

For Release
Friday, February 21, 1969
10:00 A.M., E.S.T.

Guidelines For Monetary Policy--
The Case Against Simple Rules

A paper delivered at the
Financial Conference of the
National Industrial Conference Board
New York City
February 21, 1969

by

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GUIDELINES FOR MONETARY POLICY--
THE CASE AGAINST SIMPLE RULES

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There are several things that seem worthwhile mentioning by way of a prelude to the substance of my remarks. First, I do not regard it as my function to defend, explain, or otherwise comment on the course of monetary policy during the past several years. My comments will be confined to the more general question of running monetary policy by simple rules, and what the empirical evidence seems to say about the issue. Second, of necessity, I must take the Federal Reserve off the hook for what I have to say. I could scarcely present a Federal Reserve consensus in any brief period without grossly misrepresenting someone's position, since there is at least as much diversity of view within the Federal Reserve as elsewhere on the appropriate guidelines for monetary policy. You might already have guessed that from reading the November 1968, Review of a certain Mid-western Reserve Bank, whose brand of monetary policy is known around the Board as Brand X.

Third, I do not intend to present a personal point of view on how a central bank should run its affairs. My function is to present sympathetically the case against simple rules in monetary management--and in particular the case against rules defined in terms of growth rates of the money stock, or related monetary aggregates. In this role, I find myself in something of a quandry.

* The views expressed in this paper are the responsibility of the author alone, and are not necessarily shared by the Board of Governors or by the author's staff colleagues.

Among my friends outside the Board, I seem to have developed a reputation, such as it is, for being an anti-quantity theory man, perhaps even a violent one. At the Board, on the other hand, I am not infrequently accused of having dangerous leanings in the opposite direction, since I have a habit of insisting that a yo-yo is not the appropriate physical analogy for monetary policy.

Fourth, since my subsequent remarks about simple rules and quantity theories will be rather critical, it seems appropriate to emphasize at the outset that the fields of monetary economics and stabilization policy, in my judgment, owe an enormous debt to Professor Friedman for insisting that the role of money as a determinant of national income be given more careful consideration than it was from the period of roughly 1935 to 1965. Apart from a few lonely souls such as Milton Friedman, monetary economists argued for about 3 decades that central banking was largely wasted motion, and sneered at those with contrary ideas. Professor Friedman fought for more careful attention to monetary variables when the going was the roughest--and he deserves our commendation.

The danger now is that the pendulum has swung too far in the other direction. Recognition of nonmonetary factors as a potential disequilibrating influence in the economy is in grave danger of being overlooked. An increasing proportion of economists, financial writers, and others appear to be reaching the conclusion that nonmonetary factors can be safely disregarded as important

potential sources of economic turbulence, and that fiscal policy is the wet noodle among our economic stabilization tools.

The case for discretionary monetary management starts from the premise that money matters, and matters a great deal. But other things can and do matter too--specifically, fiscal policy and changing propensities to spend in the private sector. The case also hinges on the assumption that we have learned enough about the sources and the nature of economic fluctuations to do something useful about them, and that the prospects for learning more remain bright.

Let me begin the defense of this case by discussing a grubby statistical problem. Technical arguments may be a little boring, but this one cannot be avoided if the evidence supporting the case for steady growth of the money stock is to be evaluated properly.

As you are well aware, one of the principal supports for the monetarist position is the empirical evidence of a relatively stable relation between money and income, or between changes in these variables--evidence of the kind represented by Professor Friedman's extensive studies or by the Andersen-Jordan paper in the November 1968 issue of the St. Louis Fed's Review. In the latter study, changes in GNP from 1952 through mid '68 are regressed on variables taken as proxies for monetary and fiscal actions, with the monetary variables alternately defined as changes in the money stock or in the monetary base i.e.,

currency plus total bank reserves. In the Andersen-Jordan regressions, fiscal variables turn out not to bear a statistically significant relation to changes in nominal income. The results, therefore, cast serious doubts about the role of fiscal policy as a stabilizing instrument and by implication on the significance of all nonmonetary factors as determinants of nominal income. Meanwhile, monetary variables come booming through as important determinants of GNP.

The problem with this study, and with others of its kind that I am familiar with, is that they are potentially biased, in a statistical sense, towards overemphasis of monetary factors as determinants of income. I use the word "potentially" advisedly since it is hard to prove one way or another, even though the nature of the argument is straight-forward. The argument runs as follows.

If the central bank sits on its hands and does nothing, a rise in GNP resulting from (say) an expansive fiscal policy tends to increase the money stock, mainly because it induces banks to borrow more from the central bank and to reduce excess reserves, but partly also because the induced rise in interest rates reduces demand for time deposits, and thus permits an increase in demand deposits and the money stock. The money stock is not independent, in a statistical sense, of current changes in GNP. Consequently, a regression of GNP on the money stock combines the effects of GNP on money with those of money on GNP. Regressions of GNP on money would not, therefore, yield statistically unbiased estimates of the effects of monetary

policy on the economy. Rather similar arguments hold if the monetary variable used is the monetary base.

On the other hand, if the Fed has not sat on its hands, but has behaved the way monetarists often claim, the potential bias in the historical data is much larger. Professor Friedman, for example, has argued that the Federal Reserve's inept performance in monetary management (as he sees it) results heavily from the fact that too often it leans against the trend of the credit markets--moderating upward pressure on interest rates during economic expansion, and cushioning the downward rate adjustments that occur in recessions. As a result, he argues, the money stock tends to accelerate or decelerate at just about the time it should be doing the opposite.

If you believe that story, it follows that regressions of GNP on the money stock, with or without other variables to represent fiscal policy, are biased even more towards overestimating the effects of monetary factors as economic determinants. Indeed, a close correlation between money and GNP could occur in those circumstances even if monetary policy had no effect at all on national income.

This problem of statistical bias is an old and familiar story--and monetarists as well as their critics are quite well aware of it. The question at issue, of course, is whether it is a serious enough problem to really worry about. I suggest that it is.

Consider for a moment the implications of concluding that fiscal policy has no discernible effect on money income, apart from

its effects on the money stock. This is the conclusion you would reach, presumably, if you accepted as reliable, and statistically unbiased, the evidence set forth in the St. Louis Bank article mentioned earlier, in which fiscal variables were found not to bear a statistically significant relation to money income. The properties of an economic system in which fiscal policy acts the way it does in the Andersen-Jordan model have been discussed in the economic literature for 100 years or more, and are reasonably well understood. It is widely known that fiscal policy would have no effect on money income, apart from induced changes in the money stock, if and only if the demand for money were completely interest-inelastic. And if that were true, changes in private spending propensities also would have no effect on money income, except through their impact on the demand for, or the supply of, money. Indeed, in such a world, the behavior of the money stock would completely determine the course of money income if the demand function for money were stable.

The demand function for money has probably been estimated statistically as many times, and perhaps more, than any single behavioral equation commonly used in economics. While the nature of the public's demand for money is not understood to anyone's full satisfaction, the empirical evidence accumulated over the past 10 to 15 years--of which a significant part comes from the monetarist camp itself--points overwhelmingly to the conclusion that the public's desired holdings of money balances are interest-sensitive. And this

is true whether money is defined narrowly to exclude time deposits of commercial banks, or broadly to include them.

In view of this, it seems to me, Andersen and Jordan should not have concluded that their regressions had satisfactorily sorted out the relative roles of monetary and fiscal policy as determinants of GNP. Rather, they should have concluded that something was rather badly wrong with their method.

As I noted, this bias problem is an old familiar one; nevertheless, precious little has been done about it until just recently. I commend for your reading, in this respect, a "Comment" on the Andersen-Jordan study by two staff members at the Board (Frank de Leeuw and John Kalchbrenner) to be published shortly in the St. Louis Fed's Review. de Leeuw and Kalchbrenner find that different results emerge from the Andersen-Jordan equations if the monetary and fiscal variables are redefined in such a way as to reduce the degree of statistical influence running from GNP to the policy variables. Most importantly, the monetary policy variable is redefined as the monetary base less the public's holdings of currency and member bank borrowings. With this definition, monetary factors decline in importance, and fiscal variables turn out to have significant effects on GNP after all. Also, the relative potency of monetary and fiscal policies resulting from use of the Andersen-Jordan equations, as modified by de Leeuw and Kalchbrenner, turn out to be in the same ball park as those

emerging from the larger and more elaborate FRB-MIT model developed by the Board staff working jointly with Professors Ando and Modigliani. Since the structure of the FRB-MIT model differs markedly from the Andersen-Jordan single-equation models, the coincidence of results would seem to be more than accidental.

Let me move now to the next point, which is that, even taken at face value, regressions relating GNP to the money stock (or relating changes in these variables) over the long sweep of history generally are quite consistent with the view that nonmonetary factors play a significant role in determining national income. In elaborating this contention, it seems appropriate to concentrate particularly on the empirical work of Professor Friedman, the leading advocate of the monetarist view.

An article of his published in The Journal of Law and Economics a couple of years ago discussed a simple regression equation relating annual changes in current income to annual changes in M_2 -- that is, the money stock defined to include time deposits. Friedman defines money this way for pragmatic reasons-- M_2 is more closely related to GNP, over the long run, than M_1 . What I have to say about the flexibility of the M_2 - GNP relation thus applies in spades to the relation between M_1 and GNP.

Friedman's equation, based on data from 1870 to 1963, shows a correlation between annual changes in M_2 and GNP of $.70\frac{1}{2}$. This means that half of the annual changes in nominal income are explained by contemporaneous changes in M_2 , and the other half are not. The significance of that degree of accuracy can be illustrated by considering what Friedman's equation says about changes in nominal income during recent years.

From 1962 onward, the equation predicts better than in earlier years. Given knowledge of the annual percentage change in M_2 and the previous year's income, it predicts levels of nominal income for the years 1962-66 with an accuracy of about 1-1/4 per cent. This is worth about \$11 billion in GNP, given the present size of the economy, an error that is not negligible when we are talking about average annual levels. Indeed, I suspect a prediction that GNP in 1969 will hit an annual average of \$921 billion (the CEA forecast) plus or minus \$11 billion would strike almost everyone in this room as unusually imprecise. But in the preceding 10 years--that is from 1952 to 1961--the predictions from Friedman's equation are far worse. The mean absolute error over the 10-year period is roughly 3-1/4 per cent, or about \$28 billion in terms of today's GNP. What would you do with a 1969 GNP forecast of \$921 billion, plus or minus \$28 billion?

1/ Milton Friedman, "Interest Rates and the Demand for Money," The Journal of Law and Economics, Vol. 9, October 1966, p. 78.

A 3-1/4 per cent average prediction error produces a strange picture of short-term economic developments during the 1950's. Annual percentage changes in current income predicted by Friedman's equation are about equal for the three years 1953-1955, though you will remember that income growth turned negative in the recession year 1954 and rose sharply in 1955. His equation also predicts an acceleration of income growth in the recession year 1958 and a slight reduction in the boom year 1959. And if its description of short-term economic changes leaves something to be desired, its longer-term predictions are even more astonishing. The predicted growth of nominal income over the ten years 1952-1961 as a whole is only a bit over one-half as large as the actual growth that took place.

If these results surprise you, they shouldn't, since there has always been a good deal of variability in the M_2 - GNP relation. The facts are there to read in Professor Friedman's Monetary History of the U.S. Annual variations of 3 per cent or more in the income velocity of M_2 are the rule, not the exception. They occur in 2/3's of the some 90-odd years covered by the study. Even if the first 12 years of this period of history are thrown out on grounds of unreliable data, as Friedman suggests, and if the years of the Great Depression and the two World Wars are also discarded, for reasons that are not so clear, annual velocity changes of 3 per cent or more still occur in more than one-half of the remaining years.

As I read the historical evidence, therefore, one of the two main pillars on which the monetarist position rests is a bit shaky. The second one strikes me as even less stable. It is the contention that the money stock should grow at a constant rate because, to quote Professor Friedman, ". . . we simply do not know enough, we are not smart enough, we have not analyzed sufficiently and understood sufficiently the operation of the world so [that] we know how to use monetary policy as a balance wheel."^{2/} Consequently, he argues, we ought to convert monetary policy from a factor that he contends has been positively destabilizing to one that is neutral.

The argument has intuitive appeal, but not much more. If we do not know how to use monetary instruments to offset the disequilibrating effects of nonmonetary factors, then we do not know enough to accentuate these effects either--or to judge whether the central bank has done so.

To strike an analogy, Friedman's argument is that the central bank is like a person lost near the edge of a forest, with insufficient evidence as to the shortest way out.

Friedman advises the wanderer to stay put, since otherwise he may wander deeper into the woods. He may, but then again he also may wander out. Friedman's advice is sound if the wanderer can

^{2/} "The Federal Reserve System after Fifty Years," House Banking and Currency Committee, 88th Congress, Vol. 2, Hearings, 1156.

be reasonably sure that a rescue party is on the way. But if there is no rescue party, the poor lost soul might just as well start walking-- he might just stumble onto some tracks that lead him home.

The point I am making is perhaps obvious, but I did not originate it. The credit goes to Professors Lovell and Prescott, who deal with the question at considerable length, and in a theoretical fashion, in a recent article.^{3/} They conclude that in the absence of knowledge about the strength and timing of monetary changes, it cannot be demonstrated that a policy rule specifying a constant growth rate of the money stock is superior, in terms of smoothing out income fluctuations, to a rule specifying that interest rates be stabilized. Also, one cannot demonstrate the superiority of either rule over any specific set of policies pursued by the central bank.

Rational conduct of monetary policy--whether by the pursuit of rigid rules or by allowing central banks substantial discretion in deciding the course of monetary affairs--cannot be specified if we assume complete lack of knowledge. Our understanding of how the economic system works is imperfect, and we must recognize that an optimal policy strategy has to take uncertainty into account. But we must begin with what we know, and build on it. The Lovell-Prescott approach is an excellent example of one direction of fruitful inquiry.

Perhaps I am a hopeless optimist on this score, but I think we have learned a great deal in the past ten years or so about

^{3/} Michael C. Lovell and Edward Prescott, "Money, Multiplier Accelerator Interaction, and the Business Cycle," Southern Economic Journal, Vol. 35, July 1968, pp. 60-72.

the use of stabilization policy--and particularly monetary instruments. The most hopeful sign, in this regard, is the fact that we are gradually whittling away the wide diversity that once existed as to the effects of monetary policy on the economy. A consensus has developed that monetary policy is vitally important to economic performance, and the estimates of the money multipliers seem to be converging. Our understanding of the paths of transmission has increased greatly, and here too, people from opposing camps find they have more in common than they thought. Professors Tobin and Friedman speak much the same language when they are talking about the processes of monetary policy. And the Board's staff, working together with Professors Ando and Modigliani, has developed a model in which the wealth effects of monetary policy, working through the markets for equities, bear directly on consumer spending in a way that would warm even Milton Friedman's heart. This is a far cry from the simple-minded Keynesianism of the 1930's and early 1940's or the equally naive quantity theories expounded at that time.

Lags, of course, there are, but they are not hopelessly long. I understand Professor Friedman's current view is that the average lag is something like six months between changes in the growth rate of money and changes in the growth rate of GNP. Our own empirical work at the Board suggests the average lag may be slightly longer, but we, too, find that significant economic effects can be obtained within the space of half a year by manipulating the

instruments of monetary policy. We are making progress, also, in understanding why the lags are variable, and how to estimate the lengths of lags in economic systems in which this variation occurs.

Above all, we are learning how immensely complex the economic and financial world really is. Money, however we define it, is not unique, in any meaningful sense of that word. Demand deposits substitute for CD's, for other classes of commercial bank time and savings accounts, for claims on nonbank intermediaries, and for market securities.

This does not mean, of course, that the central bank can ignore the money stock and concentrate on (say) interest rates. The behavior of the money stock contains useful information for measuring and interpreting monetary policy, more information, I think we should acknowledge, than most economists other than the monetarists have recognized. Reducing the growth rate of bank demand deposits, and hence the narrowly defined money stock, does reduce the growth rate of GNP. But so also does a reduction in the growth rate of commercial bank time deposits, or a decline in the growth rate of savings and loan shares or mutual savings bank deposits. In fact, there is no reason in theory for regarding a dollar change in the growth rate of claims against nonbank intermediaries as any less significant, in terms of its effects on GNP, than a dollar change in M_2 or in M_1 . We ignore fluctuations in commercial bank time deposits or in claims against nonbank intermediaries at our peril in a world in which all sectors of the financial market are becoming more closely related, and in which the processes of monetary policy are increasingly extending beyond the boundaries of the narrowly-defined money stock.

Surely, Professor Friedman would not deny, in principle, that we ought to try to take into account these more complex aspects of the effects of central bank policies on economic activity in the formulation of monetary policy. What is needed is an analytic framework, a conceptual apparatus, to do this more systematically and with greater success than we have been able to in the past. That is precisely the goal of our research effort at the Board, and I am fully convinced that these efforts are paying off, in the sense that we have been already, are now, and will be in the future, getting informational inputs that are useful for improving monetary policy decisions.

We occasionally hear remarks that belittle the usefulness of large econometric models such as ours, on the grounds that such models are unstable, not robust, poor predictors, and so on. If, by those comments, it is meant that the art of building large mathematical models is still undeveloped and needs improvement, I fully agree. But if it means that such models are in a substantive sense inferior to the one-equation models produced by Professor Friedman or by Andersen and Jordan, I disagree wholeheartedly.

Finally, let me note that models of monetary policy variables and their effects on the economy, whether they be one-equation models

or more complex ones, never can be (and I would argue never should be) push-button devices that provide automatic, unqualified answers to policy questions--answers that human judgment cannot then refine further, or discard altogether if it seems appropriate. We send spaceships to the moon with human lives aboard mainly to permit on-the-spot reaction to developments that cannot always be anticipated and allowed for in advance. Changes in plans made in such a context must, obviously, take into account what we know, as well as what we don't know. Spacemen are not allowed to play God in the decision-making process, and central bankers should not have such freedom either. But reducing them to sub-humans, grinding out a constant growth rate of money, is not justified by logic or by empirical fact.