)

and he also proposed that, as an empirical matter, the relationship in question was a stable one. In the context of the macroeconomics of the 1950s this claim was bound to be controversial.

It was then conventional wisdom that the demand for money function was highly interest elastic, and volatile too, while, on the other hand, the economy’s marginal propensity to consume out of current income was a well determined parameter. From this conventional wisdom flowed an interpretation of inter-war economic history – the Great Depression was the result of a collapse in investment, amplified by the multiplier, and monetary policy had been powerless to offset it – and a recipe for stabilisation policy – rely on fiscal measures which would work through the multiplier, with the role of the monetary authorities being reduced to ensuring that interest rate fluctuations did not interfere with their effects. *Friedman’s Theory of the Consumption Function* (1957) would soon challenge the idea of a stable marginal propensity to consume out of current income, and hence of a stable multiplier, and in 1956 he was suggesting that it was the demand for money function which was the stable relationship in the economy. The implications were soon to be spelled out: the business cycle in general, and the Great Depression in particular were, after all, largely monetary phenomena, (*Friedman and Schwartz* 1963), and autonomous expenditure variables (including those

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<sup>4</sup> The seminal paper on the relationship between *Friedman’s* “Restatement ...” and the “Chicago Tradition”, so called, was *Don Patinkin* (1969). See also the exchange between *Patinkin* and *Friedman* on these matters in *Friedman* (1974).

under the control of the fiscal authorities) had far less explanatory power over the time path of money income than did fluctuations in the quantity of money (*Friedman and Meiselman 1963*).

It was, of course, some way from an *a priori* claim that the demand for money was a stable function of a few arguments to these empirical conclusions. To link them, it needed to be established first of all that the demand for money function was indeed as empirically stable as Friedman claimed it to be, and the early years of the monetarist episode saw many studies of this issue. There is no need to describe this literature in detail here. Suffice it to say that by the middle of the 1960s *Friedman's* (1959) difficulties in finding any role for the interest rate to play in the demand for money function had been overcome, and that a number of studies all supported essentially the same conclusion, namely that the demand for real balances was indeed systematically related to real wealth (or permanent income) and a representative nominal interest rate, with its elasticity with respect to the latter variable being modest (ie. probably less than unity even for a long rate) and showing no signs of increasing at low interest rate levels. Only the question of the appropriate empirical specification of money remained a little contentious, with *Friedman* (1959, 1966) tending to a broader M2 definition and *Meltzer* (1963) leaning towards the narrower M1.<sup>5</sup>

By the mid-1960s, what seemed to be still missing from Friedman's version of Monetarism was an explicit formal model of a complete macroeconomic system. He had, in more than one place (e.g. in *Friedman and Meiselman* (1963)) given informal accounts of a "transmission mechanism" cast in terms of asset substitution and interest rate effects on aggregate demand that some of his critics simply chose to ignore.<sup>6</sup> However, *Friedman and Schwartz's* (1963) *Monetary History* had included no explicit account of the formal model which underlay its historical analysis. The seriousness with which this omission was taken varied very much from commentator to commentator, but in any event, Friedman sought to close it in two essays originally published in the *Journal of Political Economy* in 1970 and 1971, but whose most accessible source nowadays is *Friedman* (1974).

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<sup>5</sup> My own main contribution to the monetarist debate of this period was to survey the state of theoretical and empirical knowledge about the demand for money function in *Laidler* (1969).

<sup>6</sup> As early as (1962), *Harry Johnson* had noted the essential similarity of the views of *Friedman, Brunner* and *Tobin* on the transmission mechanism whereby monetary shocks affected aggregate demand, choosing to illustrate this with a quotation from *Brunner* (1961).

*Brunner and Meltzer's* (1974) comment on these essays both succinctly sums up competing views on the importance of this gap in Friedman's earlier work, and sets out the motivation of their own attempts to develop a coherent framework in terms of which monetarist propositions could be presented

"A prevalent view among economists is that hypotheses involving empirical regularities must be 'supported' by a higher-level theory from which the lower-level proposition can be derived. We do not share this view; in fact, we dissent strongly and so does the modern literature on the philosophy of science. However, if theories generate useful empirical conjectures ... the expected gain from more discriminating tests derived from more fully developed hypotheses increases." (1974, p. 64, fn. 1.)

For them, that is to say, as for Friedman, an empirical proposition supported by evidence was to be taken seriously regardless of the assumptions from which it was derived. But Brunner and Meltzer also thought the attempt to develop more comprehensive and carefully articulated premises worthwhile, not for its own sake, but because it promised to generate further testable hypotheses.<sup>7</sup> Thus, though the critique of Friedman's work from which the above quotation is taken is sometimes read as an attack on Friedman, this is a mistake. Brunner and Meltzer's intention was to show how their own work supplemented Friedman's, not to claim that it supplanted it in any way.

From the late 1950s onwards Brunner and his students at UCLA had been working on those very "specifications about the conditions of the supply of money" (*Friedman* 1956, p. 52) which Friedman had suggested needed attention if a theory of the demand for money was to become a component of a theory of "output ... money income, or of the price level" (1956, p. 52). Their work had led them to regard the then prevailing IS-LM model as an inadequate tool for analysing monetary and fiscal policy. That model, formally speaking, treated the quantity of money as exogenous. It also ignored the monetary effects flowing through asset markets of variations in the public debt generated by fiscal policy. Their disappointment when *Friedman* finally set out his "Monetary Frame-

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<sup>7</sup> *Friedman's* own well known views on the importance of empirical evidence about predictions, relative to the truth or otherwise of assumptions were set out in (1953). I am not arguing here that there were any differences of substance between him and *Brunner and Meltzer* on these matters, only differences of emphasis. *Friedman* was less willing than *Brunner and Meltzer* to engage in theoretical speculation that was not clearly related to a well defined empirical issue.



work” (1974) in terms of a series of variations on a standard IS-LM model was, then, understandable. They did not, of course, disagree with Friedman’s comment that

“... substantial changes in the supply of nominal balances can and frequently do occur independently of any change in demand. The conclusion is that substantial changes in prices or nominal income are almost invariably the result of changes in the nominal supply of money”. (*Friedman* 1974, p. 3)

Indeed, they characterised this contention as “largely correct”, but they also suggested that it was “misleading” (*Brunner and Meltzer*, 1974, p. 71).

Friedman’s brand of Monetarism had by no means neglected the money supply process. Appendix B of the Monetary History was devoted to an extensive (33 pages) analysis of “Proximate Determinants of the Nominal Stock of Money”; a series of Chicago Ph. D. theses, including the original versions of *James Meigs’* Free Reserves and the Money Supply (1962) and *George Morrison’s* Liquidity Preferences of Commercial Banks (1966) had made important theoretical and empirical contributions to its analysis; while *Phillip Cagan’s* Determinants and Effects of Changes in the Stock of Money 1875 - 1960 (1965) was an integral and important part of that series of NBER studies which began with *Friedman* and *Schwartz’s* Monetary History ... All of these studies were, however, conceived in terms of the conventional Crick-Meade money multiplier, and sought behavioural foundations for the various asset ratios which figured in that expression.

Supplemented by such analysis, Friedman’s analytic framework could and did cope easily enough with those feedback effects running from output and prices to the money supply which he was so often accused of ignoring. His conclusion about money supply behaviour during “The Great Contraction” of 1929 - 1933, namely that “... the decline in the stock of money and the near collapse of the banking system can be regarded as a consequence of nonmonetary forces ...” (*Friedman* and *Schwartz* 1963, p. 300) provides one striking example of this capacity. But its application did not permit analysis of the interactions among money, credit creation, and the portfolio choices of the non-bank public of a type required to address those questions about the financial consequences of fiscal policy which, in the 1960s and early 1970s, so concerned American Keynesians, notably James Tobin and his associates.

*Meltzer* had commented at length on these aspects of Friedman’s work in his (1965) review essay on Friedman and Schwartz’s Monetary History

..., and in (1974) it was this same limitation which prompted Brunner and Meltzer's abovementioned characterisation of Friedman's analysis as "misleading". Their more extensive macroeconomic framework incorporated a government budget constraint, and modelled the private sector's portfolio behaviour in a way that permitted feedbacks from the creation of government debt to the demand as well as the supply of money to be analysed in a way that IS-LM supplemented by a money multiplier did not.<sup>8</sup>

Friedman's response to this particular criticism of his framework was guarded: he suggested that Brunner and Meltzer had "... inflated the role of the common [IS-LM] model ..." (*Friedman*, 1974, p. 136) in his work, but agreed with them that

"... the framework I present is only a beginning. Their own specific model ... is a special extension and development of that framework. I applaud and welcome their efforts in that direction, without necessarily accepting the details of their model." (1974 p. 137)

Friedman's caution here seems to have stemmed from no cause more profound than that Brunner and Meltzer's model was not obviously inconsistent with his own work, and that, at that particular time, he had other more hostile critics with whom to deal. Even so, the particular version of monetarism implicit in Brunner and Meltzer's specific model, which allowed for the logical possibility of important credit market effects, and significant fiscal policy impacts, on aggregate demand, was somewhat closer to James Tobin's "New View" of monetary mechanisms than Friedman's. Like Tobin, Brunner and Meltzer found the IS-LM model's aggregation of assets into two categories, money and all others, misleadingly simplified, and like Tobin they were uneasy about using the Crick-Meade money-multiplier to go behind the LM curve in order to endogenise the determination of the money supply. But they differed from Tobin and his associates too on one all important matter of research tactics.

Where proponents of the New View usually chose to elaborate the portfolio choice aspects of their models, even at the expense of treating output and prices as parametric in order to keep their analytic exercises

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<sup>8</sup> *Brunner and Meltzer* (1964) contains an account of the relevant money supply analysis which, though by no means the first or most comprehensive exposition, is particularly useful in being integrated with a simultaneous empirical investigation of the demand for money function. It is also worth noting that *Brunner and Meltzer* gave a systematic presentation of the basic macroeconomic model to the First Konstanz Conference (See *Brunner and Meltzer* (1972)).



manageable, Brunner and Meltzer regarded the explanation of prices and output as too important, and the feedbacks running from these variables to asset markets as too empirically significant, to be neglected. Hence when faced with the same need to keep things manageable, it was the analysis of portfolio behaviour that they simplified. To proponents of the New View their analysis thus seemed unacceptably crude, and to Monetarists working under Friedman's influence it simultaneously seemed unnecessarily elaborate and open-ended. But Brunner and Meltzer provided an analytic framework capable of yielding either side's predictions, about for example, the significance of monetary impulses in generating inflation, or the effects of fiscal policy on aggregate demand, thus establishing that these issues were empirical rather than theoretical. The open-endedness of their theoretical framework, however, did not prevent them from forming and defending strong empirical judgments about these questions. In particular, when, in the early 1970s, the influence of fiscal policy was for a while the main bone of contention among macroeconomists, it was *Brunner and Meltzer* who most fully developed the monetarist side of the controversy, as is evident, for example from the contents of the well known (1976) volume, edited by Jerome Stein, which, though it dealt mainly with fiscal policy, bore the title *Monetarism*. In particular, Brunner and Meltzer's "Reply" to their discussion in this volume is an outstandingly clear, non-technical, exposition of that position.

### III. The Nature of Money

I have already mentioned the broad consensus that had emerged by the mid-1960s about the empirical formulation of the demand for money function, but I have also drawn attention to one substantive difference between the views of Friedman and those of Brunner and Meltzer. The difference in question concerned the appropriate specification of the money stock variable to be used in empirical work. Friedman favoured a broader M2 specification. Brunner and Meltzer, if they had to use a single aggregate, tended to prefer M1, and in their analytic work were more comfortable with a framework which distinguished between demand and time deposits.<sup>9</sup> Underlying this difference of opinion, which,

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<sup>9</sup> The fact that *Brunner and Meltzer* preferred to discuss demand and time deposits as distinct entities meant, of course, that bank credit was not simply the other side of a balance sheet on which the only liability was "money", hence making it important for them to distinguish between money and credit when analysing monetary policy.

be it said, was not at the time regarded as a serious one, lay another deeper issue, namely the criteria to be applied in making the choice in question. *Friedman* and *Schwartz* (1970) treated it as an empirical matter, to be decided by the data; *Brunner* and *Meltzer*, despite their basic agreement with *Friedman* about the ultimate primacy of empirical evidence in economics, in this instance paid some, though not overriding, attention to theoretical criteria. Specifically, their interest in M1 was related to their view that money's primary role in economic life was that of means of exchange.<sup>10</sup>

This was, from the early 1960s onwards, an important theme in their work. It appeared first in *Brunner* and *Meltzer* (1964, pp. 258 - 261), again in *Brunner* (1971, pp. 5 - 19), and was the sole subject of *Brunner* and *Meltzer* (1971).<sup>11</sup> The starting point of the argument was an economy lacking any centralised agency for the generation of information and its transmission to agents, in which, therefore market activity was characterised by a high degree of uncertainty on the part of individual transactors about the qualities of the items to be transacted. Its conclusion was that

“Divergent distributions of knowledge about the quality characteristics of assets generate a pattern of implicit and actual exchange ratios that induce cost-saving and wealth-maximising agents to use some specific assets as a focal point of all transactions chains. Holding such assets thus becomes advantageous. ... If the variability of information patterns is extended to cover the possible exchange operations emerging in the future, the advantages of holding *some* positive amount of the asset with minimal relative variance (of exchange ratios) and to use it as the focal point of optimal transaction chains becomes even more pronounced.” (*Brunner* and *Meltzer* (1964, p. 261, italics in original)

The social productivity of money therefore derived from the cost savings which its use generated *in all market transactions*, and its demand was a matter of transactions and precautionary motives. This, in *Brunner* and *Meltzer*'s opinion, was in sharp contrast to the implications of the *Baumol-Tobin* model of demand for money which focused only on *transactions costs between money and other stores of value*. Here was a fun-

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<sup>10</sup> It should be noted, though, that *Brunner* (1971 p. 19) pointed out that this line of argument did not preclude the existence of a stable demand function for a broader M2 aggregate which might then usefully be treated as “money” in empirical applications involving the explanation of price level behaviour.

<sup>11</sup> Their discussions of this issue owed much, as they explicitly indicated, to discussions with *Armen Alchian*, who finally published his own variation on this particular theme in (1977).

damental difference between their monetary economics and the “New View” of James Tobin and his associates. In Brunner’s words:

“It is easily understandable ... that the ‘New View’ found little basis to differentiate between money, i.e. media of exchanges [sic], and financial assets which are not media of exchanges. The analytic frame chosen prevents recognition of the magnitude and nature of the productivity associated with money.” (1971, p. 15)

Hence the emphasis which the other strand in Tobin’s analysis laid upon interest rate uncertainty as an influence on the demand for money was, in their view, misplaced:

“... uncertainty concerning interest rates only implies per se portfolio adjustments involving securities and other assets. Money is introduced by assumption in order to discuss the portfolio adjustments between securities and money”. (1971, p. 6)

Brunner and Meltzer’s basic vision of the role of money in the economy thus differed sharply from that underlying the “New View”, but it seemed to them to be quite consistent with Friedman’s analysis, provided the latter was carefully interpreted.<sup>12</sup> As I have already noted Friedman had modelled the demand for money “as if” the demand for a durable good yielding a flow of services to its holders. The Brunner-Meltzer analysis of the productivity of money “... denie[d] the occurrence of an original ‘convenience yield’ properly expressed by marginal utility” (*Brunner* 1971, p. 19) but it nevertheless suggested that money’s

“... medium of exchange function ... assures a positive marginal private ... productivity to money and thus determines a positive allocation of individuals’ wealth to money inventories. Once such inventories occur ... a utility yield may naturally attach to them similar to the utility yield attached to other wealth items. ...” (*Brunner* 1971, p. 19)

There is nothing of which I am aware in Friedman’s writings to indicate that he has ever found anything to disagree with in this analysis of

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<sup>12</sup> Indeed, this analysis harked back to 19th century ideas about money’s means of exchange role, not least those of *Carl Menger* (1892). These ideas had become lost from view as money’s store of value role received increasing emphasis in the wake of the *Keynesian* revolution. Note that another approach to discussing money’s means of exchange role emphasises its evolution from repeated transactions among agents that are initially mediated by credit. It is not clear whether this line of reasoning, particularly associated with *Hicks* (1989) and *Clower* and *Howitt* (1994) who emphasise the role of firms in creating such ongoing market relations, is an alternative, or rather an important supplement, to *Brunner* and *Meltzer*’s.



Brunner and Meltzer, but the fact is that he has seldom discussed the underlying social experiment out of which monetary exchange might develop in the first place. His most extensive, but still extremely brief, treatment of these issues occurs in his (1969) essay on “The Optimum Quantity of Money”. There the abstract economy which Friedman discusses is one in which

“The society, though stationary, is not static. Aggregates are constant, but individuals are subject to uncertainty and change. Even the aggregates may change in a stochastic way, provided the mean values do not” (*Friedman* 1969, p. 2)

and it is from this individual uncertainty that the demand for money derives.

“Why ... should people want to hold money? The basic reason is to serve as a medium of circulation, or temporary abode of purchasing power, in order to avoid the need for the famous ‘double coincidence’ of barter. ... This separation of the act of sale from the act of purchase is the fundamental productive function of money. It gives rise to the transactions motive stressed in the literature. A second reason ... is as a reserve for future emergencies. ... This reason corresponds to the ‘asset’ motive for holding money.

It is worth noting that both reasons depend critically on ... the existence of individual uncertainty. In a world that is purely static and individually repetitive, clearing arrangements could be made once and for all that would eliminate the first reason, and there would be no unforeseen emergencies to justify holding money for the second reason.” (1969, p. 3)

But that is all Friedman had to say in this, his most substantial essay on monetary theory, about the social nature of money – the bare minimum needed to justify the extensive discussion of cash balance mechanics and the welfare effects of inflation to which the essay is devoted, and in which, as in his original “Restatement ...” of 1956 “The analysis of the demand for money ... [is] made formally identical with that of the demand for a consumption service” (1956, p. 52). It is hard to resist the conclusion that, quite simply, Friedman did not find this matter very interesting, or judge it likely to be the source of fruitful empirical hypotheses.<sup>13</sup>

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<sup>13</sup> In a personal letter to the author, dated June 19, 1994, *Friedman* remarks that this conclusion is “... to the best of my recollection, the right conclusion ...”.

The consequence of this was, I shall now argue, of considerable importance for subsequent developments in monetary economics, because, though *Friedman* told his readers that money's means of exchange function was fundamental, he also told them that, in his economy "Relative prices are determined by the solution of a system of Walrasian equations." (1969, p. 3) This Walrasian real economy of *Friedman's* 1969 essay, which is perhaps subject to *aggregate* stochastic shocks, and in which a demand for money derives from *individual* uncertainty, is not quite *Lucas's* New-classical system of (1972 - 1973); in particular, *Friedman's* reference, by no means the only one in his writings of this period, to "the solution of a system of Walrasian equations" should not be read as an early and conscious endorsement of *Lucas's* later insistence on equilibrium modelling. But it is nevertheless a manageable step from *Friedman* to *Lucas*, much more so than from *Brunner* and *Meltzer* to *Lucas*, for in their work the system of monetary exchange is explicitly presented as an alternative to the Walrasian market.

#### IV. The Phillips Curve and *Friedman's* Missing Equation

It has been noted earlier that, when *Friedman* (1974) set out his "Theoretical Framework" he used an IS-LM model as his basic expository device. The model in question was underdetermined, consisting of six equations and seven unknowns, and *Friedman* discussed three approaches to providing what he called "The Missing Equation". First, there was "the simple quantity theory", which assumed that "real income is determined outside the system. In effect, it appends ... the Walrasian equations of general equilibrium" (1974, p. 31); second, there was "the simple income-expenditure theory" which postulated that "the price level is determined outside the system" (1974, p. 32); and third, there was *Friedman's* own preferred approach, which involved

"... bypassing the breakdown of nominal income between real income and prices and using the quantity theory to derive a theory of nominal income rather than a theory of either prices or real income." (1974, p. 34)

All this presents a considerable puzzle, since a fourth, altogether more satisfactory, approach to the problem at hand was already clearly visible, though not fully worked out, in *Friedman's* own earlier work: namely his (1968) extension of *Phillips's* (1958) discussion of the inflation unemployment trade-off to incorporate endogenous inflation expectations and the idea of a "natural unemployment rate ... ground out by the

*Walrasian* system of general equilibrium equations” (1968, p. 102) (i.e. the unemployment rate associated with the level of output taken as given by the simple quantity theory in his 1974 exposition).<sup>14</sup> The fact that *Friedman* did not in (1974) make this link is all the harder to understand in the light of the fact that he there explicitly recognised *Phillips’* (1958) study as an attempt to come to grips with the problem of establishing “... a link between real magnitudes and the rate at which prices change from their initial historically determined level.” (1974, p. 32). But the fact remains that this was the first and last suggestion in this particular essay that a *Phillips* curve of any sort might be used in this fashion.

Even so, *Friedman’s* 1968 treatment of the *Phillips* curve is of considerable relevance to matters under discussion here, because it is there, more so than anywhere else, that those tensions in monetarist doctrine, which were later to lead to its influence being shared between New-classical and New-Keynesian economics, are (with benefit of hindsight) most clearly evident. In the 1960s, the standard interpretation of the role of the unemployment rate in the *Phillips* curve was as a proxy measure for pressures being exerted on money wages by excess demand. *Friedman* argued that *Phillips’* analysis “... contains a basic defect – the failure to distinguish between *nominal* wages and *real* wages” (1968, p 102, italics in original), but he continued to accept that wage dynamics were driven by excess demand. Once inflation got going in response to monetary expansion,

“Employees will start to reckon on rising prices of things they buy and to demand higher nominal wages for the future. ‘Market’ unemployment is below the ‘natural’ level. There is an *excess demand* for labor so real wages will tend to rise toward their initial level.” (1968, p. 104, italics added)

This story of a market out of equilibrium is of course quite compatible with *Friedman’s* earlier observation that, after an unanticipated monetary shock, “To begin with, much or most of the rise in income will take the form of an increase in output and employment rather than in prices.” (1968 p. 103), but less so with another remark that occurs in an intervening paragraph.

“Because selling prices of products typically respond to an unanticipated rise in nominal demand faster than prices of factors of produc-

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<sup>14</sup> It is well known, but worth noting explicitly nevertheless, that *Edmund Phelps* (1967) represents another, non-monetarist, source of the expectations augmented *Phillips* curve.



tion, real wages received have gone down – though real wages anticipated by employees went up .... *Indeed, the simultaneous fall ex post in real wages to employers and rise ex ante to employees is what enabled employment to increase.*" (1968 pp. 103 - 104, italics added)

Here the behaviour of a key real variable, employment, is presented as an equilibrium response to price changes that *have already occurred*, but have been perceived differently on different sides of the market. It is hard to see how the employment change in question could simultaneously have *preceded* those same price changes. What we have in this passage, then, is not an alternative description of a disequilibrium interpretation of the Phillips curve augmented by the idea of inflation expectations, but something rather closer to Lucas's alternative aggregate supply curve interpretation, albeit without the idea of rational expectations.<sup>15</sup>

In (1968), then, *Friedman* was ambivalent about the analytic basis of the expectations augmented Phillips curve, and, as we have seen, he did not refer to it in his 1974 theoretical framework. In 1975, however, this ambivalence seemed to disappear. Then, in addition to re-iterating his comments on the importance of distinguishing between real and nominal wages, he compared *Phillips'* (1958) analysis with that to be found in a then recently rediscovered paper by *Irving Fisher* in the following terms.

"There was, however, a crucial difference between Fisher's analysis and Phillips', between the truth of 1926 and the error of 1958, which had to do with the direction of causation. Fisher took the *rate of change of prices* to be the independent variable that set the process going." (1975 p. 12 italics in original)

Now it is unlikely that, in this passage, *Friedman* intended to endorse the then emerging New-classical economics. It takes on far more significance in the light of subsequent developments in monetary economics than it could have had at the time. And it would certainly be going too far to argue that *Friedman's* adoption of an aggregate supply curve interpretation of the Phillips curve in 1975 was a logical consequence of his having, in 1969, motivated the existence of money by referring to individual uncertainty within an economy whose overall behaviour could

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<sup>15</sup> But it should be noted that the aggregate supply curve interpretation of the relationship appeared in *Lucas's* work (*Lucas* and *Rapping*, 1969) three years before he joined it up to the rational expectations hypothesis. Though *Lucas* and *Rapping* do refer to *Friedman* (1968), the reference is perfunctory, and suggests that their analysis should be regarded as a parallel development to, rather than a development of, *Friedman's* discussion.

be described in terms of “Walrasian equations”. Even so, there can be no denying the compatibility of these two aspects of his economic thought.<sup>16</sup> Nor, surely, is it an accident that Brunner and Meltzer, who paid far more attention to the fundamentally non-Walrasian nature of an economy characterised by monetary exchange always described the interaction of the prices and quantities in their system in terms of an equation describing “... the private sector’s *price setting behavior* as a function of output [etc.]” (*Brunner and Meltzer 1978 p. 5, italics added*).<sup>17</sup>

*Brunner* had, in (1971), recognised and extensively discussed the connections between his analysis of the information problems underlying monetary exchange and the behaviour of prices and quantities in the short run.

“It follows ... that longer-run behavior functions are approximately homogeneous with respect to prices and nominal values, whereas shorter-run behavior functions are definitely non-homogeneous in terms of prices. This divergence ... follows from the behavior of information and adjustment costs in the two runs ... The second major implication ... is the reversal of the Marshallian ordering of output and price adjustment velocities. The short-run adjustment velocity of quantities exceeds the corresponding velocity of prices in all cases requiring substantial costs of producing information about prevailing market opportunities. Relative short-run price inflexibility thus emerges as a rational consequence of wealth-maximizing behavior in the context of incomplete market information and costly information production. ... Changes in monetary impulses necessarily modify real variables, total output, employment and the real rate of interest. The

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<sup>16</sup> *Friedman* uses the aggregate supply curve analysis of the *Phillips* curve in his (1987) *New Palgrave* essay on “The Quantity Theory of Money”, but he nevertheless continues to draw attention there to the empirical tendency of monetary shocks to create output fluctuations that precede those in the price level. The facts in question seem to me to be incompatible with a model in which quantities respond to misperceived price changes or even to price changes whose significance is different on different sides of the market, and it is fair to conclude, therefore, that a large element of ambivalence has remained in this aspect of *Friedman’s* work. Indeed, in his personal letter of June 19, 1994, reiterates his view “... that there is still no satisfactory solution to ... how to predict the fraction of a change in nominal income that will take the form of a change in prices rather than in output”.

<sup>17</sup> I choose this particular source to quote at this point, because it was written late enough for us to be sure that *Brunner* and *Meltzer* had had time to consider and digest the implications of *Lucas’s* work. They had done so selectively, taking in and using the idea of rational expectations, but choosing not to switch to *Walrasian* micro-foundations for their work.

same impulse gradually induces an adjustment of the price-level with a speed determined by the load imposed with the impulse and the nature of the cost functions governing production of information and adjustments.” (Brunner 1971, p. 21)

This particular passage is supplemented by no references, but the phrase “the reversal of the Marshallian adjustment velocities” echoes the usage of *Axel Leijonhufvud* (1968, eg. p. 37), while the influence of “... interminable discussions with A. A. Alchian” on Brunner and Meltzer’s views on monetary exchange had already been acknowledged when these issues had been briefly discussed in *Brunner and Meltzer* (1964, p. 258, fn. 19). The point to be stressed here is that the particular microfoundations which these two colleagues of Brunner’s were then developing for macroeconomic analysis were anything but Walrasian. Leijonhufvud was explicitly concerned with elaborating the analysis of quantity constrained processes which *Robert Clower* (1965) had initiated; and Alchian had recently published a search theoretic model of inflation and employment interaction that stressed information asymmetries between buyers and sellers of labour, warning his readers that

“The analysis can be expressed more conventionally, but *not* as follows: ‘A reduction in demand involves a lag of wage rate decreases behind prices – which is a rise in real wage rates. This rise implies lower employment because of diminishing marginal returns to labor inputs.’ ... wage rates and all other prices can fall at the *same* rate.” (Alchian (1970), p. 43, italics in original)

Alchian thus explicitly ruled out explaining the observed Phillips curve as a movement along an aggregate supply curve, and indeed, with acknowledgement to Leijonhufvud, went so far as to suggest that his analysis might provide an analytic foundation for Keynes’s notion of involuntary unemployment. (cf. 1970 p. 44, fn. 27)

Now the different approaches to price-output interaction taken by Friedman on the one hand and Brunner and Meltzer on the other seem more important with benefit of hindsight than they did at the time. What initially attracted attention when *Lucas* published his two seminal papers (1972, 1973) was not so much his adoption of Walrasian foundations for business cycle theory as the idea of rational expectations. While, in the next few years, monetary economists were self-consciously digesting the latter idea, they were also, often unknowingly, swallowing the former as well, and it was not until the late 1970s that the analytic



tensions I have been dealing with here began to attract attention.<sup>18</sup> By then, though, they involved much more than a minor disagreement among monetarists. Rather they were seen to represent fundamentally different theoretical visions, and were coming to mark the crucial dividing line between New-classical economics and its so-called “New-Keynesian” critics.

To discuss this later stage in the development of monetary economics would take me beyond the scope of this essay – though I cannot resist noting one feature of subsequent work within the Monetarist tradition. As *Brunner* and *Meltzer* further refined their analysis of price setting behaviour in the late 1970s, they did so by opposing the relative-price-general-price-level confusion, invoked by exponents of New-classical economics to explain real responses to monetary shocks, with the alternative hypothesis that agents face problems in distinguishing between transitory and permanent shocks to nominal aggregate demand, an approach which Meltzer had first broached in his (1965) discussion of Friedman and Schwartz’s (1963) business cycle analysis. Such problems would, of course, have no real consequences in a Walrasian economy where nominal prices were costlessly and continuously variable, but in Brunner and Meltzer’s monetary economy, permeated with uncertainty, it was easy to argue that they could indeed give rise to real responses.<sup>19</sup> The transitory-permanent distinction (though not as applied to monetary shocks) was, however, clearly Friedman’s property before it was Brunner and Meltzer’s, as *Meltzer* (1965) had acknowledged; and there is considerable irony in the fact that this idea has found an important role to play in a style of macroeconomics so radically different from the New-classical theory which developed out of Friedman’s version of Monetarism.

## V. Concluding Comments

There can be no question that the Monetarism of twenty-five years ago has left a lasting imprint upon macroeconomics. First, and so obvious that it is easy to overlook, the once heretical view that inflation is a monetary phenomenon susceptible to control by monetary policy is now

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<sup>18</sup> I will let others speak for themselves on this matter. The record shows that, allowing for editorial lags, it took me until 1977 or thereabouts to understand that the difference in question was important enough that one had to choose sides on the matter. See *Laidler* (1978.)

<sup>19</sup> Much of *Brunner* and *Meltzer*’s third *Mattioli* lecture is devoted to these issues. See (1993, pp. 128 - 157).

close to conventional wisdom.<sup>20</sup> Second, that style of analysis which blends economic and econometric theory as a means of dealing with a world subject to stochastic shocks, of which Friedman's Theory of the Consumption Function was the path-breaking work, is now standard. Third, less fundamental, but still worth noting, current debates about the relative importance of credit and monetary channels in the transmission of policy impulses to aggregate demand, are in some measure reopening issues which were debated between Brunner and Meltzer and proponents of the "New View" a quarter century ago. All of these are good reasons for being interested in the monetarist episode as history.

As I have tried to show in the latter part of this essay, however, the monetarist literature of about 1970, also contains alternative interpretations of the interaction of real and nominal fluctuations, cast in terms of Walrasian markets on the one hand, and markets subject to nominal stickiness on the other. In the early 1970s, this issue had hardly been noticed, but it already differentiated Friedman's Monetarism from that of Brunner and Meltzer, and divisions of opinion on this matter are still at the very heart of current macroeconomic controversy. Though we probably all have our own views about how the matter should be resolved, it is still very much open.

Now it should be stressed that, in drawing attention to this difference between Friedman and Brunner and Meltzer, I intend to pass no judgement on the relative merits of their work. Rather, I intend only to comment on a difference between their intellectual styles, and on the (surely unforeseeable) consequences that this difference had. Friedman, it seems to me, was content to use just as much theoretical analysis as was necessary to deal coherently with whatever empirical issue was at hand, and saw no point in engaging in theoretical speculations that did not seem to bear upon empirical puzzles. That there might be, at some deeper theoretical level, a basic incompatibility involved in analysing monetary phenomena in terms of a model characterised by "Walrasian equations" did not trouble him, just so long as empirical evidence appeared to be generated "as if" by such a model.

When the methodological priorities of economists shifted towards demanding coherent microfoundations, expounded in terms of so-called "deep parameters" characterising tastes, technology, and endowments,

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<sup>20</sup> Anyone whose professional memory is shorter than twenty-five years is invited to compare *Bronfenbrenner and Holzman's* (1963) survey of the literature on inflation with *Laidler and Parkin's* (1975) treatment of the same issue to see how much impact the monetarist episode had on our understanding of this topic.

Friedman's analysis was, whether he liked it or not, a natural starting point for the resulting work.<sup>21</sup> In contrast to Friedman, Brunner and Meltzer were more willing to speculate at length about theoretical issues, to discuss assumptions for their own sake, and, as I have tried to show in the latter part of this essay, their speculations about the nature of monetary exchange made their version of Monetarism proof against assimilation into New-classical theory.

Conflicts about the appropriate theoretical foundations for monetary analysis are now of more central importance than they were twenty five years ago when they first surfaced in the monetarist literature. That is perhaps the most important single reason why Monetarism is no longer distinct school of thought, but it is also why this aspect of its development is still of more than merely historical interest.

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<sup>21</sup> I suspect that he did not particularly like it, and that the feature of New-classical economics least congenial to him would be the weight it gives to "sound micro-foundations" as opposed to empirical content in judging competing models. Certainly *Brunner* and *Meltzer*, whose methodological position is similar to *Friedman's*, take this position in (1993, p. 45): "Real business cycle theory is a triumph of ingenuity and technical virtuosity over observation".



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## Summary

### Some Aspects of Monetarism Circa 1970 – A View from 1994

Viewed from 1994, the monetarism of 1970 was not nearly as homogenous a body of doctrine as it seemed at the time. Three areas of difference between the doctrine's leading proponents, *Milton Friedman* on the one hand and *Karl Brunner* and *Allan Meltzer* on the other, are now apparent. First: Brunner and Meltzer paid more attention to the role of credit markets than did Friedman; second: Friedman's empiricism made him relatively uninterested in discussing issues raised by money's status as a social institution, a matter that Brunner and Meltzer frequently discussed; third: this second characteristic of Friedman's work led him to discuss the interaction of inflation and unemployment in terms that clearly prefigure the New-classical analysis of *Robert E. Lucas*, et al., while Brunner and Meltzer's emphasis on the role of money as a means of coping with information and co-ordination problems led them to anticipate many of the insights of what is now called "New Keynesian" economics. Thus the monetarist literature of 25 years ago sheds important light upon modern debates.

## Zusammenfassung

### Gewisse Aspekte des Monetarismus um 1970 – aus der Sicht von 1994

Aus der Sicht von 1994 war der Monetarismus des Jahres 1970 nicht annähernd eine so homogene und in sich geschlossene Doktrin, wie dies der Fall zu sein schien. Drei unterschiedliche Ansätze zwischen den Hauptproponenten der Doktrin – *Milton Friedman* einerseits und *Karl Brunner* und *Allan Meltzer* andererseits – sind jetzt offensichtlich. Erstens: Brunner und Meltzer haben der Rolle des Kreditmarkts mehr Aufmerksamkeit gewidmet als Friedman. Zweitens: Wegen

seines empirischen Ansatzes war Friedman an einer Diskussion von Fragen, die von der Sozialfunktion des Geldes aufgeworfen werden, relativ wenig interessiert, während sich Brunner und Meltzer häufig mit diesen Fragen auseinandergesetzt haben. Drittens: Dieses zweite Charakteristikum der Arbeit Friedmans veranlaßte ihn zu einer Erörterung der Wechselwirkung von Inflation und Arbeitslosigkeit in Begriffen, die eindeutig die neoklassische Analyse durch *Robert E. Lucas* und andere vorwegnehmen, wohingegen Brunner und Meltzer wegen der Betonung, die sie auf die Rolle des Geldes als Mittel der Bewältigung von Informations- und Koordinierungsproblemen gelegt haben, dazu veranlaßt worden sind, viele der Einsichten dessen vorwegzunehmen, was heutzutage die „neue keynesianische Wirtschaftslehre“ genannt wird. Somit wirft die monetaristische Literatur der Zeit von vor 25 Jahren ein aufschlußreiches Licht auf die Debatten unserer Zeit.

### Résumé

#### Quelques aspects du monétarisme des années 70 – Une vue de 1994

Vu de 1994, le monétarisme de 1970 n'était pas une doctrine aussi homogène qu'elle ne semblait l'être à l'époque. On voit aujourd'hui trois différences entre les doctrines principales de *Milton Friedman* d'une part et de *Karl Brunner* et *Allan Meltzer* d'autre part. (1) Brunner et Meltzer attachèrent plus d'importance au rôle des marchés du crédit que ne le fit Friedman; (2) l'empirisme de Friedman le laissa relativement désintéressé à la discussion sur les résultats soulevés par le statut de la monnaie en tant qu'institution sociale, un sujet fréquemment discuté par Brunner et Meltzer (3) la deuxième caractéristique du travail de Friedman le conduisit à discuter l'interaction de l'inflation et du chômage en termes qui préfigurent clairement l'analyse néo-classique de *Robert E. Lucas* et autres, tandis que l'insistance de Brunner et Meltzer sur le rôle de la monnaie comme un moyen de faire face aux problèmes d'information et de coordination les amena à anticiper beaucoup de connaissances de ce qu'on appelle aujourd'hui le «nouveau keynésianisme». Donc, la littérature monétariste d'il y a 25 ans éclaire de façon importante les débats modernes.