PANEL

JAMES TOBIN

Believe it or not, New Haven is in the First Federal Reserve District, the Boston-Harvard region. I had to come to Nantucket in 1969 to find out that my concerns about debt management policy in 1961 and 1962 were of any concern to the Treasury. I was worried a little bit in those days, when we at the Council with the help of Bob Roosa and others at the Treasury had persuaded the Federal Reserve to *buy* long-term bonds, why it should also be good policy for the Treasury to *sell* them at the same time. Bob Roosa explained to me at some length—I couldn't learn it very well—that it made a lot of difference who was buying and who was selling and how it was being done. Maybe if I were a more practical man, I would understand these things.

I don't know if it's worse to follow Samuelson, who uses all your arguments, or Meiselman, who refers to them by number.

I will concentrate on the question of evidence, which is crucial to the great debate. One kind of evidence, which has been presented at some length, is timing evidence: namely, the leads of changes in stock of money, or of changes in the rate of change of the stock of money, or of other monetary aggregates over income, or over the rate of change of income or over other measures of economic activity. A large amount of the work of Friedman and Schwartz in their *Monetary History of the U.S. 1867-1960*[1] and in their article, "Money and Business Cycles,"[2] is concerned precisely with pinning down these timing patterns. Dave Meiselman mentioned timing evidence this morning also. Now I think it is clear that timing evidence—leads, lags and so on—is no evidence about causation whatsoever. This is argued very eloquently, and I think correctly, by Solow, Kareken, and Brown in their CMC paper.[3]

I have engaged in a little irreverent exercise which constructs two models: on the one hand, one of these British models that Paul Samuelson was referring to, an ultra-Keynesian model where money has no causal relationship to anything, and on the other hand, a Friedman-like model in which money is the driving force of the business cycle. I have then compared the timing patterns of money

Mr. Tobin is Sterling Professor of Economics at Yale University, New Haven, Connecticut.

and the change in money relative to money income and the change in income implied by these two different worlds. As it turns out, the Radcliffe world, the ultra-Keynesian world, produces a pattern of leads and lags in business cycles that superficially looks much more like money causing income than the Friedman world in which money actually is causing income. Moreover, the ultra-Keynesian model produces patterns of leads and lags in business cycles which coincide precisely with the summary of empirical results about such timing that appears in the Friedman-Schwartz article, whereas the implications of Friedman's and Schwartz's own theory diverge considerably from their own empirical findings.

Milton Friedman has responded that he knows better than to think that timing evidence has anything to do with causation. If this is stipulated, we can regard as descriptive but irrelevant detail all those pages about timing that an unwary reader might think were there for the purpose of making some point about causation.

There is a related point about evidence, which has to do with the effects on the data of the sins of the Federal Reserve and other monetary authorities in the past. Now let me give you a ridiculous example to make the point. Don't take it too seriously. Suppose that some statistician observes that over a long period of time there is a high association, a very good fit, between gross national product and the sales of, let us say, shoes. And then suppose someone comes along and says, "That's a very good relationship. Therefore, if we want to control GNP, we ought to control production of shoes. So, henceforth, we'll make shoes grow in production precisely at 4 percent per year, and that will make GNP do the same." I don't think you would have much confidence in drawing this second conclusion and policy recommendations from the observed empirical association.

Over the years, according to the monetarists, the Federal Reserve has been acting like the producers and sellers of shoes. That is, the Fed has been supplying money on demand from the economy instead of using the money supply to control the economy. The Fed has looked at the wrong targets and the wrong indicators. As a result, the Fed has allowed the supply of money to creep up when the demand for money rose as a result of expansion in business activity, and to fall when business activity has slacked off. This criticism implies that the supply of money has, in fact, not been an exogenously controlled variable over the period of observation. It has been an endogenous variable, responding to changes in economic conditions and credit market indicators via whatever response mechanism was built into the men in this room and their predecessors.

The evidence of association between money and income reflects, to a very large degree, this response mechanism of the Federal Reserve and the monetary authorities. It cannot be used simultaneously to support the reverse conclusion: namely that what they have done is the *cause* of the changes in income and GNP. Perhaps the monetarists will be sufficiently persuasive of the Federal Reserve and of Congressional committees to bring about, in the future, a controlled experiment in which the stock of money is actually an exogenous variable.

Much evidence has been presented purporting to show the superior power of monetary variables over fiscal variables and private investment measures in explaining changes in GNP. This evidence comes in what I call pseudo-reduced-forms.

The meaning of the term *reduced-form* is this: If you think of the economy as really a complex set of equations—basic structural relationships describing business investment, demands for loans, demands for money, the consumption function and so on—conceivably you could solve such a system and relate the variables in which you are ultimately interested, such as GNP, to the truly exogenous variables including the instruments of the monetary and fiscal authorities. Such a solution of a big complicated model you would call a *reduced-form.* And then one possible way of estimating a model of the system would be not to estimate the structural equations, the building blocks of the system, but to estimate the condensed equations which relate the ultimate outputs like GNP to the ultimate causal factors. That would be reduced-form estimation.

There are a lot of difficulties in that procedure. Therefore, most builders of big and small models of the economy do not proceed in that way; but, instead, try to estimate the individual structural equations one by one. What I mean by a pseudo-reduced-form is an equation relating an ultimate variable of interest, like GNP, to the supposedly causal variables, but one which doesn't come out of any structure at all. Instead, the investigator just says, "Here are the effects and here are the causes, let's just throw them into an equation." The form and content of the equation—the list of variables and the lag structure—are not derived from any structural model. That is what we have had presented to us as the main evidence for the supposed superiority of monetary variables in explaining GNP.

When, in contrast, we try to take a *theory* of how money affects the economy, and test it in the form it is presented, we have to look at one of two things: either a demand for money equation, or some complicated set of linkage equations through which changes in the money stock affect investment demand, consumption demand, etc. As far as the demand for money equation is concerned, as Paul Samuelson mentioned, the crucial assumption of some monetarists is that interest rate variables are of no importance, so that there is a tight linkage between the stock of money and GNP. If real GNP and prices, current and lagged, are the only important factors in the demand for money balances, then we know that control of money stock is uniquely decisive, and we don't have to look elsewhere in the system. However, all the tests that I know in which interest rates are allowed to enter demand for money equations, indicate that interest rates have important explanatory power.

If we do not really know that the demand for money is exclusively determined by income, then things other than income may absorb changes in money supply. There is no short cut. We have to look for the effects of changes in the stock of money, and it is hard work. We have to look through the system of structural equations to see how money enters directly and indirectly into investment demand and consumption demand and so on. We have to examine long chains of causation. In those chains there could be many slips, and there could be many structural changes, innovations in markets and institutions. That is the purpose, I suppose, of the hard work involved in large econometric models, work which these other attempts to find evidence try to short-circuit completely.

[1] Friedman, Milton and Schwartz, Anna Jacobson, *A Monetary History of the United States, 1867-1960* (Princeton, N.J.: Princeton University Press, 1963).

[2] Friedman, Milton and Schwartz, Anna Jacobson, "Money and Business Cycles," *Review of Economics and Statistics,* XLV (Supplement: February, 1963), 32-78.

[3] Ando, Albert, Brown, E. Cary, Solow, Robert M., and Kareken, John, "Lags in Fiscal and Monetary Policy," Report of the Commission of Money and Credit, *Stabilization Policies* (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1963), 1-163.