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Macroeconomics**

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Milton Friedman and the Evolution of Macroeconomics*

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*An introductory essay for Milton Friedman's collected writings on macroeconomic topics. I thank Robert Hetzel, Susan Howson, Allan Hynes, Robert Leeson, Perry Mehrling, Donald Moggridge, and John Munro for helpful comments on earlier drafts, and exonerate them of any errors that remain in this one.

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Milton Friedman was a rarity, an economist well known among the general public but also acknowledged within his discipline as having made contributions to it of lasting importance, and rewarded for them with a Nobel Prize in 1976. In the twentieth century, only John Maynard Keynes had a claim - by no means undisputed - to a higher rank than Friedman as a public figure and economic scientist. The reputations of the two are, furthermore, deeply intertwined within the evolution of macro-economics - that branch of the subject that deals with the behaviour of the economy as a whole. In the late 1930s, with the Great Depression still hanging over the world's market economies, Keynes was pivotal in the very creation of macro-economics as a separate sub-discipline, and his work also helped to direct it in a particular policy direction. Beginning in the 1950s, Friedman would play a key part in bringing about a radical re-assessment of macro-economics' central scientific tenets, not least as they appertained to the explanation of the Great Depression, and of their policy implications too. And yet, as I shall argue in due course, Friedman and Keynes belonged to the same intellectual tradition in economics - that associated with Alfred Marshall. This tradition has lately fallen into neglect, and ironically so, since this has come about, in some measure, because of Friedman's work.

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

The Consumption Function

Keynes's 1936 *General Theory* sought to explain the occurrence and persistence of large scale unemployment, and it did so in a way that proved readily amenable to a degree of simplification, of which Alvin Hansen (1953) is the definitive example. By the 1950s this version of "Keynesian economics" was the stuff of intermediate and even elementary textbooks. The overall level of unemployment, so it was said, varied with the economy's real output (Y), which, when resources were unemployed, was able to respond more or less passively to satisfy the demand for goods and services. This demand, in turn, came from three sectors of the economy: households (consumption, C), firms (investment, I) and the government (government expenditure, G).

Investment was said to be largely autonomously determined by the "animal spirits" of firms, unstable over time, and sufficiently impervious to influence from monetary policy that

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

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

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

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

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

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

Now, quite independently of Keynes, the 1920s and 1930s had seen a rapid growth of explicitly empirical economics.¹ Data were systematically collected, and a wide variety of statistical techniques began to be developed to analyse them. The National Bureau of Economic Research (NBER), closely associated in the 1930s with Columbia University was at the forefront of such efforts in the US. Friedman took courses from the Bureau’s founder Wesley C. Mitchell while a graduate student at Columbia, and his Ph.D thesis, which extended work originally begun by Simon Kuznets and was supervised by Mitchell’s collaborator and successor as the Bureau’s director, Arthur F. Burns, was published by the Bureau in 1945, with Kuznets as joint author, under the still well-known title, *Income from Independent Professional Practice*.

¹In the UK indeed, these developments were hampered by Keynes’s own hostility to them. See Don Patinkin (1976)

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 too did his familiarity with the difficulties that empirical economists were having with Keynes's stable psychological law. Consumption did indeed seem to vary as a fraction of income, but the quantitative relationships involved were problematic, and much work on these matters was done under NBER auspices. Over the "long run" of a few decades, the consumption-income relationship seemed to be one of strict proportionality with, to put matters in terms of equation (1), c being stable and positive and a being equal to zero. Over shorter periods c seemed to be smaller, and a a positive but shifting up over time. So already there was a problem, but Keynes's fundamental law was also supposed to apply to households in general, and when cross section data emanating from budget studies were analysed, they yielded a wide variety of estimates for both parameters. There is neither need nor space here to discuss the rich literature that these empirical anomalies generated from the early 1940s onwards, and which Allan Hynes (1998) has carefully discussed³. Suffice it to say that many of the ingredients of Friedman's 1957 analysis were to be found in that literature, to which he himself had indeed been a contributor, but that the particular way in which he then put them together in his *Theory of the Consumption Function* would not only have a direct impact on contemporary macroeconomic orthodoxy, but would also, in the longer run, come to be seen as a fundamental turning point in the way in which macroeconomic theory was done.

To a generation of economists brought up to analyse the economy one market at a time, the idea that the demand for any particular good would, *ceteris paribus*, vary with income was a common-place, and I conjecture that the vast majority of them thought of Keynes's fundamental psychological law as a simple generalisation of this to the level of consumption as a whole. But there is a massive fallacy of composition here. Friedman the microeconomic theorist, with some

²On 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.

³But Franco Modigliani's work on the consumption function nevertheless merits citation here as an independently arrived 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, rather than forward looking behaviour *per se*, nor did it 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 the contributions sufficiently distinct that they had no difficulty in awarding Modigliani a Nobel Prize for his work, having earlier rewarded Friedman's work on the same topic.

acknowledged help from Irving Fisher (1907, 1930), understood that the relevant objects of choice in the microeconomics of the consumption function had to be consumption now and in the future, and that the constraint upon that choice was defined by income now and in the future as well as the terms upon which it could be loaned or borrowed. On the assumption of a perfect capital market, the typical consumer could be thought of as able to sell his expected future income stream and purchase an annuity with the proceeds, and it was this hypothetical annuity, the consumer's *permanent* income, that Friedman postulated to be relevant to the choice of today's consumption. Current consumption, the outcome of a forward looking maximising decision about that variable's overall time path, would only vary with *current* income to the extent that variations in the latter affected permanent income. *Transitory* income, the difference between permanent and current income, would have no influence on consumption.⁴

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

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

but the model estimated by least squares regression was

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

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 $Y(t)$.

⁴The 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) drew on his work, it is tempting to think of Friedman as self-consciously reviving the important Fisherian tradition in American economics. However, there is no reference to Fisher in Friedman and Kuznets (1943), the capital theory set out in Friedman's (1962) *Price Theory - a Provisional Text* derives from Frank Knight, with no reference to Fisher, while Fisher's name appears in neither the Index nor the Bibliography of Friedman and Friedman (1998). Without discounting it totally, therefore, one should not over-emphasise a direct Fisherian influence on Friedman. I am grateful to Perry Mehrling for discussion of this issue.

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

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

To begin with, the particular method that Friedman chose to implement the idea of permanent income for empirical aggregate time series purposes, which were the ones that mattered for macro-economics and hence attracted most of the attention, considerably lessened the impact of his work. In this context he measured it as a geometrically declining weighted average of current and past aggregate income (multiplied up by an adjustment factor to allow for the fact that such a technique shifted the mean of the series back in time, and hence, given economic growth, would understate its current value in a growing economy.)⁵ Specifically, with $b < 1$, and ignoring this growth adjustment for simplicity, permanent income became

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

This was, at best, a rough empirical approximation to Friedman's basic theoretical concept, but, when used in the consumption function, it had the effect of preserving the stability of both a

⁵The "adaptive expectations" idea that underlies this formulation was, as Philip Cagan (2000) has noted, picked up by Friedman in 1952 from conversations with A. W. Phillips. Under Friedman's influence, it had already been successfully deployed to proxy inflation expectations by Phillip Cagan in his (1956) study of hyper-inflations, and was being used by David Meiselman (1962) in a study of the role of interest rate expectations in determining their term structure. It also had the virtue of providing a "good fit" to US time series data on consumption. One can see easily enough why it attracted Friedman in this context.

“long run” marginal propensity to consume - c - and a “short-run” one - bc - and hence of long-run and short-run multipliers too, these being linked by the dynamics inherent in distributed lags. Hence, it distracted attention from the permanent income hypothesis’ likely implications for the stability and reliability of the multiplier as a fulcrum for policy. Instead, it emphasised the dynamics with which this process worked out over time, hardly a novel, let alone disturbing, insight to econometricians already working on the quantification of Keynesian macroeconomics, who had by the late 1950s already learned a considerable amount about the usefulness of distributed lags when it came to fitting the data.

Money and the Rate of Interest

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

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

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

This explanation relied on the theory of *liquidity preference*, the very monetary complication that, by the 1950s, it had become customary to omit from elementary textbook accounts of Keynes’ 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 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 would have a well determined demand for a stock of *real* money - ie. money measured in units of constant purchasing power. In the writings of its originators, this demand was said to emanate from money's use as the economy's means of exchange and reflected what we would now call transactions and precautionary motives. They argued that this demand would usually represent a stable fraction of the money-holders' "resources", but they were routinely unclear as to whether this word referred to wealth, a stock, or income, a flow. Also, though they recognised that wealth not held as money could be held in other income-yielding forms - among Marshall's examples were "a horse" and "furniture" - the insight that the demand for money might be systematically related to some measure of the opportunity cost of holding it - a rate of interest on a representative financial asset, for example - eluded them. It was Keynes, in his *Treatise on Money* (1930) who finally brought clarity to these matters in a way that attracted widespread attention - though the priorities of Frederick Lavington (1921) in sketching out the relevant ideas should be acknowledged.⁷

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

⁶The pioneers of the Cambridge approach - Alfred Marshall (eg. 1871) and Arthur C. Pigou (eg. 1917) - did not refer to their model *per se* as the quantity theory, but preferred to say that it yielded the same prediction of proportionality between the quantity of money and the price level as did that older model, which was explicitly based on the concept of the velocity of circulation.

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

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

As is well known, if instead of being totally autonomous, investment also depends on the rate of interest, equations (1) - (3) yield an IS relationship defining those combinations of the rate of interest and real income at which investment and saving are equal to one another. If we then follow Keynes himself in characterising the interaction of the supply and demand for real money balances (M/P) in the following terms,

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

we have the LM relationship which defines combinations of these same two variables that equate liquidity preference (the demand for money) to the money supply. And as is also well known, the reduced form of the resulting IS-LM system – which is essentially Hansen’s (1953) version of the Keynesian model - approaches equation (4) above as the demand for money becomes more and more interest sensitive, but, as that sensitivity disappears, it instead approaches

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

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

Thus, just what kind of message about the workings of the economy followed from the IS-LM model that had been extracted from Keynes’ work hinged in an important way on empirical propositions about how the supply and demand for money interacted with one another. That is why, while Friedman’s work on the consumption function tended to undermine confidence in the stability of simple multiplier analysis and its policy applications, his essentially contemporaneous work on the demand for money function can be viewed as promoting this relationship as an alternative and more empirically stable fulcrum for the explanation of economic fluctuations and for the design of policies to deal with them.⁸

⁸There 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).

The Revival of the Quantity Theory and the Importance of Money

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

Hence, when in (1956) Friedman proposed that the demand for money was fundamentally a demand for real balances, the outcome of a portfolio allocation decision, and would vary with real income and a number of measures of the opportunity cost of holding it, his formulation of the relationship stood only a little apart from contemporary discussions of the topic on matters of substance, the most important difference here being his explicit claim that the relationship was empirically stable.⁹ However, the title of the (1956) essay, "The quantity theory of money, a restatement", and its publication as the introductory essay to a set of *Studies in the Quantity Theory of Money*, in and of themselves matters of style rather than substance, were calculated to be much more controversial, as Patinkin (1969) pointed out. In the 1950s, if the quantity theory was discussed at all, it was as a meaningless tautology that had been part of the erroneous classical doctrine that Keynes had successfully overthrown.

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

⁹Among 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. In 1956 the latter would have been seen as being only tenuously connected to Fisher's own (1911) transactions velocity formulation.

It was a key message of the (1956) *Studies* that this proposition was well supported by empirical evidence - Nazi Germany (John J. Klein) being a counter-example - but it was also shown that the parameter values underlying the relevant dynamics were such that it would have been possible to bring the inflations studied under control by reducing the rate of monetary expansion.¹⁰ Even so, (and with the exception of Richard Selden's paper on the long-term monetary experience of the United States) the *Studies* dealt with rapid inflation in rather far-off times and exotic places, and few readers seemed to have thought that they had much to say about contemporary advanced economies, where the low but persistent inflation that was being experienced was usually attributed to institutionally driven cost-push forces rather than monetary factors.¹¹

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

¹⁰The 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.

¹¹Though this was the view from the US and Europe, it was a different matter in Latin America, where the quantity theory as a theory of inflation played a critical role in the *Monetarist-Structuralist* debates that began in the mid-1950s. See Baer and Kershenetsky (eds.) (1963) for a representative collection of contributions to this debate. In general, Friedman's revived quantity theory found more immediate applications in less developed economies than at home. A subsequent collection of essays based on Chicago Ph.D. theses edited by David Meiselman (1970) contained studies of Chile (J. V. Deaver) Argentina (A. C. Diz), Post-War Japan (M. W. Keran), South Korea and Brazil (C. D. Campbell), as well as of Canada (G. Macesich) and a cross section of 47 countries, 26 of which were, however, located in either Asia and Latin America (M. Perlman).

attributed to variations in interest rates. Perhaps, he suggested, the strong evidence that others had found of an important interest sensitivity to the demand for money was the result of their having erroneously used current rather than permanent income in their regressions.

This study was followed up in short order by another - Friedman and Meiselman (1963) - which used 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 autonomous expenditure - (I + G) in equation (3) - and compare the outcome to that obtained when the money supply was used as an independent variable, as well as to estimate equations containing both variables. Its results seemed to show that, except in the 1930s, money dominated autonomous expenditure as an explanatory variable. Taken together, these two studies suggested that a demand 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 that a theory which could beat the Keynesian model, already weakened by Friedman's work on the consumption function, had indeed been found.

These papers made a considerable impact in the early 1960s, but with the passage of time, both of them turned out to be flawed.¹² Their longer-term influence 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 most influential work. Though not published by the NBER until 1963, this work was the product of a collaboration between Friedman and Anna J. Schwartz that had begun more than a decade earlier and had been influencing Friedman's monetary economics throughout the 1950s. The *Monetary History* was a work of quantitative, though not econometric, history, systematically tracing the causes and effects of variations of the quantity of money on the US economy since 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 feed-back effects from economic activity to money, but overall the picture seemed to be clear.

¹² 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).

This was particularly the case for the so-called the *Great Contraction* of 1929-33, the very episode when, according to the conventional wisdom prevalent in the 1950s, market mechanisms had most clearly failed, and the weakness of monetary policy had most vividly been demonstrated. Quite to the contrary, Friedman and Schwartz claimed that a typical cyclical downswing had started in late summer of 1929, but had been first allowed to get out of hand, and then exacerbated, by Federal Reserve policy. This had permitted the money supply to collapse amid banking crises that a sufficiently vigorous response on its part, by way of lender of last resort activities and large scale open market operations, could have prevented. Rightly or not, and that is not the point here, many more readers were eventually to be convinced by this narrative than by, say, the Friedman-Meiselman study, that Friedman's message about the importance of money had to be taken seriously.

It is important to grasp just how deep that message went in undermining the Keynesian consensus described at the outset of this essay. That it seemed to favour a version of the IS-LM model that downgraded the macroeconomic significance of real shocks and fiscal policy and attached increased importance to money was evident enough from the outset, of course, and this generated considerable controversy in its own right. But Friedman's message also contradicted the view, so much taken for granted in the early 1960s that it was rarely debated, that a modern monetary economy is fundamentally incapable of effectively allocating resources over time - of co-ordinating savings and investment while maintaining full employment - so that active and continuous government intervention is required to ensure its stability. This implication of his work sank in only slowly, though it would eventually come to leave deep marks on both academic economics and economic policy from the 1970s onwards.¹³ At considerable risk of oversimplification, it perhaps required Axel Leijonhufvud's (1968) success in making economists once again self-conscious about Keynes' vision of the flaws inherent the monetary economy's co-ordination mechanisms to enable them to appreciate the full extent of Friedman's challenge to received orthodoxy.

As Robert Clower stressed in his (1964) review of the *Monetary History*, Friedman's analytic methods were Marshallian, and in this respect his macro-economics bore a close affinity to that of Keynes. Each sought to construct a simple macro-economic framework around a single empirically stable relationship, and one within which the economy's responses to shocks could be analysed as evolving over time as the constraints imposed by various short-run rigidities were relaxed. Nevertheless, Friedman's specific framework seemed to support a vision of the monetary economy's workings essentially identical to that of those Austrian economists who, using a theory of real economic fluctuations 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.¹⁴ For Friedman, every bit as much as for

¹³I 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.

¹⁴As 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

von Mises or von Hayek, markets were stable and capable of dealing efficiently with allocative challenges. If they failed to meet them, this was not because they were inherently flawed, but because misconceived monetary policies had been visited upon them. Friedman's views on just what constituted misconceived policies, and how they inflicted harm, differed considerably from those of the Austrians, but he agreed with them wholeheartedly that activist policies, far from being needed to stabilize the market economy, were the principal source of its instability. Unlike them, however, he derived these conclusions from careful empirical analysis of competing models, rather than from any set of first analytic principles.¹⁵

Monetary Policy for a Dynamic Economy

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

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

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 persuasive case for treating Marshall as a general equilibrium theorist. See however, fn 20 below on the later evolution of the Marshallian-Walrasian distinction.

¹⁵Nicholas Kaldor (1970) was early among Friedman's critics in noticing an affinity between his work and that of the Austrians. However, there were and are important differences here too. For example, Hayek's views on the dangers of fighting depression with "a little inflation" (see, eg, 1936, p. 125) are sometimes read as having anticipated Friedman's (1967) accelerationist doctrine, discussed below. However, Hayek was discussing contemporary policy issues, and he was referring to inflation of the money-supply, not of the price level. Hence his remark stands in flat contradiction to Friedman's later conclusion that monetary expansion was exactly what was needed to come to grips with the contraction of 1929-33.

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 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 contemporary policy doctrine than a desire to substitute monetary for fiscal measures. He also insisted that monetary shocks impinged upon the economy with time lags that are (now famously) *long and variable*, and that this fact both required policy to be forward looking and exposed it to serious risk of error into the bargain, for the simple reason that his policy makers were much less well informed than Keynes's.

In an IS-LM model in which the demand for money is more or less independent of the rate of interest, fluctuations in Keynesian animal spirits do not affect income and employment at all provided that the money supply is held constant, but such analysis does not do justice to Friedman's policy vision. For him, there was ample elasticity in the system to permit real shocks to have consequences, either because the economy could function temporarily with less money in circulation than was demanded, or because that demand depended in any case on permanent rather than current income. In such circumstances offsetting policy measures could in principle improve matters, but in practice they risked making them worse, as he showed, for example, in (1953b)¹⁶ By the time they began to take effect, they could be end up not stabilising, but further destabilising an economy that was already responding to new shocks. For Friedman, then, the principal problem in the implementation of policy was not to create institutions that would facilitate the rapid and continuous discretionary deployment and withdrawal of economic stimulus - *fine tuning* as such measures were called in the 1960s without a trace of irony - but that would impose constraints to prevent policy makers over-reaching themselves. Already in (1948), he had, in this spirit, proposed a system that would exploit the built-in stabilising effects of having the counter-cyclical deficits generated by the interaction of stable government expenditure and progressive income taxation automatically funded by money creation, but as his work progressed and he became convinced of the inherent stability of an economy that was not subject to monetary shocks, his attention shifted to devising an institutional framework that would prevent their occurrence.

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

¹⁶This paper provides another example of Friedman's pioneering use of statistical theory in the analysis of a problem in economics.

In the first, less important sense, Friedman's money growth rule involved the deployment of monetary policy as a built-in stabiliser, and was defended on the quite practical economic grounds that, given the state of macroeconomic knowledge at the time, this would maximise monetary policy's contribution to the stability of the economy. In the second, it invoked the political principle that decisions impinging upon the stability of the monetary system, and hence of the market economy, were too important to be delegated to unaccountable functionaries, and were proper objects for legislated, or even constitutional constraints. Here it will suffice to recall Sir Robert Peel's 1844 Bank Charter Act, Irving Fisher's tireless efforts in the 1920s to persuade Congress to legislate the Federal Reserve system into the single minded pursuit of price level stability, and Henry Simons' celebrated advocacy of similar measures in (1936), to demonstrate how deeply embedded was Friedman's proposal in the economic agenda associated with what used to be called *classic liberalism*.¹⁷ The empirical foundations of this doctrine seemed to have been thoroughly undermined by the interpretation of the experience of the 1930s as clear evidence of a fundamental flaw in the workings of a monetary economy, but by 1960 Friedman's positive work was well on its way to restoring these foundations as we have seen, and hence was in close accord with his policy proposals.

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

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

Phillips himself had said next to nothing about the theoretical basis for his hypothesis that the rate of change of money wages would vary systematically with unemployment, but Richard Lipsey's (1960) careful elaboration of the few hints he had given soon became widely accepted.

¹⁷The direct influence here on Friedman was Simons, as he himself acknowledged in (1967)

First, frictional and structural factors ensured that the supply and demand for labour would be equal to one another at a positive unemployment rate, and when they were, money wages would be stable. Second, any deviation of unemployment from that same level was a sign of an excess demand (or supply) for labour, and money-wage-change was a response to it. The Phillips curve, in short, was simply the result of applying ordinary supply and demand analysis to a particular market.

Not so, was Friedman's response. The price that cleared the labour market was the *real* wage, not the *money* wage, and excess demand there should generate real, not money, wage changes. Agents bargained over money wages of course, so excess demand would initially cause money wages to rise, but this would soon feed through to the behaviour of prices and engender inflation expectations that would feed back into money wage bargaining. If excess demand was then held constant by policy, the inflation rate would have to rise, setting in motion a never ending upward spiral. Therefore, any efforts on the part of policy-makers to buy, at the price of a little inflation, an unemployment rate permanently lower than the *natural* - structural and frictional - properties of the labour market could deliver, were doomed to collapse in the face of a perpetually accelerating price level. Long before 1968, Friedman had argued that monetary policy should provide a background of macroeconomic stability against which agents could then pursue their various private interests, but now he was able to be much more precise about just what was involved here. The *only* macroeconomic variable that monetary policy could influence on anything other than a purely temporary basis was the inflation rate, and Friedman had earlier argued, in company with Phillips, be it recalled, that short-run fine tuning was too difficult to be practicable. "The role of monetary policy", then, was to stabilise inflation in the medium term, and that was all.¹⁸

The Transmission Mechanism and its Missing Equation

In the early 1960s, many economists expressed doubts about the transmission mechanism that underlay Friedman's claims for the importance of money, even though rather elaborate accounts

¹⁸In 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.

of it were to be found in Friedman and Meiselman (1963) and Friedman and Schwartz (1963b).¹⁹

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

Friedman and Schwartz documented the workings of this mechanism, which bears a strong family resemblance to that postulated even before the Great Depression by Irving Fisher (eg. 1911, ch. 4) and Ralph Hawtrey (eg.1913), on a cycle by cycle basis in the *Monetary History* (1963a) and showed that its precise operations in any particular episode depended on the extent to which the economy was simultaneously being affected by other shocks, not mention the nature of the monetary policy regime that was in place, which would determine the nature and extent of the money supply's endogenous responses to swings in real income and inflation. Nevertheless their work also seemed to confirm that a hard core of common elements were always present, three of which were of particular importance. First money was substitutable across a wide range of margins, a characteristic that followed from Friedman's conception of it as a "temporary abode of purchasing power" whose demand was determined like that for any other service-yielding durable good. This implied that monetary policy's transmission mechanism was not just a matter of interest rate effects in organised financial markets. Second, the stock-flow interactions that lay at the transmission mechanism's heart were inherently dynamic and drawn

¹⁹It 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.

out over time. Third, though it thus seemed possible to tell a coherent story about how and why variations in money growth induced changes in the time path of nominal income, the factors determining precisely how those changes were divided up between real income and prices in any particular episode remained elusive beyond a general tendency for variations in output and employment to get under way before price level effects came through.

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

Perhaps the solution to this puzzle is to be found in the fact that two mutually inconsistent sketches of the theoretical underpinnings of the curve in question were in fact present in the (1968) paper, and that Friedman was at least partially aware of the problems this posed for his work. The first of these, 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 see precisely what is involved here, it is convenient to consider the Phillips curve in its price-inflation-output rather than its wage-inflation-unemployment form, (though that is not how Friedman himself discussed the matter).

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

$$p = gy \tag{10}$$

Once inflation expectations come into the picture, this becomes

$$p = gy + p(e) \tag{11}$$

which is equivalent to the first of Friedman's two formulations of it in wage-change-

unemployment space. To get to the second formulation, we start from a conventional aggregate supply curve

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

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

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

Equations 11 and 11' are observationally equivalent as written here, but note that, if it is necessary do so in order to match empirical evidence, and it is, there is no logical difficulty about lagging y on the right hand side of equation 11, but it is nonsensical to do this in the case of equation (11'). That is because these two equations embody fundamentally different visions of the economy's workings. The individual behaviour underlying 11 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. 11' 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.²⁰

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 added that Phillips had also gotten the direction of causation underlying his curve wrong: he had quantities affecting prices - unemployment causing wage changes - instead of prices affecting quantities - wage behaviour causing variations in unemployment. By 1975, that is to say, Friedman seems to have recognised the tensions between the two theories of the expectations augmented Phillips curve that uneasily co-existed in his Presidential address, and to have opted for the second of them. Certainly it is this second rationalisation that is to be found in the *New Palgrave* version (1987) of his major essay on the Quantity Theory of Money. Even so, Friedman seems never to have adopted the rational expectations hypothesis that usually complements the Phillips-curve-as-aggregate-supply-curve in New-classical economics. Though respectful of this idea's theoretical usefulness, he did not find it "the open sesame to unravelling

²⁰In 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 Leijinhufvud (1968) who forced macroeconomists to pay attention to this question. On this matter, see Laidler (2006).

the riddle of dynamic changes that some of its more enthusiastic proponents make it out to be.” (Friedman and Schwartz 1981, p.630)²¹

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

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

The (1969) collection of essays, *The Optimum Quantity of Money*, contained only one new paper - its title essay - albeit a lengthy one, that developed the case for a fiat monetary system’s aiming at a stable rate of *deflation*, equal in absolute value to the economy’s real rate of interest, so as to maximise the productivity of the economy’s stock of real balances. This essay is still much cited, but it was only one of a number of papers on this and related issues published at about the same time, and its survival where others of comparable quality, for example Johnson (1969), have faded from view perhaps owes as much to its author’s overall reputation, as to anything unique in its content. Friedman’s already cited contributions to Robert Gordon’s (1974) edited volume *Milton Friedman’s Monetary Framework*, were also aimed at his fellow economists, but their intention was to summarise previously expounded ideas and defend them, not to break new ground, and that was the main aim of his (1977) Nobel Prize lecture too. *Monetary Trends in the United States and the United Kingdom* (Friedman and Schwartz 1981) was a substantial work by any measure, but this, the final product of the monetary history project that had started in the early 1950s, had originally been intended to deal with trends *and cycles*, and had been in a more or less complete first draft form as early as 1969.²²

²¹I am grateful to Allan Hynes for discussion of this point

²²This long delay in publication did much to reduce this book’s impact, not least because

Meanwhile, Friedman was becoming increasingly visible and respected as a commentator on economic policy, sometimes writing alone - notably as a *Newsweek* columnist - and sometimes with his wife Rose Director Friedman.²³ It is beyond the scope of this essay to weigh the merits of Friedman's broader policy contributions, but his influence over macroeconomic policy from the 1970s onwards does require attention here. At the very beginning of that decade Harry Johnson (1971) had noted that Keynesian economics was at its strongest in dealing with the problems posed by unemployment, while Friedman's approach, by then known as *monetarism*, had the advantage when it came to inflation.²⁴ Because unemployment was the more serious policy issue, Johnson went on, Friedman's critique of Keynesian orthodoxy was likely to prove ultimately ineffective. Even as he wrote, however, inflation was fast becoming the main macroeconomic policy problem facing the US and those countries linked to it through the Bretton Woods system, and it would soon generate that ugly phenomenon with the equally ugly name, *stagflation*.

As was noted earlier, the conventional wisdom prevailing at this time was that inflation in advanced economies was not a monetary phenomenon at all, but a matter of institutionally driven cost push forces, and the simultaneous occurrence of rising inflation and rising unemployment gave superficial plausibility to this explanation. That plausibility did not, however, survive the abject and extremely visible failure in the early 1970s of wage-price controls unsupported by monetary measures in such economies as the US and the UK, and it soon became apparent that Friedman's monetary explanation of inflation, supplemented by his expectations-augmented

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.

²³Their 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)

²⁴Johnson 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.

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

At the serious risk of over-generalising, monetarist policies everywhere foundered on the same factors. To begin with, as implemented from the mid-1970s onwards, these policies were only distantly related to Friedman's (1960) *Program*. This had proposed a legally binding money-growth rule in order to maintain stability in an already smoothly functioning macroeconomic 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 dis-aggregated their data only to cycle phases. Short-run demand for money functions are tools of monetary fine-tuning, which Friedman had never advocated.²⁵

He (and everyone else writing on the demand for money before the mid-1970s) were, however, more culpable in having failed to grasp the impact that institutional change within the financial sector could have on the economic significance of any specific monetary aggregate, including that (or those) chosen as the fulcrum of monetary policy. Some of this change in the 1970s was the result of regulatory interference with banking practices on the part of authorities who were simultaneously anxious to hit money-growth targets and to avoid some of the consequences of doing so (Goodhart's Law), some was the consequence of the private sector's reaction to the adoption of new policies (the Lucas critique), and some was caused by the application to banking of then recent developments in computing technology; but added together,

²⁵If 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.

these factors undermined not just short-run relationships but even the longer term stability of the demand for money functions on which policy was beginning to rely. Such instability in earlier times is evident in the historical results presented in *Monetary Trends* (1981) but there is no trace of Friedman, or anyone else, having recognised this problem early enough for such information to have been useful to the conduct of money growth targeting.

Furthermore, Friedman's very public (1984) prediction that the rapid money growth the US was then experiencing would soon lead to a resurgence of serious inflation did further harm to the reputation of his policy doctrines. This prediction should not have been made, of course, because the rapid fall in the inflationary opportunity cost of holding money that was in motion in the early 1980s was creating ample room for the demand for money to absorb ongoing money growth, and indeed, in its absence, recovery from the steep recession with which that decade had begun might well have been aborted. The prediction was made, however, and its failure, which in fact provided further strong evidence in favour of Friedman's monetary model of inflation, was, quite wrongly, widely regarded as discrediting it.

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

The story of money-growth targeting and its aftermath seems at first sight to suggest that Friedman's influence on macroeconomic policy was short-lived and even superficial, but nothing could be further from the truth. Even in the mid-1970s, let alone the 1950s when he first began to develop them, his claims that inflation 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 has now been dominated for a decade or more by the preconception that gross macroeconomic instability is more likely to be the result of policy-induced monetary shocks than of any fundamental flaw in the workings of a monetary economy; monetary policy has displaced fiscal policy at the centre of things; and a medium term nominal objective, usually a quantitative

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

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

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

Friedman's unwillingness, as late as (1992) to use his own aggregate supply curve interpretation of the expectations-augmented Phillips curve as the "missing equation" in his essentially Marshallian account the links between money, prices and output has already been noted, as it has that Keynes too was a thoroughgoing Marshallian. Though any verdict on Friedman's place in the development of macroeconomics must acknowledge his importance in undermining Keynes's influence on policy, it must also recognise that the above-mentioned aggregate supply curve was the starting point for Robert E. Lucas's work on his (1972) "money supply surprise" model of the cycle. That is why it must also entertain the possibility that, perhaps inadvertently, he set in motion the demise of the Marshallian tradition in macro-economic theory of which both he and Keynes were such distinguished exponents. How

important his influence here will ultimately prove to be, must await the further development of the sub-discipline, but viewed from a present-day perspective, the irony implicit in it is striking.

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