Comment on Barry Friedman's Demand for Money

Allan H. Meltzer
Carnegie Mellon University, am05@andrew.cmu.edu
COMMENT ON BARRY FRIEDMAN'S DEMAND FOR MONEY

Recently many economists have argued that money is a buffer stock held solely for transactions. On this interpretation there can be deviations between desired and actual money balances because of lags in adjustment to changes in the desired volume of transactions or changes in interest rates. But each of the writers in this tradition denies the existence of an asset demand for money and for this reason, denies that money is a part of the desired portfolio of assets and that wealth influences the demand for real money balances. In defense of this view, many cite the conclusions of James Tobin's justly famous paper on liquidity preference, namely: In a world with close substitutes for money such as time deposits, money is "dominated" by other assets that are said to have all of the properties of money and to have the additional property or advantage of bearing interest. Hence there is no reason for economic units to hold money as an asset, according to this analysis.

Barry Friedman's paper starts with the refreshing announcement "The difficulty is that the empirical evidence appears to strongly support the assets approach," as opposed to the transactions approach. To rescue what I shall call the pure transactions approach from the weight of evidence against it, Friedman (that is Barry Friedman) formulates a hypothesis based on two types of lagged adjustment. First, money holders are assumed to adjust, gradually, the amount of money that they hold. This is the now traditional hypothesis that time is required to adjust the actual stock of anything to the desired level. Second, individuals are assumed
to adjust their payments schedules. This is a radical departure from the pure transactions approach and is an important notion that has been discussed, but never formulated as an empirical hypothesis. Moreover, it is at the heart of the issue that divides proponents of the "pure transactions" approach from proponents of the asset demand for money.

First let us look at the argument and the issue, stated in a relatively simple way. The pure transaction group argues that there is only one reason for holding money — to bridge the gap between the receipt of money and the payment of money. The payment of money is identified with some volume of transactions, so that the volume of transactions and some other variables determine the desired money balance. Those who take the assets approach stress that money is a productive asset that furnishes a variety of services. What are these services? One of them is certainly related to the role of money in minimizing the cost of making transactions or payments. Individuals receive payments in money on both wealth and income account. They are assumed to want to maximize wealth. Part of the process of maximizing wealth requires the individual to choose — from among all relevant payment schedules — the particular schedule that minimizes the cost of achieving the optimal composition of assets and debts in his terminal wealth position. Because an individual's payments are constrained by his present wealth position, his present wealth appears as an argument in his payment schedule and thus wealth affects the desired money balance through the payments schedule.
This brief outline brings out the point that I want to emphasize. The standard transactions approach, that is generally identified with the work of Baumol and Tobin on the transactions demand for money, is based on the assumption of a fixed, invariant payments schedule. By making the payments schedule variable, Barry Friedman has let wealth in through the back door. Aside from the fact that the terminology becomes misleading, I don't mind if he wants to call this the "transactions approach" and he shouldn't mind if I prefer to call it the wealth-adjustment approach. The point is that we have agreed on the general features of the hypothesis explaining money, and we have some evidence to support the general view. To reiterate, the area of agreement, we both assume that individuals adjust payment schedules and search for an optimum. Once we agree on this, I do not see how either of us can accept the view that the sole purpose of money balances is to bridge the gap between receipts and payments or to carry out a given volume of transactions. Desired money balances now depend on the payments schedule and thus on relative prices, wealth and the cost of making transactions. The open question now is whether or not money depends on current transitory income.

Let me turn to the empirical evidence in the paper since it bears on the same issue. Barry Friedman's hypothesis contains two lagged adjustment equations, and as a result, current income does not appear in the demand function and a weighted average of past income becomes a constraint on money balances. Again, Barry Friedman and I agree that in his formulation, we cannot interpret the effect of income on money as support for the transactions approach. The other Friedman used a similar weighted average of past income to measure wealth.
To his credit, Barry Friedman is very much concerned with the problem of discriminating between alternative hypotheses. He uses two main arguments for this purpose. First, he claims that short-term interest rates are relevant for the transactions approach because they "measure the yield on actual money substitutes. In the case of the asset demand," he writes, "all rates could be relevant, even the rate on physical capital." I fail to understand the argument because I cannot see why the desired payment schedule should depend only on the short-term rate. When individuals are making payments and reallocating assets, all rates are relevant. However, it is not a short-term rate but a weighted average of past rates on commercial paper and on a variety of time deposits that enter his regression equations. I find it hard to decide whether the rate on time and savings deposits is a short-term or long-term rate. The concept of maturity is difficult to apply to such assets, but I am inclined to believe that the rate paid on these assets is more closely related to the long-term rates on assets like mortgages that dominate the portfolios of savings and loan associations and mutual savings banks. In any case, the problem of discriminating between hypotheses by using short- and long-term rates is made more difficult because weighted averages of past rates are used in the paper. Recent work suggests that these weighted averages are either unbiased or at least approximate estimates of long-term rates. For this reason, I believe that a test based on the use of long-term versus weighted averages of short-term rates fails to discriminate between the two hypotheses empirically.
A second test is proposed in the paper. Both the Milton Friedman and the Barry Friedman equations are estimated using data for the same time period. The Milton Friedman hypothesis, augmented by interest rates, wins on the basis of standard error of estimate and on the principle of parsimony. Barry Friedman points out, however, that on his hypothesis, he should iterate to incorporate the effect of the lagged residuals. After doing this for both hypotheses, he obtains a lower standard error of estimate for his equation than for the permanent income equation and concludes that the evidence gives mild support to his approach.

Unfortunately, he apparently neglected a simple but important point. His equation has velocity as the dependent variable; the permanent income hypothesis has real money balances. The variance of velocity is smaller than the variance of real money balances. Hence, the lower standard error of estimate that he finds for his equation does not say that his hypothesis explains more variance and predicts "better." At most, it says that he makes a slightly smaller error in "predicting" a number that varies substantially less. The correlation coefficients, which adjust for the variance of the dependent variable, tell an entirely different story. The wealth or assets approach wins again.

Before concluding, I want to point out that in general Barry Friedman's paper is a careful and competent piece of work. I have some reservations about the assumption that the real demand for money equation is independent of the price level at every point in time, so that no learning process or
gradual adjustment of money balances to changes in price is permitted. Nevertheless, Barry Friedman has given us the first empirical hypothesis about the adjustment of payments schedules. This is an important feature of the process by which individuals adjust money and wealth, and I hope he will continue his interesting work on it.