Monetary Theory and Monetary History

By Prof. Allan H. Meltzer, Carnegie Institute of Technology, Pittsburgh

This volume is a delight to the economist. The book is clearly destined to be a classic, perhaps one of the few emerging in that role rather than growing into it. The reader cannot fail to be impressed by the size of the task to which the authors committed themselves, by the authors’ ability to treat the broad sweep of a century of monetary history without being overcome in the mass of detail that they carefully examine, by the originality and scholarship that are everywhere displayed, and by a host of other considerations most of which are conveyed by the word “classic”.

The scope of the book and the array of topics covered force the reviewer to devote principal attention to some particular features. Others have discussed the general sweep of the analysis, the main conclusions, the capital theory underlying the authors’ discussion of the banks’ demand for reserves in the ’thirties, and the use made of the purchasing power parity theory. This review will concentrate on the monetary theory that underlies the author’s interpretation of monetary history. After discussing the authors’ treatment of the money supply and velocity, I will consider the implicit theory of price and income determination that emerged from my reading of the book and some related papers. Needless to say, my interpretation, though based on their work and consistent with many of their conclusions, may not be a completely correct statement of the authors’ views. But the authors do not formally present their theory, though their discussion of the effects of monetary change on various aggregate measures suggests that they use one.

The framework presented here suggests the source of some of the conclusions for monetary theory or policy in the authors’ study of monetary history – particularly the denigration of the influence of interest rate movements and the denial of an important role to interest rates in the demand function for

1 A discussion of Milton Friedman and Anna J. Schwartz, A Monetary History of the United States, 1867–1960. A study for the National Bureau of Economic Research (Princeton: University Press, 1963). I am indebted, as always, to Karl Brunner and wish to acknowledge helpful discussions with Harry G. Johnson and Zvi Griliches and financial support from the National Science Foundation. Comments on an earlier draft by Milton Friedman and Anna Schwartz were extremely useful and clarified a number of points. An earlier version of this paper was presented at the American Bankers Association, Conference of University Professors at Princeton, September 1, 1964.

money. Moreover, the framework leads to an understanding of the central role that the stock of money plays in the authors' analysis of income and employment and of the preeminent position of money vis-a-vis its more or less formidable rivals, the budget deficit or the supply of pins, a position that is more fully discussed in some of the authors' related work.

Acceptance of Friedman and Schwartz' implicit or explicit interpretations of history or policy must rest on an acceptance of their underlying theory. This is the main theme of the review and leads to its main conclusion: The Friedman and Schwartz theory is primarily a theory of the price level and not a theory of fluctuations in output. Their discussion of cyclical fluctuations requires the introduction of additional hypotheses some of which entail rather specific treatments of particular cycles. Despite the authors' numerous, imaginative applications of economic theory to a variety of problems, the explanation of cyclical variations retains a rather ad hoc character.

The Money Supply

Monetary history consists of two separable series of events. One is an account of the forces acting on the money supply. This part encompasses the influence of policy actions, changing institutional arrangements, the behavior of the banks and the public, the interrelation of these forces and the response of the money supply to them. The other is a statement of the ways in which income, domestic prices, exchange rates, etc., respond to the level or rate of change of the stock of money when allowance has been made for the influence of non-monetary factors. The link between the money supply and prices or income is, of course, velocity. In this section some problems raised by Friedman and Schwartz' discussion of the money supply are considered. The following sections provide an interpretation of the authors' underlying theory of the demand for money or velocity and the resulting macro-economic theory.

The money supply theory that underlies Friedman and Schwartz' penetrating analysis of monetary history is presented in their Appendix B. The basic elements to be explained by the theory are described in four equations, the solution in equation 5.


2 Some of the symbols are changed or introduced here. Friedman and Schwartz do not use symbols for r and k and refer to what is here called the monetary base as "high powered money", H. They use D for what I have called D + T. See Appendix B, esp. pp. 790-791. Note also that Friedman and Schwartz do not write the specific relations that are equations (2) and (5) of the text. However, as the discussion in the text notes below, the equations are discussed particularly on pp. 785-788.
\[ M = C + D + T, \]  
\[ B = R + C, \]  
\[ R = r(D + T), \]  
\[ C = k(D + T), \]

so that

\[ M = \frac{1+k}{r+k} B, \]

where \( M, C, D, T, R \) and \( B \) are the money supply, currency, demand deposits, time deposits, bank reserves and the monetary base, respectively.

Equation (5) has two quite different roles in the authors' discussion, a difference that leads to two concepts of the money supply that are not carefully distinguished: "expected" and actual. At the start of most chapters, equation (5) is used as an "arithmetical" relation, a framework for dividing the changes in the money supply into changes produced by variations in \( B, r \) and \( k \). Used in this way equation (5) is an identity. But Friedman and Schwartz also refer to \( B, r \) and \( k \) as "proximate determinants" of the money supply and indicate the principal factors on which the proximate determinants are assumed to depend. This procedure makes \( r \) and \( k \) desired ratios and converts equation (5) into a statement about the quantity of money that is expected to result from the behavior of the banks, the public and the policymakers.

If \( B, r \) and \( k \) always adjust rapidly to changes in the variables on which they depend, the actual money supply and the expected long run money supply would be approximately equal at each moment. But many of the interesting episodes in U.S. monetary history are those in which the expected and actual money supply are not equal. There have been periods of bank "runs" and bank failures as well as periods in which banks accumulated a relatively large volume of reserves. These are the more obvious occasions for a discrepancy between actual and expected long-run monetary variables.

Suppose that we elect to treat some of these episodes by assuming that there is a lag in the adjustment of actual \( k \) or \( r \) to its long-run desired position. For example, assume that an unanticipated demand for currency, or a fall in interest rates, raises the short-run desired reserve ratio above the long-run desired ratio in a particular month or year. The short-run response of the reserve ratio is larger than the expected long-run response, and the money supply expected to result from the current value of the monetary base is smaller than the amount expected in the long-run. When the actual money supply differs from the amount expected, the difference may be described in terms of a lag or by introducing some variables on which desired \( r \) or \( k \) -- and hence the expected short-run money supply - depend, given the monetary base.
Once \( r \) and \( k \) are introduced as desired ratios, it is no longer appropriate to regard them as relatively exogenous variables. The discussion throughout *Friedman* and *Schwartz*’ book, and the visual summary in Chart 64, indicates clearly that the ratios \( r \) and \( k \) have exhibited substantial short-run variability. The author’s explanation of this variability makes \( r \) and \( k \) endogenous variables and describes some of the factors on which the two ratios are assumed to depend. But this dependence is not incorporated as part of a maintained money supply hypothesis or used to provide a consistent explanation of the departure of the actual from the expected long-run money supply.

If the desired reserve and currency ratios depend on interest rates and/or other variables, the expected money supply is dependent on these variables also. Since real income and prices depend at least partially on the money supply, they, too, depend on interest rates, even if the demand for money is found to be independent of interest rates. The procedure used by *Friedman* and *Schwartz* permits them to ignore the systematic effect of interest rates (and other variables) on the money supply and hence on the equilibrium levels of real income and prices. Three examples of the treatment of interest rates on the supply side will be noted.

First, *Friedman* and *Schwartz* use a definition of the money supply that includes time deposits at commercial banks. The effect on the expected money supply of a redistribution of deposits between demand and time accounts in response to relative movements of interest rates is obscured by this choice of definition. An increase in the interest rate paid on time deposits, other rates remaining unchanged, induces a reallocation from time to demand deposits. Changes in the ratio of time to demand deposits introduce a discrepancy between the actual and the expected money supply. This is particularly true in the U.S. after 1917 when the reserve requirement ratios for the two types of deposits were set at different levels. If *Friedman* and *Schwartz* had introduced the ratio of time to demand deposits as a systematic influence on the money supply, i.e., as a proximate determinant, they would most likely have assigned a more important role to interest rates in their analysis of the money supply.

Second, neither the arithmetic treatment of equation (5) nor the discussion of proximate determinants carefully distinguishes between borrowed and un­borrowed reserves. The authors correctly note that most of the changes in the monetary base or high powered money, \( B \), have been relatively exogenous, either controlled by the monetary authority or dependent on prior events that generated specie flows. But after 1914 one portion of the base, borrowed reserves, is an endogenous variable, dependent on the decisions of the banks particularly their response to interest rates. Neglect of the distinction between borrowed and un­borrowed reserves permits the authors to neglect another channel by which interest rates influence the money supply.
Third, the authors' remaining proximate determinants, \( r \) and \( k \), are described as primarily endogenous variables. The weighted average reserve ratio, \( r \), is made dependent on interest rates, panics, legal reserve requirements, the probability of currency drains, and other factors\(^1\). The currency ratio, \( k \), is said to be affected by relative prices in the form of interest rates, service charges on checking deposits, possibly also by the composition of deposits, by income and other endogenous variables. Still other influences are discussed when they are considered relevant\(^2\). But these influences on \( r \) and \( k \), though discussed from time to time, are given no long-run or persistent role in the authors' discussion. No doubt some of the factors mentioned above do not have a persistent influence. For example, \textit{Friedman} and \textit{Schwartz} note that the introduction of deposit insurance eliminated banking panics from the list of factors affecting the desired short-run currency and reserve ratios. But changes in interest rates or redistributions of deposits between time and demand account continue to influence the magnitude of the response in the money supply to policy changes. Recent experience in the U.S. following changes in Regulation \( Q \) testifies clearly that short-run movements in the money supply remain dependent on the interest rate paid on time deposits and asset reallocations that change the volume of time deposits relative to demand deposits.

The point at issue is not that the authors neglect or fail to consider the factors I have mentioned in their discussion of particular episodes. Neither can it be claimed that they neglect the influence of market interest rates or time deposit rates on the time deposit ratio or of market rates and the discount rate on the banks' desired borrowing ratio. All of these factors are mentioned in one chapter or another. But they are not systematically related to the authors' proximate determinants or included among them. As a result, the reader is uncertain about the role that many of these factors played during periods in which they are not discussed and the authors feel free to omit them from their long-run analysis.

\(^1\) Pp. 785–786. The reserve ratio is also affected by such factors as shifts in the composition of bank deposits between classes of banks and types of deposits as is implied by the description of \( r \) as a weighted average. See, for example, p. 124. Factors such as legal and institutional changes (for example, vault cash requirements) that affect \( r \) are discussed on pp. 208–209 along with lags in adjustment of desired to actual reserves. The latter is an important element in the authors' explanation of the ratio for the 'thirties along with confidence in the willingness of the Federal Reserve to lend reserves to meet deficiencies (pp. 534–543).

\(^2\) See p. 787. For each half-cycle, there is a discussion of \( r \), \( k \), and \( B \) indicating the specific factors that the authors regard as the primary influences on the relative costs of holding money in various forms. For \( k \) these are primarily the factors emphasized by \textit{Phillip Cagan}, "The Demand for Currency Relative to the Total Money Supply", Journal of Political Economy (1958).
We are left with the monetary base, the currency ratio and the reserve ratio as the (principal) proximate determinants and a money supply hypothesis that differs little from an “arithmetical” description. The authors’ approach to money supply theory – particularly the ad hoc character of the explanation offered for particular periods – is all the more puzzling in view of their insistence on a single explanation of the demand for money that is applicable to each sub-period. Moreover, the denial of a persistent role to interest rates in their money supply theory has important implications for their discussion of the behavior of prices and income, as will be shown below.

Velocity and the Demand for Money

The discussion of demand for money or velocity departs from much recent work. Friedman and Schwartz indicate at different places several variables that influence the movement of velocity. Among these, real income, the costs of and yields from holding money, “expectations about the degree of future economic stability”, uncertainty about the monetary standard, bank failures, the development of money substitutes, and others receive attention. A separate chapter (12) discusses the influence of many of these variables on the movement of velocity. The chapter follows, but expands upon, an earlier paper by Friedman where two concepts of velocity – measured and permanent – are distinguished and where per capita permanent real income is suggested as the principal determinant of permanent velocity.

Permanent velocity is defined as the ratio of permanent income to the stock of money, while measured velocity is the traditional variable, the ratio of measured income to money supply. Over long periods of time, e.g., decades, the two concepts are closely related. Permanent income is a measure of expected income, and permanent velocity may be regarded as the long-run expected value of velocity – the value that would be obtained if measured and expected or permanent income were equal. During business cycles, the authors contend, and the evidence suggests, that measured velocity rises in the upswing and falls in the downswing by more than permanent velocity.

In the past there has been much discussion about two related points: (1) the now time honored issue of whether or not there has been a declining

1 It is noteworthy that the three proximate determinants are the same elements that Pigou stressed many years ago. Cf. A. C. Pigou, “The Value of Money”, Quarterly Journal of Economics (1917), reprinted in F. Lutz and L. Mints (eds.), Readings in Monetary Theory (Philadelphia: Blakiston, 1951). The authors’ treatment differs from Pigou’s.

trend in velocity, and (2) the variables that enter as arguments in the velocity function, measured or permanent. The first issue is largely a dispute about the definition of money balances. If time deposits are excluded from the definition of money, there is no apparent trend in velocity for the twentieth century as a whole. The part of the second dispute that will be discussed here is the importance of interest rates in the demand function for money or velocity equation.

Much of the authors’ discussion in Chapter 12 is about the postwar movement of measured velocity. According to the permanent income hypothesis, the increase in per capita permanent real income should have lowered velocity during the fifties; i.e., it should have resulted in a secular movement opposite to the rise observed. The authors correctly conclude that the prolonged increase in velocity in the postwar period cannot be explained without amending their long-run hypothesis.

A number of alternatives are proposed as possible explanations of the discrepancy between actual movement of postwar velocity and the movement expected on the basis of the authors’ permanent income hypothesis. The principal alternatives include the effects of: (1) rising interest rates; (2) expectations of rising prices; (3) the growth of financial intermediaries; (4) the combined effect of the first three factors; and (5) changed expectations about future economic stability. Friedman and Schwartz reject each of the first four and conclude: “...changing expectations about economic stability seem at the moment a more plausible explanation of postwar movements in the velocity of money than any of the other factors we have examined.” Of principal interest in the light of recent studies is the authors’ rejection of interest rates as an important influence in the explanation of the movement of velocity. Space does not permit a detailed discussion of their argument here or the presentation of a counter-argument or evidence. I will confine my remarks to three simple points, one of which the authors partially acknowledge.

First, Friedman and Schwartz rest part of their case on the reasons for choosing between per capita permanent real income and interest rates. A finding that

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3 Ibid., p. 675.

4 “If we put together the items considered to this point, it seems unlikely that, even all together, they can account for as much as half of the 45 per cent postwar rise in the velocity of money. Perhaps the most important factor is the postwar reaction to wartime accumulation induced by the unavailability of durable goods. The other factors—the rise in interest rates, shifts in price expectations, and improvement in quality of saving and loan association shares—seem of much less importance.” Ibid. p. 672.

5 Friedman and Schwartz write: “If a single explanation is to be used for the whole
interest rates alone do not explain movements in the authors' measure of velocity is not sufficient grounds for dismissing them all but completely. The influence of interest rates on velocity when real income or wealth is held constant is far from negligible (see references cited in footnote 2). This argument is a weak ground for dismissing the relation of interest rates to velocity or the demand for money.

Second, the definition of money as the sum of currency plus total deposits rather than as the sum of currency plus demand deposits reduces the interest elasticity of the demand function as has been shown elsewhere. The authors defend their choice of definition largely on the grounds that they are interested in developing a series for "money" that will extend back to 1867. They argue with some force that demand and time deposits cannot be separated reliably prior to 1917 and perhaps later. And they recognize (p. 652 and elsewhere) that their choice of definition may imply the lower interest elasticity that they observe. But Friedman and Schwartz dismiss this point since they argue that interest rates alone cannot explain the movement in velocity for the period since 1867 for either definition of money.

Third, the authors point to two particular periods as the principal examples of movements in velocity that cannot be explained solely in terms of interest rates. The years chosen are 1880–1914 and 1942–1946. In some previous work, I have shown that most of the fall in velocity from 1880–1900 and for the subsequent period can be explained in terms of interest rates and real wealth using the Friedman-Schwartz definition of money.

Further, I doubt that we should expect to explain the fall in velocity from 1942–1946 solely in terms of market interest rates on financial assets. While the movements of interest rates on financial assets generally will be related to the movements of rates of return on a variety of non-financial assets, it is most unlikely that this relation will prevail in a period during which bond prices are pegged, most commodity prices are controlled and resources are allocated by government decisions.

period, then whether the analysis is concerned with currency plus demand deposits adjusted or with our concept of money, the movements in income and interest rates alone cannot explain the postwar rise in velocity. The postwar rise in both velocities is contrary to the first, and more rapid than the other variable alone could have been expected to produce." Ibid., p. 655. Italics added. See also p. 644 where the authors present their argument for interest rates as a determinant of the demand for money.

1 Ibid., p. 657.
3 See previous footnote of this review.
If the bond rate is taken as the measure of asset yields, should velocity be related to bond rates during periods of wartime price control? Or should we expect that velocity will be low relative to what would be inferred from quoted market interest rates? Wartime regulations reduced the expected return to investment representing real capital used for the production or distribution of many consumer goods. The excess profits tax and high wartime tax rates on earned income reduced the net return to investment in other non-financial assets. The fact that the percentage increase in velocity exceeded the amount expected to result from the percentage increase in interest rates in 1946 to 1951 – when bond rates remained controlled and commodity prices were not controlled – supports my alternative interpretation. If velocity responds to a broad spectrum of asset returns that are summarized in the term interest rates, the fall in velocity from 1942–1946 and the rise from 1946–1951 do not appear to reject the long series of observations suggesting that velocity and interest rates are closely related. Moreover, both rates and velocity fell from 1942–1946. The inconsistency in this period – if there is one – is in the magnitude of the response in velocity to a decline in interest rates, not in the direction.

The empirical case for excluding interest rates from the empirical velocity relation is rather weak. Moreover, the authors do not provide a convincing alternative explanation of the cyclical and secular movement in velocity from 1867 to 1960. The postwar rise in velocity is clearly troublesome to them; other periods are troublesome also. These problems are acknowledged and discussed by the authors. But they tentatively conclude that variations in their measure of velocity are largely independent of interest rate changes, and are explained by the movement of real permanent income and changing expectations about future economic stability.

The resulting velocity relation is shown in equation (6)

$$V_p = a \left( \frac{Y_p}{N} \right)^b S \quad (0 > b > -1), \quad (6)$$

where $V_p$ is permanent velocity, the ratio of permanent income to the money

\footnote{This interpretation is based on the quotation reproduced earlier in the text of this review. See the text at footnote 3, p. 410. The interpretation is supported by other statements that the authors make, including the following: "As a consequence we are inclined to regard them [interest rate movements and the growth of money substitutes] as explaining at best only a minor part of the postwar movement and to accept instead an alternative interpretation that emphasizes ... changed expectations ... about the likely degree of future economic stability." (Friedman and Schwartz, op. cit., p. 645. Italics added.) The text of the chapter makes clear that they are accepting changed expectations of future economic stability as a supplement to per capita real permanent income and not as an alternative.}

412
stock (as defined by Friedman and Schwartz), the permanent income, \( y_p \) is real permanent income, \( S \) is a measure of expectations about economic stability, and \( N \) is the population.

The emphasis that the authors tentatively assign to the behavior of \( S \) as an explanation of the discrepancy between measured and permanent velocity is difficult to accept. As noted earlier, Friedman and Schwartz set out to explain why measured velocity, \( V \), increased during the postwar period while the value of permanent velocity computed from the long-run relation of \( V_p \) to \( y_p/N \) fell. The tentative attempt to interpret the growing discrepancy as the influence of \( S \) on permanent velocity seems to require either that the effect of \( S \) increased during the period or that expectations of future stability continued to improve. Friedman and Schwartz choose the latter interpretation. But it is difficult to reconcile the notion of continued increase in expectations of stability, rising \( S \), with the increasing talk about inflation in the middle 'fifties and about lagging growth rates at the end of the decade. While this does not rule out the influence of \( S \), it seems to require an increased effect of \( S \) — rather than the effect of increased \( S \) — as the principal influence on permanent velocity. If this is the case, the authors' argument will not succeed in producing the uniform explanation that they seek. A decreased influence of \( y_p/N \) or an increased effect of interest rates would have accounted for the discrepancy just as well.

These doubts are reinforced when the discrepancy that must be explained by rising \( S \) is examined. If the public expects and experiences smaller fluctuations in income, the difference between aggregate permanent income and aggregate measured income in any year is reduced, and the ratio of measured to permanent income \( (Y/Y_p) \) is closer to unity. Since

\[
V_p \frac{Y}{Y_p} = V,
\]

the difference between \( V_p \) and \( V \) should have been smaller during the postwar period than during earlier periods, rather than larger, if Friedman and Schwartz are correct about the increase in economic stability. To explain the discrepancy between measured and permanent velocity in terms of \( S \) means that the positive effect of \( S \) on permanent velocity must have been large enough not only to reverse the negative influence on \( V_p \) of growing permanent income, but also to make the difference between \( V_p \) and \( V \) smaller than the average for earlier decades.

Finally, if \( S \) has an important influence on the demand for money at times, it is likely to affect consumption also. The introduction of \( S \) has implications for Friedman's earlier analysis of the consumption function as well as for the macro-economic theory that is implicit in A Monetary History of the United States. Some of the macro implications are discussed in the following section. The effects on consumption cannot be considered without going beyond the subject matter of A Monetary History.
The Long-Run Relation of Money to Prices and Income

The money supply and demand equations above are part of a macro-economic theory that underlies the authors’ analysis of price and income changes in the U.S. To complete the theory, some additional equations must be introduced, and the authors’ short- and long-run analysis must be separated. In this section, I present the long-run macro-economic model, that emerged from my reading of the book, and some of its implications. My interpretation of the authors’ analysis of business cycles is presented in the following section.

One important distinction between Friedman and Schwartz' short- and long-run analysis was discussed earlier. Two of the “proximate determinants” of the money supply, \( r \) and \( \kappa \), may diverge from their long-run expected values under the influence of short-run changes in interest rates or other variables. In the long-run, expected and actual \( r \) and \( \kappa \) coincide, and there is little or no discrepancy between the expected and actual money supply resulting from a given value of the monetary base. Since the base has been a relatively exogenous variable through most of our history and since authors assume that \( r \) and \( \kappa \) are known, the determination of the money supply is reduced to a problem in arithmetic.

A long-run theory of nominal income, the quantity theory of money in the Friedman and Schwartz form, is obtained by combining the money supply with equation (6),

\[
MV_p = Y_p, \tag{7}
\]

where \( Y_p \) is the nominal value of permanent or expected income. Since \( V_p \) depends in the long-run on real permanent income, \( y_p \), population, \( N \), and perhaps on expectations about economic stability, \( S \), information must be provided about these variables if equation (7) is used to predict \( Y_p \). The stock of population may be taken as exogenous. The value of real permanent income can be obtained or approximated as an exponentially weighted average of past income, a procedure that Friedman has used in *A Theory of the Consumption Function* and elsewhere. The relation is stated in equation (8).

\[
y_p(T) = \beta \int_{-\infty}^{T} e^{\beta(T-t)} y_t \, dt. \tag{8}
\]

The expectations variable, \( S \), does not seem to have an important long-run influence on the movement of \( V_p \). Friedman and Schwartz seem to suggest that \( S \) had an important influence on the movement of permanent velocity during the 'fifties and a smaller influence at other times. They suggest that the path of \( V_p \) prior to 1951 or 1954 can be explained quite well without \( S \) and conjecture

\[1\] Friedman and Schwartz, op. cit., pp. 642-643.
that the influence of $S$ may have diminished by 1960\(^1\). If this interpretation is correct, the long-run movement of $Y_p$ can generally be approximated quite well without information about $S$.

With a value of $\beta = .4$ obtained by iterative procedures as a part of Friedman's estimate of the consumption function, the value of permanent income in the current ($T$) period can be computed from equation (8). The only missing information is the value of current measured income. For most years, measured income can be inferred from the growth rate of income and the level of income in the previous year. The error will be small, approximately equal to transitory income and of little long-run significance. Moreover, the effect of the error on the estimate of permanent velocity is damped by both the value of $\beta = .4$ and the value of $b$ in the velocity equation. The error in a particular year usually can be further reduced by knowledge of current conditions, for example, by estimating that the change in measured income will be above or below trend.

Once real permanent income has been used to estimate permanent velocity and nominal permanent income has been obtained as the product of the money supply and $V_p$, the price level, $P$, can be obtained, also:

\[
P = \frac{Y_p}{\gamma_p}.
\]

Equations (5) to (9) thus form a system that determines $V_p$, $\gamma_p$, $Y_p$, $P$ and $M$ to a first approximation, given the values of $B$, $r$, $k$, $N$ and lagged real income, $\gamma$.

Interest rates, the yield on real capital and similar variables representing the operation of relative prices on portfolio allocations do not appear in the long-run model. If they were included, some additional equations would have to be introduced and Friedman and Schwartz would be forced to develop their analysis of the real sector of the economy and of the determination of real income in more detail. The long-run theory is primarily a theory of the price level and not a theory of the real sector.

Before turning to the authors' analysis of fluctuations in income (business cycles), it is useful to note two consequences of the long-run model. The first concerns the long-run theory, as I have presented it. The second is the policy conclusion that seems to emerge from the theory.

It should be clear from remarks above, and in any case it is abundantly clear to the reader of the book, that Friedman and Schwartz do not deny that interest rates and relative price changes have an influence on the quantity of money demanded and supplied and on the real magnitudes. On the contrary, the authors' discussion repeatedly mentions the operation of interest rates and other relative prices as determinants of both monetary and real variables. It would be misleading, indeed wrong, to suggest the opposite since the authors' skill in

\(^1\) Ibid., p. 675.
applying relative price theory to economic history is one of the most interesting
and exciting features of the book.

Friedman and Schwartz do not deny that changes in interest rates and prices
have an important effect on the pace of economic activity in the short-run, but
their long-run theory\(^1\) clearly does. Can these conflicting views be reconciled so
that the long-run model can be applied to short-run fluctuations in income? If I
judge correctly, the authors suggest that the long-run model is applicable to
periods that are not subject to marked instability in the growth rate of the
money supply\(^2\). The following arguments support their position: (1) The empirical
equations hold subject to error. The omitted variables will influence the size
of the errors if the model is used in any particular year. (2) Changes in expecta-
tions may modify the value of \(\beta\) to be used in the estimate of permanent income
applicable to a particular year. Transitory and permanent income are approxi-
mations; the value of \(\beta = 0.4\) is not presented as a constant imposed on the
economic system. It is obtained by numerical methods and may change slightly
from year to year if there are minor changes in expectations. (3) The value
of transitory income in the current year is not known a priori. This introduces
an additional source of error into the level of prices, etc., estimated from the
model.

Moreover, the five equations go a long way. Lagged real income gives the
value of real permanent income, and the money equations determine the price
level to a first approximation once real permanent real income has been com-
puted. To obtain more a variety of interest rates, yields, prices, etc., are required.
In the judgment of the authors, two or three more equations for investment,
consumption, and «the» interest rate will not do. They see the real sector as far
too complicated to be approximated by such simple hypotheses. For them there
is more than one kind of investment, more than one interest rate that has to be
determined. Even if all of the relations could be formulated consistently, where
would one get the data to estimate the parameters?

Besides, the consumption function provides much of the missing information
for both short- and long-run analysis. From permanent income and the con-
sumption function the amount of permanent income not consumed in the
current year can be estimated; this is the (approximate) aggregate value of the
addition to wealth, the amount saved by the community up to the transitory

\(^1\) An additional point should be noted in connection with the absence of interest rates
in the empirical demand (or supply equations) for money. Contrary to much recent mo-
netary theory, e.g., Patinkin and Metzler, an open market exchange of money for
bonds has no permanent effect on interest rates. Interest rates (or “the interest rate”) remain real phenomena if my interpretation of the Friedman–Schwartz long-run model
is correct.

\(^2\) Friedman and Schwartz, op. cit., p. 678.
The equality of saving and investment implies that the amount saved is an estimate of the amount invested by business and households up to the transitory component in saving. Again, one equation does a large part of the work of many and, if my interpretation of the authors’ position is correct, obviates (for them) the need for additional equations expressing the households’ demand for each of a number of durable goods, business’ demand for plant and equipment, etc. If we are willing to ignore «transitory» changes, there is little more that we need to know for the prediction of aggregate real income and the price level in most years.

Two considerations prevent me from accepting this conclusion. First, transitory income is only another name for the business cycle. A part of past transitory income is, of course, included in current permanent income. But the differences between real income and permanent income have at times been quite large. Like most other economists, I would like to know more about the factors producing cyclical changes in real income or in the magnitude called transitory income. Second, the errors in one of the important equations, the money demand function, are at times large in periods that show no marked change in the growth rate of the money supply. Hence the model is capable of generating relatively large errors. For example, at the turn of the century, the permanent velocity hypothesis consistently overestimated velocity and consequently overestimated the level of nominal permanent income and of the price level. For the years 1910–1940 and 1951–1958, the model summarized in equations (5) – (9) would have made an average absolute error of 7 per cent in the prediction of nominal permanent income or the price level. Incorporating measures of interest rates and transitory income reduces the predictive error to 4.6 per cent, a substantial improvement ².

The data seem to tell us that we cannot neglect the transitory component in income if we are to understand the cyclical behavior of income and prices. But to obtain more information about the behavior of transitory income, we must expand the analysis of the real sector and extend what I have interpreted to be the Friedman and Schwartz model to include interest rates and other variables.

When we look at the model as a guide to policy, we do not need to look far. By operating on the monetary base, we can, given \( r \) and \( k \), make the money supply what we want it to be. But despite the “arithmetical” method of determining the money supply by combining \( r \), \( k \) and \( B \), it is quite clear from the authors’ ¹

¹ In a sense the role of the consumption function is parenthetical to the book that is under discussion since the authors do not dwell on the consumption function. But I believe that the point is germane to the theory that underlies A Monetary History for reasons suggested in the text.

discussion that $r$ and $k$ have lives of their own. Hence neither the reader nor the authors can be certain that by controlling $B$ they will in fact get the money supply in the short-run that will keep income rising and prices stable. Again, the problem is easily resolved. Just look at the money supply. Since lagged income is the principal determinant of permanent income, there is nothing else in equations (5) – (9) that is a better guide to price stability. In fact, there is nothing else for a policy-maker to change.

The only open policy question left to us is whether we set the growth rate of the money stock once and for all time, control it on a daily basis, or choose some intermediate monetary policy. We have no better lever and, for a first approximation, we don’t need any for long-run policies. If the reader of A Monetary History concludes that errors in policy at crucial episodes in history suggest that rules would have done better than judgment, Friedman will not be unhappy. But that is neither a central argument of the book nor, I would say, a central element of the theory. What is of crucial importance is the notion that steady monetary growth will damp the fluctuations in transitory income and reduce the need for better information about the real sector. It is this notion that is the core of the authors’ discussion of major depressions and to a lesser extent of major inflations.

The Analysis of Economic Instability

Most readers will find the descriptions of periods of major depression among the most illuminating in the book. Friedman and Schwartz present a convincing case that substantial decreases in the stock of money or in the growth rate of the money supply are followed by major depressions. For each depression, the authors present a detailed account of the reasons for the decline in the money supply. The record is one of missed opportunities, hesitant and belated responses, mistakes, and faulty analysis. The authors’ careful appraisal of each of these periods and the assembling of the record for the past century is a major contribution, hopefully one that will help to avoid repetition of the errors.

The core of the authors’ explanation of depression or inflation is that major changes in the growth rate of the money supply produce large short-run changes in the growth rate of real income or prices. During such periods, the long-run model of the previous section is not applicable. The deviation of nominal income from its expected value $(\text{M}\text{V}_p)$ is too large to be assigned to “transitory income” and remain unanalyzed.

The theory outlined in the previous section is applicable to inflationary episodes, however, if one modification is introduced: The demand for money, or velocity, becomes significantly affected by the expected rate of change of prices$^1$.

The expectation of price changes, obtained by a procedure analogous to equation (8), raises velocity. With the value of real permanent income obtained from equation (8), the money supply and permanent velocity again determine nominal permanent income and the price level to a first approximation. Deviations or errors are assigned to the transitory component in a manner similar to the one used in the discussion of mild recessions.

During deep depressions, the difference between expected and measured income and the variation of measured income are too large to be ascribed to “transitory” changes. The authors introduce the empirical generalization that large declines in the money supply or its growth rate induce large declines in real income and industrial production. Sudden increases in the rediscount rate in 1920 and 1931 and the doubling of reserve requirements in 1936–1937 are presented as examples of policy changes that caused larger contractions in the money supply than could be expected from the long-run relation of the money supply to the monetary base.

Many of the variables that were excluded from the long-run model and given little importance in the discussion of income changes during mild recessions acquire a prominent position in the authors’ explanation of deep depressions. For example, reductions in interest rates raised the banks’ desired short-run reserve position. The large substantial increase in the rediscount rate in 1920 and again in 1931 is described in terms of its effect on market interest rates and banks’ desired reserve positions. Liquidation forced upon the banking system by untimely and inappropriate policies raised the public’s desired currency ratio, produced a further increase in the banks’ desired short-run reserve position and intensified the pressure on the banking system. The short-run response of the money supply to the monetary base is reduced or made negative by the influence on the desired reserve and currency ratios of interest rates, rediscount rate, currency drains, bank failures, etc.

Contemporary explanations of the behavior of money during deep depressions most often take the supply of money as given or determined by the public’s desire to borrow and discuss the influence of an increased demand for money or reduced velocity on the reduction in real income. Friedman and Schwartz also discuss the behavior of velocity and the influences acting upon it. But their discussion places the money supply, rather than the demand, at the center of the forces causing or accelerating the decline in real income. This approach is supported by such a large body of evidence that economists may become convinced that money supply theory is a part of monetary theory that is worthy of more attention than it has received.

Unfortunately, Friedman and Schwartz did not choose to lend their weight to the development of a clearly specified theory of the money supply. By exclud-

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1 For examples see pp. 231–238, p. 307, pp. 682 and ff.
ing or minimizing the influence in non-depression periods of many of the variables that have a dominant position in their discussion of deep depressions, they produced a rather ad hoc analysis of the factors influencing the money supply. Since it is the deviation of actual from expected money supply that has a decisive influence on the movement of real income during deep depressions, their explanation of depressions retains the ad hoc character of their analysis of the money supply.

Conclusion

This then is the monetary theory that Friedman and Schwartz use to explain monetary history. It is relatively simple, and in the hands of the authors it goes a long way. The Monetary History presents a strong case that the theory describes and predicts reasonably well. As I have stressed repeatedly, the authors never claim or seek perfection. They seem to tell us only that if the goal of policy is to maintain full employment real income and stable prices, their theory works reasonably well for long-run analysis and for mild recessions. With a large expenditure of effort, the most we can hope to do is reduce the stochastic (error) term in the demand equation for money and get a better explanation of the movements of current transitory income.

Had the authors systematically incorporated interest rates or asset yields as a determinant of velocity or of the money supply, they would have been forced to do what they have otherwise avoided doing, develop a more extensive analysis of the real system to supplement their treatment of the monetary sector. It is for this reason that Friedman and Schwartz cannot have interest rates as an important determinant of the demand for money and generally reduce the determination of the money supply to a problem in arithmetic rather than a result of the interaction of interest rates, policy and other variables. The appealing simplicity of their approach vanishes if their assumptions are rejected.

Why do many economists refuse to accept this model? It seems correct to say that there is not yet an alternative model that explains as much of the economic history of the past century. In part this is necessarily true since data are not available for many of the variables that enter the larger, more complicated models. But even in periods for which the alternative models can be compared, the power and simplicity of the Friedman and Schwartz quantity theory have great appeal. I will suggest four partly overlapping problems of their approach as answers to the question above.

There is, first, the puzzling and unexplained series of observations cast up by monetary history, many of which the authors discuss in detail. One basic problem is the failure of the movement in per capita permanent income to describe or
predict the movement in permanent velocity in the past decade and in some other periods as I have noted above. A theory that takes the demand function for money as the central relation must perforce rely heavily on the adequacy of that relation. The evidence compels us to abandon per capita permanent income as the principal explanation of permanent velocity. By doing so, it forces us to expand the set of equations and to incorporate interest rates, i.e., to analyze the real sector. This and other examples that might be provided suggest the need for an alternative or expanded theory. Indeed it is the task of good theory to raise problems that cannot be explained with existing tools and thus to point us in the direction that will lead to a more powerful theory. That the authors have been able to do this in so many places is a strength as well as a weakness of their book.

A second problem is the explanation of the accumulation of excess reserves in the banking system of the ’thirties. As the reviews by Professors Brunner and Tobin suggest, the authors’ explanation is not sufficiently persuasive to remove doubts about the power of monetary policy. Can those doubts be removed without an expanded theory of the money supply and bank credit applicable to the demand for reserves or for earning assets by banks and the supply of earning assets to banks for the period of the ’thirties as well as for other years? A more adequate theory will doubtless cause us to incorporate interest rates and other factors among the variables to be explained and thus force the development of a more extensive analysis of the real sector.

Third, there is the role assigned to transitory income. It is not sufficient to treat mild recessions up to the value of transitory income if, as I have argued, transitory income is another name for the business cycle. The positive, short-term association between measured velocity and the index of transitory income is a large part of the cyclical problem that remains to be explained. In sum, we need a theory that accurately predicts measured real income in the current period before we have an adequate approximation to a theory of the business cycle.

Fourth, the authors’ theory is an incomplete guide to policy. Policymakers often choose to use policies other than monetary policy. Perhaps the policymakers will refuse to permit exchange rate flexibility and will fail to expand the stock of money as a consequence. Must we accept the resulting deflation and/or unemployment? The results of the past few years suggest that many economists would want to suggest an alternative policy, and I am hopeful that they will want to know more than they currently know about the result of doing so. For example, they may wish to know the effect on one or more asset yields of financing a budget deficit partly with money and partly with debt. The Friedman and Schwartz theory makes the effect of expansionary fiscal policy financed by increased debt one of the factors influencing transitory income. I suspect that

economists will not be satisfied with a theory that tells them no more than that they may have succeeded in increasing transitory income to the extent they financed a deficit with debt rather than money.

The business of the social scientist is to construct models or theories that predict. But prediction is a means and not an end of science. We want prediction, that is confirmation, so that we can understand and influence or control what is happening in the social space. Professor Friedman and Mrs. Schwartz have taken us a long way toward acquiring that understanding, and we will gain greatly from their outstanding contribution. But those of us who are not convinced that much of the business cycle must remain hidden in the movement of transitory income will not be content. Much of our future work will benefit from a careful reading of their splendid contribution and the numerous problems and suggestions that make the reading of their book both a pleasure and a worthwhile investment for all economists.