WHAT have we learned from the Commission on Money and Credit about processes and responses in monetary policy? So far as I can see, from the Report itself, very little — but perhaps experts in monetary economics were not a significant part of the audience at whom the Report was aimed. So the most fruitful course may be to turn primarily to the staff papers.

Here there is substantial evidence of the quantity theory reborn — at least in the sense that money matters a good deal in determining aggregate spending on current output — and of an important postwar change in monetary theory. As noted in my introduction to this volume, this postwar shift in theory is centered in the "portfolio" approach to the demand for money and other assets. In this theory, the channels through which variations in the money supply affect the levels of income and employment include not only a change in "the interest rate" but also changes in relative prices of all assets — real and financial — which in turn lead to shifts in spending on existing assets and currently produced goods and services. Thus in its extreme form the new approach suggests that we need to look not at one interest rate but at an extremely large number, including the implied interest rates on all real assets and including consumer goods of any degree of durability.

In logical terms, this approach offers an elegant rapprochement for the devotees of the "Keynesian" and the "quantity-theory" approaches to the role of money, and for the monetary-versus-fiscal policy disputes since the 1930's. If the new look prevails, we may look back at much of this controversy as a good deal less significant than it has seemed en route. The great controversy over "Is savings really equal to investment?" of the late 1930's and early 1940's springs to mind.

But agreement on this mechanism doesn't necessarily tell us how important money is quantitatively. If we look at the staff papers (or at least, at the sample I managed), what does the professional support for the renaissance of money and monetary policy amount to? Friedman and Meiselman, as might be expected, plump for money as a prime determinant of the level of spending on current output, and show convincingly that a simple, traditional monetary model versus a simple traditional Keynesian model test gives the verdict clearly to stability for velocity over stability for the ratio of autonomous investment to income, including cases where reasonable lags are introduced. Moreover, they go on to spell out the portfolio balancing mechanism as at least a plausible mechanism through which this monetary effect may be exerted. If we take Section VI of their paper as a statement of the "new monetary orthodoxy," on intellectual grounds at least a good deal of the basis for the long quarrel between the monetary and the Keynesian economists has been reasoned (or compromised) away. Few, even the most ardent neo-Keynesians, would disagree that the impact of open market operations may be through the spreading net which Friedman and Meiselman spell out — not merely through one (bond) interest rate alone acting on "investment" decisions. The major challenge to the now generally accepted fiscal policy position as our really powerful stabilization tool becomes a strong one if a reasonably stable demand for money is added to the new mechanism — as at least Friedman and Meiselman argue. The C.M.C. staff papers contain no empirical answer to the Friedman and Meiselman challenge to show better results with another model.

Tobin, in his paper on debt policy, provides an elegant statement of a very similar mechanism through which changes in the money stock and liquidity may influence spending decisions on current output through the rebalancing of asset portfolios.

But I hope it will not be too dissonant a note to suggest that we really know very little empirically about the validity of this description of the channels of monetary policy; and that the elaborate portfolio-balancing general equilibrium approach lacks intuitive appeal
as a description of what most of us do in managing our own economic affairs, so that empirical validation becomes all the more important. Is it true that the interest rates that matter most include whole elaborate net of yields on assets in spreading the impact of more money? It would be reassuring if we had more behavioral evidence at the micro level to confirm the over-all empirical support advanced by Friedman and others on the new channels of monetary policy.

The staff papers are not barren on empirical evidence of the actual channels. Brown, Solow, Ando, and Kareken attack the lag problem empirically, and intriguingly seem to end up confirming the usability of active monetary policy to a higher degree than Friedman does, since they assert that the lag is probably only about six months. The multiple authors of this paper are modest in their claims for their results, and I fear I must agree that the results call for such modesty. Leaving aside the now well-argued statistical issue on comparing rates of change against levels, the Brown, Solow, et al. case for the six-month lag could have been stretched a good deal either way without much implausibility. And, on what to me is an even more important issue—the variability of the lag—neither they nor anyone else in the staff papers pretends to throw much, if any, light except on purely a priori grounds. In my judgment the verdict has to be: result still uncertain on even average length of lags, and more evidence desperately needed on the critical issue of variability.

Modest empirical knowledge is added on channels of impact by Meltzer, Okun, Friend, and Bach, who trace the spread of restrictive policy through the banking and certain nonbank sectors of the economy, with the conclusion that such restriction does not distort seriously the otherwise established patterns of market relationships. Friend produces direct data that the nonbank financial intermediaries pose less of a problem to effective monetary policy than Shaw and Gurley and others have claimed. Okun shows that monetary policy seems to act effectively on certain interest rates, and produces quantitative estimates of the changes in rates attributable to specific policy changes on the basis of recent experience. Brownlee and Conrad try to specify the channels through which alternative policies act in checking inflation and to quantify the results at different stages.

All this adds up to a good deal of support—theoretically and empirically—for reestablishing a major role for money and monetary policy. But I have an uncomfortable feeling that the foundation is not happily solid yet; the parallel of the great intellectual faith in money during the prosperity of the 1920's comes to mind. At the risk of sounding like a Monday morning quarterback, let me conclude by suggesting some questions the Commission's research directors might have asked and apparently did not.

(1) To what extent historically can we isolate cases where changes in the money supply were independent of the public's spending and output decisions? Given the agreed close correlation between the two, we need to establish whether changes in the money stock were really caused by changes in economic activity rather than vice versa. I suspect we may find that supply changes can be established as quite independent in many important cases, such as the big inflations and depressions, though much less clearly so in minor fluctuations. Cagan and Friedman may have these answers for us in their forthcoming volumes.

(2) To what extent can the monetary authorities actually control the stock of spendable money (currency and demand deposits) by controlling the total volume of currency and deposits, which is all they have within their proximate power through their control over reserves? Over long periods and with comparable interest rates, the relation of time deposits to total currency plus deposits looks pretty stable. But for short periods, the relationship obviously may be very unstable, as in recent years.

(3) Lastly, and perhaps most important, which model (in a sophisticated form that makes its proponents happy) really gives the best results—a basically "monetary" model or an "income-expenditures" model with money explicitly built in? Ultimately it is through development and careful testing of two or three more complex models against one
The new look in monetary theory is intellectually attractive. What we need most now is some more figures to flesh out the model.

COMMENT

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The Report of the Commission on Money and Credit does not, in my judgment, fully deserve the caustic criticisms directed at it by Martin Bronfenbrenner in his paper. It is true that the Report is quite fuzzy in places and that the analysis not infrequently bears little relation to the ensuing recommendations. It deals inadequately with policy objectives and glosses over the difficulties arising out of conflicts between objectives, and it is wishy-washy on the balance-of-payments problem. But, in spite of its weaknesses, I believe it is a valuable document which contains many useful recommendations. I happen to agree with the philosophy underlying the Report on such matters as the need for greater executive discretion in fiscal policy and on the choice of the proper mix of monetary and fiscal policies.

Some of the recommendations relating to the relaxation of portfolio and interest-rate restrictions applicable to financial institutions would, I suspect, be accepted by practically everyone attending this conference. If adopted, these recommendations would increase efficiency in the allocation of capital and perhaps modestly strengthen monetary policy. Some of us would want to go further than the Commission does and allow commercial banks to pay interest on demand deposits.

It seems to me that it is much too early to judge the impact of the Report on economic policy — indeed it may never be possible to assess its impact with much precision. Its publication was undoubtedly a significant factor in encouraging the Kennedy Administration to put before Congress proposals for increasing the President's discretion in fiscal policy, proposals which parallel quite closely many of the recommendations of the Commission. In addition, the President has appointed three interagency working groups to follow up and appraise the Commission's proposals relating to (1) regulation of banks and other financial institutions, (2) federal lending and loan guarantee programs, and (3) private pension funds. Even if the recommendations of the Commission are not immediately adopted, it is quite possible that the Report may help to set in motion a process of reappraisal which will eventually produce some important changes.

Processes and Responses in Monetary Policy

The conclusions concerning monetary policy do not appear to be based to any appreciable extent on new knowledge that was uncovered in the course of the study. The Commission surveyed the same ground that others of us have gone over, and it encountered the same difficulties. Since many of the issues are quite technical and since the Commission was, for the most part, composed of persons who are not specialists in monetary policy, one may suppose that the conclusions it arrived at were based largely on the analyses of the staff and the advisory board. Here and there the Report makes reference to shreds of new evidence, but — and I found this annoying — in no case was the precise nature or source of this evidence disclosed.

After expressing general confidence in the effectiveness of monetary policy, the Report goes on to recognize that, except in the area of residential construction, firm evidence of its effects is quite limited. As a consequence, the Report places considerable emphasis on the influence of monetary policy working through changes in the availability of credit and through changes in attitudes and expectations and on secondary interindustry and macro effects. The Commission's proposals for strengthening monetary policy include abandonment of the "bills only" policy, some streamlining of the admin-
two that present new evidence most relevant to the assessment of the effectiveness of monetary policy are a study of lags by Brown, Solow, Ando, and Kareken, and a quantitative study of the effects of monetary and debt operations on interest rates by Okun.

The Brown, Solow, Ando, and Kareken study breaks down the lag in monetary policy as follows: (1) the "inside" lag between the need for action and the Federal Reserve's response; (2) the lag between action by the Federal Reserve and the effect on interest rates and credit terms experienced by private spending units; and (3) the "outside" lag between changes in interest rates and credit terms and the effect on real output. The most notable contribution of the study lies in the effort to estimate the outside lag. The study deals separately with fixed investment in nonelectrical machinery and with inventory investment.

The study of fixed investment uses new orders for nonelectrical machinery as the decision variable that is influenced by interest rates — that is, the model is based on the rationale that interest rates affect orders with a lag and that production is adjusted to orders with a further lag. Several types of distributed lags having different time patterns are tried out, and new orders are found to depend on the index of industrial production, corporate profits, and the industrial bond yield. The interest elasticity of investment (at the point of means) is found to be about -0.4 to -0.5. However, the effects are somewhat stretched out in time, with about 45 per cent occurring in the quarter in which a change in interest rates takes place, 25 per cent in the next quarter, and around 15 per cent in the third quarter — thus, about 85 per cent of the total effect occurs within three quarters or so.

To the lag between changes in interest rates and changes in orders must be added the lag between changes in orders and changes in production. Again using a relationship containing


The study also deals with the lags in fiscal policy.
a distributed lag, the conclusions are roughly
that 17 per cent of the effect of a change in
orders is reflected in production of machinery
within three months, 30 per cent within six
months, and about 50 per cent within the first
year.

While these results are somewhat tentative,
they do strongly suggest (1) that the ultimate
effect of a change in the long-term interest rate
on fixed investment may be quite substantial,
but (2) that these effects are spread out over a
considerable period of time, thereby creating
a troublesome problem of timing for the mon-
etary authorities.

With regard to inventory investment, the
study employs a modified inventory accelerator
type of equation and indicates that the elastic-
ity of demand for inventories (total accumu-
lated change in stock) with respect to the
average interest rate on short-term bank loans
is about —.4. However, there is again a dis-
tributed lag involved, with the result that about
one-quarter of the total inventory investment
will take place within one quarter after the
change in the interest rate, about two-thirds by
the end of four quarters, and about nine-tenths
by the end of eight quarters. This again sug-
ests serious problems of timing in using mon-
etary policy to control inventory investment.

The Brown, et al., study has some shortcom-
ings, and the authors are careful to emphasize
the tentative nature of their findings. In the
case of fixed investment, the difficulties include:
failure to take account of supply conditions in
the capital goods industry, which would surely
condition the response of production to a
change in interest rates; and inability to make
allowance for the fact that the lags involved
will vary with the state of expectations and the
stock of inventories held by capital goods pro-
ducers. But since the lags in the effects of mon-
etary policy and the size of the responses that
occur are very important and interrelated mat-
ters, this study is representative of the type of
work that needs to be done if our knowledge of
the working of monetary policy is to be signifi-
cantly increased. In addition to further exten-
sion of this work on plant and equipment and
inventory investment, similar studies are need-
ed in other areas, such as residential construc-
tion, and state and local government expendi-
tures.

Okun's study is a quarterly analysis which
attempts to estimate the effects of various mon-
etary and debt operations on the short-term
interest rate (Treasury bill rate) and the long-
term interest rate (yield on long-term Treasury
bonds). The analysis is so formulated as to
permit quantitative estimates of the effects on
the two interest rates that would result from
open market operations in debt of various
maturities, changes in reserve requirements,
changes in the Federal Reserve discount rate,
changes in the level of income, and operations
by the Treasury or Federal Reserve that change
the maturity composition of the publicly-held
debt.

The most striking conclusion of the Okun
study is that debt management operations
which change the maturity composition of a
given debt have very weak effects. Several
alternative sets of equations are presented, but
a typical conclusion is that the retirement of
$1 billion of Treasury bills and the simulta-
neous issuance of $1 billion of 20-year bonds will
lower the bill rate by about two basis points and
raise the long-term rate by less than one basis
point. In general, the study suggests that the
maturity structure of interest rates is primarily
determined by the interest rate expectations of
private lenders and borrowers and is not very
sensitive to changes in the relative supplies of
debt of different maturities — a conclusion that
accords well with the results of other recent
work on the determinants of the interest rate
structure, such as that of David Meiselman.
As Okun points out, his study suggests that the
issue of "bills only" was not nearly so impor-
tant as many of us thought it to be.

In addition to its implications with respect
to the interest rate structure, the Okun study
also indicates that rather large monetary opera-
tions are needed to produce substantial changes
in the level of interest rates. For instance, a
typical result is that open market sales of bills,
notes, or other short-term issues in the amount
of some $6 billion will be needed to raise the
long-term interest rate by 50 basis points.
Nearly the same volume of sales of 20-year
bonds would be needed to produce the same
result. The conclusion seems to be, as Okun points out, that vigorous action is necessary if monetary policy is to accomplish much.

The studies by Brown, Solow, Ando, and Kareken and by Okun are certainly by no means the last word on their respective topics. However, they do seem to me to typify the kind of quantitative research that is necessary if our understanding of monetary phenomena is to be significantly improved.