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MONEY AND INCOME: POST HOC ERGO PROPTER HOC? *

JAMES TOBIN

Milton Friedman asserts that changes in the supply of money M (defined to include time deposits) are the principal cause of changes in money income Y. In his less guarded and more popular expositions, he comes close to asserting that they are the unique cause.[1] In support of this position Friedman and his associates and followers have marshaled an imposing volume of evidence, of several kinds.

Historical case studies are one kind of evidence. For example, in their monumental *Monetary History of the United States 1867–1960*,[2] Friedman and Anna Schwartz carefully analyze and interpret the role of money and monetary policy in the important episodes of American economic history since the Civil War. Summary regressions of time series of economic aggregates are a second type of evidence. Presumed effects are simply regressed on presumed causes; the single equations estimated are something like the econometrician's "reduced forms." In a study with David Meiselman,[3] Friedman concluded that his monetary explanation of variations in money income fits the data better than a simple Keynesian multiplier model. More recent studies in the same vein claim that monetary policy does better than fiscal policy in explaining postwar fluctuations of money income.[4]

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1. See, for example, his column in *Newsweek*, Jan. 30, 1967, p. 86, "Higher Taxes? No." He says, "To have a significant impact on the economy, a tax increase must somehow affect monetary policy — the quantity of money and its rate of growth. . . . The Federal Reserve can increase the quantity of money by precisely the same amount with or without a tax rise. The tax reduction of 1964 . . . encouraged the Fed to follow a more expansionary policy. This monetary expansion explains the long-continued economic expansion. And it is the turnabout in monetary policy since April 1966 that explains the growing signs of recession."

2. National Bureau of Economic Research, *Studies in Business Cycles*, No. 12 (Princeton: Princeton University Press, 1963).

3. Friedman and David Meiselman, "The Relative Stability of Monetary Velocity and the Investment Multiplier in the United States 1897–1958," Commission on Money and Credit, *Stabilization Policies* (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1963), 165–268.

4. Leonall Anderson and Jerry Jordan, "Monetary and Fiscal Actions: A

A third type of evidence relates to timing, specifically to leads and lags at cyclical turning points. Much of the work of Friedman and his associates at the National Bureau of Economic Research has been devoted to this subject.[5] Turning points in the rate of change of money supply, M',[6] show a long lead, and turning points in the money stock, M, itself (relative to trend) a shorter lead, over turning points in money income, Y. A great deal of the popular and semiprofessional appeal of the modern quantity theory can be attributed to these often repeated facts.

However, the relevance of timing evidence has been seriously questioned.[7] Friedman himself says, "These regular and sizable leads of the money series are themselves suggestive of an influence running from money to business but they are by no means decisive."[8] The apparent leads may "really" be lagged responses — either positive or negative ("inverted") — of money to previous changes in business activity. Friedman cautiously rejects this possibility. He finds that the M series conforms more closely to the NBER reference cycle on a positive basis with money leading than on an inverted basis with money lagging, and he regards the business-money causal nexus as very likely to be inverted. Having satisfied himself that the dominant association of M' and business activity is positive, Friedman concludes, ". . . it is not easy to rationalize positive conformity with a lead as reflecting supply response,"[9] i.e., response of the supply of money to changes in business activity.

The purpose of the present paper is to spell out the lead-lag timing implications of alternative theoretical models of the relation

Test of their Relative Importance in Economic Stabilization," *Federal Reserve Bank of St. Louis Review* (Nov. 1968), 11–24.

5. See Friedman, "The Lag in the Effect of Monetary Policy," *Journal of Political Economy*, LXIX (Oct. 1961), 447–66; Friedman and Schwartz, "Money and Business Cycles," *Review of Economics and Statistics*, Feb. 1963 Supplement, 32–64; Friedman, "The Monetary Studies of the National Bureau," *Annual Report of the National Bureau of Economic Research 1964*.

6. Throughout this paper x' will denote the time derivative of x, $\frac{dx}{dt}$, and x'' the second derivative of x with respect to time.

7. By, among others, J. Kareken and R. Solow, "Lags in Monetary Policy," Commission on Money and Credit, *Stabilization Policies* (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1963), 14–25. They pointed out that a rate of change like M' will generally lead a level like Y, in the manner that a cosine series "leads" a sine series. Friedman replied in "The Lag in the Effect of Monetary Policy," *loc. cit.*, that both M' and Y have the dimension of a flow and that in any case he finds M' leading Y' and trend-corrected M leading Y. Kareken and Solow found little lead, if any, of M' over the rate of change of the industrial production index, but they should have used a monetary rather than a real measure of business activity.

8. In "The Monetary Studies of the National Bureau," *loc cit.*, 13.

9. *Ibid.*, 14.

between money and money income. In one model, a version of the ultra-Keynesian theory that Friedman is so often attacking, monetary developments are just a sideshow to the main events. In the other model, one of Friedman's own, monetary developments are of decisive causal significance. What kinds of observed relations between money and money income and their rates of change do the opposing models generate? Do they imply different lead-lag patterns?

In the ultra-Keynesian model, changes in the money supply are a passive response to income changes generated, via the multiplier mechanism, by autonomous investment and government expenditure. This makes it possible to see what kinds of observations of money stock M and its rate of change M' would be generated in an ultra-Keynesian world. These can then be compared with the observations that would be generated by a Friedman economy. Here it is necessary to express Friedman's hypothesis with more precision and simplicity than it is usually expounded. However, this can be done with the help of the model of the demand for money set forth in his article with Anna Schwartz, "Money and Business Cycles."[1]

I hasten to say that I do not believe the ultra-Keynesian model to be exhibited (nor would Keynes), any more than I believe Friedman's. I do think, nevertheless, that the exercise points up the dangers of accepting timing evidence as empirical proof of propositions about causation.[2] I shall show that the ultra-Keynesian model — in which money has no causal importance whatever — throws up observations which a superficial believer in *post hoc ergo propter hoc* would regard as more favorable to the idea that money is causally important than does Friedman's own model. What is even more striking and surprising is that the ultra-Keynesian model implies cyclical timing patterns just like the empirical patterns that Friedman reports, while the Friedman model does not.

An Ultra-Keynesian Model

The ultra-Keynesian multiplier model has

$$Y = m(G + K') \tag{1}$$

where Y is net national product, G is the current rate of government expenditure, and K' is net capital accumulation, all in nominal units.

1. *Loc. cit.*
2. The same methodological lesson is given by the simulations of more complicated models in William C. Brainard and James Tobin, "Pitfalls of Financial Model Building," *American Economic Review* (*Papers and Proceedings*), May 1968, 19–122.

(The division of cyclical fluctuations in income between real output and prices is inessential to the argument of the paper and is ignored throughout.) The multiplier m is derived routinely from the identity:

Saving + Taxes = Government Expenditure + Net Investment

$$s(1-t)Y + tY = G + K' \tag{2}$$

where s is the marginal propensity to save from income after taxes and t is the constant tax rate (net of transfers). Therefore the multiplier,

$$m = \frac{1}{s(1-t)+t}. \tag{3}$$

The determination of income by equation (2) is illustrated in the familiar textbook diagram, Figure I.

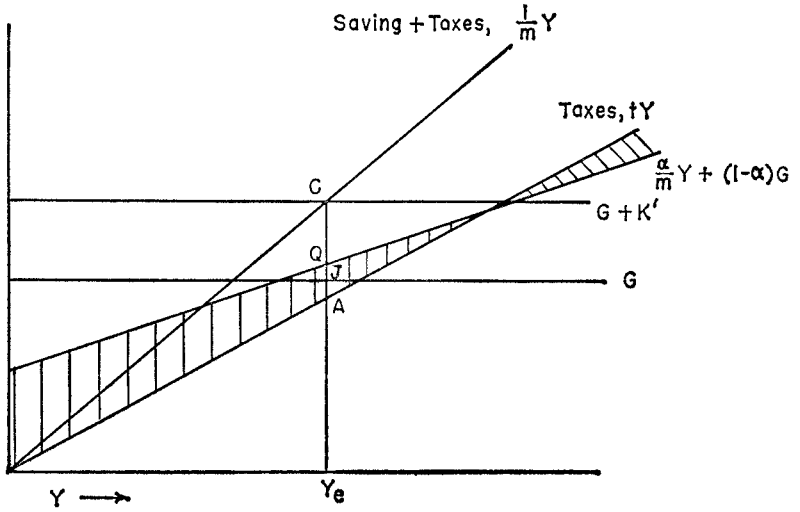

FIGURE I

Private wealth W is the capital stock K plus the government debt D (whether monetized or not), the cumulative total of past deficits, $G - tY$. Saving, the change in private wealth, is

$$s(1-t)Y = W' = K' + G - tY = (K' + G)(1 - tm). \tag{4}$$

In Figure I government deficit is AB, and net capital accumulation is BC.

The public's balance sheet is

$$W = K + D = K - L + M + B \tag{5}$$

where B is the public's holdings of the nonmonetary debt (bonds) of the government, L is the debt of the public to the banking system,

and M is the public's holdings of the monetary liabilities of the government and the banking system. To be consistent with Friedman's model and his empirical findings, M includes time deposits as well as demand deposits.

The portfolio behavior of the public in this ultra-Keynesian world is very primitive. Real investment is autonomous; indeed, exogenous fluctuations in the pace of capital formation are the source of the business cycle. This implies that there are autonomous shifts in the proportions in which the public wishes to allocate its wealth among the available assets. During investment booms, capital becomes more attractive relative to money and bonds; during investment recessions, the reverse occurs.[3] By the same token, borrowing from banks rises in booms and falls in recessions. Specifically, the public's debt to the banking system is taken to be a fixed proportion of the capital stock:

$$L = a\,K \qquad (0 < a < 1). \tag{6}$$

The only portfolio decision left is the allocation of the remainder of the public's net worth — $(W - K + L)$, which is equal to $(D + a\,K)$ — among the two remaining assets, money (currency and bank deposits) and bonds (interest-bearing government debt). This is the choice of Keynesian liquidity preference theory. The demand for money can be written as the sum of two components, an asset demand related to the interest rate and to allocable wealth and a transactions demand proportional to income:

$$M = a_0(r)\ (D + a\,K) + a_1 Y \tag{7}$$

where r is the interest rate on bonds and the derivative $a'_0(r)$ is negative. By subtraction, public demand for bonds is

$$(1 - a_0(r))\ (D + a\,K) - a_1 Y.$$

The main point of the exercise can be made by assuming that the monetary authority provides bank reserves as necessary to keep r constant, so that a_0 is a constant. The monetary system responds to the "needs of trade." With the help of the monetary authority, banks are able and willing to meet the fluctuating demand of their borrowing customers for credit and of their depositors for money. In Friedman's terms, this is a "supply response" with "positive conformity" of money to business activity. It is indeed a response which he regards as all too common in central banking, one for which he has severely criticized the Federal Reserve. If these criticisms are justi-

3. It might seem more Keynesian to let bonds alone bear the brunt of the autonomous shifts to and from capital. But "money" here includes time deposits.

fied, then this endogenous response must have played an important role in generating monetary time series.

The relation among flows corresponding to (7) is

$$(8) \qquad M' = a_0(D' + a\,K') + a_1Y' \\ = a_0(G - tY + a\,K') + a_1Y'.$$

Using (1) converts (8) into

$$(9) \qquad M' = a_0[G(1 - a) + Y(\frac{a}{m} - t)] + a_1Y'.$$

Thus, for given G, M' is a linear function of Y and Y', and these vary in response to autonomous changes in investment K'. The relationship to Y' is, of course, positive. Consider now the relationship to Y. In Figure I, at income level Y_e, D' is represented by AJ. Let JQ equal aJC, the amount of real investment covered by new indebtedness to banks. Then AQ represents $D' + a\,K'$, the quantity which the public divides between accumulations of money and of bonds. Imagine that G is held constant, while K' varies autonomously and carries Y with it. Then the vertical distance through the shaded area, of which AQ is an example, is $D' + a\,K'$. This declines with Y, as illustrated, provided the line through Q has a slope smaller than t, i.e., that a/m is smaller than t. (For example, if the multiplier is 2–1/2 and the tax ratio is 1/5, the loan-to-investment ratio a must be smaller than 1/2.) In this case $D' + a\,K'$ will become negative, as illustrated, at sufficiently high values of Y, where the government budget is in large surplus.

The financial operations of the government and the banks are as follows: The government and the monetary authority divided the increase of debt D' between "high-powered money" and bonds in such manner as to keep the interest rate on target. If we assume no change in currency holdings by the public, the increase M' in money requires an increase of kM', where k is the required reserve ratio, in bank reserves. Banks' loan assets increase by $L' = a\,K'$. The difference $(M'(1 - k) - L')$ the banks allocate between excess reserves and bond holdings, in proportions that depend on the interest rate. Thus the monetary authority provides enough new high-powered money to meet increased reserve requirements and any new demand for excess reserves. The remainder of the increase in public debt D' takes the form of bonds, and it is just enough to satisfy the demands of the banks and the public. This can be seen as follows: The increase in public demand for bonds is $W' + L' - K' - M' = D' + aK' - M'$. The increase in the banks' demand for bonds is $M' - L' - H' = M' - a\,K' - H'$, where H' is the increase in required and

excess reserves. Adding the two together, we see that the increase in demand for bonds is $D' - H'$, just equal to the supply. In short, Walras' law guarantees that if the money market is cleared, the bond market is also cleared.

A dollar increase in government spending has the same effect in raising income and tax receipts as a dollar increase in private investment. Both raise income Y by the multiplier m, and taxes by tm. However, they have different effects on $D' + a K'$ and thus on M'. An increase in government expenditure raises $D' + a K'$ by $1 - tm$; an increase in private investment, by $a - tm$. Since a is less than 1, the demand for money is raised more by an increase of government expenditure. This fact is clear from (9). For given Y, a dollar increase in G (replacing a dollar of K') increases M' by $1 - a$.

A tax cut sufficient to create the same increase in income would entail an even larger rise in $D' + a K'$ and in the demand for additional money. Our ultra-Keynesian would not be surprised to find the money supply rising especially fast in an income expansion propelled by deficit spending. He would not even be surprised if some observers of the accelerated pace of monetary expansion in the wake of a tax cut conclude that monetary rather than fiscal policy caused the boom.[4]

Let us return, however, to a model cycle generated by fluctuation in private investment K', with government expenditure and the tax rate constant. The model abstracts from trends in Y and its components. However, private wealth grows over the model cycle, and this growth is responsible for an upward trend in M. What will be the cyclical behavior of the money supply M and of its rate of change M', in reference to the cycles in money income Y and its rate of change Y'?

There are two components of M', one related to Y and one to Y'. The Y-component has already been discussed. Its relationship to Y is shown in Figure II, as the downward sloping line. Y_{tr} and Y_{pk} are the trough and peak of the cycle. In the illustration, M' for stationary Y does not become negative, even at Y_{pk}. The second or transactions component is simply proportional to Y': $a_1 Y'$ in equation (8) or (9) above. This can be added to Figure II, provided we know the relation of Y' to Y. That relation is illustrated in Figure III, on the assumption that the cycle in K' and Y is a sine wave. The circle, with arrows, shows Y' zero at the trough of Y, Y' at its own peak, Y at its peak with Y' again zero, Y' at its trough, and so on.

4. Note Friedman's comment in *Newsweek* quoted above, note 1, page 301.

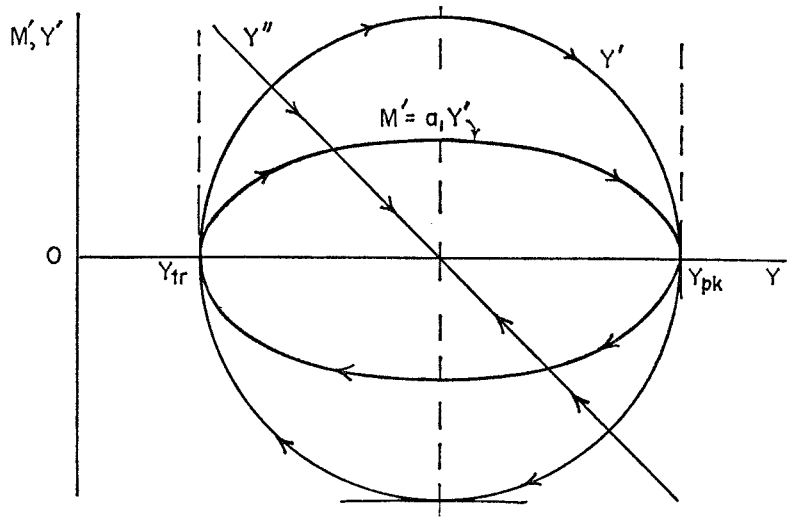

FIGURE II
Ultra-Keynesian Cycle

The ellipse within the circle represents the corresponding cycle in the second component of M'.

In Figure II, this component is added vertically to the line representing the first component. The squashed ellipse in Figure II shows the cycle of M' as income moves from Y_{tr} to Y_{pk} and back. The order of events in the cycle can be read by following the perimeter of the squashed ellipse clockwise. In Figure II there is a brief period of the cycle when M' is negative. Thus M has a late peak and early trough, and grows on balance over the cycle. It can easily be imagined, however, that the ellipse in Figure II lies entirely above the axis, so that M grows continuously but at varying rates. Or, if the first or level component of M' became negative before Y reached its peak, then M would lead Y at the peak as well as at the trough. In any case, it is clear that M' not only has a long lead over Y, more than a quarter of a cycle, but also leads Y'.

The horizontal line through the squashed ellipse represents the average value of M'. The stock of money M, corrected for trend, will reach its peak and trough when actual M' is equal to average M'. These points are also indicated in Figure II. They precede turning points in Y but not in Y'.

It is easy to modify Figure II to allow for a rise in interest rates during expansions of money income and a decline in contractions. In an ultra-Keynesian world this "leaning against the wind" by the

monetary authorities would be irrelevant to stabilization. But it might occur nonetheless, because the monetary authorities mistakenly believe in their own powers or are just operationally conservative in changing the supply of high-powered money, or because they worry about the balance of payments. Anyway, it would be represented in Figure II by a steepening of the central line. This would result in a still longer lead of M' with respect to Y' and Y.

Equation (8) would read

$$(8') \qquad M' = a_0(r)\ (D' + a\,K') + a'_0(r)\ (D + a\,K)r' + a_1Y'.$$

As r rises with Y, the decline in a_0 reinforces the decline in $D' + a\,K'$. Assuming that r' is positively related to Y', and given that $a'_0(r)$ is negative, the second term contributes a negative relation of M' to Y'. This would make the ellipse of Figure III flatter, as well

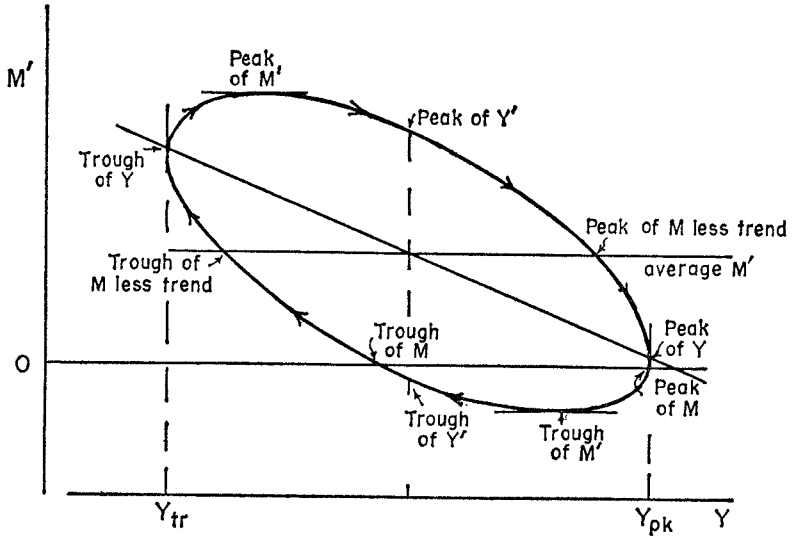

FIGURE III

as distorting its shape. Indeed, it could conceivably reverse the net effect of Y' on M' and therefore reverse the order of events in the cycle. But the central bank surely does not lean against the wind so hard as that, especially in an ultra-Keynesian world.

The results would also be reinforced if a term in Y', with a positive coefficient, were added to the basic demand for money equation (7). The logic of such a term would be that changes in wealth are in the first instance absorbed in cash balances, with more permanent portfolio allocations following later. Thus demand for money

would be especially high when income and saving are rapidly increasing. This, after all, is what one would expect of money as "a temporary abode of purchasing power," to use Professor Friedman's famous phrase.

A Y' term in expression (7) for M means a Y'' term in expression (8) for M'. In a cycle of the type illustrated in Figure III, Y'' is inversely related to Y. Therefore a Y'' component of M' will be high at low levels of Y and low at peak levels. Like the interest rate effect, this will increase the slope of the central line in Figure II and accentuate the lead-lag pattern there depicted.

There is nothing sacred about sine waves, and neither is a sine-curve cycle crucial for the timing pattern shown in Figure II. The reader is invited to experiment with noncircular shapes of the relation of Y' to Y in Figure III. He will find it easy to change the lengths of the lags and leads, and in extreme cases to produce some coincidences and ambiguities. But the essential message of Figure II comes through, provided that M' is related negatively to Y and positively to Y'.

A Friedman Model

I turn now to the cyclical pattern implied by Friedman's own "permanent income" theory of the demand for money. For present purposes this may be expressed as follows:

$$(10) \qquad 1nM = A + \delta\, 1n\, Y^*_p.$$

Here M is the same quantity of money as in the ultra-Keynesian model; Y^*_p is permanent income; δ is the elasticity of the demand for money with respect to permanent income, estimated by Friedman to be of the order of 1.8. Income and permanent income grow secularly at an exponential rate β. As above, we abstract from this trend of income and consider the deviations from trend, Y_p and Y. Since $1n\, Y_p = 1n\, Y^*_p - \beta t - C$, equation (10) can be restated as

$$(11) \qquad 1n\, M = B + \delta\, 1n\, Y_p + \delta\beta t.$$

For rates of change, (11) implies

$$(12) \qquad M'/M = \delta(Y'_p/Y_p) + \delta\beta.$$

Permanent income, corrected for trend, is a weighted geometric average of current and past actual incomes, also corrected for trend, with the weights receding exponentially. Thus when actual and permanent income differ, the public changes its estimate of permanent income by some fraction of their relative difference. Specifically,

$$(13) \qquad Y'_p/Y_p = w(1n\ Y - 1n\ Y_p), \text{ or}$$
$$1n\ Y = \frac{1}{w}\ (Y'_p/Y_p) + 1n\ Y_p.$$

Friedman has estimated, mainly in connection with his work on the consumption function, that revision of permanent income eliminates about one-third of its deviation from actual income within a year. In other words, the weight of the current year's income is one-third, and the weights of past years' incomes two-thirds, in the calculation of permanent income. If the revision is taken to be continuous, as in (13), rather than discrete, these weights imply a value of 0.40 for w.

In this model the supply of money and its rate of change are autonomous. The demand for money must adjust to the supply at every point of time. Permanent income is the only variable involved in the demand for money; so it must do the adjusting. But much of permanent income is past history; the only part that can adjust is current income. Roughly speaking, Friedman's numerical estimates imply that permanent income must rise 0.55 per cent to absorb a 1 per cent increment in the supply of money. But in the short run money is much more powerful. Current year's income must rise by 1.65 per cent to make permanent income rise 0.55 per cent. Thus in a cyclical boom, in which the supply of money keeps rising, current income must rise even faster. In this way the theory explains why the velocity of money moves up and down with income in business cycles and reconciles this observation with Friedman's finding that secularly velocity declines as income rises.

An explicit relation of income to money supply can be obtained from (13) by using (11) to express $1n\ Y_p$ in terms of $1n\ M$ and (12) to express Y'_p/Y_p in terms of M'/M:

$$(14) \qquad 1n\ Y = \frac{M'/M}{\delta w} + \frac{1n\ M}{\delta} - \beta t - \frac{\beta}{w} - \frac{A}{\delta}$$

$$(15) \qquad Y'/Y = \frac{g'_M}{\delta w} + \frac{g_M}{\delta} - \beta\ ,$$

for convenience letting g_M *denote* M'/M and g'_M, its time derivative. Equation (15) will be used for the analysis of cyclical timing patterns. It relates the rate of change of income, abstracting from trend, to the rate of change of the money stock and to the change in that rate. Note that if g_M is held steady at $\delta\beta$ then Y'/Y will be zero and income will be on trend.

This exposition is based on Friedman's theory as set forth in his

article with Anna Schwartz.[5] I have used continuous rather than discrete time, and I have related money demand to money income, ignoring the complication that real income and price level enter Friedman's formula somewhat differently. These simplifications do not impair the essential message of the theory for the present purpose.[6]

Consider a business cycle generated by a sine wave in g_M. What will be the resulting movement of Y'/Y? This is, according to (15), the sum of two components, one linear in g_M itself, the other proportional to g'_M. The first is indicated by the positively sloped line in Figure IV. The trough and peak of g_M are indicated by g_{Mtr} and g_{Mpk}.

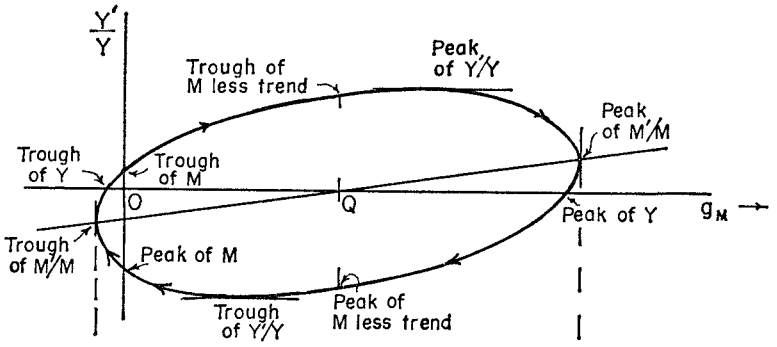

FIGURE IV
Friedman Cycle

The average value of g_M over the cycle is positive, specifically $\delta\beta$, while the average value of Y'/Y is, of course, zero. These average values are shown as point Q in Figure IV. To show the second component on the same diagram, we must use the relationship between g'_M and g_M, depicted by the circle in Figure V. The large ellipse in which the circle is inscribed is $g'_M/\delta w$, where $1/\delta w$ exceeds one, in keeping with Friedman's theory and numerical estimates. It is this which must be added vertically to the line of Figure IV to exhibit the total change in income Y'/Y. As in the case of Figure II, the order of events in the cycle may be read by following the perimeter of the misshapen ellipse in Figure IV clockwise.

5. "Money and Business Cycles," *loc. cit.*, 56–59.

6. Elsewhere, with Craig Swan, I have considered the permanent income theory in full detail and tested the model and Friedman's numerical estimates of the parameters against postwar U.S. data. See J. Tobin and Craig Swan, "Money and Permanent Income: Some Empirical Tests," *American Economic Review*, LIX (May 1969), 285–95.

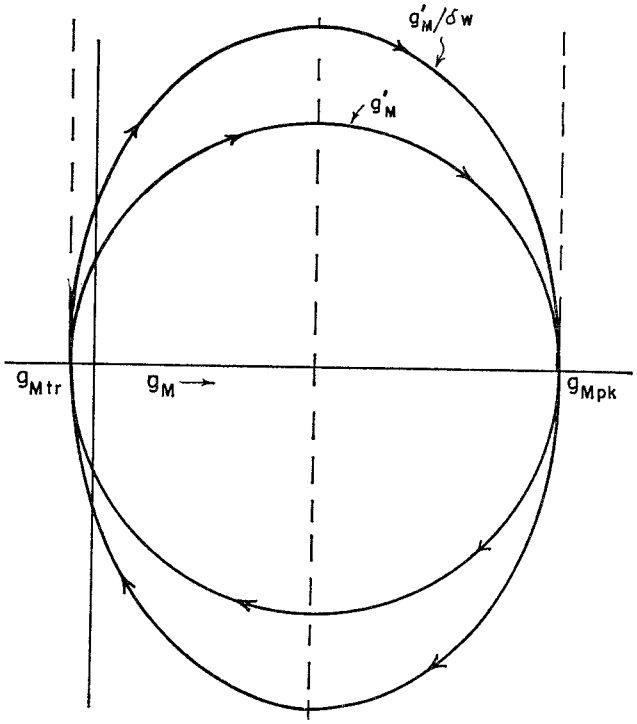

FIGURE V

In this monetary model of business fluctuations, M'/M *lags* Y'/Y and has only a short lead over Y itself. The money stock itself lags Y at peak and trough. However, as in the other model, there might be no cycle in M at all: M'/M might never be negative. This would be shown in Figure IV by moving the vertical axis entirely to the left of the ellipse. If it were moved part way, the trough in M might precede the trough in Y. But the major conclusions remain.

As in Figure II, it is also possible to indicate in Figure IV the peak and trough in the deviation of the money stock from trend. The average level of M'/M is shown by the dashed vertical line through Q. When actual M'/M equals this average, trend-corrected M reaches its peak and trough. Figure IV shows that these turning points lag the corresponding turning points in Y.

As in the case of the ultra-Keynesian model, the cycle does not need to be a sine wave in order to produce the basic order of events over the cycle.

Comparisons of Timing Implications

In Table I, I have summarized the timing implications of the two models, as indicated in Figures II and IV.

TABLE I

Order of Events in Model Cycles

Ultra-Keynesian	Friedman
trough of Y	trough of Y or [trough of M]
peak of M'	[trough of M] or [trough of Y]
peak of Y'	trough of M corrected for trend
peak of M corrected for trend	peak of Y'/Y
peak of Y or [peak of M]	peak of M'/M
[peak of M] or peak of Y	peak of Y
trough of M'	peak of M corrected for trend
trough of Y' or [trough of M]	trough of Y'/Y or [peak of M]
[trough of M] or trough of Y'	[peak of M] or trough of Y'/Y
trough of M corrected for trend	trough of M'/M
trough of Y	trough of Y or [trough of M]

Note: events in brackets [ ] need not occur at all.

Clearly the monetary-causal model implies a much less impressive lead of money over business activity than its opposite.

Consider now the empirical evidence. The cyclical timing patterns reported by Friedman and Schwartz are as follows: [7]

(a) For "mild depression cycles" they find no cycle in M.

(b) For "deep depression cycles" they find a cycle in M, mildly lagging the NBER reference cycle, with which money income is roughly coincident, at peaks.

(c) They find that the rate of change of the money stock leads at peaks and troughs. This lead is dramatically long, so much so "as to suggest the possibility of interpreting the rate of change series as inverted, i.e., as generally declining during reference expansion and rising during reference contraction."

(d) They show a generally procyclical behavior of velocity Y/M, but with some tendency for velocity to start declining before the reference peak.

Friedman has also summarized the evidence in an earlier article, as follows:

> . . . peaks in the rate of change of the money stock precede reference cycle peaks by 16 months (on the average) . . . peaks in the deviation of money

7. Friedman and Schwartz, "Money and Business Cycles," *loc. cit.*, especially Charts 2, 4, and 6, and p. 36.

stock from its trend do so by five months . . . such absolute peaks as occur in the money stock precede reference cycle peaks by less than five months and may even lag . . . peaks in the rate of change of income precede such peaks as occur in the stock of money . . . they probably also precede peaks in the deviation of the money stock from its trend . . . they probably also follow peaks in the rate of change of money.[8]

In comparing these findings with the patterns of Figures II and IV, it is helpful to recall that sixteen months is roughly three-eighths and five months roughly one-eighth of a complete cycle. Figure II agrees with the empirical summary not only in order of events but also in the lengths of these leads or lags.

Every single piece of observed evidence that Friedman reports on timing is consistent with the timing implications of the ultra-Keynesian model, as depicted in Figure II. This evidence actually contradicts his own "permanent income" theory and lends support to the ultra-Keynesian model.

As the quotation in (c) above indicates, Friedman himself has worried whether the very long lead of M' over Y and the reference cycle may not prove altogether too much. It might be a lag instead of a lead. "An inverse relation," he says elsewhere, "with money lagging would be much easier to rationalize in terms of business influencing money than of money influencing business. . . ."[9]

It is only fair to notice, however, that there are two Friedmans when it comes to describing the causal mechanism from money to money income. One is the Friedman of the permanent income hypothesis, with the implications set forth above. The logic is that the demand for money is quite insensitive to current income, because current income has only a fractional weight in permanent income. This has the virtue of explaining why the monetary multiplier in the cyclical short run is so large and why velocity varies procyclically. But the cost of this explanation, as we have seen, is that it implies an immediate response as well as a powerful response. What is gained from the hypothesis in explaining amplitude is lost in explaining timing.

Friedman recognizes some of the limitations of the permanent income model. He sees that it cannot be applied without modification to quarterly as well as annual data. Since the current quarter of income experience has presumably even less weight in determining permanent income, and thus the demand for money, than the cur-

8. "The Lag in the Effect of Monetary Policy," *loc. cit.*, 456.
9. *Ibid.*, 458.

rent year of income experience, the money multiplier should be much larger (three to four times as large) on a quarter-to-quarter application of (15) than on a year-to-year application.[1]

Faced by this sort of *reductio ad absurdum*, Friedman says:

> In generalizing to a quarterly basis, it will no longer be satisfactory to suppose that actual and desired money balances are always equal. It will be desirable to allow instead for a discrepancy between these two totals, which the holders of balances seek to eliminate at a rate depending on the size of the discrepancy. This will introduce past money balances into the estimated demand equation not only as a proxy for prior permanent incomes [as in (14) and (15)] but also as a determinant of the discrepancies in the process of being corrected.[2]

The second Friedman explains the money-income causal nexus, and the reason that it takes some time to operate, in much more conventional and less controversial terms. This description relies heavily on discrepancies of the type just discussed. Excessive money balances, for example, are not immediately absorbed by mammoth spurts of money income. They are gradually worked off — affecting interest rates, prices of financial and physical assets, and eventually investment and consumption spending.[3] This account, though not yet expressed with the precision of the permanent income hypothesis, can doubtless be formulated so as to be consistent with the observed evidence on timing. But at a cost. It cannot attribute to money a large short-run multiplier or explain the procyclical move-

1. When the model is formulated in discrete rather than continuous time, equation (14) becomes (here interpreting M, as well as Y, as trend-corrected)

$$1n\, Y(t) = \frac{1}{\delta w} 1n\, M(t) - \frac{(1-w)}{\delta w} 1n\, M(t-1) - \text{const.}$$

Since w, the weight of current period income, varies inversely with the length of the period, the multiplier of $1n\, M(t)$ is larger the shorter the period.

A formulation free of this paradox would relate trend-corrected "permanent money balances" to trend-corrected "permanent income":

$$1n\, M_p(t) = \sum_{-\infty}^{t} v(1-v)^{t-\tau}\, 1n\, M(\tau)$$

$$= \delta \sum_{-\infty}^{t} w(1-w)^{t-\tau} 1n\, Y(\tau) = \delta Y_p(t)$$

$$v\, 1n\, M(t) + (1-v)\, 1n\, M_p(t-1) = \delta w\, 1n\, Y(t) + \delta(1-w)\, 1n\, Y_p(t-1)$$

$$1n\, Y(t) = \frac{v}{\delta w} 1n\, M(t) + \frac{w-v}{\delta w} 1n\, M_p(t-1).$$

Since v/w is presumably independent of the time period chosen, this formulation avoids the *reductio ad absurdum*. But it also has different implications both for policy and for estimation.

2. "Money and Business Cycles," *loc. cit.*, 59.

3. Passages describing this mechanism may be found in each of the Friedman articles previously cited.

ment of velocity. Indeed it leaves room for interest rates and other variables to affect velocity. Therefore it cannot have those clear-cut implications regarding monetary and fiscal policy with which Professor Friedman has so confidently identified himself.

YALE UNIVERSITY

COMMENT ON TOBIN

MILTON FRIEDMAN

Professor Tobin writes as if the two ingenious models that his paper contains contradict or cast doubt on views that I hold or have stated in print. The fact is that his models are not inconsistent with any conclusions about the role of money that I hold or, to the best of my recollection, have published. On the contrary, they exemplify a proposition that I have stated repeatedly in the past decade. The appearance to the contrary arises only because Tobin's paper gives a thoroughly misleading impression of my conclusions.

Needless to say, my conclusions about the role of money are tentative and may be wrong. Indeed, like all conclusions that have some content, they will undoubtedly be shown to be wrong as we learn more about the phenomena that they deal with. That is what scientific progress consists of. But Tobin's article casts no doubt on them.

In this comment, I shall first discuss the major issues on which Tobin gives a misleading impression of my views. I shall then state the proposition that Tobin's models exemplify and indicate how they do so.

I. THE INFLUENCE OF MONEY ON BUSINESS

Tobin begins his paper, "Milton Friedman asserts that changes in the supply of money . . . are the principal cause of changes in money income. In his less guarded and more popular expositions, he comes close to asserting that they are the unique cause." This is the whole of what Tobin says in this paper about my views on the influence of money on business.

Elementary canons of scholarship call for documentation of such statements, yet Tobin gives not a single reference in support of the first sentence and only an excerpt from a *Newsweek* column of mine in support of the second. And that excerpt, read carefully, gives no support whatsoever to his statement.[1]

1. The excerpt (in Tobin's note 1) says that a *tax increase* will not have a significant impact on nominal income unless it somehow affects the quantity

Tobin's second sentence is simply wrong. The first is a different matter. To the best of my recollection, I have never made such an assertion. Yet it does give the right flavor of my views, and I have no wish to quibble over wording.

What impresses — or depresses — me about the statement is less its inaccuracy as a description of my views than its imprecision as a piece of scientific writing. What does "principal" cause mean? If there were an unambiguous way to count "causes," presumably it would mean, "accounts for more of the variance of money income than any other single cause" — which, if the causes were numerous enough, might be consistent with its accounting for only 1 per cent, say, of the variance of money income. Since there is no unambiguous way to count causes, the only way to assure that changes in the supply of money are *the* "principal cause," with no further qualification, would be for them to account for more than half of the variance of all changes in money income — from minute to minute, day to day, month to month, and so on — for periods of all durations. I do believe that changes in the supply of money have accounted for more than half the variance of money income for reasonably long periods and for changes measured over intervals of a year or more. But they certainly have not done so for all periods and all intervals.

Moreover, if the statement goes too far in this respect, it does not go far enough in another. Suppose that my policy prescription of a constant rate of change in the quantity of money were adopted by the monetary authorities, but that instead of choosing a rate of 3 to 5 per cent for the United States, as I have recommended, they chose a rate of zero — i.e., they held the quantity of money constant. Changes in the supply of money would then account for zero per cent of the variance of nominal income. Would Tobin then say that money is of no importance? Assuredly not.

What I have actually said is:

> Money does matter and matters very much. Changes in the quantity of money have important, and broadly predictable, economic effects. Long-period changes in the quantity of money relative to output determine the secular behavior of prices. Substantial expansions in the quantity of money over short periods have been a major proximate source of the accompanying

of money and its rate of growth. This does say that taxes by themselves are unimportant for the course of nominal income; it does not say that variables other than taxes and money may not have important effects on money income.

Of the instruments available to the government to affect nominal income, I believe that changes in the quantity of money are far and away the most important. But that is a far cry from saying that they are "the unique cause." Indeed the particular excerpt Tobin quotes is, taken by itself, entirely consistent with the belief that changes in the supply of money are a minor cause (to use Tobin's language) of changes in nominal income.

inflation in prices. Substantial contractions in the quantity of money over short periods have been a major factor in producing severe economic contractions. And cyclical variations in the quantity of money may well be an important element in the ordinary mild business cycle.[2]

II. Evidence for the Independent Influence of Money on Business

Tobin's whole paper, as most clearly shown by its title, has no point whatsoever *as a criticism of my views* unless my conclusion that money exerts an independent influence on business[3] primarily, or at least heavily, rests on the observation that the rate of change of money turns down well before peaks of business cycles, and turns up well before troughs.

Tobin does not explicitly state that my conclusion rests primarily on this observation. Indeed he quotes me as saying that the leads are by no means decisive — selecting part of a longer quote that I shall give in full later. Yet I think that no fair-minded reader of his paper can come away with any other impression than that the paper is intended as a criticism of my views — which it is if and only if I have, in Tobin's words, "accepted timing evidence as empirical proof of propositions about causation." Tobin gives the impression that I have done so not only by imprecise wording but even more by ignoring large parts of our argument.

It may be that in the early years of my work on money, I regarded the lead of money at peaks and troughs as decisive evidence of the independent influence of money. I doubt that I did even then, because the "acceleration principle" makes it clear how tricky timing evidence can be. But in any event, for at least a decade I have been fully aware of the limitation of timing evidence as "empirical proof" of the independent influence of money. In that period, I have published two careful summaries of the evidence on the independent influence of money, one in Anna Schwarz's and my *Monetary*

2. "Monetary Studies of the National Bureau," from 44th Annual Report of NBER, reprinted in my *Optimum Quantity of Money and Other Essays* (Chicago: Aldine, 1969), quotation from p. 277 of latter.

Other similar summaries of my conclusions are *ibid.*, pp. 177, 179 (reprinted from a 1958 Joint Economic Committee Print), p. 234 (reprinted from an article, "Money and Business Cycles," written jointly with Anna J. Schwartz and initially published in *Review of Economics and Statistics*, Feb. 1963); in Milton Friedman and Anna J. Schwartz, *A Monetary History of the United States, 1867–1960* (Princeton: Princeton University Press for the NBER, 1963), pp. 676, 695.

3. Which does not of course exclude a reciprocal influence from business to money as well. See my *The Optimum Quantity of Money*, pp. 79, 240, 265. for samples of statements to this effect.

History, the second in my contribution to the 1964 *Annual Report* of the National Bureau of Economic Research.

The first summary, which runs ten pages, does not even mention timing evidence. Yet we conclude:

> Changes in the money stock are . . . a consequence as well as an independent source of changes in money income and prices, though, once they occur, they produce in their turn still further effects on income and prices. Mutual interaction, but with money rather clearly the senior partner in longer-run movements and in major cyclical movements, and more nearly an equal partner with money income and prices in shorter-run and milder movements — this is the generalization suggested by our evidence.[4]

The second and later summary does cite timing evidence to support the conclusion that "the major direction of influence is from money to business." That evidence is striking and important, and nothing in Tobin's article is in the slightest way inconsistent with my discussion of it. On the contrary, as I show later, Tobin's first example reinforces my analysis. However, the timing evidence is one of five different kinds of evidence we cite — it occupies about one-quarter of the space and part of that is devoted to explaining why the timing evidence is "suggestive . . . but . . . by no means decisive." Expunge that section from the summary, and I would not myself — and neither, I believe, would any impartial reader — be led to change to any major extent the confidence attached to our basic conclusion.[5]

Tobin refers to the two careful summaries [6] and to some, though not all, of the other evidence we cite in support of our conclusion. Yet, to judge from his article, he apparently is himself persuaded that we attach primary importance to "timing evidence as empirical proof of propositions about causation," and that our protestations to the contrary are not to be taken seriously. How come?

I suspect that the main reason is that, in Tobin's words, "Much of the work of Friedman and his associates at the National Bureau of Economic Research has been devoted to" evidence on timing. If timing relations are not decisive evidence on direction of influence, why have we devoted so much effort to trying to pin them down?

4. *A Monetary History*, summary on pp. 686–695, quotation on p. 695.

5. In addition to those two careful summaries, I have published less extensive summaries. For example, in a 1961 article on "The Lag in Effect of Monetary Policy," a reply to an article by John Culbertson, I referred to the timing evidence in the same words as above — as "suggestive" but not "decisive" — then wrote, "there is other and much stronger evidence for the largely independent role of money," and proceeded to summarize the other evidence. Reprinted in *The Optimum Quantity of Money*, quotations from pp. 240 and 241.

6. And also to the 1961 article referred to in the preceding note.

The answer is two-fold: First, *given that money exerts an independent influence on income*, the use of that proposition for either policy or prediction depends critically on knowing what the relation between money and income is, and especially on what lags are involved. If a monetary change today has its predominant influence on income within a month, I shall make very different predictions than if its predominant influence is delayed six months, or nine months, or twelve months, or eighteen months. Similarly, the lag between monetary changes and their effects is critical for the kind of monetary policy that is possible and desirable. My own strictures against a discretionary policy rest heavily on the belief that the lags are long and variable.

Second, failure to allow for lags between monetary changes and income changes will bias any estimates of relations between the two and will give a misleading impression of the closeness of the relation. For example, velocity as ordinarily calculated is much more variable than it is if the numerator and the denominator are for time intervals separated by the mean lag.

We have long since accepted the proposition about direction of influence. Hence, we have been devoting our research efforts to trying to pin the relationships down more closely. Tobin, still unpersuaded or at most a very recent and reluctant convert, looks at everything through different glasses and takes it for granted that we wear the same glasses he does.

III. Tobin's Two Models

The proposition of mine that Tobin's models exemplify is stated most fully in my contribution to the 1964 *Annual Report* of the National Bureau of Economic Research, from which Tobin quotes selectively. Let me give the relevant quotation a bit more fully.

> These regular and sizable leads of the money series are themselves suggestive of an influence running from money to business but they are by no means decisive. One reason is that both the monetary changes and the business changes might be the common consequence of some other influences which have their effect on money more promptly than on business. A second is that the characteristics of business change affecting money may not be those that are dated by the Bureau reference dates.
>
> The most important reason, however, why the consistent leads of the money series are not decisive is that, given a recurrent cyclical process, these leads may be simply a reflection of an earlier influence of business on money; they may be a statistical artifact resulting from our matching the turning points in money with the wrong turning points in business. Instead of matching a peak in the money series with the subsequent reference peak, we could

match it with the prior reference trough; similarly, we could match the rate of change trough with the prior reference peak.[7]

I illustrated the first reason in a 1961 article in reply to an article by John Culbertson. "Movements in stock market prices," I wrote, "on the average precede movements in business. My own conjecture is that the explanation is neither that the stock market exerts any significant influence on business nor that traders in the market are good forecasters but that both the stock market and business reflect the influence of monetary changes, which precede both but operate more quickly on prices of equities than on flows of money expenditures."[8]

Tobin's "Ultra-Keynesian" Model

Tobin's "ultra-Keynesian" model illustrates both the second and third reasons: the relation in his equation (9) between M' and Y illustrates the third reason, and it is this element in his model that accounts for the most striking of his timing results; the relation in his equation (9) between M' and Y' illustrates the second reason — the Bureau dates peaks and troughs by Y, not Y'.

The interesting feature of this model of Tobin's is that the key result — the inverted conformity between M' and Y — does not derive from the alleged ultra-Keynesian character of the model, but rather from a thoroughly un-Keynesian feature of it — a highly peculiar demand for money equation.

Consider Tobin's Figure I. At the income level where the two lines defining the shaded area cross (call this Y_c), M' is zero if Y' is zero for the interest rate that Tobin assumes fixed by the monetary authorities. Since the monetary authorities passively supply the amount of money demanded, this means that, at this income level, the quantity of money demanded is constant over time, though, on Tobin's assumptions, real capital grows steadily. At *lower* levels of income, the quantity of money demanded (under the same conditions of $Y' = 0$ and a fixed interest rate) *rises* over time, and the lower the level of income, the more rapidly the quantity of money demanded rises. At levels of income higher than Y_c, the quantity of money demanded falls, and the higher the level of income, the greater the absolute rate of fall.

7. *Optimum Quantity of Money*, p. 269. Tobin properly notes that we reject the third reason on the basis of considerable empirical evidence that a positive relation between money and business with a lead fits the facts better than an inverted relation with a lag. Because he does not quote the first two reasons, he leaves the erroneous impression that, having rejected the third, we have no further doubts about the import of the timing evidence.

8. *Optimum Quantity of Money*, pp. 240–241.

An income higher than Y_c is, of course, an impossible long-run equilibrium position, because the community would at some point run out of money entirely. This neglect of the long-run implications of short-run equilibrium positions is one strictly Keynesian feature of the model. The other Keynesian feature of the model is the determination of income by the level of investment. But neither of these Keynesian features produces the timing results. As Tobin himself says, "the essential message of Figure II comes through provided that M' is related negatively to Y and positively to Y'." Both these relations are imbedded in the peculiar demand equation. The rest serves only to give an air of verisimilitude to the model as a whole.

The demand for money equation is un-Keynesian — and also un-quantity-theory — in several respects. First, Keynes and his disciples typically relate the quantity of money demanded, not its rate of change, to the interest rate and the level of income. Second, and more important, they invariably regard the quantity of money demanded as positively, not negatively, related to the level of income.

The peculiar character of the demand for money equation is not obvious on the surface of equation (7). That equation looks like a standard Keynesian equation. The gimmick in it is that D (government debt) and K (capital stock), while fixed at any moment of time, change over time for given Y (net national product). Let Y, for example, be equal to Y_c and stay there. Then, under Tobin's assumptions, K is growing over time (because of investment) and D is declining over time (because of a government surplus) at a rate equal to a (the ratio between the public's debt to the banking system and the capital stock) times the rate of growth of K, so $D + aK$ remains constant, which is why the quantity of money demanded remains constant. At lower levels of income, D falls less rapidly than aK rises, and at still lower levels of income (below the level at which the tax line in Figure I cuts the horizontal G line) both D and aK rise. Conversely, at higher levels of income, D falls more rapidly than aK rises, so $D + aK$ falls, reducing the quantity of money demanded. Note that this is all for *given levels* of income.

Equation (7) would be a strictly Marshallian demand curve linking the demand for money to wealth and income if a were replaced by unity, since in Tobin's model $D + K$ = total wealth. But in that case, Tobin's results would no longer follow because wealth would grow more rapidly, the higher the income, hence the relation between M' and Y for $Y' = 0$ would be positive, not negative.[9]

9. By definition, $D' = tY - G$.

Tobin is himself misled about the meaning of his demand for money equation (7) in the context of his model. After presenting this equation and making the assumption that the monetary authorities act to keep r (interest rate on bonds) constant, he remarks, "In Friedman's terms, this is a 'supply response' with 'positive conformity' of money to business activity." It is a supply response, but because of his peculiar demand function, with *inverted*, not *positive*, conformity. Had he used the Marshallian demand function, he would have found positive conformity but synchronously, not with a lead, and thus would have illustrated my remark, "it is not easy to rationalize positive conformity with a lead as reflecting supply response."[1] As it is, he illustrates the third reason I gave why timing observations may not be decisive on direction of influence: because what appears to be a positive relation with a lead may be an inverted relation with a lag. In addition, his example reinforces my conclusion that the timing observations cannot be interpreted as a supply response. In order to generate a supply response yielding an inverted synchronous relation, Tobin finds it necessary to resort to an untenable demand function for money.

His model illustrates also another proposition on a much more general level: "Observed facts are necessarily finite in number; possible hypotheses, infinite. If there is one hypothesis that is consistent with the available evidence, there are always an infinite number that are."[2] However, Tobin's particular hypothesis is contradicted by some finite facts not taken into account in its construction, namely, those on the characteristics of the demand for money, and those that support positive rather than inverted conformity of money to the business cycle.

IV. The Friedman Model

Just as Tobin's "ultra-Keynesian" model is not Keynesian, so his "Friedman model" is not Friedmanian. Mrs. Schwartz and I described the demand equation that he erects into a complete busi-

From equation (2), $K' = s(1-t)Y + tY - G$.
Hence

$$D' + K' = W' = (s - st + 2t)Y - 2G.$$

The coefficient of Y is positive for both s and t positive and less than unity. Q.E.D.

More generally, as Tobin points out, the dividing line between a positive and negative relation between M' and Y is $a = tm$. For higher values of a, the relation is positive; for the lower values assumed to prevail by Tobin, negative.

1. *Optimum Quantity of Money*, p. 270.

2. Milton Friedman, "The Methodology of Positive Economics," *Essays in Positive Economics* (Chicago: University of Chicago Press, 1953), p. 9.

ness cycle theory as "one element of a theory designed to account for the observed tendency of cyclical fluctuations in income to be wider in amplitude than cyclical fluctuations in money." [3] We did not present it as accounting for the timing observations. We agree fully with Tobin's finding that this element by itself does not yield the observed timing relations between money and business. Indeed, that is precisely the reason why we regard it as only an element and why we pointed out, as Tobin acknowledges, that in generalizing to shorter time units it would be necessary to allow for other elements. Consistency of a hypothesis with timing observations is evidence for a hypothesis but by itself not decisive evidence (as witness Tobin's ultra-Keynesian hypothesis). But inconsistency with the timing observation requires rejection of the hypothesis.[4]

Tobin speaks of "two Friedmans when it comes to describing the causal mechanism from money to money income." There are two Friedmans — one, the caricature drawn by Tobin; the other, the real Friedman. The real Friedman has never presented the particular demand equation that Tobin singles out as a complete model of business cycles or as the essential element linking money to money income. The real Friedman has presented the permanent income hypothesis as a partial explanation of one structural equation in a complete model. In every explicit discussion of monetary theories of cyclical fluctuations which I can recall, including the article from which Tobin extracts his caricature, the real Friedman has emphasized that the "necessity for overshooting in the rate of price change and in the rate of income change (though not necessarily in the level of either prices or income) is in my opinion the key element in monetary theory of cyclical fluctuations." [5] And this statement is in a context in which the permanent income element that Tobin extracts is not present in the analysis at all! [6]

V. Conclusion

I am so happily blessed with critics that I have been forced to adopt the general rule of not replying to them. Even the latest de-

3. *Optimum Quantity of Money*, p. 235.

4. To quote again from my essay on methodology: "The hypothesis is rejected if its predictions are contradicted ('frequently' or more often than predictions from an alternative hypothesis); it is accepted if its predictions are not contradicted; great confidence is attached to it if it has survived many opportunities for contradiction. Factual evidence can never 'prove' a hypothesis; it can only fail to disprove it, which is what we generally mean when we say, somewhat inexactly, that the hypothesis has been 'confirmed' by experience." (*Essays in Positive Economics*, p. 9.)

5. *Optimum Quantity of Money*, p. 13.

6. See also *ibid.*, pp. 233–34, 255–56.

velopments in science have not taught me how to stretch my days to eighty hours while keeping theirs to twenty-four.

I have departed from the general rule in this instance because of my great respect and admiration for James Tobin's scientific ability and because this article is thoroughly out of character. Unlike most of his work, it is imprecise, inaccurate, and misleading.

Tobin is right that timing evidence cannot be accepted as "empirical proof of propositions about causation." But he does not give a satisfactory explanation of why it cannot be and, even more important, what its proper role is. His examples are ingenious but largely irrelevant to this basic issue. The impression — which is completely erroneous — that these examples in some way contradict or are inconsistent with views that I have expressed is produced by a series of straw men. One straw man is an inaccurate and incomplete statement of my views. A second straw man is a non-Keynesian "ultra-Keynesian" model that produces its effect by incorporating an unacceptable demand for money function. A third straw man is a non-Friedmanian Friedman model that produces its effect by a myopic concentration on a single element out of a complex mosaic. At most, these examples illustrate some of the reasons that I have given in the past why timing evidence, important as it is, cannot by itself be decisive.

UNIVERSITY OF CHICAGO

REJOINDER

JAMES TOBIN

Professor Friedman's tone of pained indignation obscures his essential agreement with the main points of my paper. Indeed, much of his reply says that this agreement should already have been clear to any reader of his work. Nevertheless I am glad to have his current stipulations.

He agrees that cyclical leads of money over money income say virtually nothing about direction of causation; he says that he studies and reports these timing sequences for quite other reasons. He agrees that, while consistency with timing observations is not decisive evidence for a hypothesis, inconsistency with those observations requires rejection of the hypothesis. He agrees that his permanent income theory of money demand "does not yield the observed timing relations between money and business." Therefore, he says, it is "only an element" of a complete theory, an element "designed to account for the observed tendency of cyclical fluctuations in income to be wider in amplitude than cyclical fluctuations in money."

That was my concluding point: The element of Friedman's monetary theory that accounts for his amplitude evidence — that is, the procyclical movement of velocity — is not consistent with his timing evidence. Likewise, as well as I can understand other "elements" of his theory, they may be consistent with timing evidence but cannot account for the amplitude observations.[1] Perhaps Friedman can provide a formulation of the money-income nexus that is consistent with both kinds of evidence, so that it is not necessary to vary the model with the evidence to be explained. Recognizing the

1. Consider, for example, the distributed lag equation

$$\frac{\triangle Y_t}{Y_t} = \sum_{i=0}^{s} b_i \frac{\triangle M_{t-i}}{M_{t-i}}$$

where all $b_i > 0$ and $\sum_{i=0}^{s} b_i = 1$. This has the "right" timing implications; cyclical fluctuations in the rate of growth of the money supply will precede those in the rate of change of income. But what about amplitudes? Velocity is increasing, zero, or decreasing as the following expression is positive, zero, or negative:

$$\frac{\triangle Y_t}{Y_t} - \frac{\triangle M_t}{M_t} = \sum_{i=0}^{s} b_i \left(\frac{\triangle M_{t-i}}{M_{t-i}} - \frac{\triangle M_t}{M_t} \right)$$

when $\frac{\triangle M}{M}$ is rising, the expression is negative — money income is growing more slowly than money, and velocity is declining.

shortcomings of the permanent income theory, he may wish to consider whether the observed procyclical movement of velocity might be related, via "liquidity preference" considerations, to procyclical fluctuation of interest rates.

My ultra-Keynesian model exemplifies the fact that timing sequences consistent with observations, and superficially favorable to hypotheses stressing the causal importance of money, can be generated by a structure in which money has no causal role. Friedman doesn't like the structure, but I believe he accepts the methodological point. Whether the particular model I used to illustrate the point deserves the label I gave it is not a question of great moment.[2]

Milton Friedman's work on money is important and influential; it commands the attention of economists, policy-makers, journalists, and men of affairs throughout the world. That is why it deserves and receives serious critical discussion. I am continually perplexed by Friedman's propensity in professional debate to evade by verbal quibbling the responsibility and the credit for the characteristic propositions of "monetarism" associated with his name.

YALE UNIVERSITY

2. Friedman objects that the money demand function is neither Keynesian nor plausible. But his objection is based not on the relation of money to income in the money demand equation taken by itself but on the relation of money to income in the model as a whole. The fact that model-wide implications differ from single-equation impacts is, of course, the basic point of my paper.