Friedman and Schwartz on Monetary Trends in the USA and the UK from 1867 to 1975 *: A First Assessment **

1. The volume under examination is the third one to be published over the last twenty years by Milton Friedman and Anna Schwartz under the auspices of the NBER, and will, they assure us, be the last of the series. The first volume, *Monetary History,*[1] offers a chronological and largely qualitative analysis of the evolution of the quantity of money, of the factors responsible for that evolution, and of the influence of the stock of money on other magnitudes. The second volume, *Monetary Statistics,*[2] describes the construction of the new estimates of the quantity of money and formulates explicitly the criteria which have led the two authors to choose as their definition of money M_2 (currency plus adjusted demand and time deposits of commercial banks held by the public). The third volume, *Monetary Trends*, presents "a statistical and theoretical analysis of the relations between the quantity of money and other key economic magnitudes over periods longer than those dominated by cyclical fluctuations — hence the term *trends* in the title".[3]

The importance of the three volumes does not derive from the over two thousand pages composing them, nor solely from the massive

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* M. Friedman and A. Schwartz, *Monetary Trends in the United States and the United Kingdom. Their Relation to Income, Prices, and Interest Rates, 1867-1975*, The University of Chicago Press for NBER, Chicago, 1982, pp. xxxi, 664.

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[1] FRIEDMAN and SCHWARTZ (1963b).

[2] FRIEDMAN and SCHWARTZ (1970).

[3] FRIEDMAN and SCHWARTZ (1982), p. xxviii. CAGAN (1965) also forms part of the series. After having identified in high-powered money, in the ratio of deposits at banks to their reserves and in the ratio of the public's holding of deposits to its holdings of currency the determinants of the quantity of money, he studies the cyclical and secular evolution of these determinants. Since Friedman and Schwartz have announced that they do not propose to publish the fifth volume envisaged, which was to have been devoted to the study of "monetary cycles", FRIEDMAN and SCHWARTZ (1963a) should also be included in the series.

attempt to provide information, or, as the more severe critics object, disinformation.[4] Their importance should mainly be assessed in relation to the methodological approach which, according to Friedman, must be adopted in judging a theory. It is well known that, for Friedman, the choice between the infinite range of possible theories must fall on the one which, in addition to being simple (and simplicity is perhaps the only merit which he admits in the Keynesian theory), has the best predictive power.[5] Hence the crucial importance of tests in judging the conformity of theoretical predictions with experience. Even if one does not agree with the methodological approach proposed by Friedman, one must not underestimate the persuasive power of an empirical fit. In this respect the three volumes of the NBER series are particularly important for the further reason that the coauthor is the initiator and the most prestigious representative of the new quantity school. Critics should show, as they have done in certain cases,[6] that tests are not able to perform the crucial role assigned to them by Friedman's methodology.

The third volume, the one dealt with here, is important as regards another aspect. It supplies the reader with a sufficiently compact and explicit exposé of Friedman's whole up-to-date theoretical position.[7] It is also instructive to read through the volume, because it gives the impression that the numerous theoretical updatings, elaborations and amplifications offered by Friedman in the last twenty years have also been prompted by the difficulties — and they were far from few — met with in completing this volume.

Monetary Trends has the following structure. It begins (Chapter 2) by presenting the general theoretical model, which we can think of as divided into the core (that is, the central nucleus of the theory) and a series of additional propositions,[8] which Friedman regards as more or less tentative, and failure to confirm which empirically does not, he thinks, in any way affect the core. That is composed of the theory of

[4] KALDOR (1970).

[5] FRIEDMAN (1953).

[6] See for example the theoretical-empirical debate on the relative stability between velocity of circulation and the multiplier of autonomous spending between FRIEDMAN and MEISELMAN (1963, 1965), ANDO and MODIGLIANI (1965) and DE PRANO and MAYER (1965). See also the observations in KALDOR (1970) and DESAI (1981).

[7] We do not believe we are wronging Anna Schwartz if we attribute to Friedman alone the paternity of the theoretical part of the book, at least of that part concerning the core of the theory.

[8] For a similar subdivision, but presented in terms of chronological development, see LAIDLER (1981).

the demand for money and the related theory of nominal income. The additional propositions concern essentially certain transmission mechanisms including the adjustment process of nominal income started by changes in the stock of money, the way in which a variation in nominal income is subdivided into variations in output and variations in prices, and the influence of money on interest rates.[9] Next, in Chapters 3 and 4, comes the presentation of the statistical framework and of the basic data for the two countries. Chapter 5 describes with a broad brush the secular movements of money, the velocity of circulation, and prices and income. The study then goes on to the testing of the core (Chapters 6-8) and to that of the additional propositions (Chapters 8-10). Before the chapter recapitulating the analysis (12), there is a discussion of the existence and nature of long swings in growth rates (Chapter 11).

The limited scope of the present article makes it impossible to offer a thorough and exhaustive presentation and discussion of all sections of the volume. We will therefore try to pivot the discussion on two questions which appear to us to be crucial and about which we feel that Friedman's previous works have not succeeded in dissipating legitimate doubts. We refer to the views put forward by Friedman and by certain neo-Keynesians[10] regarding the absence of theoretical divisions between "good" monetarists and Keynesians, and to the ability of tests to play a crucial part in the choice between conflicting theories. We will therefore deal with the presentation and discussion of the core of the theory (sections 2 and 3) and of the additional propositions (section 4) before examining if their predictions conform to empirical evidence (sections 5 and 6 respectively).

2. We have asserted that the core of Friedman's theoretical analysis is formed of the theory of the demand for money and of the theory of nominal income. According to Friedman, "the quantity theory ... on an analytical level, ... is an analysis of the factors determining what quantity of money the community wishes to hold; on an empirical level, it is the generalization that changes in desired real balances (in the demand for money) tend to proceed slowly and gradually or to be the result of events set in train by prior changes in supply, whereas, in contrast, substantial changes in the supply of nominal balances can and

[9] The theoretical discussion of some of these subjects is taken up again and amplified in the subsequent chapters in relation to the difficulties raised by the empirical evidence.

[10] MODIGLIANI (1977).

frequently do occur independently of any changes in demand. The conclusion is that substantial changes in prices or nominal income are almost invariably the result of changes in the nominal supply of money".[11]

If we felt that we had to exclude all references to empirical evidence from the theoretical core, we would have the alternative of including in it only the theory of the demand for money or considering the two empirical generalizations (the stability of the demand function for money and the exogeneity of the money supply) as if they were theoretical hypotheses. In the latter case, the demand function and the two additional hypotheses taken together lead to the theory of nominal income, according to which variations in nominal income are "almost invariably the result of changes in the nominal supply of money".[12]

The presentation of the core of Friedman's theory may be set out in broad terms, since it does not differ from the versions which have long been known.[13] The demand function for money is:

$$(1) \qquad m = f\ (y,\ w;\ R_M^*,\ R_B^*,\ R_E^*,\ g_P^*;\ u)$$

where m is the demand for money in real terms; y real income; w the fraction of income derived from property; the asterisk indicates expected values; R_M, R_B, R_E are the nominal rates of return on money, fixed-value securities and equities; g indicates the rate of percentage change; P the price level, and u is a residual variable.

Friedman tends to emphasize two aspects of this formulation: the decisions concern the quantity of money in real terms (absence of monetary illusion); the advantages connected with the possession of money are set against those derived from an extremely wide range of assets. However, it does not seem to us that these two characteristics succeed in giving some kind of "special" property to equation (1). That equation can be introduced into any neo-Keynesian model without modifying its results to a marked extent.[14] The equation proposed by Friedman, that is, does not seem to us to be able of itself to confer a

[11] FRIEDMAN and SCHWARTZ (1982), p. 19. From here on references to this work will be placed directly in the text.

[12] We will revert to the theory of nominal income with greater precision in section 3.

[13] In effect, this part of Chapter 2 of *Monetary Trends* is almost a straight reproduction of FRIEDMAN (1974), which in turn is an expansion of earlier writings. The theoretical treatment in Chapter 2 differs from FRIEDMAN (1974) in the parts devoted to what we have called the additional propositions.

[14] This is a way of reaffirming the thesis of PATINKIN (1969) according to which Friedman's theory of the demand for money constitutes a reformulation of the Keynesian theory of liquidity preference rather than a reformulation of the quantity theory.

crucial characterization on the quantity theory, even if it is admitted that opinions may differ as regards the way in which that particular formulation is arrived at.

We can therefore move on to the two additional hypotheses which Friedman regards in much the same way as empirical generalizations. As to the question of stability, Friedman himself admits that any function can be made stable by including in it a sufficient number of independent variables; hence, stability can only be meaningful if defined in terms of "a small number of variables". He then adds that this small number cannot be specified on a strictly analytical level.[15] This means, that, if we abstract from the excessively "lucky" case of stability in a single variable, there is no objective criterion for deciding on the degree of a function's stability: hence the empirical status attributed by Friedman to this proposition. The judgement must then be closely linked to the reasons why the stability of the demand function for money is regarded as essential for the quantity theory. As has recently been affirmed: "What is being sought in a stable demand function is a set of necessary conditions for money to exert a predictable influence on the economy so that the central bank's control of the money supply can be a useful instrument of economic policy".[16] In other words, the stability of that function should be defined and judged in relation to its predictive power which is crucial for Friedman as regards the acceptability of a theory. If put in these terms, the problem does not however concern only the number of the independent variables considered, but also their nature. Given that the predictive value of a theory is verified in relation to past experience mainly as a means of affirming its validity for the future, the link between the phenomenon and the variable which represents it must be of such a nature as to be able to be postulated quantitatively *a priori*. We will see below that, for the function estimated by the two authors, that is at times somewhat problematic.

The stability of the demand function for money is in any case a necessary though not a sufficient condition for affirming the casual pre-eminence of the quantity of money over the other variables of the system. To that end, we have to accept the second empirical generalization, that is, the exogeneity of the supply of money in nominal terms.[17]

[15] FRIEDMAN and SCHWARTZ (1970), p. 197.

[16] JUDD and SCADDING (1982), p. 993.

[17] The two authors specify that by the exogeneity of the supply of money they mean the independence of the supply of the variables which determine the demand for money; it follows from this that supply enters as an exogenous variable into the determination of nominal and real

Monetary Trends dwells only briefly on this crucial point and restates the position assumed in earlier works by the two authors.[18] They hold that on the theoretical level there are valid arguments in support of both the exogeneity and of the endogeneity of the quantity of money. The pre-eminence of the one over the other may therefore be affirmed only with reference to empirical evidence. The evidence supplied by various studies, including *Monetary History* and Cagan (1965), would appear to confirm that, even if there are influences of demand on supply, they are not very significant and the quantity of money is generally exogenous (pp. 34-5).

As is well known, the possibilist position expressed by Friedman on the theoretical level does not appear satisfactory to that part of the Keynesian theorists who consider the authorities' action to be largely constrained by the necessity not to create serious disturbances for the financial system, and who regard a variation in the quantity of money as the immediate result of an excess or lack of demand for credit in terms of the existing volume.[19] This does not mean that, in specific circumstances and for short intervals, the authorities are not regarded as in a position to control the quantity of money, at least within certain limits. What is in discussion is the possibility that the supply may remain exogenous for lengthy periods.

The existence of divergences on the theoretical level could lead us to agree with Friedman in considering the exogeneity of money as a generalization calling for empirical evidence. Given the crucial nature of this generalization, the empirical evidence would thus come to play the decisive role called for by the Friedman methodology. Unfortunately, even a cursory examination of the results obtained in this field shows that, at least so far, testing has not been in a position to offer an objective basis for reaching a decision. Friedman and Schwartz refer in particular to their *Monetary History* and to Cagan's volume in the NBER series. The findings obtained by Cagan are much less clearcut than the two authors seem to believe; he concludes that for mild cyclical fluctuations the main nexus is from nominal income to the quantity of money; deep depressions

income, prices and rates of interest. They then affirm that at a different level of analysis the supply of money, too, can become endogenous, since there are systematic causes responsible for its variations. See FRIEDMAN and SCHWARTZ (1982), pp. 35-6 and note 24.

[18] See for example FRIEDMAN and SCHWARTZ (1963b), pp. 686-95.

[19] See for example KALDOR (1970) and CRAMP (1971a, 1971b). HAWTREY (1928) is one of the first to affirm, vigorously, that the variations in the quantity of money are the effect and not the cause of variations in the quantity of credit created. It is perhaps significant that, in the rich bibliography contained in *Monetary Trends*, there is no reference to these authors nor to the school of American post-Keynesians.

seem the result of contractions in the quantity of money; in the long period, variations in the quantity of money are the result of previous variations, of the opposite sign, in prices, while there is a close simultaneous relation between increases in prices (and in nominal income) and increases in high-powered money.[20] *Monetary History* adds little to these conclusions. The only lengthy period in which the exogeneity of money is considered to be clearer is that of 1897-1914, when an increase in the world production of gold is linked to an expansion at the same time of the monetary indicators. The contractionary phases regarded as most significant (January-June 1920, October 1931 an July 1936-January 1937) are of brief duration.

These results do not seem to us to be such as to support the Friedman thesis. The empirical evidence for the normal cyclical fluctuations is markedly favourable to the hypothesis of the endogeneity of money. The evidence for the most violent cycles (which in any case is cited only for the phases of depression) is favourable to the thesis that a sudden monetary squeeze may have a profound influence not only on the level of activity and on prices, but also on the stability of the financial structure — a thesis completely in line with Keynes' theory, since among other things the evidence shows that there were at the same time increases in the rates of interest.[21] In addition, as Kaldor has noted, the '29-'31 depression cannot be imputed to a diminution of high-powered money. Hence, a deep depression is not always the result of a monetary squeeze.[22] For the long period, Cagan's correlations have little to do with the direction of causality; what is more important is a qualitative analysis like the one carried out in *Monetary History*. We have seen that, in this latest work, the period in which the exogeneity of money is held to be clearest is that of 1897-1914, a period of only eighteen years (which is a short stretch in the Friedman perspective) and is related to a gold standard regime. Even without wishing to contest the Friedman-Schwartz thesis for this period, we have no "proof" relating to a monetary system like the one prevailing after the second World War. Lastly, in a review of the literature for the period after 1973, two

[20] CAGAN (1965), Summary.

[21] See FRIEDMAN and SCHWARTZ (1963b), graphs 20, 29, 57 and 59.

[22] KALDOR (1970), pp. 272-6. The argument in *Monetary History* (pp. 691-3) and in *Monetary Trends* (pp. 228 and 625) that the 1929-31 depression stemmed from the failure of the Federal Reserve to pursue an expansionary policy in the face of the rise in the liquidity preference function is a rather unconvincing attempt to save the exogeneity of supply by sacrificing to it the stability of the demand function for money.

authors not suspected of Keynesianism such as Judd and Scadding go so far as to affirm that "empirical evidence on whether money is exogenous with respect to income and interest rates is mixed... The evidence does not on balance suggest a strong case for the exogeneity of money".[23]

Friedman seems conscious of these difficulties when in the first passage quoted, in which he defines the quantity theory, he makes full use of his linguistic skill: the variations in the quantity of money "can" happen, and "frequently" do happen independently of demand; "substantive" variations in nominal income are "almost invariably" the result of variations in the nominal supply of money. What Friedman and Schwartz seem to us to succeed in proving empirically is the power of the authorithies to produce deep depressions when their action is violent and unexpected. But this is also the thesis of that section of Keynesians who regard the quantity of money as substantially endogenous given the normally responsible behaviour of the monetary authorities which is designed to avoid giving rise to, or is intended to obviate financial crises.[24]

3. The theory of the demand for money, the stability of that function and the exogeneity of money are not, however, sufficient to produce definite results; as Friedman had asserted on previous occasions as well,[25] the system is underdetermined. With the closure of the model, Friedman intends to arrive at what he defines as "a monetary theory of nominal income", a theory which would make it possible to describe the movements of nominal income with reference solely to the variations in the money supply, with a relation which is quantitatively independent of how the variations in nominal income are composed of variations in prices and of variations in the quantity produced. It is obvious that this is possible if we succeed in accounting for the constancy of the velocity of circulation, or at any rate in showing that variations in it are foreseeable and independent of the "composition" of variations in nominal income.

An initial problem is raised by the dependence of the demand for money on interest rates. In an earlier work,[26] Friedman had laid at the basis of the theory of nominal income Fisher's suggestion that the nominal rate of interest be considered as the sum of the real rate of interest and of the anticipated rate of inflation, the real rate being taken as constant.

[23] JUDD and SCADDING (1982), p. 1013.
[24] In addition to the authors cited in the previous note 19, see also MINSKY (1980, 1982).
[25] FRIEDMAN (1974); *Monetary Trends*, p. 59.
[26] FRIEDMAN (1971).

Putting the expected rate of inflation as a function of current and past inflation, and the latter as a function of the past growth of the quantity of money, the nominal rate of interest, and hence the velocity of circulation, come to depend predictably on monetary growth. The fact is that not only may we well remain perplexed at the hypothesis of the constancy in the real rate of interest,[27] but the argument requires a close relation between the growth in prices and that of the quantity of money which cannot be justified within the core of Friedman's theory and which indeed represents one of the results at which he means to arrive later on.

Even if we admit the unimportance of interest rates as autonomous factors making for disturbance, it still has to be shown that the velocity of circulation is not affected by the "composition" of the variations in nominal income. As has been noted,[28] since the elasticity of the demand for money with respect to prices is unity, only if the elasticity of the demand for money with respect to real income is also equal to unity, it is unimportant whether the quantity of money influences prices and quantities in different proportions. It should be noted that the two unit elasticities imply the constancy of the velocity of circulation. The fact is that there is no theoretically plausible explanation for such a hypothesis; indeed, when, in *Monetary History*, the two authors found for the United States an elasticity of money with respect to income higher than unity, they regarded it as compatible with the nature of money as a luxury good. Putting forward a simplified version of the theory of nominal income, with the variations in income as a function of the excess supply of money (p. 62), *Monetary Trends* leaves the theoretical questions unanswered, and at bottom refers the reader to the empirical evidence. If that evidence did not deny the close relation between rates of variation in money and income, Friedman's methodological approach would lead us to conclude that it is "as if" the theory of nominal income was true. The fact is that Friedman's theory may not be the only one from which it emerges, at least for the long run, that there is a close relation between money income and quantity of money.

In addition to affirming that the direction of causality runs mainly from the supply of money to nominal income, Friedman seeks at bottom to prove that the velocity of circulation does not act as a shock-absorber

[27] Alternatively it would be necessary to postulate the constancy of the difference between the real rate of interest and the rate of growth of output; the arguments advanced by Friedman for both hypotheses are regarded as unconvincing by nonKeynesians such as MAYER (1982) as well.
[28] LAIDLER (1978) and MAYER (1982).

of conflicting pressures from income and the supply of money. Friedman attributes to Keynes and to the old guard of the Keynesians a conception of the velocity of circulation which regards it as a will o' the wisp, that is, as a magnitude without a theoretical personality of its own. If, as he affirms, Keynes' theory is based on absolute liquidity preference (pp. 51-7), variations in the supply of money would have repercussions only on the velocity of circulation which would thus perform a purely passive role. Friedman claims for the quantity approach the long-run stability of the velocity of circulation and a short-run behaviour which reinforces and does not damp down the variations in the supply of money (p. 57).

In his interpretation of Keynes, Friedman is far more prejudiced and factious than Keynes was in dealing with the "classics", although having the advantage over Keynes of a more formal training in economics. In the first place, there is no work of Keynes in which the economic cycle is not described with a procyclical behaviour of the velocity of circulation and of the quantity of money. When Keynes then speaks of the limits to the effectiveness of an expansionary monetary policy, he is carrying out a logical experiment which need not necessarily find frequent confirmation in real life, especially if the monetary authorities are aware of their own limits. In any case there are at least two periods to which Keynes refers and which seem to prove him right, the two great depressions of the 1890s and 1930s. In these periods, an active anticyclical monetary policy proved incapable of influencing nominal income.[29] Furthermore, the United States' experience of the last few years shows that an intentionally restrictive monetary policy has produced an increase in the velocity of circulation.[30] If we move on to the behaviour of the velocity of circulation in the medium/long period, Friedman may be in disagreement with Keynes' explanation, but he must not conceal the fact that Keynes affirms that "the net effect of fluctuations over a period of time will be to establish a mean figure in conformity with the stable proportion between the national income and the quantity of money to which the psychology of the public tends sooner or later to revert".[31] Hence Keynes too expects that in the long period money and nominal income will move in parallel.

[29] To see how strongly this emerges from the data, one has to refer to high-powered money and not the M_2 used by Friedman and Schwartz; on these subjects, see TONVERONACHI (1983).

[30] JUDD and SCADDING (1982).

[31] KEYNES (1936), p. 307.

Summing up, the theoretical developments considered so far show that Friedman's theory is mainly characterized by the exogeneity attributed to the money supply. Since, among other things, the empirical evidence proves incapable of enabling us to overcome the theoretical divergences on this point, it may be concluded that the differences between quantity theorists and Keynesians (but those closest to the Cambridge tradition) are primarily of a theoretical and not empirical nature.

In line with Friedman's definition of the quantity theory, we have affirmed that the core of that theory stops at the propositions concerning nominal income. That means that Friedman ought not to regard as crucial the validity of the additional propositions concerning, among other things, the influence of the quantity of money on prices and production. In reality, Friedman's position is open solely as regards the short-period disequilibrium processes, during which he admits that even production may vary as a result of an excess supply of money; that is, he admits that, at most, the Keynesian theory can hold good for the short or very short period, ie. that it can describe initial and transitional effects. The ways diverge sharply as regards the permanent or long-term effects of variations in the quantity of money, since Friedman denies the most innovative result of Keynesian theory, ie. the possible emergence of underemployment equilibria. Friedman affirms that "we shall regard long-run equilibrium as determined by the Walrasian equations of general equilibrium, which determine the real variables, plus the quantity theory, which, for the given real variables, determines the price level" (p. 60). The approach is therefore the traditional one, with money unable to influence the determinants of long-run equilibrium (resources, preferences and technology). In addition, the equilibrium is regarded as stable in view of the operation of the wealth effect, which by itself is capable of closing the Keynesian chapter on underemployment equilibrium (pp. 42-3). As is shown by significant gaps in the bibliography attached to the volume, Friedman takes no account either of the criticisms long addressed to this type of utilization of the Walrasian model (Hahn 1965, 1971 and 1980) nor of the perplexities at the general validity of the wealth effect (Tobin 1980); in addition, like the supporters of the neoclassical synthesis, he does not seem to realize that the wealth effect is, if anything, able to bring into full equilibrium the markets of goods, but not necessarily the labour market (Tonveronachi 1983).

Friedman cannot conceal the weakness of his theoretical position behind affirmations such as the one according to which only "noneco-

nomists, opponents of the market system" do not accept the Keynesian proposition as wrong (p. 43). Moreover, in referring to the full Walrasian equilibrium, he has chosen what is apparently the easiest path, but not the most meaningful one. For his thesis requires that the quantity of money should have no influence on the determinants of long-run equilibrium, be it of full or not full utilization of all resources; but this is a proposition which is even more difficult to prove than the previous one.

Since, as regards long-run equilibrium, Friedman takes up a strictly theoretical position, with hypotheses which are not empirical generalizations, the differences from Keynes' theory are again of a theoretical nature. In that case, then, the methodology of positive economics cannot be invoked in order to affirm that, if the empirical evidence agrees with the theoretical predictions, it is "as if" the theory was true: empirical evidence cannot bridge over the internal inconsistency of a theory.

4. There still remains the need to discuss — briefly — the additional propositions which elaborate the core of the quantity theory; we shall begin with the adjustment process of nominal income, prices and production following an excess supply of money.

The variations in nominal income, or rather the divergences from its permanent or expected rate of growth, are put as a function of the excess supply of money (p. 62); that means that a difference between the quantity of money possessed and that desired (at current prices and income) leads to a process of portfolio readjustment, the effect of which is an increase in expenditure and hence in nominal income.

For the readjustment of prices and production, the hypothesis is adopted that their rate of change is a function of the expected rate of inflation and of the difference between current production and full employment production (p. 60). In the equations, therefore, we find the expected, or permanent or full-employment values for prices, real income and nominal income; the readjustment to the new equilibrium is also produced by a revision of the expected values in response to the values experienced (p. 64).

Friedman and Schwartz are aware of the two problems arising from this formulation. The first derives from the hypothesis that, for each variable, expectations are determined by the history of that same variable and not by the entire set of phenomena experienced. The two authors feel the fragility of this hypothesis, especially as regards the

theory of rational expectations; they reply that this theory has not yet been able to supply "empirically testable hypotheses about the formation of expectations" (p. 65). The second problem is that of a possible overdetermination of the system, which is now composed both of the expanded system of Walrasian equations and of the feedback equations now illustrated. They affirm generically that "the problem is to assure that at long-run equilibrium these two determinations do not conflict" (p. 66). This thus brings out the mainly empirical meaning of these feedback equations which, it should be added, are not derived from specific behavioural hypotheses. The only significant theoretical constraint placed on these equations is the final value of real income, that is, its full employment value; in this way the final effect of an excess supply of money cannot fail to affect more than prices.[32]

What is more interesting from the theoretical point of view is the discussion on the determination of interest rates.[33] The point round which the discussion revolves is the distinction between real and nominal rates of interest. The real rate of interest is determined in the real part of the model (the often cited but never explicitly stated Walrasian equations), and thus proves to be linked to long-run equilibrium. Friedman and Schwartz affirm that the real rate is crucially dependent on the community's time preferences (p. 499). Nominal and real rates of interest are equal in equilibrium only in the absence of price movements. According to the two authors, the complexity of the relation between quantity of money and interest rates depends on the interaction between monetary and real disturbances, which they analyze separately.

Monetary disturbances consist of variations in the rate of growth in the quantity of money. For the sake of simplicity, the analysis assumes a once-and-for-all increase in that rate. A distinction is made between an impact, an intermediate and a final effect. The impact effect may be described by means of the Keynesian theory of liquidity preference and/or by means of the loanable funds effect; in both cases there is a lowering of the rate of interest, though with a different behaviour in adjustment over time. The intermediate effect consists of a wider readjustment, which can be described either on Keynesian lines (with an

[32] The authors briefly discuss the possible effect of a variation in the permanent rate of inflation on the rate of equilibrium growth of income; they propend for a negligible influence, and if anything one with a negative sign (pp. 66-8).

[33] This discussion is not, like the other ones, contained in Chapter 2 of *Monetary Trends*, but in Chapter 10, section 10.1.

increase in investment and subsequent multiplier effects on spending), or on portfolio adjustment lines (according to which the attempt to unload excessive liquidity leads to a greater flow of expenditure in all directions); whether the greater expenditure induces variations in production or also in prices, the result is in any case an increase in the demand for money and hence an increase in the rates of interest; in this way, the trend set in motion by the impact effect is reversed. Since Friedman assumes that the monetary disturbance does not influence the determinants of long-run equilibrium, sooner or later production must fall in order to approximate to the equilibrium value, leaving the prices to bear the whole impact of the greater rate of increase in the quantity of money. The final effect recalls the Fisher relation, that is, the determination of the nominal rate of interest through the real rate and the expected rate of inflation. As the effects of the initial disturbance are concentrated on prices, the greater rate of inflation becomes expected and the nominal rate has to increase in order to produce a real rate equal to the initial one.[34]

The importance of this discussion also lies in the different behaviour of the nominal and real rates of interest depending on whether the greater degre of inflation is or is not fully anticipated. With correct expectations, the real rate is constant and the nominal rate is a function of the rate of inflation; where expectations underestimate variations in prices, the nominal rate only partially adjusts and the real rate falls. As we shall see (section 6), this is the basis from which Friedman and Schwartz then start to discuss the Gibson paradox.

In order to describe the disturbances of a real nature, the two authors refer to the surge of innovations hypothesized by Wicksell and Schumpeter which induce initial increases in the real rate of interest. Friedman and Schwartz are right in regarding a systematic analysis of the real disturbances as difficult since the effects depend largely on the nature of these disturbances; but the innovations which the two authors have in mind must be very strange if the terminal value of the real income is put equal to the initial one (p. 498).[35] In any case, the process is described as follows: the greater real rate of interest induces an increase in the demand for capital goods and loans; the nominal rate of

[34] For the real rate of interest the arguments in note 32 also apply; see *Monetary Trends*, pp. 491-4.

[35] We do not feel we are wrong in affirming that the innovations referred to by Wicksell and Schumpeter are of the type which free resources and hence imply a new equilibrium characterized by greater production.

interest increases and the velocity of circulation grows (the two authors prefer in this case, too, to assume the quantity of money as being exogenous). With time, production reverts to the initial value, and the increased expenditure is reflected solely in prices. During the process of adjustment to the new equilibrium, the real and nominal rates of interest are both higher than their initial values; their final position depends on the time preferences of the community which characterize the long-run equilibrium.

The simultaneous presence of the two types of disturbance explains, according to the authors, why it will later be difficult to identify empirically simple and stable relations between rates of interest, prices and quantity of money.

5. The aim of this section, and of the one following it, is not that of presenting a detailed analysis of the weighty empirical work contained in *Monetary Trends*. The discussion can only embrace those arguments which have proved crucial in the previous pages, with a view to examining whether the empirical results presented by Friedman and Schwartz are in a position to provide unambiguous replies to the many questions raised. In this section we will deal with the empirical evidence regarding the propositions which characterize the core of Friedman's theory.

A brief reference to the basic data. Since the aim is to study the relations for periods longer than those dominated by cyclical fluctuations, the whole period is divided into phases of cyclical contraction and expansion; the absolute values are calculated as an average over a cycle phase and the rates of change refer to three successive phase averages. It is obvious that the results are sensitive not only to the statistical techniques used, but also to the criteria which have led to the identification of the cyclical phases.

From the series thus constructed for the quantity of money (M_2) and for nominal income, the series for the velocity of circulation are obtained. Although with some oscillation, the velocity declines in the United States at a fast rate from 1867 to 1903, at a more moderate rate from 1904 to 1929, and hence with renewed vigour up to the end of the second world war. The period after that war experienced first a sustained increase in velocity (up to the middle of the 'fifties) and then a more restrained growth. In the United Kingdom, on the contrary, velocity shows a moderate increase up to the end of the first world war, and then declines at a sustained rate, and with marked swings, up to

the end of the second world war. In the period after that war, there is an even more sustained increase than in the United States, interrupted by a fall in the first half of the 'seventies. At the beginning of the period, the velocity of circulation in the United States is almost triple that of the rate in Britain; the distance then narrows rapidly up till 1903. After ups and downs, the difference tends to disappear towards the end of the second world war, and again emerges, with the opposite sign, in the period after the second world war (see graphs 5.5A, p. 178).

These rough series do not however satisfy the two authors. For the United States the period up to 1903 is regarded as one of increasing financial sophistication, which simply means a diminution in the fraction of production for own use; the authors then estimate at 2.5% a year the increase in the demand for money due to this reason alone, and the series of M_2 is corrected in consequence. For both countries there are then introduced two dummy variables: one which makes the function of the demand for money shift upwards in line with periods of marked economic depressions or wars; the other relates to the readjustment of the two postwar periods and has a negative effect on the demand for money for the period after the first world war and a positive effect for that after the second one. The three corrections affect about 66% of the whole period considered for the United States, and over 40% for the United Kingdom. As we will have occasion to argue shortly, the intervention effected on the series of the quantity of money is severe not only because of the magnitude of adjustments and the length of the period involved, but especially because of the very nature of the readjustments.

Coming now to the demand for money in real terms, we find that this is a function, as well as of the three adjustment variables just described, of real income, of nominal rates on short-term securities and of the rate of growth of nominal income, considered as a proxy variable of the nominal yield on real assets.[36] The authors affirm that six variables are not very many for such a long period and for two different countries, so that the function of the demand for money may be regarded as stable.

At this point we can take up again some considerations discussed in section 2. In the first place, since a function's stability cannot be subject to an absolute assessment, and since the two authors do not

[36] For criticisms of the proxy variables used in *Monetary Trends*, see MAYER (1982) and GOODHART (1982).

compare their results with those obtained or obtainable from alternative functions, their statement on the relative stability of that function is not very meaningful.[37] In addition, the studies conducted by others on the function of the demand for money for the 'seventies show the insufficiency of that type of function and the need to incorporate in it a further dummy variable in order to take account of phenomena of financial innovation.[38]

In the second place, we have already affirmed (section 2) that stability has meaning if defined with respect to variables whose quantitative link with the phenomenon which they represent can be postulated *a priori*. The three adjustment variables do not appear to respect this criterion. It has been noted for example that it seems incomprehensible that a dummy variable can have the same coefficient for the two countries during the first world war, when Great Britain was deeply committed for four years and the United States was by comparison hardly affected.[39] In addition, the dummy variable used for the depression of the 'thirties accounts, according to the authors, for the increase in liquidity preference caused by an increased degree of uncertainty (p. 228). This leads Goodhart to wonder: "If that is the argument, is it right to treat such a behavioral hypothesis by a dummy?". Moreover, since "what has happened before could happen again, does not this reliance on dummy variables to account for several major changes in velocity seriously weaken the case for monetary rules?"[40] But Goodhart is perhaps too optimistic as regards the possibility of transforming that dummy variable into a real explicative variable. It certainly cannot be thought that, once uncertainty is introduced, it has no influence even in less turbulent times (and the frequent oscillations of the velocity of circulation show that turbulence is never absent). However, at this point the problem arises of linking uncertainty on to measurable magnitudes, that is, the problem of conducting the analysis on a strictly quantitative level. In assessing the stability of the function of money, it is therefore essential to remember that three of the six explicative variables are dummy ones, or at any rate adjustment ones whose links with the phenomena which they represent

[37] On these subjects see also MAYER (1982), pp. 1533-4.

[38] See JUDD and SCADDING (1982).

[39] GOODHART (1982), p. 1544. Goodhart's argument also finds quantitative support in the effect of the war on real per capita income which shows a much sharper fall in Great Britain than in the United States. See *Monetary Trends*, graph 5.3B, p. 162.

[40] GOODHART (1982), p. 1544.

are difficult to postulate *a priori*. If we then wish to extend the period covered in order to include in it the decade just elapsed, the need to insert a further dummy variable in the function to account for phenomena of financial innovation, shifts the explicative burden even more dangerously on to this type of variable. It seems to us obvious that recourse to these variables stems from the desire to remove from the statistical series events regarded as accidental or extraordinary with a view to bringing back the long-run movements of the velocity of circulation within the same logic; without the adjustments carried out with the dummy variables, one might in fact be led to make a more episodic and institutional analysis of the demand for money. And it is also obvious that it is precisely this type of interpretation at which the two authors do not wish to arrive.

Friedman and Schwartz show themselves satisfied at having found that real income is the more significant statistical determinant of the demand for money in real terms. This finding, however, has implications which are the cause of visible embarrassment throughout the volume. Even with the adjustments mentioned above, the velocity of circulation proves to be decreasing in the United States and increasing in the United Kingdom. The result is an elasticity of the demand for money with respect to income of more than 1.1 in the United States and less than 0.9 in the United Kingdom.[41] Some commentators are unable to explain the embarrassment with which these results are presented and commented on in *Monetary Trends*; they affirm that the difference between the two elasticities is minimal, that much more marked differences between the two countries are shown in the studies carried out for consumer goods,[42] and that the difference should not cause surprise given the different institutional developments in the two countries.[43] But the point is that these results have important repercussions on Friedman's *theory*. In the first place, the difference between the two elasticities and between each one and unity, are statistically significant; and it is obvious that the corrections effected in the M_2 series have had the consequence of minimizing them. In the second place, elasticities significantly different from unity contradict one of the hypotheses which, as has been seen, are necessary for the theory of

[41] Since in *Monetary History* the M_2 series for the United States was not adjusted to take account of the increasing financial sophistication, the elasticity was 1.8.

[42] MAYER (1982), p. 1533.

[43] GOODHART (1982), p. 1544.

nominal income, that is, in order to affirm the unimportance of the different variations in prices and production for the relation between quantity of money and nominal income.[44] It should also be noted that, given the variability of the velocity of circulation, that relation is also less easy to postulate for the short/medium period, for which it had originally been thought up. In the third place, these values of elasticity mean, according to the logic of Friedman's theory (p. 222) that, while the Americans regard money as a luxury good, the British think of it as an inferior good. This difference cannot but be regarded as a serious blow for a theory which regards as stemming from money the greatest and perhaps only regularities and uniformities both in time and as between countries.

In the fourth place, to bring in the different institutional developments in order to explain these differences, would result in the entry into the theoretical picture of considerations which Friedman has always tried to exclude. It should be remembered that one of the greatest reasons for satisfaction expressed in *Monetary History* was the fact of having found stable relations throughout a long period of major institutional changes.[45] In *Monetary Trends* the comparison with the United Kingdom has forced the two authors to insert an institutional factor such as the growing financial sophistication; however, these factors must remain secondary, with at most an episodic role, in order not to transform the quantity theory into a quality theory.

From the estimated demand function for money, Friedman and Schwartz then derive a function for nominal income. The regression shows a good correlation, for both countries, between quantity of money and nominal income. It has already been observed that this result is significant for Friedman's theory only if the quantity of money is regarded as exogenous. The fact is that, as noted by Mayer,[46] the results of other regressions presented by the two authors tend if anything to weaken that crucial hypothesis. For these regressions show that better results are obtained if all that is considered is just current money in relation to versions using both current and lagged money. The result is compatible with causation running from income to money or with the hypothesis that money and income are determined by a common series

[44] Perhaps this is also the reason which impels Friedman in *Monetary Trends* to favour a simplified and theoretically uncertain version of the theory of nominal income over the more complete and explicit version of FRIEDMAN (1971).

[45] FRIEDMAN and SCHWARTZ (1963b), p. 683.

[46] MAYER (1982), pp. 1534-5.

of other causes. To sum up, the empirical results presented in *Monetary Trends* appear unable to resolve the more controversial points regarding the core of the quantity theory.

6. In assessing the estimates of the relations between quantity of money, prices and real income, too, it is crucial to reason as if money was exogenous.

The average rate of growth of real income proves to be constant and greater in the United States than in the United Kingdom. The average rate of inflation on the contrary is higher in the United Kingdom. While the (present and past) rate of growth of the quantity of money seems to show a good relation with the rate of inflation, its links with the rate of growth of real income are weak in the United States and not significant in the United Kingdom. According to the two authors (p. 623), that operates in favour of the crude version of the quantity theory, that is, of the independence of real income from the quantity of money and represents a failure of the more sophisticated version based on the feedback equations (see above, section 4).

Let us begin with prices. Friedman and Schwartz show that the current rate of inflation is correlated with the current and past growth of money; a good result is also obtained by the regression between the current rate of inflation and the rates of inflation experienced in the past. By recourse to annual data for Great Britain, Goodhart also obtains a good estimate of the current rate of inflation using the current or lagged rate of monetary growth and past inflation as explicative variables. Goodhart then points out that the results are "strongly monetarist" only if the growth of money is assumed to be independent of current inflation. In other words, these regressions could also be used to support the thesis of the dependence of the rate of growth of the quantity of money on the rate of anticipated inflation (estimated in *Monetary Trends* by means of current and past inflation).[47]

Friedman and Schwartz then show that the rate of inflation is not explained by the degree of utilization of productive capacity; they do not succeed in deriving from the data the normal Phillips curve, but if anything a Phillips curve with a positive slope. This is considered as the result of the inverse and not direct relation between the rates of

[47] GOODHART (1982), pp. 1546-7. Goodhart affirms that "when Friedman turns from statistical analysis to policy prescriptions, this is often exactly what he accuses Central Banks of so misguidedly doing", *ibidem*, p. 1547.

variation of prices and production observed for the whole period in the United Kingdom and from the period after the second world war in the United States (graph 9.2, p. 401). The two authors consider these results as an obvious negation of the Keynesian theses, which, as usual, are not presented with due objectivity.

In the first place, the positive relation is postulated in the *General Theory* and by the Keynesians of the neoclassical synthesis for the *levels* of prices and quantities (at a given money wage), with reference to cyclical fluctuations and not for the medium/long run, to which the data of *Monetary Trends* refer; nor should it be forgotten that in 1939 Keynes convincingly affirmed that his theory had everything to gain from the abandonment of the direct relation.[48] If we move from the levels to the rates of change, it should not be forgotten that a significant part of the Keynesian theory links, for the medium/long period, greater rates of real growth with larger increases in productivity, and hence, at least potentially, with lower rates of inflation.[49] This is precisely the result of the observations of Friedman and Schwartz. The arguments of the two writers, moreover, do not always seem consistent. They are convinced that the rate of real growth is greater the farther away one is from full employment (we should remember their feedback equation for real income); but then the inverse relation between rate of growth and rate of inflation ought to lead them to affirm the existence of a Phillips curve with a negative slope.

Their affirmation that in the long run the growth of real income is independent of monetary dynamics should also be interpreted with caution. What ought to give cause for reflection and give rise to further investigation is the constancy of the average rate of long-run growth. The few data contained in *Monetary Trends* do not help to penetrate its significance. (We are not, for example, given the series for the rate of growth of product by unit of labour, the rate of growth of the private sector alone, or the secular trend of unemployment).

As regards the influence of money on real income, the two countries show different types of behaviour. The fact that influence

[48] KEYNES (1939).

[49] KALDOR (1966). At the limit, Kaldor's theory can help to explain why Great Britain, which for the whole period shows a lower rate of growth than the United States, experiences a higher average rate of inflation; this difference is not explained in *Monetary Trends*. Independently of increases in productivity, according to SYLOS LABINI's oligopoly theory (1964), a larger growth of demand leads to a lowering of the barriers to entry which induces the price leaders to lower their prices or to keep down increases.

cannot be statistically shown for the United Kingdom might be explained by the constraints on the growth of income in an economy always subject to marked balance of payments constraints and by the responsible behaviour of the monetary authorities. We could conclude that "the United Kingdom has not suffered from such large disturbances in monetary conditions as to generate large enough cycles to show a strong statistical relationship between m [money growth] and O [output growth]".[50] For the United States, on the contrary, the relation between money and real income proves significant; Friedman and Schwartz go so far as to affirm that the fluctuations in the rate of growth of real income may be regarded as residual fluctuations of the great depressions of the years 1890s, 1920s and 1930s. But, since they are convinced that a depression is always caused by monetary factors, that ought to mean that money has a considerable and prolonged influence on production. As can readily be understood, these questions have a marked normative significance. If it can be shown that substantial variations in the rate of growth of the quantity of money have a negligible and short-term effect on real income and a predominant and lasting effect on the rate of inflation, it follows that it is possible to make uninhibited use of the monetary lever in order to wipe out inflation. Experiments in this direction have been tried in recent years, but, as far as one can judge, and as might also be expected from the empirical verifications just examined, with results which do not bear out Friedman's optimism. Lastly, it must be added that these results cannot in any way obscure the serious deficiencies in Friedman's theory of long-run equilibrium.

It remains for us to analyze the relation between quantity of money and rates of interest. Friedman and Schwartz deny any validity to the Keynesian theory of liquidity preference since the evidence does not show, for the medium/long period, an inverse relation between the quantity of money and nominal rates of interest, but if anything a direct relation. On this point it is time to clarify the situation, since Friedman has the habit of accusing Keynes of not having distinguished between nominal and real values. In Chapter 17 of the *General Theory*, Keynes defines the own rates of money interest including in them a coefficient of expected appreciation for every asset with respect to money; the liquidity effect thus shows itself for any given rate of appreciation, and should therefore be measured with reference to the real and not the

[50] GOODHART (1982), p. 1548.

nominal rates.[51] Since Friedman and Schwartz prove the existence of an inverse relation between monetary growth and real rates, the Keynesian theory is not at all contradicted.

The most interesting part of the discussion, however, is about the Gibson paradox, that is, the direct relation between movements in prices and in nominal interest rates. Apart from in the two world wars, this relation can be observed in the whole period up to 1960; subsequently, interest rates seem more closely connected to the rate of inflation. Since real interest rates show a greater variability than nominal rates (both referred to short-term securities), Friedman and Schwartz then conclude that in the whole period inflation has never been fully anticipated, and bring this result to the attention of the rational expectations school. Though recognizing that they are not in a position to interpret these results within a unitary system, the two authors incline to regard as plausible, although not very satisfactory, the explanation of the Gibson paradox provided by Fisher and to deny validity to explanations of the Wicksell-Keynes type. Fisher's explanation assumes that inflationary expectations adjust only with considerable delay to current inflation; hence nominal interest rates adjust gradually, so that the desired real rate diverges systematically from the one realized. Friedman and Schwartz agree with Fisher's approach, but recognize that his formulation would have to be modified in order to insert in it a logic of the variation of real interest rates and to make the adjustment of expectations less mechanical.

The explanation on Wicksell-Keynes lines of the Gibson paradox is based on an autonomous change of the expected yields on investments, which influences aggregate demand, rates of interest and prices. The criticism formulated by Friedman and Schwartz is based on the fact that evidence seems to show that the real rate of return on physical assets is not correlated with the price level, and that the monetary expansion which accompanies the increase in prices is in general the result of an increase in high-powered money and not of bank-created money. Mayer, however, points out that the variable with which the two authors approximate the real rate of physical return on assets (the rate of change of real income) is not reliable; in addition, for the expansion of money it is sufficient to postulate that the greater expected return on investments, and hence the higher rates of interest, succeed in attracting

[51] This is moreover consistent with the fact that, in the *General Theory*, income, consumption, investment etc. are all measured in terms of the wage unit.

funds from abroad and in stimulating the supply of money.[52] We thus come back to the ever present problem of the exogeneity of money. But, once again, objections must also be made to the attempt to present Keynes' theory as if it ignored the distinction between nominal and real rates, and denied the influence on the former of inflationary expectations; the explanations of Fisher and Keynes are not necessarily antithetical, and it would hence be possible to weld them together. But is that not what Friedman and Schwartz propose when they envisage a "generalization" of the Fisher approach?[53]

Conclusions

The problems tackled in *Monetary Trends* are many, indeed too many for an article such as the present one to take them fully into account and permit an appreciation of the enormous amount of work which it has cost the two authors. Even if the volume does not contain developments which break really new ground as compared with the previous contributions of Friedman and Schwartz, it is certainly stimulating to read, even and perhaps above all for critics of the quantity theory. Nevertheless, and perhaps contrary to the expectations of the two authors, its powers of persuasion are very limited. The conviction derived from it is that this vast work of collection, manipulation and interpretation of the data has not produced results capable of making these tests crucial for the choice between conflicting theories. Since, on the contrary, this is the role which Friedman assigns to empirical evidence, the volume may have a good reception as a point of organic reference for the debate in the future, but it will not constitute the decisive element in that debate.

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[52] MAYER (1982), pp. 1537-8.

[53] It should, however, be made clear that, if this "generalization" is carried out in a Friedman perspective, it is reasonable to expect that it will clash with Keynes' theory of the own rates of interest. On this point, see HARROD (1971).

BIBLIOGRAPHY

ANDO A. and MODIGLIANI F. (1965), "Velocity and the Investment Multiplier", *American Economic Review*, September.

CAGAN P. (1965), *Determinants and Effects of Changes in the Stock of Money, 1875-1960*, Columbia University Press for NBER, New York.

CRAMP A.B. (1971a), "Monetary Policy: Strong or Weak?", in *Conflicts in Policy Objectives*, edited by N. Kaldor, B. Blackwell, Oxford.

CRAMP A.B. (1971b), *Monetary Management*, George Allen and Unwin, London.

DESAI M. (1981), *Testing Monetarism*, Frances Pinter Ltd., London.

DE PRANO M. and MAYER T. (1965), "Tests of the Relative Importance of Autonomous Expenditure and Money", *American Economic Review*, September.

FRIEDMAN M. (1953), "The Methodology of Positive Economics", in Friedman, *Essays in Positive Economics*, The University of Chicago Press, Chicago.

FRIEDMAN M. (1971), "A Monetary Theory of Nominal Income", *Journal of Political Economy*, March-April.

FRIEDMAN M. (1974), "A Theoretical Framework for Monetary Analysis", in *Milton Friedman's Monetary Framework. A Debate with His Critics*, edited by R.J. Gordon, The University of Chicago Press, Chicago.

FRIEDMAN M. and MEISELMAN D. (1963), "The Relative Stability of Monetary Velocity and the Investment Multiplier in the United States, 1867-1958", in *Stabilization Policies*, Commission on Money and Credit, Prentice Hall, Englewood Cliffs, N.J.

FRIEDMAN M. and MEISELMAN D. (1965), "Reply to Ando and Modigliani and to De Prano and Mayer", *American Economic Review*, September.

FRIEDMAN M. and SCHWARTZ A. (1963a), "Money and Business Cycles", *Review of Economics and Statistics*, February.

FRIEDMAN M. and SCHWARTZ A. (1963b), *A Monetary History of the United States, 1867-1960*, Princeton University Press for NBER, Princeton.

FRIEDMAN M. and SCHWARTZ A. (1970), *Monetary Statistics of the United States*, Columbia University Press for NBER, New York.

FRIEDMAN M. and SCHWARTZ A. (1982), *Monetary Trends in the United States and the United Kingdom. Their Relation to Income, Prices, and Interest Rates, 1867-1975*, The University of Chicago Press for NBER, Chicago.

GOODHART C. (1982), "Monetary Trends in the United States and the United Kingdom: A British Review", *Journal of Economic Literature*, December.

HAHN F. (1965), "On Some Problems of Proving the Existence of an Equilibrium in a Monetary Economy", in *The Theory of Interest Rates*, Hahn and Brechling, eds., MacMillan, London.

HAHN F. (1971), "Professor Friedman's Views on Money", *Economica*, February.

HAHN F. (1980), "Monetarism and Economic Theory", *Economica*, February.

HARROD R. (1971), "Discussion Paper on M. Friedman's 'A Monetary Theory of Nominal Income'" in *Monetary Theory and Policy in the 1970s*, ed. by Clayton, Gilbert and Sedgwick, Oxford University Press, Oxford.

HAWTREY R.G. (1928), *Currency and Credit*, Longmans, London.

JUDD J.P. and SCADDING J.L. (1982), "The Search for a Stable Money Demand Function: A Survey of the Post-1973 Literature", *Journal of Economic Literature*, September.

KALDOR N. (1966), *Causes of the Slow Rate of Economic Growth of the United Kingdom*, Cambridge University Press.

KALDOR N. (1970), "The New Monetarism", *Lloyds Banks Review*, July, reproduced in *Money and Banking*, ed. by A.A. Walters, Penguin Books, Harmondsworth, Middlesex.

KEYNES J.M. (1936), *The General Theory of Employment, Interest and Money*, MacMillan, London.

KEYNES J.M. (1939), "Relative Movements of Real Wages and Output", *Economic Journal*, March.

LAIDLER D. (1978), "Money and Money Income: An Essay on the Transmission Mechanics", *Journal of Monetary Economics*, April.

LAIDLER D. (1981), "Monetarism: An Interpretation and an Assessment", *Economic Journal*, March.

MAYER T. (1982), "Monetary Trends in the United States and the United Kingdom: A Review Article", *Journal of Economic Literature*, December.

MINSKY H. (1980), "Capitalist Financial Processes and the Instability of Capitalism", *Journal of Economic Issues*, June

MINSKY H. (1982), "Debt Deflation Processes in Today's Institutional Environment", in this *Review*, December.

MODIGLIANI F. (1977), "The Monetarist Controversy or, Should We Forsake Stabilization Policies?", *American Economic Review*, March.

PATINKIN D. (1969), "The Chicago Tradition, the Quantity Theory, and Friedman", *Journal of Money, Credit and Banking*, February.

SYLOS LABINI P. (1964), *Oligopoly and Technical Progress*, 2nd edition 1969, Harvard University Press, Cambridge, Mass.

TOBIN J. (1980), *Asset Accumulation and Economic Activity*, B. Blackwell, Oxford.

TONVERONACHI M. (1983), *J.M. Keynes: dall'instabilità ciclica all'equilibrio di sottoccupazione*, Nuova Italia Scientifica, Roma.