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Abstract. Milton Friedman's contributions to and influence on macroeconomics are discussed, beginning with his work on the consumption function and the demand for money, not to mention monetary history, which helped to undermine the post World War II "Keynesian" consensus in the area. His inter-related analyses of the dynamics of monetary policy's transmission mechanism, the case for a money growth rule, and the expectations augmented Phillips curve are then taken up, followed by a discussion of his influence not only directly on the monetarist policy experiments of the early 1980s, but also less directly on the regimes that underlay the "great moderation" that broke down in the crisis of 2007-2008. Friedman's seminal influence on the development of today's mainstream, stochastic, but essentially Walrasian, macroeconomic theory, rooted in his explicit deployment of econometric theory in the analysis of forward-looking maximising behaviour in 1957, and in his later work on the Phillips curve, is also assessed in the light of his own preference, which he shared with Keynes, for a pragmatic Marshallian approach to economic theorising.

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Milton Friedman was an economist both well known among the general public and also acknowledged within his discipline as having made contributions to it of lasting importance, and rewarded for them with a Nobel Prize in 1976. Among twentieth century economists, only John Maynard Keynes has a claim, by no means undisputed, to a higher rank. The reputations of the two, both as public intellectuals and economic scientists, are deeply intertwined within the evolution of macroeconomics - that branch of the subject that deals with the behaviour of the economy as a whole - as we shall now see.

An overview

Keynes had been pivotal in the evolution of macroeconomics into a distinct sub-discipline in the wake of the Great Depression, and in helping to direct it in a particular policy direction, and in this essay I shall argue that, beginning in the 1950s, Friedman would play a key part in bringing about a radical re-assessment of the central tenets of macroeconomic theory that Keynes had helped to establish, not least as they appertained to the explanation of the Great Depression of the 1930s, and of their policy implications too. I shall also argue, however, that, if not as public intellectuals, then certainly as economic scientists, Friedman and Keynes belonged to the same intellectual tradition; namely, that associated with Alfred Marshall. This tradition has lately fallen into neglect, and ironically so, since this has happened, in some measure, because of Friedman's work.

Friedman's generation of economists came to intellectual maturity during the Depression, and it would have been natural for him to have been concerned with macroeconomic questions from the very outset of his career. Anecdotal evidence (see, for example, Friedman and Friedman 1998, p. 81, fn).suggests that, initially, he was a rather uncritical supporter of Franklin Delano Roosevelt's *New Deal* - in its own right, a largely independent source of many of the dirigiste policy ideas that in the post-war years would come to be labelled, not always fairly, as "Keynesian". But Friedman's earliest academic interests were more directed to pure micro-economic theory and mathematical statistics than to macroeconomics, and as I shall show below, when Friedman began his efforts to transform macroeconomics in the 1950s, it was by bringing his expertise in precisely these areas to the very centre of the study of consumption behaviour, a topic which had also engaged his attention from the 1930s onwards.

A Theory of the Consumption Function (1957) was by no means Friedman's first publication on either the economics of consumption in particular, or macroeconomics in general, but it was utterly central, both to his own work and to the evolution of the discipline, so this is where I shall begin the following account of his contributions. I shall then take up his work on monetary theory, monetary history and monetary policy, and end with a brief assessment of his overall influence on the development of macroeconomics from the early 1970s onwards, by which time his own contributions to the area were already mainly complete.

Textbook "Keynesian" Economics and the Consumption Function

Keynes's 1936 *General Theory* sought to explain the occurrence and persistence of large scale unemployment such as had been experienced during the Great Depression,, and it did so in a way that proved readily amenable to a degree of simplification, of which Alvin Hansen (1953) provides the definitive example. By the 1950s this simplified version of "Keynesian economics" was the stuff of intermediate and even elementary textbooks.

Textbook macroeconomics in the 1950s

According to this analysis, the overall level of unemployment varied with the economy's real output Y, which, when resources were unemployed, was able to respond more or less passively to satisfy the aggregate demand for goods and services. This demand, in turn, came from three sectors of the economy: households (consumption, C), firms (investment, I) and the government (government expenditure, G). Investment was treated as largely autonomously determined by the "animal spirits" of firms, unstable over time and sufficiently insensitive to influence from monetary policy that even the reasons for this imperviousness could safely be neglected in elementary expositions. Households were said to divide their incomes between consumption and saving according to a "fundamental psychological law" - Keynes's own phrase (1936, p.96) – according to which a stable fraction (c, called by Keynes the *marginal propensity to consume*) of changes in income were spent on goods and services. Since, at the level of the economy as a whole, the real value of output was paid out to households as real income, the most elementary version of this system could be written down as follows

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

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

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

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

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

Friedman's empiricism and the marginal propensity to consume

Now, quite independently of Keynes, the 1920s and 1930s had seen a rapid growth of explicitly

empirical economics.[1] Data were systematically collected, and statistical techniques developed to analyse them. The National Bureau of Economic Research (NBER), closely associated in the 1930s with Columbia University was at the forefront of such efforts in the US. Friedman took courses from the Bureau's founder Wesley C. Mitchell while a graduate student at Columbia, and his Ph.D thesis, which extended work originally begun by Simon Kuznets, was supervised by Mitchell's collaborator and successor as the Bureau's director, Arthur F. Burns, and was published by the Bureau in 1945, with Kuznets as joint author, under the still well-known title, *Income from Independent Professional Practice*. Friedman's abiding respect for data, and his insistence that economic models were there to explain them, marked him as an heir to the NBER tradition, as indeed did some of his specific empirical techniques, which often differed from those that would, under the influence of the Cowles Commission, in due course come to dominate orthodox econometrics.[2]

More to the point under discussion here, so did Friedman's familiarity with the difficulties that empirical evidence created for Keynes's stable psychological law. From the very outset, statistical studies, many of them done under NBER auspices, showed that, although consumption did indeed seem to vary as a fraction of income, the quantitative relationships involved were much more complicated than Keynes had suggested. Over a "long run" of a few decades, the consumption-income relationship seemed to be one of strict proportionality with, to put matters in terms of equation (1), c being stable and positive and a being equal to zero. Over shorter periods c seemed to be smaller than in the long run, and a positive, but shifting up over time. Keynes's fundamental law was supposed to apply to households in general, furthermore, and when cross section data drawn from budget studies were analysed, they yielded a wide variety of estimates for both parameters.

There is neither need nor space here to go into the many studies in which these empirical anomalies were analysed, and which Allan Hynes (1998) has already carefully surveyed[3]. Suffice

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[1]In the UK indeed, these developments were hampered by Keynes's own hostility to them, at least until the onset of World War 2. See Don Patinkin (1976)

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

[3]Franco Modigliani's work on the consumption function merits citation here as an independent and slightly earlier variation on the same theme that Friedman developed. See for example Modigliani and Brumberg, (1954). Modigliani's work differed in emphasising "life cycle" effects on consumption, rather than forward looking behaviour *per se*, and did not display the seamless integration of economic and econometric theory that marked Friedman's contribution, and on which I comment below. The Swedish Academy of Sciences found Friedman's and Modigliani's contributions sufficiently distinct that they had no difficulty in awarding separate Nobel Prizes to each of them.

it to say that many of the ingredients of Friedman's 1957 analysis are to be found in this earlier literature, to which he himself had already contributed. What mattered in 1957 was the particular way in he which integrated them into a *Theory of the Consumption Function* that would not only have a direct impact on contemporary macroeconomic orthodoxy, but would also, in the longer run, turn out to have been a fundamental turning point in the way in which macroeconomic theory was done.

The permanent income hypothesis

To the inter-war generation of economists, brought up to analyse the economy one market at a time, the idea that the demand for any particular good would, *ceteris paribus*, vary with income was a common-place, and I conjecture that many of them thought of Keynes's fundamental psychological law as a simple generalisation of this to the level of consumption as a whole. But there is a massive fallacy of composition here. Friedman the microeconomic theorist, acknowledging the priorities of Irving Fisher (1907, 1930) and Kenneth Boulding (1948) – see Friedman (1957, p. 7, fns.) - understood that the relevant objects of choice in the micro-economics of the consumption function had to be consumption now and in the future, and that the constraint upon choice was defined by income now and in the future as well as the terms upon which it could be loaned out or borrowed against.

On the assumption of a perfect capital market, the typical consumer could be thought of as able to sell his current and expected future income stream and purchase an annuity with the proceeds, and it was this hypothetical annuity, the consumer's *permanent* income, that Friedman postulated to be relevant to the choice of today's consumption. Current consumption, the outcome of a forward looking maximising decision about that variable's overall time path, would only vary with *current* income to the extent that variations in the latter affected permanent income. *Transitory* income, the difference between permanent and current income, would have no influence on consumption.[4]

Friedman the statistician then developed the implications of this elementary micro-economic theory in terms of the statistical theory underlying the least squares estimation of the parameter c in econometric work. Specifically, he noted that, if the true model determining consumption was

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[4]The vocabulary of *permanent* and *transitory* components of income, their statistical interpretation as systematic and random components of the variable, and indeed a more general analytic approach based on forward looking maximisation, are already present in Friedman and Kuznets (1945). Given Irving Fisher's emphasis on forward looking maximisation, albeit in a non-stochastic environment, and the fact that Friedman (1957) cited his work, some might be tempted to think of Friedman as self-consciously reviving the important Fisherian tradition in American economics. However, there is no reference to Fisher in Friedman and Kuznets (1943), the capital theory set out in Friedman's (1962) *Price Theory - a Provisional Text* derives from Frank Knight, with no reference to Fisher, while Fisher's name appears in neither the Index nor the Bibliography of Friedman and Friedman (1998). It is hard, therefore, to make a case for a direct Fisherian influence on Friedman. I am grateful to Allan Hynes and Perry Mehrling for discussion of this issue.

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

but the model estimated by a least squares regression was

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

where

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

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

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

Implications for "Keynesian" economics

Thus did Friedman (1957) offer a seamless blending of forward looking microeconomic analysis and statistical theory to resolve the empirical puzzles that earlier studies of consumption behaviour had revealed, and he proceeded to show that his explanation had exceptionally strong explanatory power over many of the detailed problems that these had uncovered. Friedman's permanent income hypothesis also implied that Keynes's marginal propensity to consume out of current income and therefore the multiplier as well were anything but stable, and provided a shaky foundation either for explaining the behaviour of the economy or designing policy. The full implications of this analysis were not at first widely appreciated, however.

To begin with, the particular method that Friedman chose to implement the idea of permanent income for empirical aggregate time series purposes, which were the ones that mattered for macroeconomics not to mention its policy applications, considerably lessened the initial impact of his work. In this context Friedman measured permanent income as a geometrically declining weighted average of current and past aggregate income (multiplied up by an adjustment factor to allow for the fact that such a technique shifted the mean of the series back in time, and hence, given economic growth, would understate its current value in a growing

economy.)[5] Specifically, with b < 1, and ignoring this growth adjustment for simplicity, permanent income became

$$Y(p) = by + b(1-b)Y(-1) + b(1-b)(1-b)\,Y(-2) \ldots \qquad (7)$$

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

A second and more basic factor also lessened the initial impact of Friedman's work on macroeconomics more generally. To quote one of his favourite aphorisms, "it takes a theory to beat a theory" and if systems built around equations 1 - 3 were to lose their dominant position in the mainstream of macroeconomics, something else, preferably as simple, had to replace them. By 1957 Friedman's revived *quantity theory of money* was in fact already on offer as this "something else", but it did not fully assume this role for another decade or so, as I shall now explain.

The Quantity of Money and the Rate of Interest

In 1936, Keynes had frequently contrasted his new, and, as he thought of it, revolutionary theory with what he called *classical economics*. The essential difference between the two systems, he insisted, was that, in his, shifts in the level of investment created shifts in income and employment, so that prolonged depressions could be attributed to a chronic lack of investment opportunities. In what he presented as prevailing classical orthodoxy, on the other hand, such shifts would create variations in the rate of interest sufficient to ensure that investment would always stay at a level high enough to fill the gap between income and consumption – i.e. saving - at full employment.

Classical economics as described by Keynes was a gross caricature. From the 1890s onwards, an increasing number of economists had argued that market economies seemed to have a hard time co-ordinating the allocation of resources over time – that is in keeping saving and investment in equilibrium with one another at full employment. By 1936 there already existed a

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[5]The "adaptive expectations" idea that underlies this formulation was, as Philip Cagan (2000) has noted, picked up by Friedman in 1953 from conversations with A. W. Phillips. Under Friedman's influence, it had already been successfully deployed to proxy inflation expectations by Phillip Cagan in his (1956) study of hyper-inflations, and was being used by David Meiselman (1962) in a study of the role of interest rate expectations in determining their term structure. It also had the virtue of providing a "good fit" to US time series data on consumption. One can see easily enough why it attracted Friedman in this context.

large and complex literature that pointed both to this failure as the source of real economic fluctuations and to the workings of the monetary system as the source of the trouble, but it had achieved no consensus about just how these two factors might be linked. Thus, there was nothing original about Keynes' stress on the unreliability of inter-temporal co-ordination mechanisms that were supposed to work through the interest rate in a monetary economy, but his specific explanation of why they might fail was nevertheless coherent and highly original, and it lies at the heart of the *General Theory's* contribution to economics.

Liquidity preference

This explanation centered on the theory of *liquidity preference*, the very monetary complication that, by the 1950s, was often omitted from elementary textbook accounts of Keynes's macroeconomics, though it was, of course, included in more advanced expositions that followed Hansen (1953). This theory built upon what is nowadays known as the *Cambridge* version of the quantity theory of money, which, in the hands of Alfred Marshall (1871) and Arthur C. Pigou (1917) had initially applied supply and demand analysis to the stock of nominal money in order to determine its purchasing power.[6] The central proposition underlying the Cambridge model was that any representative economic agent, and hence the economy in aggregate, would have a well determined demand for a stock of *real* money – i.e. measured in units of constant purchasing power. In the writings of its originators, this demand was said to emanate from money's use as the economy's means of exchange and reflected what we would now call transactions and precautionary motives. They argued that this demand would usually represent a stable fraction of the money-holders' *resources*, but they were routinely unclear as to whether this word referred to wealth, a stock, or income, a flow. Also, though they recognised that wealth not held as money could be held in other income-yielding forms - among Alfred Marshall's own examples were "a horse" and "furniture" - the insight that the demand for money might be systematically related to some measure of the opportunity cost of holding it - a rate of interest on a representative financial asset, for example - eluded them. It was Keynes, in his *Treatise on Money* (1930) who finally brought clarity to these matters in a way that attracted widespread attention, though the priorities of Frederick Lavington (1921) in sketching out the relevant ideas should be acknowledged, as Keynes notably did not.[7]

In the *Treatise* Keynes argued that the demand for money (at least that part of it related to what he called the *financial circulation*) should be thought of as the outcome of a portfolio allocation decision, and that the relevant constraint here was wealth. Crucially, he also argued that the rate of interest paid on financial assets such as bonds represented an opportunity cost of holding stocks of money, particularly those whose demand derived not from transactions in markets for goods and services but from keeping options open in the face of the risks posed by

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[6]The pioneers of the Cambridge approach did not refer to their model *per se* as the quantity theory, but preferred to say that it yielded the same prediction of proportionality between the quantity of money and the price level as did that older model, which was explicitly based on the concept of the velocity of circulation.

[7]These developments are discussed in detail by Don Patinkin (1974) and Laidler (2004, ch. 13 [1980]).

financial market activities.

With a few modifications that need not concern us here, he carried these ideas over into the *General Theory*, suggesting there that, in a monetary economy, the rate of interest had two roles to play; namely, to maintain equilibrium in the inter-temporal allocation of resources as so-called classical economics stressed, but also, and mainly, to equilibrate the supply and demand for money, particularly that component of the latter which sprang from *speculative* motives associated with uncertainty about the future prices of financial assets, and hence about the future time path of the rate of interest itself. Keynes argued, therefore, that the rate of interest had too much work to do in a monetary economy, and could not be relied upon to keep saving and investment in equilibrium. Crucially, since holding money always enabled agents to keep their options open, but holding bonds exposed them to the risk of making capital losses if the rate of interest rose, an eventuality whose likelihood increased when rates were at low levels, the phenomenon of liquidity preference would set a floor below which the rate of interest could not fall, even though, when investors' animal spirits were low, this floor might nevertheless keep the interest rate too high to induce a full employment level of investment.

This was the state of affairs that, in 1936, Keynes suggested prevailed in Britain and the US, and, because, as his popularisers soon noticed, a rate of interest that was "stuck" at a low and more or less constant level could simply be dropped as a determinant of investment, his ideas were in due course simplified into the type of system encapsulated in equations (1) - (3) above which, as already noted, was widely presented in the elementary textbooks from the 1950s onwards.

IS-LM

If instead of being totally autonomous, investment, still with an autonomous component that we might label A, also varies with the rate of interest, r (let us call the relevant parameter i) then equations (1) - (3), suitably extended, yield not a model of the determination of real income, but a so-called IS relationship defining those combinations of the rate of interest and real income at which investment and saving are equal to one another. If we then characterise the interaction of the supply and demand for real money balances (M/P) in the following terms,

$$Ms/P = Md/P = mY - l(r) \qquad (8)$$

we have the so-called LM relationship, which defines combinations of these same two variables that equate liquidity preference (the demand for money) to the money supply. Combining these curves yields a linear version of Hansen's (1953) famous IS-LM model, which was widely accepted in the 1950s and '60s as an accurate representation of Keynes's analysis, and quickly became the stuff of intermediate and even advanced textbooks.[8] This model's reduced form may be written as

$$Y = 1/[1 - c + (i/l)m][A + G] + 1/[m + (l/i)(1 - c)]Ms/P \qquad (9)$$

which, as already noted, approaches equation (3) above as the demand for money becomes more

[8] How much of Keynes's own economics was actually preserved in this system was, and still is, a much debated matter. For a recent treatment, See Roger Backhouse and Laidler (2004)

and more interest sensitive and l goes to infinity, but, which, as this sensitivity disappears and l goes to zero, instead approaches

$$PY = (1/m)Ms \qquad (10)$$

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

Thus, just what kind of message about the workings of the economy followed from the IS-LM model that by the 1950s had been extracted from Keynes' work and embedded in macro-economics as an appropriate representation of it hinged in an important way on empirical propositions not just about the consumption function but also about the demand for money function. We have already seen that Friedman's work on the consumption function questioned the stability of simple multiplier analysis and its policy applications, without in and of itself providing an alternative to it. As we shall now see, his essentially contemporaneous work on the demand for money function addressed precisely this second challenge by promoting a particular form of this relationship as an empirically stable alternative to the consumption function that could replace it as the key to explaining economic fluctuations and designing policies to deal with them.[9]

Friedman's revival of the quantity theory

By the mid-1950s, Keynes's theory of liquidity preference had already prompted a number of empirical studies that had seemed to establish that the demand for money was indeed interest-sensitive. Less directly, his theory also lay behind theoretical work on the transactions demand for money (Baumol 1952, and Tobin 1956), and on the demand for money as a financial asset (Tobin 1958). Hence, when in (1956) Friedman proposed that the demand for money was fundamentally a demand for real balances, the outcome of a portfolio allocation decision, and would vary with real income and a number of measures of the opportunity cost of holding it, his general formulation of the relationship stood only a little apart from contemporary discussions of the topic in matters of substance, the most important difference here being his explicit claim – similar to that made by Keynes on behalf of his consumption function - that the relationship was empirically stable.[10]

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

[10]Among other differences, Friedman referred to money as a "temporary abode of purchasing power", avoiding then usual distinctions among transactions, precautionary and speculative motives for holding it, and he paid only passing attention to the "liquidity trap", as economists had come to call that region of the by then standard intermediate textbook version of the function. where its interest elasticity approached infinity. In effect he treated real money balances "as if" a consumer durable good, thus forging an unusual link between his Fisherian theory of the consumption function and the Cambridge approach to the quantity theory.

However, the title that Friedman gave this (1956) essay, "The quantity theory of money, a restatement", and its publication as the introductory essay to a set of *Studies in the Quantity Theory of Money,* in and of themselves matters of style rather than substance, were calculated to be much more controversial, as Patinkin (1969) pointed out. In the 1950s, if the phrase quantity theory of money was deployed at all, it was as label for a meaningless tautology that had been part of the erroneous classical doctrine that Keynes had successfully overthrown.

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

It was a key message of the (1956) studies by Philip Cagan of European hyper-inflations, and Eugene Lerner of the Confederacy, that this explanation was well supported by empirical evidence - Nazi Germany (John J. Klein) however, provided a counter-example - and it was also shown that the parameter values underlying the relevant dynamics were such that it would have been possible to bring the inflations studied under control by reducing the rate of monetary expansion.[11] Even so, (and with the exception of Richard Selden's paper on the long-term monetary experience of the United States) the fact that the 1956 empirical *Studies* dealt with rapid inflation in rather far-off times and exotic places, tended to lessen the impact of their message for contemporary advanced economies, where the low but persistent inflation that was then being experienced continued, well into the 1960s, to be attributed by most observers to institutionally driven cost-push forces rather than monetary factors.[12]

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

[12]Though this was the view from the US and Europe, it was a different matter in Latin America, where the quantity theory as a theory of inflation played a critical role in the *Monetarist-Structuralist* debates that began in the mid-1950s. See Baer and Kershenetsky (eds.) (1963) for a representative collection of contributions to this debate. In general, Friedman's revived quantity theory found more immediate applications in less developed economies than at home. A subsequent collection of essays based on Chicago Ph.D. theses edited by David Meiselman (1970) contained studies of Chile (J. V. Deaver) Argentina (A. C. Diz), Post-War

Be that as it may, the challenge to contemporary macro-economic orthodoxy implicit in Friedman's invocation of the quantity theory of money in 1956 was given added substance, and placed firmly in a US context too, by his 1959 "The demand for money: some theoretical and empirical results". This paper's main theoretical innovation was to affirm that the measure of income upon which the demand for money ought to depend was, like consumption, its permanent and not its current value, from which proposition there seemed to follow a startling empirical result, which Friedman developed using not conventional econometric techniques, but statistical methods derived from the NBER tradition. Specifically, treating each NBER-dated business cycle as a single observation, he ran the regression of real money holdings on real income, which he argued was essentially equal to permanent income when measured over a complete cycle. He then projected annual average money holdings by substituting into this equation estimates of annual permanent income obtained in his study of the consumption function, and showed that there seemed to remain no systematic within-cycle variations in the demand for money that could be attributed to variations in interest rates. Without drawing attention to the potential inconsistency between this result and those about the systematic effects of variations in the opportunity cost of holding money on the demand for it presented only three years earlier in the (1956) studies, he suggested that perhaps the strong evidence that others had found of an important interest sensitivity to the demand for money in U.S. data was a misleading consequence of their having erroneously used current rather than permanent income in their regressions.

This paper was soon followed up by another - Friedman and Meiselman (1963) - which used a more conventional technique - multiple regression analysis - to relate variations in nominal consumption (instead of income of which autonomous expenditure was itself a component) to variations in a measure of nominal autonomous expenditure – i.e. (I + G) in equation (3), except that Friedman and Meiselman deployed nominal rather than real variables - and to compare the outcome here to that obtained when the nominal money supply was used as an independent variable – i.e. essentially equation (10) again with nominal variables substituted for real - as well as to estimate equations containing both variables. Their results seemed to show that, except in the 1930s, money dominated autonomous expenditure as an explanatory variable. Taken together, Friedman (1959) and Friedman and Meiselman (1963) suggested that a demand for money function in which the rate of interest played no significant role could usefully replace the Keynesian consumption function as the crucially stable empirical relationship around which explanations of macro-economic instability could be constructed and policies to counteract it designed. In short, taken at face value, they seemed to show that there did indeed exist an alternative but equally simple theory - essentially an income velocity version of the quantity theory of money – that could beat the basic Keynesian model, already weakened by Friedman's work on the consumption function.

Japan (M. W. Keran), South Korea and Brazil (C. D. Campbell), as well as of Canada (G. Macesich) and a cross section of 47 countries, 26 of which were, however, located in either Asia and Latin America (M. Perlman).

The Monetary History of the United States

These papers attracted a great deal of attention in the early 1960s, but with the passage of time, both of them turned out to be flawed.[13] Their longer-term influence on the development of macroeconomics in any event pales in comparison with that of *A Monetary History of the United States,* which Robert Hetzel (2007) suggests, with considerable justice, to have been Friedman's single most influential work. Though not published by the NBER until 1963, this book was the product of collaboration between Friedman and Anna J. Schwartz that had begun more than a decade earlier and had been influencing Friedman's monetary economics throughout the 1950s. The *Monetary History* was a work of quantitative, though not econometric, history, systematically tracing the causes and effects of variations of the quantity of money on the US economy since 1867, and it drew on an extremely large background literature dealing with specific historical episodes and/or issues, some produced by other NBER affiliates, and some by Friedman's Chicago graduate students. Not surprisingly, furthermore, its analysis revolved around the interaction of the supply of money with a demand function very like that postulated in Friedman's 1956 and 1959 papers, although no explicit model expounding the details of these mechanisms was set out.

The story that the *Monetary History* documented, on a cycle by cycle basis and in considerable detail, was that variations in the rate of growth of the money supply seemed systematically to lead the cycle, and in all probability to play a significant role in causing it too. The evidence was stronger for some cycles than for others, to be sure, and often showed strong and systematic feed-back effects from economic activity to money, but overall the picture seemed to be clear. This was particularly the case for the so-called the *Great Contraction* of 1929-33, the very episode when, according to the conventional wisdom prevalent in the 1950s, market mechanisms had most clearly failed, and the weakness of monetary policy had most vividly been demonstrated.

Quite to the contrary, Friedman and Schwartz claimed that a typical cyclical downswing, presaged by a slowdown in money growth, had started in late summer of 1929, but had been first allowed to get out of hand, and then actually exacerbated, by Federal Reserve policy. This policy had been slow to respond to initial signs of weakness in the banking system that had followed on the October stock market crash and then had permitted the money supply to collapse as a consequence of the series of banking crises that in due course followed, when a sufficiently vigorous response on the Fed's part, by way of lender of last resort activities and large scale open market operations, could have prevented this disaster and the depression that followed. Rightly or not, and that is not the point here, many more readers were eventually to be convinced by this narrative than by, say, the Friedman-Meiselman study, that Friedman's messages about the importance of money for ensuring the stable behaviour of the economy, and about how seriously the then prevailing "Keynesian" orthodoxy underestimated this factor, had to be taken seriously.

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

It is important to grasp not only how deeply Friedman's historical work undermined the Keynesian consensus described at the outset of this essay, but also how slowly this was initially recognised. That Friedman and Schwartz's account of the Depression downgraded the significance of swings in investment and of fiscal policy too for macroeconomic behaviour, and attached increased importance to money was evident enough from the outset, of course, and generated considerable controversy in its own right. But their conclusions also ran counter to the more general view, so much taken for granted in the early 1960s that it was rarely debated, that a modern monetary economy is fundamentally incapable of effectively allocating resources over time - of co-ordinating savings and investment while maintaining full employment - so that active and continuous government intervention is required to ensure its stability. This implication of their work was grasped much more slowly, though it would eventually leave deep marks on both academic economics and economic policy from the 1970s onwards.[14] At considerable risk of oversimplification, it perhaps required Axel Leijonhufvud's (1968) very success in making economists look beyond Hansen's IS-LM version of Keynesian economics and become self-conscious about Keynes' own vision of the flaws inherent the monetary economy's co-ordination mechanisms to enable them also to appreciate the full extent of the challenge that Friedman was mounting to that vision's empirical significance.

Friedman's Marshallianism

Friedman's challenge was nevertheless empirical, not ideological. As Robert Clower stressed in his (1964) review of the *Monetary History*, though Friedman differed radically from Keynes on matters of substance, when it came to analytic methods, Keynes and Friedman alike were Marshallians. Both sought to construct simple macroeconomic frameworks around empirically stable relationships, within which the economy's responses to shocks could then be analysed as evolving over time – a factor that the standard IS-LM version of macroeconomics totally neglected - as the constraints imposed by various short-run rigidities were relaxed.

It is of course true that Friedman's specific framework differed from Keynes's, and also seemed to support a substantive vision of the monetary economy's workings that in some important respects more closely resembled that of those Austrian economists who, using a theory of economic fluctuations grounded explicitly in Walrasian general equilibrium theory, had been Keynes's principal rivals in the 1930s in the competition to shape the then emerging sub-discipline of macroeconomics.[15] Like the Austrians, and unlike Keynes, Friedman argued that

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[14]I am grateful to Susan Howson for making me pay attention to the relative slowness with which the full implications of Friedman's work on money made themselves felt.

[15]As he made clear in (1953a), Friedman regarded the essential difference between Marshallian and Walrasian methods as lying not in the distinction between partial and general equilibrium analysis, but in that between economic theory used to formulate refutable hypotheses and hence empirically useful, and economic theory constructed so as to encompass all logically possible outcomes, and hence empirically vacuous. As Allan Hynes has pointed out to me, his (1949) interpretation of "The Marshallian Demand Curve" made a powerful case for treating Marshall as a general equilibrium theorist. See however, fn 22 below on the later evolution of the Marshallian-Walrasian distinction.

markets were stable and, if left to themselves, were capable of dealing efficiently with allocative challenges, and also that if they failed to meet those challenges, this was not because their mechanisms were inherently flawed, but because misconceived monetary policies had been visited upon them. Like the Austrians also, and again counter to Keynes, he argued that activist policies, far from being routinely needed to stabilize the market economy, were in most circumstances the principal source of its instability.[16]

But quite unlike the Austrians, and just like Keynes, Friedman derived these conclusions from empirical analysis of competing simple models and from confronting them with facts as revealed by narrative history, rather than from any unquestionable set of first analytic principles. His differences with Keynes were thus, as the foregoing discussion has been meant to show, scientific and debatable within a set of rules that give primacy to the ability of economic analysis to withstand the tests of logical consistency and to confront empirical evidence. Though these differences certainly had profound implications for how any society might think about organising its economic life, they were not, in and of themselves, ideological. Friedman's views on macroeconomic policy in general and monetary policy in particular, which we shall now discuss, were also grounded in these substantive views about how the economy functions.

Monetary Policy for a Dynamic Economy

It should already be clear that the full extent of Friedman's contribution to macroeconomics cannot be grasped within the logical confines of IS-LM analysis and the dominance of this framework in the 1950s and 1960s perhaps provides yet another reason why the full significance of those of his contributions to macroeconomic theory and monetary history discussed so far came to be appreciated only slowly. In particular, as Backhouse and Laidler (2004) have argued, the IS-LM model, being a comparative static construct, helped to create an intellectual climate in which, for a while, the central fact that economic activity happens in time became obscured.

Forward looking behaviour, time lags and monetary policy

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

[16]Nicholas Kaldor (1970) was early among Friedman's critics in noticing some affinities between his work and that of the Austrians.

easy business.

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

In an IS-LM model in which the demand for money is more or less independent of the rate of interest, fluctuations in Keynesian animal spirits do not affect income and employment at all provided that the money supply is held constant, but such analysis does not do justice to Friedman's policy vision. For him, there was more than enough elasticity in the economic system to permit real shocks to have consequences, either because the economy could function temporarily with less money in circulation than was demanded, or because that demand depended in any case on permanent rather than current income. In such circumstances offsetting policy measures could in principle improve matters, but in practice they risked making them worse, as he showed, for example, in (1953b)[17] By the time they began to take effect, they could end up not stabilising, but further destabilising an economy that was already responding to new shocks.

For Friedman, then, the principal problem in the implementation of policy was not to create institutions that would facilitate the rapid and continuous discretionary deployment and withdrawal of economic stimulus - *fine tuning* as such measures were called in the 1960s without a trace of irony - but that would impose constraints to prevent policy makers over-reaching themselves. Already in (1948) when he was still apparently something of a new-dealer in his thinking, he had, in this spirit, proposed a system that would exploit the built-in stabilising effects of having the counter-cyclical deficits generated by the interaction of stable government expenditure and progressive income taxation automatically funded by money creation, but as his work progressed and he became more convinced of the inherent stability of an economy that was not subject to monetary shocks, his attention shifted away from seeking ways to co-ordinate monetary and fiscal policy towards devising an institutional framework that would prevent monetary shocks occurring.

Policy rules

The *Program for Monetary Stability* that Friedman laid out in (1960) amounted to no more, and no less, than the imposition on the Federal Reserve system of a legally binding requirement that the money supply grow at a constant percentage rate on a month by month basis. It thus involved a *rule* for monetary policy, in two distinct senses. First, the behaviour of the money supply was to be *systematic* as opposed to *arbitrary,* and second, such behaviour was to be achieved not by *persuading* the central bank to *choose* it, but by *constraining* it by law to do so, by taking away

[17]This paper provides another example of Friedman's pioneering use of statistical theory in the analysis of a problem in economics.

from it the *discretion* to do otherwise.

In the first less important sense of the word, Friedman's money growth rule developed from his (1948) proposals. It involved the deployment of monetary policy as a built-in stabiliser, and was defended on the quite practical economic grounds that, given the state of macro-economic knowledge at the time, this would maximise monetary policy's contribution to the stability of the economy. In the second more fundamental sense, it represented something of a break with that earlier work inasmuch as it explicitly invoked the political principle that decisions impinging upon the stability of the monetary system, and hence of the market economy, being too important to be delegated to unaccountable functionaries, were proper objects for legislated, or even constitutional constraints. Here it will suffice to recall Sir Robert Peel's 1844 Bank Charter Act in Britain, Irving Fisher's tireless efforts at the time of the Federal Reserve system's founding, and into the 1920s to persuade Congress to legislate it into the single minded pursuit of price level stability, and Henry Simons' celebrated advocacy of similar measures in (1936), to demonstrate how deeply embedded was Friedman's proposal in the economic agenda associated with what used to be called *classic liberalism*.[18] The empirical foundations of this political doctrine seemed to have been thoroughly undermined by the interpretation of the experience of the 1930s as clear evidence of a fundamental flaw in the workings of a monetary economy, but, by 1960. Friedman's positive work was well on its way to restoring these foundations as we have seen, and hence provided a scientific supplement to his policy proposals' political basis.

The Phillips curve and monetary policy

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

For Friedman, already sceptical about the possibilities of fine tuning, claims about the theoretical and empirical robustness of the policy trade-off implicit in the Phillips curve were problematic. They seemed inconsistent with everything else that he thought he knew about how economies functioned, but it was one thing to be aware of this tension and quite another to

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[18]The direct influence here on Friedman's monetary policy proposals was Simons, as he himself acknowledged in (1967). His self-conscious adoption of classical liberalism as a political creed seems to have been heavily influenced by his interactions with colleagues in the Mont Pelerin society from the late 1940s onwards, notable Hayek, as is acknowledged in Friedman and Friedman (1998)

explain precisely where the problem lay. It was the latter task that he undertook in his 1967 Presidential address to the American Economic Association (Friedman 1968).

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

Not so, was Friedman's response. The price that cleared the labour market was the *real* wage, not the *money* wage, and excess demand or supply there should generate real, not money, wage changes. Agents bargained over money wages of course, so excess demand would initially cause money wages to rise, but this would soon feed through to the behaviour of prices and engender inflation expectations that would feed back into money wage bargaining. If excess demand was then held constant at a positive value by policy, the inflation rate would have to rise, setting in motion a never ending upward spiral. Therefore, any efforts on the part of policy-makers to buy, at the price of a little inflation, an unemployment rate permanently lower than the *natural* - structural and frictional - properties of the labour market could deliver, were doomed to collapse in the face of a perpetually accelerating price level. Long before 1968, Friedman had argued that monetary policy should provide a background of macroeconomic stability against which agents could then pursue their various private interests, but now it was possible to be much more precise about just what was involved here. Though Friedman himself was usually content to argue that monetary policy could influence only nominal variables, this proposition taken in conjunction with the natural unemployment rate hypothesis implied that the *only* macroeconomic variable that monetary policy could influence on anything other than a temporary basis was the inflation rate. Furthermore Friedman had earlier argued, in company with Phillips, be it recalled, that short-run fine tuning was too difficult to be practicable. "The role of monetary policy", then, could be no more than to stabilise inflation in the medium term.[19]

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[19]In this, Friedman's views on the policy implications of what came to called *the expectations augmented Phillips curve*, differed sharply from those of Edmund Phelps (1967), who worked out the same analytics as Friedman at about the same time. Phelps treated this new relationship not as presenting an obstacle to the fine tuning of an inflation-unemployment trade off, but as introducing a dynamic element into the optimization problem that policy makers faced. In the late 1960s, when some empirical evidence still seemed to suggest that the practical implication of the Friedman-Phelps curve was that the inflation unemployment trade-off became steeper in the long run than in the short, but did not vanish, Phelps' work received at least as much attention as Friedman's. More recently it has become recognised as the starting point for much of the work on monetary policy optimization in a dynamic setting that underlies so-called "flexible" inflation targeting, which is a major reason why Phelps was finally honoured for it with a Nobel Prize in 2006.

The Transmission Mechanism

In the early 1960s, many economists expressed doubts about the transmission mechanism that underlay Friedman's claims for the importance of money as a determinant of aggregate demand, even though rather elaborate accounts of it were to be found in Friedman and Meiselman (1963) and Friedman and Schwartz (1963b).[20]

The cyclical context

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

Here it is worth noting that Friedman's account of the cycle played no special attention to the behaviour of investment expenditure by businesses, even though this component of aggregate demand had been well known to fluctuate systematically more than others since the 19th century, was at the centre of the multiplier accelerator models that figured so prominently in the intermediate and advanced textbooks of the period – even that of his Chicago colleague Martin J. Bailey (1963) - and also provided the empirical rationale for Keynes's ideas about the role played by the animal spirits of business men in driving economic fluctuations.

Be that as it may, in the *Monetary History* (1963a), Friedman and Schwartz documented on a cycle by cycle basis the workings of the mechanisms described above and which bear a strong family resemblance to those postulated even before the Great Depression by Irving Fisher

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

(e.g. 1911, Ch. 4) and Ralph Hawtrey (e.g.1913), who had also paid virtually no attention to investment.[21] The *Monetary History* showed that the precise operations of these mechanisms in any particular episode depended on the extent to which the economy was simultaneously being affected by other shocks, not mention the nature of the monetary policy regime that was in place, which would, in particular, determine the nature and extent of the money supply's endogenous responses to swings in real income and inflation.

Nevertheless this work also seemed to confirm that a hard core of common elements were always present in the transmission mechanism, three of which were of particular importance. First money was substitutable across a wide range of margins, a characteristic that followed from Friedman's conception of it as a "temporary abode of purchasing power" whose demand was determined like that for any other service-yielding durable good. Thus the transmission mechanism was not, as so much contemporary "Keynesian" economics had it, confined to interest rate effects in organised financial markets. Second, the stock-flow interactions that lay at its heart were inherently dynamic and drawn out over time. Third, though it thus seemed possible to tell a coherent story about how and why variations in money growth induced changes in the time path of nominal income, the factors determining precisely how those changes were divided up between real income and prices in any particular episode remained elusive beyond a general, and empirically well established, tendency for variations in output and employment to get under way before price level effects came through.

The missing equation, disequilibrium and equilibrium

There thus seemed, as Friedman put it, to be one equation missing from his system; and at this point the development of his macroeconomics presents a major puzzle. In the 1940s and 50s, not just Friedman, but all macro-economists had faced a *missing equation* problem, but, as has already been noted, by the early 1960s, the curve created by Phillips in 1954 to fill just this gap in his own model of stabilization policy was being adopted by many others for the same purpose. It is easy to understand why Friedman, being suspicious of the original Phillips curve's validity, did not initially follow this trend, and the fact that his largely retrospective account of his *Monetary Framework* in (1974) presents the missing equation as a still current problem is probably to be accounted for by the fact that this particular paper was originally put together in late 1971 from two slightly earlier pieces, with a few paragraphs added from the then unpublished *Monetary Trends in the U.S and the U.K* (Friedman and Schwartz, 1981) .[22] Even so, by the early 1970s, the expectations augmented version of the curve which Friedman himself had developed in (1968) was already being taken up by others to fill precisely this gap, and he himself newer explicitly deployed it in this way.

Perhaps the solution to this puzzle is to be found in the fact that two mutually inconsistent sketches of the theoretical underpinnings of the curve in question were in fact present in

[21] The qualification here recognizes that Hawtrey presented an early account of the accelerator mechanism in (1913), in isolation from his main discussion of the cycle, and one that does not re-occur in his many later writings.

[22] I am grateful to Edward Nelson for reminding me of the significance of the provenance of this (1974) paper for the issues under discussion here. Precise details of these matters are given by Robert J. Gordon (1974, p. xi, fn. 2))

Friedman's (1968) paper, and that he was at least partially aware of the problems this posed for his work. The first of these sketches, based on Friedman's criticism of Phillips for having chosen money wage change instead of real wage change as the dependent variable of his relationship, nevertheless followed Phillips in treating variations in measured unemployment as a proxy measure of the state of excess demand or supply in the labour market. The second treated them as manifestations of supply side responses by members of a labour force who had mistaken local money wage changes, which were in fact part of economy-wide inflationary adjustments, for real wage changes. The first theoretical sketch treated the Phillips curve as a manifestation of a market *adjusting to disequilibrium*, and the second as the consequence of it remaining *in equilibrium* under conditions of less than perfect information. To keep the exposition of precisely what is involved here simple, it is convenient to consider the Phillips curve in its price-inflation-output rather than its wage-inflation-unemployment form, (though that is not how Friedman himself discussed the matter in 1968).

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

$$p = gy \qquad (11)$$

Once inflation expectations come into the picture with a unit coefficient, this becomes

$$p = gy + p(e) \qquad (12)$$

which is the price inflation – output equivalent to the first of Friedman's two formulations of the curve in wage-change-unemployment space. To get to the second formulation, we start from a conventional aggregate supply curve

$$y = h[P - P(e)] \qquad (13)$$

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

$$p = (1/h)y + p(e) \qquad (12')$$

Equations 12 and 12' are observationally equivalent as written here, but note that, if it is necessary do so in order to match empirical evidence, as indeed it usually is, there is no logical difficulty about lagging y on the right hand side of equation (12), but it is nonsensical to do this in the case of equation (12'). This is because these two equations embody fundamentally different visions of the economy's workings. The individual behaviour underlying (12) is not precisely defined, but seems to require an ability on the part of some agents, presumably sellers, to adjust prices in response to some kind of quantity signals emanating from the markets in which they operate. Its microeconomic foundations must therefore be sought in the theory of monopolistic competition. (12') on the other hand is transparently grounded in the theory of the price taking perfectly competitive agents operating in a continuously clearing market, and has obvious attractions for anyone who has come to macro-economics by way of micro-theory, let

alone someone who, like Friedman (see 1953a), had been a leading champion of the perfectly competitive model in the face of the challenges mounted to it by Edward Chamberlin (1933) and his supporters.[23]

It is not altogether surprising then, that when Friedman revisited his (1968) critique of Phillips in (1975), he repeated the charge that Phillips had discussed money wages instead of real wages, but now, referring to a recently resurrected 1926 paper by Irving Fisher (1973), added that Phillips had also gotten the direction of causation underlying his curve wrong: he had quantities affecting prices - unemployment causing wage changes - instead of prices affecting quantities - wage behaviour causing variations in unemployment. By 1975, that is to say, Friedman seems to have recognised the tensions between the two theories of the expectations augmented Phillips curve that uneasily co-existed in his Presidential address, and to have opted for the second of them, which by that time had been recognised by a younger generation of macroeconomists inspired by Robert E. Lucas (1972) as a key starting point for the so called New-classical revolution they were by then making. Certainly it is this second rationalisation that is to be found in the *New Palgrave* version (1987) of Friedman's major essay on the Quantity Theory of Money. Even so, Friedman seems never to have adopted the rational expectations hypothesis that usually complements this Phillips-curve-as-aggregate-supply-curve in New-classical economics. Though respectful of this idea's theoretical usefulness, he did not find it "the open sesame to unravelling the riddle of dynamic changes that some of its more enthusiastic proponents make it out to be." (Friedman and Schwartz 1981, p.630)[24]

But, in opting for the aggregate-supply-curve interpretation of his own expectations-augmented Phillips curve, even without rational expectations, Friedman nevertheless rendered it incapable of filling the role of the missing equation in his account of monetary policy's transmission mechanism. That account was designed to deal with the stylized facts about the interactions of money and other variables over the course of the cycle, and prominent among these was, and still is, a systematic tendency for real variables, such as output and employment, to lead inflation. Observed quantities cannot simultaneously lead observed prices and be interpreted as responding to them, so the version of the expectations augmented Phillips curve that Friedman preferred on theoretical grounds was inconsistent with the empirical evidence that

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[23]In 1953, however, Friedman treated perfect competition as a Marshallian tool, useful for empirical analysis, and classified monopolistic competition as empirically vacuous and therefore Walrasian. At that time the awkward question of who sets prices in an economy in which everyone is a price-taker was not on the agenda, and the necessity of postulating an "as if" auctioneer who continuously keeps prices at their market-clearing values as a necessary component of the microeconomic foundations of the perfectly competitive model was not understood. It was, above all Axel Leijonhufvud (1968) who forced macroeconomists to pay attention to this question. On this matter, see Laidler (2008).

[24]I am grateful to Allan Hynes for discussion of this point

it might have been required to explain.[25] Thus, one equation would always remain missing from Friedman's model of the transmission mechanism, as he affirmed as late as (1992, p. 49).

Friedman's Influence after 1968

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

The (1969) collection of essays, *The Optimum Quantity of Money,* contained only one new paper - its title essay - albeit a lengthy one, that developed the case for a fiat monetary system's aiming at a stable rate of *deflation,* equal in absolute value to the economy's real rate of interest, so as to maximise the productivity of the economy's stock of real balances. This essay is still much cited, but it was only one of a number of papers on this and related issues published at about the same time, and its survival where others of comparable quality, for example Johnson (1969), have faded from view perhaps owes as much to its author's overall reputation, as to anything unique in its content. Friedman's already cited contributions to Robert Gordon's (1974) edited volume *Milton Friedman's Monetary Framework,* were also aimed at his fellow economists, but their intention was to summarise previously expounded ideas and defend them, not to break new ground, and that was also the main and utterly appropriate aim of his (1977) Nobel Prize lecture. *Monetary Trends in the United States and the United Kingdom* (Friedman and Schwartz 1981) was a substantial work by any measure, but this, the final product of the monetary history project that had started in the early 1950s, had originally been intended to deal with trends *and cycles,* and had been in a more or less complete first draft form as early as 1970.[26]

Monetarism and stagflation

Meanwhile, Friedman was becoming increasingly visible and respected as a commentator on economic policy, sometimes writing alone - notably as a *Newsweek* columnist - and sometimes

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[25] Though, as Edward Nelson has pointed out to me, some so-called New Keynesian models of the Phillips curve deal with these timing issues by having currently observed quantities responding to expected prices,.

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

with his wife Rose Director Friedman.[27] It is beyond the scope of this essay to weigh the merits of Friedman's broader policy contributions, made as an increasingly visible public intellectual – I have briefly discussed these matters elsewhere (Laidler 2007) - but his influence over macroeconomic policy from the 1970s onwards does require attention here.

At the very beginning of that decade Harry Johnson (1971) had noted that Keynesian economics was at its strongest in dealing with the problems posed by unemployment, while Friedman's approach, by then known as *monetarism*, had the advantage when it came to inflation.[28] Because unemployment was the more serious policy issue, Johnson continued, Friedman's critique of Keynesian orthodoxy was likely to prove ultimately ineffective. Even as he wrote, however, inflation was fast becoming the main macroeconomic policy problem facing the US and those countries linked to it through the Bretton Woods system and would soon evolve into that ugly phenomenon with the equally ugly name, *stagflation.* As was noted earlier, the conventional wisdom prevailing at this time was that inflation in advanced economies was not a monetary phenomenon at all, but a consequence of institutionally driven cost push forces, and the simultaneous occurrence of rising inflation and rising unemployment in the early 1970s gave superficial plausibility to this explanation. This plausibility did not, however, survive the abject and extremely visible failure of wage-price controls unsupported by monetary measures in such economies as the US and the UK, and it soon became apparent that Friedman's monetary explanation of inflation, supplemented by his expectations-augmented Phillips curve, seemed to fit the stagflationary facts rather well.

The scene was thus set to try out a monetarist policy program, and from the mid-1970s until the early 1980s, with the precise dates varying from country to country, the rate of growth of one or more monetary aggregate became the lynchpin of anti-inflation policy in many places. Of course Friedman was not the only advocate of such policies, but it is hard to imagine that they would have become so widely popular so quickly without his specific influence, so it is also not surprising that, when they went wrong, he was expected to shoulder a good deal of the blame.

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[27]Their collaboration on popularising the policy implications of their economics had begun as early as 1962 with the initially under-appreciated *Capitalism and Freedom,* and would reach its high point in 1980 with *Free to Choose,* itself the by-product of an immensely successful television series. As the titles of these books indicate, Friedman's engagement in policy debates ranged well beyond the boundaries of macroeconomics. So did his influence, and his reputation among the public at large rests not just on his ideas about monetary policy, but on his having been, along with Friedrich von Hayek, one of the important brains behind that famous composite politician "Ronald Thatcher". Hetzel (2007) discusses these aspects of his work, as does Laidler (2007)

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

Monetarist policies in practice

At some risk of over-generalising, I would suggest that monetarist policies everywhere foundered on the same factors. To begin with, as implemented from the mid-1970s onwards, they were only distantly related to Friedman's (1960) *Program.* This had proposed a legally binding money-growth rule in order to maintain stability in an already smoothly functioning macro-economic environment, but actual policy tried to use discretionary control over money growth to restore stability in economies where inflation was uncomfortably high and still rising. Second, where Friedman had advocated that the monetary base be used as the policy instrument whereby money growth was controlled, actual policy relied on the manipulation of an interest rate. Central banks in effect used a model of the generation of the money supply in which the latter variable was posited to *adjust passively* to the behaviour of real income and prices in order to implement a policy derived from a model whose key postulate was that the money supply *actively drove* their behaviour.

Friedman is hardly to be blamed for policy failures attributable to these causes. Nor should he be expected to bear all of the responsibility for the frequency with which the quarterly and even monthly demand-for-money equations that underpinned the abovementioned policies displayed instability. In (1959) he had claimed empirical stability for a function fitted to business cycle average data, and in *Monetary Trends* Friedman and Anna Schwartz had disaggregated their data only to cycle phases. Short-run demand for money functions are tools of monetary fine-tuning, which Friedman had never advocated.[29]

He (and everyone else writing on the demand for money before the mid-1970s) were, however, more culpable in having failed to grasp the impact that institutional change within the financial sector could have on the economic significance of any specific monetary aggregate, including that (or those) chosen as the fulcrum of monetary policy. Some of this change in the 1970s was the result of regulatory interference with banking practices on the part of authorities who were simultaneously anxious to hit growth targets for specific aggregates and to avoid some of the consequences of doing so by changing the significance of those aggregates within the system (Goodhart's Law), some was the consequence of the private sector's reaction to the adoption of new policies (the Lucas critique), and some was simply caused by the application to banking of then recent developments in computing technology; but added together, these factors undermined not just short-run relationships but even the longer term stability of the demand for money functions on which policy was beginning to rely. Such instability in earlier times is in fact evident in the historical results presented in *Monetary Trends* (1981) but there is no trace of Friedman, or anyone else, having recognised this problem early enough for such information to have been useful to the conduct of money growth targeting in the 1970s and 1980s.

On top of all this, Friedman's frequent public warnings in the early 1980s, (1984 was a

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[29]If any single economist is to be singled out as having based a case for fine tuning monetary policies on the existence of a stable demand for money function, it is probably Franco Modigliani (!977). However, this attribution of responsibility, along with its accompanying relative absolution of Friedman, is informed by a good deal of hindsight. The distinctions between long and short-run stability of the demand for money function on which it rests were certainly made in the 1970s, but their crucial significance for policy was barely recognised, certainly not by this author.

particularly conspicuous example of these), that the rapid money growth the US was then experiencing would soon lead to a resurgence of serious inflation did even more harm to the reputation of his policy doctrines than they had already suffered. These predictions probably should not have been made because the rapid fall in the inflationary opportunity cost of holding money that was in motion in the early 1980s was creating ample room for the demand for money to absorb ongoing money growth, and indeed, in its absence, recovery from the steep recession with which that decade had begun might well have been aborted. They were made, however, and their failure, which arguably provided further strong evidence in favour of Friedman's monetary model of inflation, the same one that had underpinned the studies of hyper-inflation he had edited in (1956), was quite wrongly but widely regarded as discrediting it.[30]

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

Friedman's subsequent policy influence

The story of money-growth targeting and its aftermath seems at first sight to suggest that Friedman's influence on macroeconomic policy was short-lived and even superficial, but this is not really the case. Even in the mid-1970s, let alone the 1950s when he first began to develop them, his claims that inflation was always and everywhere a monetary phenomenon, that monetary policy was ill-suited for short-term stabilization of real variables, and that its only role could be to control and then stabilise inflation, were very much minority viewpoints. By the 1990s they had become commonplaces. If money growth targeting proved inadequate as a specific means of implementing monetary policy, inflation was nevertheless brought under control by monetary measures beginning in the early 1980s, and when, a decade later, that task had been completed, monetary policy in many places found an anchor in medium term inflation targeting of various degrees of formality, while labour market reforms had become the instruments of choice among mainstream economists for dealing with unemployment. As to fiscal policy, the longer term consequences of deficits and debt, particularly as they threatened to impinge upon monetary stability, pushed its potential as a short term stabilization device far into the back-ground.

In short, and to return to themes raised at the very outset of this essay: policy in most places came to be dominated in the 1980s and 1990s by the preconception that gross macroeconomic instability is more likely to be the result of policy-induced monetary shocks than of any fundamental flaw in the workings of a monetary economy; monetary policy displaced fiscal policy at the centre of things; and a medium term nominal objective, usually a quantitative

[30] Thus, the same tension that arose between the quantity-theoretic approach to hyper inflation that informed the (1956) *Studies* in which the effects of the opportunity cost of holding money were so important, and the (1959) study of the "Demand for money . . . " in which this relationship was downplayed, reappeared during this episode, which Nelson (2007, pp. 162-166) discusses in detail..

target for inflation, and sometimes a legally binding one at that, replaced short-term output and employment goals. There was not much to be seen here of the macro-economic orthodoxy that was so deeply entrenched in the textbooks of the 1950s, but there is a great deal that bears more than a passing resemblance to the alternative, then apparently outlandish, view-point that Friedman began to develop at that time. And, during the 1990s macroeconomic policy seemed to do well both in delivering low inflation, but also in maintaining a high degree of real stability too. Friedman's influence surely had a lot to do with all this.

But if this is the case, then perhaps his influence also helped to establish the policy complacency that many now blame for the severity of the international financial and real economic crisis that began towards the end of 2007. This issue cannot be settled here, and it is hard to believe that Friedman would have accepted responsibility without protest. In his defence, then, let it simply be noted that the treatment of the onset of the Great Depression in Friedman and Schwartz (1963a) provides clear-cut evidence that its authors were painfully aware that violent financial crises could arise in situations where the behaviour of the price level, and indeed the real economy, had been for some years benign, so that, if others were lulled into a false sense of security by the experience of the "great moderation" that preceded 2007, they ought not to have been had they paid attention to this work. Surely also, Friedman and Schwartz's (1963a) strong advocacy of what is now called "quantitative easing" as the appropriate but largely untried remedy for the collapse of 1929-33 can be awarded some credit for the willingness of the Fed., among other central banks, to deploy such policies more recently, even though it will take many years of work to allocate credit for staving off another depression between these measures and (surely ironically) some very Keynesian fiscal stimulus. And finally, at a time when the epicentre of monetary instability seems to have shifted to Europe, it is worth recalling that, as was only to be expected of the authoer of "The case for flexible exchange rates" (1953c), Friedman was a critic of the Euro even before its creation, and never wavered in his scepticism about this currency's long run viability.

Friedman's influence on macroeconomic theory

Friedman's influence on the development of macroeconomic theory is less immediately obvious than on the conduct of macro-policy, but as I shall now suggest, it has also been of lasting, if in some ways ambiguous, importance. To begin with, we have seen that the microeconomics that underlay his (1956) demand for money equation derived from his work on the consumption function, which, as we have also seen, he modelled as the outcome of explicitly forward looking maximising behaviour in a stochastic environment. The idea of forward looking maximisation had been pioneered by Irving Fisher, as Friedman acknowledged, but the combined influence of Keynes’s self-conscious rejection of this approach in the light of his ideas about fundamental uncertainty, of the subsequent disappearance of virtually all questions posed by the passage of time from the simplified macroeconomic model that others extracted from the *General Theory,* and of the lack of a technical apparatus within which this approach could be exploited, had led to its becoming almost lost to macroeconomics by the 1950s.

It was, above all, Friedman, not co-incidentally a micro-theorist and mathematical statistician before he was a macroeconomist, who revived this idea, at least as far as the sub-discipline was concerned. In 1957, moreover, he deployed it in the context of the explicitly stochastic framework routinely analysed by econometricians, and hence was a major force in redirecting macroeconomics towards what is nowadays regarded as the only sound way to

proceed. Contemporary macroeconomic theorists seldom if ever acknowledge the fact that, before Lucas, Sargent, Prescott et al. showed them how to make progress with the work that would eventually come to fruition under the label *Dynamic Stochastic General Equilibrium Theory*, Friedman had shown how to blend the idea of forward looking maximisation in a stochastic environment with econometric theory. They do not often cite *The Theory of the Consumption Function*, therefore, but it is arguable that, rather than simply having become irrelevant with the passage of time, this book made a contribution which, much generalised, has become so deeply embedded in the subject, and so taken for granted, that the need to cite it has simply disappeared.

Even so, Friedman might have hesitated to welcome and therefore take credit for much of modern macroeconomic theory. Today's available mathematical techniques, along with the central role played in that theory by the rational expectations hypothesis seem to dictate that essentially Walrasian micro-foundations are needed fully to exploit the idea of forward looking maximisation when the economy as a whole, with all its interactions, is analysed. As a dyed-in-the- wool Marshallian, Friedman would surely have been uncomfortable with this requirement. His already noted unwillingness fully to accept the rational expectations hypothesis, and, as late as (1992) to deploy his own aggregate supply curve interpretation of the expectations-augmented Phillips curve as the “missing equation” in his account the links between money, prices and output are both perhaps significant in this respect.

A Summing-up

In short, though any verdict on Friedman's long term influence on macroeconomics must acknowledge his importance in undermining Keynes’s influence to the point of transforming the whole intellectual climate within which macroeconomic policy debates were conducted, it must also recognise that he himself deployed the same Marshallian theoretical methods as did Keynes. And though that same verdict should also stress the fact that Friedman's adoption of forward looking maximisation as a basis for macro-theorising, not to mention his aggregate supply curve version of the Phillips curve, provided crucial starting points for those developments which in due course would lead to the complete domination of macroeconomics by Walrasian methods, this does not imply that Friedman himself either intended or would have welcomed these consequences.. How decisive this last aspect of his influence will ultimately prove to have been must of course await the further development of the sub-discipline, but viewed from a present-day perspective, the irony implicit in its current significance is striking.

References

Ando, A and Modigliani, F .1965, The relative stability of monetary velocity and the investment multiplier, *American Economic Review* 55 (Sept.) 693-728

Backhouse, R. and Laidler D. 2004 What was lost with IS-LM, in DeVroey M, and Hoover K. (eds) *The IS-LM Model: Its Rise, Fall and Strange Persistence, Annual Supplement to Vol. 36 of History of Political Economy,* Durham and London, Duke University Press

Baer, W. and Kershenetzky I. (eds.) 1963 *Inflation and Growth in Latin America,* Homewood Ill., Richard Irwin

Bailey, M. J. 1962 *National Income and the Price Level,* New York, McGraw Hill

Baumol, W. J. 1952, The transactions demand for cash - an inventory theoretic approach, *Quarterly Journal of Economics* 66 (Nov.) 545-556

Boulding, K. E.1948 *Economic Analysis* (rev. ed.) New York, Harper

Brunner, K. 1961 The report of the Commission on Money and Credit, *Journal of Political Economy* 69 (Dec.) 605-620

Brunner, K. and Meltzer A. H. 1974 Friedman's monetary theory, in R. J. Gordon (ed.) *Milton Friedman's Monetary Framwework,* Chicago, University of Chicago Press

Brunner K. and Meltzer A. H. 1993 *Money and the Economy: Issues in Monetary Analysis,* Cambridge, Cambridge University Press for the Raffaele Mattioli Foundation

Cagan, P. 1956 The monetary dynamics of hyper-inflation, in M. Friedman (ed.) *Studies in the Quantity Theory of Money,* Chicago, University of Chicago Press

Cagan, P. 2000, Phillips' adaptive expectations formula, in R, Leeson (ed.) *A. W. Phillips: Collected Works in Contemporary Perspective,* Cambridge, Cambridge University Press

Chamberlin, E. H. 1933 *The Theory of Monopolistic Competition,* Cambridge Mass. Harvard University Press

Clower, R. W. (1964) Monetary history and positive economics *Journal of Economic History,* 24 (Sept.) 364-380

DePrano, M. and Mayer, T. 1965 Tests of the relative importance of autonomous expenditure and money, *American Economic Review* 55 (Sept.) 729-752

Fisher, I. 1907 *The Rate of Interest; Its Nature, Determination and Relation to Economic Phenomena,* New York, Macmillan

Fisher, I. 1911 *The Purchasing Power of Money,* New York, Macmillan

Fisher, I, 1930 *The Theory of Interest*, New York, Macmillan

Fisher, I. 1973 I discovered the Phillips curve: a statistical relation between unemployment and price changes by Irving Fisher *International Labour Review* XIII, June, 1926 785-792, *Journal of Political Economy* 81 2, 496-502

Friedman, M. 1943 Methods of forecasting inflation, in Shoup C., Friedman M, and Mack, R. *Taxing to Prevent Inflation: Techniques for Estimating Revenue Requirements,* New York, Columbia University Press

Friedman, M. 1948 A monetary and fiscal framework for economic stability, *American Economic Review*, reprinted in *Essays in Positive Economics*, Chicago, University of Chicago Press

Friedman, M. 1949, The Marshallian demand curve, *Journal of Political Economy,* reprinted in *Essays in Positive Economics,* Chicago, University of Chicago Press

Friedman, M 1953a The methodology of positive economics in *Essays in Positive Economics,* Chicago, University of Chicago Press

Friedman M 1953b Full-employment policy and economic stability, in *Essays in Positive Economics,* Chicago, University of Chicago Press

Friedman, M 1953c The case for flexible exchange rates, in *Essays in Positive Economics,* Chicago, University of Chicago Press

Friedman, M. 1956, The quantity theory of money, a restatement, in M. Friedman (ed.) *Studies in the Quantity Theory of Money,* Chicago, University of Chicago Press

Friedman, M. 1957, *A Theory of the Consumption Function*, Princeton, NJ. Princeton University Press for the NBER

Friedman, M. 1959 The demand for money, some theoretical and empirical results, *Journal of Political Economy* 67 (June) 327-351

Friedman M. 1960 *A Program for Monetary Stability,* New York, Fordham University Press

Friedman, M. 1961 *Price Theory: a Provisional Text,* Chicago, Aldine

Friedman M. 1966 Interest rates and the demand for money, *Journal of Law and Economics* 9 (Oct.) 71-85

Friedman M. 1967 The monetary theory and policy of Henry Simons *Journal of Law and Economics* 10 (Oct.)

Friedman, M. 1968 The role of monetary policy, *American Economic Review* 58 (March) 1-17

Friedman, M. 1969 The optimum quantity of money, in *The Optimum Quantity of Money and Other Essays,* London and New York, Macmillan

Friedman, M. 1974 A theoretical framework for monetary analysis, and Comments on the critics, in R. J. Gordon (ed.) *Milton Friedman's Monetary Framework,* Chicago, University of Chicago Press

Friedman, M. 1975 *Unemployment Versus Inflation,* London, Institute for Economic Affairs

Friedman, M. 1977 Nobel lecture: inflation and unemployment, *Journal of Political Economy* 85 (June) 451-472

Friedman, M 1984 Lessons from the 1979-82 monetary policy experiment, *American Economic Review,* 74 (May, papers and proceedings) 397-400

Friedman M. 1987 Quantity theory of money, in Eatwell J., Millgate M., and Newman P. (eds.) *The New Palgrave: A Dictionary of Economics* London, Macmillan

Friedman, M. 1992 *Money Mischief: Episodes in Monetary History* New York, Harcourt Brace Jovanovich

Friedman M. and Friedman R.D 1962 *Capitalism and Freedom,* Chicago, University of Chicago Press

Friedman, M. and Friedman R. D. 1980 *Free to Choose,* New York, Harcourt Brace Jovanovich

Friedman M. and Friedman R. D. 1998 *Two Lucky People,* Chicago, University of Chicago Press

Friedman M. and Kuznets, S. 1945 *Income from Independent Professional Practice* New York, NBER

Friedman M. and Meiselman, D. 1963 The relative stability of monetary velocity and the investment multiplier in the United States, 1898-1958, in Commission on Money and Credit: *Stabilization Policies,* Englewood Cliffs NJ. Prentice Hall

Friedman M. and Schwartz A. J. 1963a *A Monetary History of the United States 1863-1960,* Princeton, NJ., Princeton University Press for the NBER

Friedman M. and Schwartz, A. J. 1963b Money and business cycles, *Review of Economics and Statistics* 45 (Feb.) 32-64

Friedman, M. and Schwartz, A. J. 1982 *Monetary Trends in the United States and the United Kingdom: Their Relation to Income, Prices and Interest Rates, 1867-1975,* Chicago, university of Chicago Press for the NBER

Gordon, R. J (1974) Introduction, to R. J. Gordon (ed.) *Milton Friedman's Monetary Framework,* Chicago, University of Chicago Press..

Hammond, J. D 1996 *Theory and Measurement - Causality Issues in Milton Friedman's Monetary Economics,* Cambridge and New York, Cambridge University Press

Hansen, A. H. 1953 *A Guide to Keynes* New York, McGraw Hill

Hawtrey, R. G. 1913, *Good and Bad Trade* London, Constable

Hendry D. F. and Ericsson, N. 1983 Assertion without empirical basis: an empirical appraisal of Friedman and Schwartz's Monetary Trends . . .in the United Kingdom, in Bank of England panel of academic consultants: *Monetary Trends in the United Kingdom*

Hendry D. F and Ericsson, N. 1991 An econometric analysis of money demand in *Monetary Trends* . . . . by Milton Friedman and Anna J. Schwartz, *American Economic Review,* 81, (Feb.) 8 - 38

Hetzel, R. L. 2007 The contributions of Milton Friedman to economics, Federal Reserve Bank of Richmond *Economic Quarterly* 93 (Winter)

Hynes, A. 1998 The emergence of the neoclassical consumption function: the formative years, 1940-52, *Journal of the History of Economic Thought* 20 (Mar.) 25-51

Johnson, H. G. 1962 Monetary theory and policy, *American Economic Review* 52 (June) 335-384

Johnson, H. G. 1969 Inside money, outside money, income wealth and welfare in monetary theory, *Journal of Money, Credit, and Banking* 1 (Feb.) 30-45

Johnson, H. G. 1971 The Keynesian revolution and the Monetarist counter-revolution *American Economic Review* 61 (May, papers and proceedings) 9-22

Kaldor, N. 1970 The new monetarism *Lloyds Bank Review* (July) 1-18

Keynes, J. M. 1930, *A Treatise on Money (2 Vols.)* London, Macmillan

Keynes, J. M. 1936, *The General Theory of Employment, Interest and Money,* London, Macmillan

Keynes, J. M. 1940 *How to Pay for the War,* London, Macmillan

Laidler, D. 1966, The rate of interest and the demand for money - some empirical evidence, *Journal of Political Economy* 74 (Dec.) 545-555

Laidler, D. 2004, *Macroeconomics in Retrospect,* Cheltenham, E. Elgar

Laidler, D. 2007 Milton Friedman – a brief obituary, *European Journal of the History of Economic Thought* 14 (2) 373-381

Laidler, D. 2008, Axel Leijonhufvud and the quest for micro-foundations – some reflections In R. E. A. Farmer (ed.) *Macroeconomics in the Small and the Large: Essays on Microfoundations, Macroeconomic app0lications and Economic History in Honor of Axel Leijonhufvud,* Cheltenham U.K. and Northampton, U. S.A., Edward Elgar

Lavington, F. 1921, *The English Capital Market,* London, Methuen

Leijonhufvud, A. 1968 *On Keynesian Economics and the Economics of Keynes,* London, New York, Toronto, Oxford University Press

Lipsey, R. G. 1960 The relation between unemployment and the rate of change of money wage rates in the United Kingdom 1862-1957, *Economica* NS 27 (Feb.) 1-31

Lucas, R. E. Jr 1972 Expectation and the neutrality of money *Journal of Economic Theory* 4, 115-138

Marshall, A 1871, Money, as published. in J. Whitaker ed. 1975, *The Early Economic Writings of Alfred Marshall,* London Macmillan, 165-177

Meiselman, D. 1962 *The Term of Interest Rates* Englewood Cliffs, NJ., Prentice Hall

Meiselman, D. (ed.) 1970, *Varieties of Monetary Experience,* Chicago, University of Chicago Press

Modigliani, F. 1977 The monetarist controversy or, should we forsake stabilisation policies? *American Economic Review* 67 (Mar.) 1-19

Modigliani, F. and Brumberg R, 1954 Utility na;lysis and the consumption function; an interpretation of the cross-section data, in K. Kurihara (ed) *Post-Keynesian Economics,* Rutgers University Press

Mundell, R. 1963 Inflation and real interest, *Journal of Political Economy* 71, 280-283

Nelson, E. (2007) Milton Friedman and US monetary history: 1961-2006, *FRB St. Louis Review* (89) May/June 153-182

Patinkin, D. 1969 The Chicago tradition, the quantity theory and Friedman, *Journal of Money, Credit and Banking* 1 (February) 46-70

Patinkin, D. 1974, Keynesian monetary theory and the Cambridge school, in H. G. Johnson and A. R. Nobay (eds.) *Issues in Monetary Economics,* Oxford, Oxford University Press

Patinkin, D. 1976, Keynes and econometrics. on the interaction between the macroeconomic revolutions of the inter-war period. *Econometrica* 44 (Nov.) 1091-1023

Phelps, E. S. 1967 Phillips curves, expectations of inflation and optimal unemployment over time *Economica* NS 34 (Aug.) 254-281

Phillips, A. W. 1954 Stabilisation policy in a closed economy, *Economic Journal* 64 (June) 290-323

Phillips, A. W. 1958 The relation between unemployment and the rate of change of money wage rates in the United Kingdom *Economica* NS 25 (Nov,) 283-299

Pigou, A. C. 1917, The value of money, *Quarterly Journal of Economics* 32, 38-65

Simons, H. 1936 Rule versus authorities in monetary policy, *Journal of Political Economy* 44 (Feb.) 1-30

Tobin, J. 1956 The interest elasticity of transactions demand for cash, *Review of Economics and Statistics* 38 (Aug.) 241-247

Tobin, J. 1958 Liquidity preference as behaviour towards risk *Review of Economic Studies* 25 (Feb.) 65-86