5-1967

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COMMENTS ON THE FEDERAL RESERVE - MIT ECONOMETRIC MODEL

by Allan H. Meltzer

There are thirteen separate sectors or sections in the Federal Reserve-MIT model, but fortunately one section contains a set of national income accounts. If each member of the Open Market Committee manipulates his own section, each of the sections will be allocated and the computer can be assigned the task of keeping the national income accounts. One can think of some obvious assignments. Governor Maisel would probably get the housing market, and the New York Bank will surely try to wrest control of the financial sector from the Board of Governors.

These comments about the size are offered to make you sympathetic to the plight of this discussant confronted with so large a model. The authors never tell for what purpose the model has been constructed, and it is difficult to know the extent to which the authors regard the model as a basis for policymaking and to what extent they regard it as an exercise in learning to live with and not become frustrated by a computer program. I have decided that their purpose is probably a linear combination of policy model and computer exercise with weights that are gradually adjusting, so that in the limit the model will approach a policymaking model with the speed of adjustment given by one member of the Solow family of lag distribution.

How does one discuss the substance of a model with 124 equations plus 54 variables that the authors call exogenous ranging from the compensation of government employees and the inventory of farm output to the seasonal

* Paper read at the SSRC Conference on the Monetary Mechanism, Board of Governors, May 26, 1967. As always joint work with and helpful comments by Karl Brunner do not permit him to be absolved of responsibility.
factors affecting the money supply? While pondering, I came across the statement almost immediately that made a formidable task seem even more formidable. The system, we are told, is in the process of revision. Some parts are completed; some are being revised; some are not available to the reader.

A discussion of the author's decision to call some variables "exogenous" and to call others "endogenous" did not seem to me to be the most fruitful line of discussion, although there are some interesting subjects that might be discussed. Also, it did not seem useful to comment in super-abundant detail on the choices that had to be made about explaining minutiae in one sector but not in another.

However, I cannot refrain from commenting on one of the more troublesome problems that this reader faced. In the November 1966 schematic framework, the only version of the model I had until ten days ago, the authors used one set of symbols; in the April draft which was apparently written about the 10th of May, they used another set — quite different from the first. A third set was used for developing the hypothesis and presenting the empirical results, so that if I wanted to see how the November equation looked when it has been re-written I had two or three sets of notations to remember. But that isn't all. The sections written by Modigliani and by Modigliani and Rasche used symbols that differ at times from the symbols used by Shapiro and Rasche. As a result, I must acknowledge the authors' responsibility for any errors that I make, and I will add the proviso that I am not sure that even the authors know whether or not I make an error when good old free reserves are Bf in November, Mfr in April, or FR in the empirical work and are defined in equation 10 as total reserves minus required reserves minus
borrowing and minus currency held by the public. The latter misfortune appears to result from the fact that the authors cannot quite make up their mind whether the variable $Z$ includes currency or doesn't. Moreover, this error in the definition of free reserves is not the only problem. Some variables are not defined at all. All I know about $Vdv$ is that it is measured in dollars (so it is probably not an advanced form of social disease) but that was more than I ever discovered about $Mgf$.

Before turning to a discussion of more serious problems, it should be noted that Franco Modigliani started his analysis of the monetary sector with one equation in 1944. About 19 years later, in 1963, the one equation had become six. Four years later, i.e. now, the six equations have become thirty-six. At the increasing rate at which equations have been spawned per unit time, we can expect 216 equations for the financial sector about next March and at least 900 symbols for the 250 variables. We can only guess about what will happen after that. One has an image of Franco Modigliani developing equations at the speed of sound — and changing notations at the speed of light — while Albert Ando, Frank deLeeuw and perhaps Dan Brill worry about whether the process of model construction and notation change converges. If that is what Franco means when he says that the system is in the process of revision, I am glad that I am not the discussant of the final version of the model.

The topics raised by the present model that I want to discuss can be grouped under a few general headings. First, I will comment on the structure of the model pointing out that as a policy model the framework
is seriously deficient in its handling of interest bearing government
debt and that this deficiency affects the analysis of the monetary sector
in important ways. Among these, are the analysis of the changes in money
and in interest rates and the conditions required for equilibrium in the
markets for money and debt, the analysis of the effect of fiscal policy and
of the interaction between monetary and fiscal policies. Next I will
discuss the demand equation for money and discuss particularly the extent
to which the evidence supports the analysis contained in the paper. Finally,
I will comment on the usefulness of the model as a policy model by looking
at the extent to which the model -- had it been available -- might have
prevented major errors in monetary policy in the past. In general, I will
not discuss particular equations since there are too many and are in the
process of revision.

A Review of the Financial Model Circa 1963

My first task as discussant as I saw it, was to ignore many equations
that were included to make the housekeeping tidy, or perhaps messy, and
to skip over the details of particular equations and irritations so that
I might get to the "heart of the matter." One part of my assigned task
is getting to the heart of the financial sector. This is a less formidable
task by 60 or 65% since the financial sector includes only 33 or 36 equations
depending upon whether one counts the cost of capital as a "financial"
variable. In seeking the core of the hypothesis, I was guided by the state-
ment in the authors' November 1966 outline that the model was "constructed
along the lines of Modigliani's Monetary Mechanism (1963)," so I turned there for guidance.

I would like to be able to say that the authors' suggestion greatly simplified my task. In fact, the statement left me puzzled and I propose to occupy the next few minutes with some of the reasons why. It may be that the statement referring to Modigliani of vintage 1963 was mainly a guide for discussants of the non-financial sector and not intended as a guide for me.

The financial sector of Modigliani's 1963 paper contains six equations. Of these one states that the quantity of money supplied in nominal terms is equal to the quantity demanded of nominal money; a second equation states the stock of government debt is held either by banks or by non-banks; the third equation presents the balance sheet of a simplified banking system and the fourth defines the quantity of money as the sum currency plus total deposits. The main things to be noted about these equations that are of interest for later developments is that the banks earning assets consist only of interest bearing government debt and that the monetary base as well as its components -- reserves and currency -- are taken as given. There is no demand function for currency and no demand function for bank reserves. Moreover, as I note below the banks are not given a demand function for government debt (or earning assets), they simply absorb in their portfolios that portion of the stock of outstanding government debt that the public does not want to hold.

It is hard to see how a financial sector could be built on these equations, since the financial sector in this model exhibits no behavior but responds passively to the whims or decisions of the public
expressed in the remaining equations of the monetary or financial sub-sector.

The fifth equation of the 1963 paper is the public's demand equation for government bonds. When this equation is combined with the four equations just discussed the stock of interest bearing government debt held by banks and the money supply are uniquely determined. I want to stress that in Modigliani's vintage 1963, the only equation capable of serving as a basis for analyses of the stock of money is the demand for government debt by the non-bank public. The banking system responds passively, absorbing government bonds that the public does not wish to hold on one side of its balance sheet and issuing deposits on the other. The dependence of the stock of deposits — and hence of the stock of money — on interest rates, wealth, income, employment and so on reflects the appearance of these variables as arguments in the public's demand function for interest bearing debt. No distinction is made between short- and long-term interest rates. There is only one interest rate which enters the saving and investment equations as well as the demand equations for bonds and for money. The time to maturity is suggested by the argument that "bonds are regarded as one-period loans or claims to future (next period) money" (p. 80). Individuals can go into debt, however, by selling short some amount of bonds. Again, the banking system passively absorbs these bonds and issues deposits. This is the only form of lending permitted.

The sixth and remaining equation of the monetary sub-system is the demand equation for money. This equation makes the quantity of nominal
money demanded homogenous of degree one in prices and in the value of financial assets, so the quantity of real cash balances demanded is a function of real income, real wealth and an interest rate. As Modigliani notes (1963, p. 81) most of the equations in his monetary sector are redundant since the system can be collapsed into a single equation, the demand equation for money as in 1944.

Comments on Earning Assets and Loans in the 1967 Model

Since the public's demand equation for bonds was the only behavior relation on the supply side as of 1963, I expected that the revised and expanded version of Modigliani's Monetary Sub-system would develop and spell out this relation between interest bearing government debt, non-interest bearing government debt, money and bank earning assets in a way similar to the one that Brunner and I used in our "Credit Market Theory of the Money Supply" (1967). But to my surprise there is not a single variable called government securities among the 124 plus 54 variables denoted as endogenous and exogenous respectively in the May paper. The closest we come is a variable Vbim which includes government bonds held by commercial banks along with mortgages, consumer loans, and bank earning assets other than business loans. This variable is a catch all, and for our purposes, it is important to note that some of the components of the variable, mortgages for example, are at least related to one of the variables, mortgages plus mortgage commitments for which there is one or more structural equations. But there is no equation "explaining" either the public's or the banks' demand for government securities, and
there is no equation expressing the relation between the cumulated
government deficit and the stock of the securities outstanding. Indeed,
as far as I can see, there is no relation explaining anything about the
stock of government debt including how the government debt is distributed
between the banks and the non-bank public. In this section, I will
argue that there are at least three main reasons why the failure to
specify relations for the stock of government debt is a serious omission
that damages the model. One reason bears directly on the analysis of the
effect of monetary and fiscal policy. A second is the treatment of the
money supply process. The third is the effect on interest rates. The
three are of course related, since they are the result of the same error.

Stripped of a great deal of complexity, many — but not all — of
the operations that we call monetary and fiscal policy operations can be
viewed as simple exchanges. The government, including the central bank,
issues interest bearing or non-interest bearing debt in exchange for goods
and services or in the case of an open market operation, the government (again
including the central bank) exchanges interest bearing for non-interest
bearing debt. The interest bearing debt is the stock of government securities,
and the non-interest bearing debt is the monetary base, reserves plus currency
minus member bank borrowing from the central bank. If the government acquires
additional goods and services and finances the purchase by issuing
additional interest bearing debt in exchange, government consumption is
higher, interest rates are higher, and present consumption in the private
sector is lower. On the other hand, if government finances it purchases
by increasing the non-interest bearing debt — that is by exchanging base
money for commodities or services — the government's consumption is higher, but since interest rates are not higher, there is no reason for the private sector to exchange present for future consumption. In the new equilibrium position that the economy reaches, the price level is higher than in the case of debt finance, but the interest rate is lower. Because the public is not required to hold as large a stock of interest bearing debt, it can hold a larger stock of real capital for the same level of real income and wealth.

My argument reaches a conclusion quite similar to the conclusion reached in Metzler's famous paper, in Modigliani's 1963 article that I cited previously and in other recent literature. It is for this reason among others that I insist that the main lines of the present analysis do not, as asserted, follow his 1963 discussion in which the only visible part of the money supply mechanism was the public's demand to hold interest bearing government debt. Of course, I expect Modigliani to reply that since consumption depends on the net worth of consumers and on the rate of return on consumer assets, the effect I have just described works through the consumption function and the result I have described is achieved. This may or may not be true. I find no place in the model where government debt is issued except possibly as a residual and no place when it is added to the net worth of the private sector. At best the result is indirect, and is achieved through some channel that I could not trace. The only equation that I found for net worth made current net worth a function of current saving and other variables. No doubt there is some means by
which the government's debt is absorbed by households, since the model has an equilibrium condition that makes total expenditure of the economy equal to total receipts. But I require enlightenment about whether all the newly issued government debt must be absorbed by households or whether it also goes to banks. If banks can acquire interest-bearing government debt, how do the authors use two residuals to allocate the debt between two sectors?

Less the point be dismissed as a fine point of no "practical" significance in a short-term policy model, let me add first that I find no way for the interest-bearing government debt to affect market interest rates even temporarily. Second, I am not sure what the Federal Reserve is expected to acquire or sell in an open market operation. To give a few more examples of the more "practical" effects of the absence of government debt in the model, I do not believe the model can explain the failure of the bond support policy to generate inflation from 1947 through 1949. One reason that inflation did not occur at that time (in fact there was a slight deflation and a recession during the period) was that the Treasury ran a surplus and retired interest-bearing debt. This lowered market interest rates and thus reduced the amount of base money that the Federal Reserve was required to issue in order to maintain the pegged level of market interest rates. In fact, the Federal Reserve reduced the monetary base during part of the period, including the early months of the 1949 recession.
Another recent period in which operations affecting interest-bearing and non-interest-bearing debt are of importance is included in the period used to estimate the parameters of the present model. During 1962-64 the U.S. was able to expand the money supply while maintaining nearly constant short-term market interest rates. This was done by running a deficit in the government budget, financing the deficit by issuing interest-bearing debt and using open market operations to maintain short-term market rates. The main reason that the policy succeeded in expanding output was that the money stock increased and the budget was expansive; a main reason that the policy maintained short-term market interest rates despite a very high rate of increase in the monetary base and in the stock of money is that the stock of interest bearing government debt held by the public increased at a relatively fast rate. I cannot understand how the Federal Reserve-MIT model can explain movements in short-term interest rates without assigning some role to changes in the stock supply of interest-bearing government debt. Perhaps something in the equation for interest rates is highly collinear with the stock of government debt during the period. If the model is eventually used by the Board's staff, and if my argument is correct, I hope that whatever it is, remains collinear.

This brings me to my third comment on the effect of ignoring changes in interest-bearing government debt: the effect of this
omission on the analysis of the stock of money. The problem here is that I am very unsure about the direction in which to proceed since the authors' complete model given in the April memo is not exactly the same as the one used to obtain parameter estimates. The money supply equation developed in the later more complete paper relates the change in total earning assets net of capital accounts to various interest rates, policy, and other variables. On the basis of the April memo, one could expect to substitute the time deposit equation and the amount of member bank reserves minus member bank borrowing to get the supply equation for demand deposits. But in practice, this turns out to be too difficult partly because Shapiro and Rasche did not present estimates of an equation for time deposit that looks very much like the one in the April memo and partly because some of the variables in the equation explaining the change in the earning assets appear in combinations that cannot be easily separated.

The solution equation of the money supply-bank earning assets system makes the change in earning assets depend on changes in policy variables, changes in interest rates, changes in time deposits and changes in commercial loans. The last term, changes in commercial loans, is of course included on both sides of the equation. In fact, I think earning assets are equal to commercial loans plus the catch all variable Vbim minus bank capital, but I am not sure because the notation used in the empirical section has been changed and as I have noted earning assets are not defined
in the same symbol dictionary that contains $V_{bim}$. The justification for including the change in commercial loans on the right hand side of the equation explaining the change in earning assets is somewhat vague but makes reference to some theory of dynamic processes and adjustments that is not clearly spelled out.

The reason that the definitional problems to which I referred a moment ago are important is that changes in earning assets are made to depend on changes in commercial loans. One dollar of additional loans does not increase earning assets by one dollar, only by seventy cents, presumably because banks sell securities or other earning assets to non-banks. Government securities are the principal assets banks might be expected to sell under these circumstances, but as far as I can find in the model there is no equation that explains who buys the securities and no equation that indicates that anyone desires to buy them. There is, as I have emphasized, no demand to hold government securities and no flow supply equation for such securities. This makes things difficult to figure out when one of the residual holders -- commercial banks -- decides to sell government securities to the other residual holder. The same kind of mystery surrounds a decision to sell state and local government securities since these are treated in an analogous way.

If you recall the earlier part of my discussion, you will remember that Modigliani, vintage 1963, based the entire monetary
mechanism on the public's desire to hold debt. The only way in
which the public could borrow from banks was by selling securities
short. When we meet the revised model four years later matters
are entirely different. The deficiency in the 1963 version of the
model was that the banking system responded passively to the public's
desire to buy or sell government debt. Now neither the banks
nor the non-banks desire to hold the government debt and the
public's demand for debt is restricted to (1) an equation relating
changes in business borrowing to interest rates and other variables
and (2) some equations for the mortgage market.

To bring out my point let me contrast the treatment of a
mortgage market and the treatment of the banks earning asset port-
folio. In the mortgage market, the rate on mortgage loans and the
volume of mortgages are jointly determined, (given some other
variables) by the interactions between mortgage lenders who
supply mortgage loans and hold mortgages and by the public which
demands mortgage loans and commitments. The quantity supplied and
demanded determine a rate on new mortgage loans and the addition
to the outstanding stock of mortgages.

If the market for bank earning assets was treated in the same
way, we would have one or more bank demand functions for loans
to the public, a demand to acquire securities by the non-bank
public, a supply equation of loans offered by the public to the
banks and a demand to acquire securities by banks. Equilibrium
on the market for bank earning assets would then require first
that the volume of loans of all types demanded by banks equal the volume of loans supplied by the public and second, that someone hold the existing stock of government securities plus new additions to that stock.

From the stock equilibrium conditions

\[ L_b = L_p \]

\[ S_b = S - S_p \]

where \( L \) is the stock of loan and \( S \) is the stock of securities and \( p \) and \( b \) indicate that the holders of these stocks are banks or non-banks (i.e., the public). It is clear that we can sub-divide \( L \) into mortgage loans, commercial loans, etc. if such detail is desirable and manageable. Each new sub-division requires an appropriate specification of behavior relations for the banks and the public and permits another market rate and another stock to be proximately determined. At present, I am concerned with combining, not sub-dividing. By adding \( L_b \) and \( S_b \) we have the banks' demand for earning assets and on the other side the public's supply of earning assets to banks. This framework states some equilibrium conditions which can be used along with some behavior relations to proximately determine interest rates on bank earning assets. The underlying process views the determination of interest rates on bank earning assets as a result of the interaction by banks and non-banks on the credit markets in much the same way that the Board of Governors-MIT model proximately determines interest rates on mortgage loans and the volume of mortgage loans.
Once the stock of bank earning assets (net of capital accounts) or the change in this stock is proximately determined, we can use the balance sheet equation of the banking system

$$\text{EA} = \text{D} + \text{T} + \text{A} - \text{R}$$

and the uses statement of the monetary base \(B = \text{R} + \text{C}\) to obtain

$$\text{EA} = \text{D} + \text{T} + \text{C}_p - (\text{R} - \text{A} + \text{C}_p),$$

that is, to obtain an equation relating bank earning assets (EA) to the money supply plus time deposits. To obtain the money supply defined as currency \(\text{C}_p\) and demand deposits \(\text{D}\), we need equations for time deposits \(\text{T}\) and for currency as the authors of the present paper make clear.

The framework I have just outlined and the behavior equations that I have not stated are the result of joint work that has been developed in several papers with Brunner. When a demand equation for money is added to the system, we have a set of equations determining the stocks of money, bank earning assets and interest rates looked at as the result of a market process responding *inter alia* to changes in the stocks of interest-bearing and non-interest bearing government debt. This would seem to be the minimum requirement for model of the financial sector designed to answer questions about the effects of monetary and fiscal policy operations on interest rates, bank credit and money.

I have now returned to the point at which I started some time ago. Because the Federal Reserve-MIT model ignores the effect of changes in interest-bearing debt, it is deficient in an important respect. In the equations of the monetary sector, the relations
for money supply, bank earning assets and interest rates are mis-specified.

Another troublesome feature in the discussion of the financial sector is the authors' insistence that the supply and demand equations for money depend upon -- and determine -- a short-term interest rate, not a long-term rate. The authors, however, develop the money supply equation from the banks demand for earning assets, as I have emphasized. Surely the authors do not maintain that the banks portfolio of earning assets is independent of time preference, i.e., independent of the public's choice of consumption paths. Since they maintain that the banks' earning assets depend on changes in business loans, time preference and long-term rates -- including yields on real capital -- affect the banks earning assets through the demand for loans. The authors' analyses makes the money supply depend on long-term interest rates and real yields on real capital. The argument about solving the supply and demand equations for money is a decision that is entirely arbitrary and bears no relation to the authors' stated hypotheses. I will return to the point about short-term rates in the discussion of the demand equation for money.

I have emphasized that the model of the money stock is developed starting from the banks' earning asset equation, rather than the reserve equation. However, the authors carry along some of the baggage that has developed around the framework they no longer
use. It has become customary in places such as Yale and MIT to talk about the demand for bank reserves as a problem of synchronizing receipts and payments. The authors introduce this picturesque terminology into their discussion. The only meaning to be assigned to the notion is in the context of a two-step procedure à la Tobin and his followers under which banks divide their portfolio into long-term and short-term assets and then subdivide short-term assets into short-term government securities and excess reserves (or cash assets.)

The use of the procedure might be justified if there were a substantial difference in the effect of open market operations using short-term securities and open market operations using long-term securities. Under the two stage procedure, the former would not affect the sum of cash assets plus short-term securities, so they would affect the economy only (if at all) by changing the term structure of interest rates. On the other hand, open market operations using long-term securities change interest rates and the stocks of reserves and long-term securities. MIT-Federal Reserve model retains the language but not the entire substance of this two stage approach. If open market operations must work through short rates to long -- as the authors suggest -- I presume the Federal Reserve can accelerate the effect of open market operations by buying long-term securities, provided, of course, that there is some holder who can be induced to construct an offer curve for
the securities he never wished to acquire.

Before leaving this section of the model, I would like to note that I have doubts about another feature of the monetary mechanism. According to the model, mortgage loans and mortgage rates are influenced by the corporate bond rate, but there is no feedback from mortgage rates to corporate rates. This seems surprising. It is even more surprising to learn that the only apparent way that mortgage rates affect commercial banks is after a lag. Then changes in the rate on pass book savings accounts are affected and after another lag posted pass book savings rates affect the amount of time and pass book savings. It is worth noting also that the multiplier of the dollar of bank reserves is assumed to be equal to the reciprocal of the reserve requirement ratio against demand deposits and independent of currency spillovers and the reserve requirement ratio against time deposits.

The Demand Equation for Money

I turn now to the demand equation for money. The theory on which this equation is based starts as a theory relating the amount of money demanded to a vector of short-term interest rates and to current income. This approach is called the neo-Fisherian approach because, according to the authors, income really doesn't mean income; income means transactions. I have no objection to the authors arguing that (1) the demand for money depends on transactions and (2) the best available measure of transactions
is income if those statements are correct, but I don't understand why we are supposed to accept them as true or meaningful statements or why Irving Fisher must be invoked to bear the brunt of the argument. What matter if income is called transactions rather than income? Fisher's arguments about short-term changes in the demand for money were largely confined to discussions of the effect of price changes on velocity changes while the separate effect of price changes has disappeared in what is now called the neo-Fisherian approach.

These preliminary matters aside, the authors proceed to develop a framework. The basis of the framework is the payments schedule. We are told that the demand for money "arises from the lack of synchronization between receipts and payments and the transaction cost of exchanging money for short-term assets." The per unit cost of transactions is then assumed to be constant, and, we are told, transactions on asset accounts are neglected for the moment. The moment turns out to be a bit longer than expected. Asset transactions are brought in, found to be "slightly significant," and dismissed because they would be inconvenient to handle. My difficulty with this framework is that I am not sure what is being said and what is being excluded. Let me state why.

First, since money and short-term securities are assets, I don't know precisely what the authors mean when they say that asset transactions are neglected. Second, variations in interest
rates in a "neo-Fisherian" world must reflect changes in time preference, at least some of the time. Hence, I would have expected the approach in this paper to emphasize asset transactions and to look at money as the part of a portfolio of assets which is held because it is productive. Third, one of the services that money holding provides is that money permits individuals to vary their payments schedules and thus to reallocate other assets in their portfolios. The authors do not insist that the payment schedule is fixed. As I understand their approach, individuals make two types of adjustment: (1) they change their notion about the stock of money they want to hold as a consequence of changes in interest rates and income and (2) they change their desired rate of payment, i.e., they change the payments schedule.

Once individuals are permitted to adjust their payments schedules, little or no content is left in the statements "money is held solely or mainly to bridge the gap between receipts and payments," and the demand for money must depend upon income and short-term interest rates and not on wealth and long-term interest rates. I expect some individuals in a "neo-Fisherian" world to borrow to hold capital, and to reduce current payments relative to current receipts while others lend and reduce current receipts relative to current payments. This is all part of the process by which wealth is maximized. Once an individual is permitted to choose the time for making payments (i.e., to vary his payments schedule)
he can change the time path of his receipts. Each individual then alters his payment and receipts schedule and searches for optimum payment and receipts schedules. The optimum change in money balance is the difference (positive, negative or zero) between optimal receipts and optimal payments; the desired stock of money is the cumulated sum of these changes. I can think of no reason for an individual not to consider the rate of return on long-term securities, his own rate of time preference and his current wealth when choosing his current schedule of payments and receipts. This is why Brunner and I believe money depends on wealth, as we have indicated in varying detail on several occasions. I do not find any of the arguments for short-term interest rates and current income very convincing in the context in which they are presented in this paper.

Let me put these points in another way. The usual defense of the so-called transactions demand for money follows the Baumol-Tobin argument in which payments schedules are fixed, receipts are known (in fact, are given) and are unchangeable. The circumstances under which the conclusion of the Baumol-Tobin analysis hold are restricted therefore by the conditions under which they have been derived. In the contemporary U.S. economy, this means that the conclusions apply to money holdings within the day, or perhaps the week, since on the average individuals have receipts at least every two weeks and business firms have more frequent receipts. It is
hard to accept as relevant a macro economy in which receipts are fixed and payments are made at a steady rate. Someone has to receive payments.

These deficiencies in the theory as developed by Baumol and Tobin should — and do — carry over to the empirical results one expects to get if one tries to use short-term interest rates and current income as arguments in the demand equation for money. The authors do not do this, of course. By introducing lagged adjustments of the desired money balance and of the payments schedule, the measure of income becomes an exponentially weighted average of past income. Since the authors themselves agree that it would be difficult — if not impossible — to separate the exponentially weighted average of past income which is used to compute permanent income from the measure of income in their theory, it is not clear to me that their hypothesis is supported by their estimates. Moreover, their theory calls for an exponentially weighted average of short-term interest rates, rather than the current short-term rate. I do not know which of these the authors used in the regression, but I do know that the authors (and many others) argue (for example, in the paper on term structure by Modigliani and Sutch) that long-term interest rates are proximately determined by a weighted average of past short-term rates, so I cannot see a way of discriminating between short-and long-term rates if the weighted average was used in the regression. In any
case, the authors add the interest rate on passbook savings deposits as a variable in the money demand regression. I believe that such deposits are regarded as intermediate or long-term "bonds" by most holders of passbook savings. One piece of evidence in support of this view is the response of these savers to the introduction of the so-called "magic fives" a few years ago.

All of this brings me to three questions. First, why do the authors talk about the demand for money as a transaction demand or argue that money is held only to bridge the gap between receipts and payments when through a cumbersome argument they reach an equation that differs little if at all from the one they would use if they started with wealth adjustment? Second, why do the authors now reject the view clearly stated by many economists and by Franco Modigliani in his 1963 article which makes the demand for money depend on wealth? Third, why have the authors abandoned the deLeeuw demand equation for currency and deposits which seems to me more consistent with the arguments advanced in the recent literature on money?

The Model as a Policy Model

There are numerous other issues on which discussion with the authors would be desirable, and I hope we will be able to get to them in the discussion period. I want now to consider the model as a policy model and to question its usefulness in that role. I will discuss the issue of policy by asking another question: Would the model have prevented major monetary catastrophes or prevented
policy makers from making major errors if it had been available in the past and if it had been used?

To answer a question as broad as that one, we need to have some notion about the principal cause of past errors. I have stated in a number of places and am prepared to defend the proposition that the main cause of major errors in monetary policy is the belief that low or falling short-term market interest rates means that monetary policy is "easy" or "easing" and that high or rising short-term market interest rates means that policy is "tight" or "tightening." This belief led the Federal Reserve astray in periods as varied as 1929, 1937, 1938. For that matter, the belief led the Federal Reserve astray in 1966. On each of these occasions, the Federal Reserve insisted that credit and money were "easy" because short-term market rates were low or falling. The fact that the money supply was declining or not rising was treated as a fact of negligible importance or ignored. The minutes of the open market committee make it very clear that during each of the earlier periods -- and most emphatically during 1929 to 1933 -- the open market committee was fully aware of the catastrophe that had occurred but most often failed to take action because conditions they then called "credit conditions" seemed "easy."

The belief that short-term interest rates indicate the prevailing policy position explains just as well why the Federal Reserve eventually engaged in expansionary monetary policy during the recessions of 1923–24 or 1926–27 or for that matter 1953–54. This belief
explains also why the Federal Reserve permitted excessive increases in the money supply during early 1966 when inflation was clearly a threat. In each case the level of or change in short-term market interest rates explains whether the policy was described as "easy" or whether it was described as "tight" and whether or not open market operations were used to raise or lower short-term interest rates. Less these statements be misinterpreted, I want to be clear that I do not assert that the Federal Reserve did not look at, talk about, ponder, discuss, weigh, and consider other variables. They did. I assert that we can predict whether or not the Federal Reserve engaged in expansive, contractive or no policy operations by looking at the level of -- or direction of change in -- short-term market interest rates. On this interpretation, the main failures of monetary policy resulted from the belief that when short-term interest rates were low or falling, monetary policy was "easy" or "easing" and that when short-term interest rates were "high" or "rising," monetary policy was "tight" or "tightening."

My answer to the question about the main cause of policy errors must, therefore, lead me to ask one of two additional questions. First, does the MIT-Federal Reserve model permit the Federal Reserve to separate the effects of their policy operations on interest rates from the effects of market variables and other policy variables, for example, fiscal policy variables? Second, if not, does the Federal Reserve-MIT model imply that the Federal Reserve
should watch some variable or variables other than interest rates to gauge the future effect of the policies they are pursuing?

I have time to pursue these questions only to a limited extent. The first question, however, has been partly answered above. I have argued at some length that the Federal Reserve-MIT hypothesis does not incorporate a demand function for interest-bearing debt and treats changes in the stock of debt as a residual. The model therefore ignores the effect on market interest rates of changes in interest-bearing debt. The argument that the model builders looked for, but did not find, any effect of changes in interest-bearing debt seems to me to be beside the point. In general equilibrium models of the Metzler type, it is the change in interest-bearing debt held by the public — and not the change in money that causes interest rates to rise or fall. This view is accepted in the Modigliani model of 1963, as I emphasized previously. I cannot but conclude that — as a model of interest rate determination — the MIT-Federal Reserve model is deficient and likely to be misleading. The variable that occupies the most important position in the Federal Reserve's traditional analysis of the monetary system should not be treated so cavalierly by the staff charged with or assigned to the development of the "Board's model."

My second question asks whether the Federal Reserve-MIT model tells the Board of Governors or the Open Market Committee how to separate the future effects on the endogenous variables of the system
that are caused by their policy operations from the effects of changes in fiscal and non-controlled variables. This is equivalent to asking if the model answers the question, has monetary policy become more or less expansive? Before taking additional policy action, it is generally useful to know whether the action you have just taken is expansive or contractive, inflationary or deflationary. This is the problem that Brunner and I have called the indicator problem. At the UCLA Conference on Indicators, one of the builders of the present model claimed that once his model was built he would have no need for an indicator of monetary policy. Since that time, the Open Market Committee has adopted the bank credit proxy as its indicator.

I hope the model builders will explain two things. First, on their analysis, is the bank credit proxy the optimal (or even a good) indicator for the Federal Reserve to watch? Second, how do they know or expect to determine whether the policy has become more or less expansive? In short, I challenge the model building team to explain how they answer the question: What is the current direction of monetary policy?

This brings me to the last comment I have time to make. We now have a number of econometric models. While these models differ in many respects, they agree in at least one important respect. In each of them, monetary and fiscal policies do not have much effect on output, employment and prices for several months or quarters. In each of the models, the length of time required before the effect
of policy changes is felt varies from cycle to cycle. This model is no exception. Most of the response of the change in deposits to a change in reserves occurs within a quarter, but it takes three or four years for the change in deposit velocity to fully reflect the change in interest rates set off by the policy operation. Moreover, final adjustments are complicated by the fact that undoubtedly price changes affect output and conversely, so there are, many reasons to believe that the lags are not only long, but more importantly, the lags are variable.

What should a policy maker do when faced with a variable lag? If the effect of policy is felt at an unknown future time, what is the point of sharp, sudden changes in monetary policy that swing the rate of change in money supply from plus 6% to minus 3% and back to plus 6% within a year as in 1966-67? Are such policy changes stabilizing or destabilizing? How does the econometric model help us to answer this question and what answer does it give? If the lags are variable -- as they appear to be -- is it not likely that even when he is armed with an econometric model, the policy maker like the proverbial general will frequently find himself fighting the last war, in this case, taking action to fight inflation when there will be unemployment in the future and fighting unemployment when the problem for the future is inflation. In short, have the large-scale econometric models provided a firm foundation for the highly variable policies that the monetary policy makers and
their staff seem so desperately eager to pursue? Or, have these models produced overwhelming evidence that frequent large changes in the size of policy operations are as likely to destabilizing as stabilizing?

Conclusion

It is the task of the discussant to raise questions, to express doubts, to criticize and to find weaknesses and suggest alternatives. In this role, it is easy to appear negative and to give the impression that the work discussed is of limited value. That was neither my desire nor my intention.

 Few people would have attempted to construct a model of this size and complexity in so short a time. Fewer still would have completed the task. None I venture to say would have done as much in so short a time as Franco Modigliani and the MIT-Federal Reserve group. It is a tribute to them that there is a model to be discussed and that it is sufficiently rich and explicit that there is something of consequence to be discussed. I hope that in the limited time that I have had available to prepare these remarks I have lived up to the standards that they have set.