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STABILIZATION POLICY AS SEEN
BY ECONOMETRIC MODELS

Remarks by

Henry C. Wallich
Member, Board of Governors of the Federal Reserve System

at the Session

Economic Fluctuations and Stabilization Policy, 1965-75:
Some Econometric Evidence

at the Annual Meeting of the
American Economic Association

in

Dallas, Texas

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It is a noteworthy finding of the model simulations presented in the main papers before us ^{1/} that even with the best of policies it would not have been possible to prevent the 1974-75 inflation and recession. I consider this result a little disappointing, even though it seems to exonerate fiscal and monetary policymakers from any charge of having brought on these unhappy events by pursuing altogether wrong policies. I must also confess to some skepticism. Surely it is always possible to do better than one did, especially in the light of hindsight,

^{1/} I did not receive the paper by Keith Johnson in time to take adequate account of it.

Note: The views expressed herein are my own and do not necessarily reflect those of the the Board of Governors or the Board's staff.

from which these model simulations have benefited. I should be sorry to have to conclude that on the whole there was not much that could have been learned from this episode.

These models do agree among themselves on a common view that policies in 1974 should have been more expansionary. However, with respect to monetary policy, on which I shall focus here, only the St. Louis model placed the optimal rate of growth for M_1 above 15 per cent during 1974, with two others briefly in the 10-13 per cent range, while the optimal path traced by the Wharton Quarterly model resulted in a growth of M_1 at less than the rate that actually prevailed for the six final quarters. I shall examine particularly those features of the tests that seem to have contributed to the findings of a need for relatively high rates of money growth. It should be remembered that these policy configurations, as I read them, are based on perfect foresight, taking into account and preparing for shocks that had not even occurred.

One possible reason is that most of the simulations terminate in the first quarter of 1975. Subsequent developments are ignored. It is a well known characteristic of many of these models that the effect of monetary policy as well as fiscal policy on prices occurs with a much longer lag than that on output and employment. Hence, the ultimate effect of high monetary growth on the rate of inflation may well have been cut off, even if the model were to reduce that growth relatively early. I know that model operators do not like long projections because some models tend to become unstable over time.

But it should be clear that, by not revealing more remote forecasts, an erroneous impression about the consequences of high monetary growth could be generated.

In the second place, it seems to be the implication of at least some of the models that a large part of the inflation of 1974 was the result of rising oil, farm, and imported materials prices, and that in effect a "one-shot" adjustment in the money supply, along with fiscal expansion, should have been made to accommodate it. Without denying the importance of these exogenous factors, my own impression of the events of 1974 was that the lifting of price controls played a considerable role. As we know from several studies, prices had been held down more than costs. Lifting of the controls brought a widespread movement to restore profit margins. This seems to be indicated by the rapid upward movement of the industrial component of the Wholesale Price Index, and by the finding of Ando and Palash, presented at an earlier session, that in the recession of 1974 markups did not shrink as they did in earlier recessions.

Had monetary (and fiscal) policy been very accommodative at that time, this might have seemed to validate the effort to raise markups and might have encouraged additional and larger increases. A monetary policy tending to restrain these forces seemed appropriate to me at the time.

Third, the notion implicit in some of the simulations of a one-shot increase in the money supply as a means of accommodating higher oil, food, and raw materials prices strikes me as questionable

for still another reason. It presupposes that people will view such a price rise as indeed a nonrecurrent event. People are expected to accept the resulting reduction in their living standard because the economy as a whole can do little to reverse it. But to most people a reduction in individual standards of living is something that they can try to fight off, by means of higher wage and salary demands. Moreover, they may not see the externally imposed rise in prices as nonrecurring, but may perceive it as simply an acceleration of the rate of inflation.

Given such public attitudes and expectations, any temporary acceleration of inflation is likely to become built into wages and thus to become permanent. Estimates of the impact of higher prices upon wages seem to run in the range of 20-70 per cent. Given expectations that are already inflationary, I would guess that such estimates, based in part on data of the past, no longer are much of a guide. We may have to contemplate the possibility of price increases being translated into wage increases on something close to a 1:1 basis. An expansive monetary policy under such circumstances would seem to validate higher wage increases.

Fourth, I doubt that our present models are very reliable in their formulation of price expectations. They assume that people base their expectations on past experience, taking into account a fairly long interval, and that a distributed lag approach therefore is the best way of approximating these expectations. This, in my

judgment, can lead to serious underestimates of inflationary expectations under today's conditions. People believe that inflation is the result of government action. Rational expectations therefore will lead them to watch what the government is doing, rather than what happened in the past when more stable conditions may have prevailed. This analysis, of course, tends to cast doubt on almost everything our models have to say about inflation, and I believe that it is generally admitted that models have grossly underestimated inflation in recent years. Policy "advice" from a model suffering from such a bias must be treated very circumspectly.

Of course, the models that called for high rates of monetary growth during 1974 wanted this policy to be brought to an end. It seems to me, however, that it is easy to underestimate the difficulty of cutting off a high rate of monetary growth. A model cannot be expected to foresee the political pressures that might arise -- that is not what it is programmed for. But to move from a 15 per cent to, say, a 5 per cent M_1 growth rate also generates pressures in financial markets. Computers do not crunch, but the market might.

A model, moreover, can proceed on the implicit assumption that a high rate of money growth will not go on indefinitely. The public, however, does not know what is ahead for monetary policy, unless it were informed at the beginning or during the execution of such a policy that the monetary growth rate would be reduced at some particular time. In that case, however, a problem of credibility would arise, and

one would have to anticipate that large parts of the public would expect high rates of money growth to continue. Inflationary expectations would mount. Conceivably they might be translated into rising prices and rising long-term interest rates much more quickly than the models might indicate. As I noted before, the models are not well designed to capture that kind of effect.

In short, the arguments for fully accommodating a severe price shock seem deficient to me. Some accommodation there probably must be, but the question of how much is a difficult one, and certainly cannot be answered on the basis of the papers before us. To summarize my impression of them, I applaud the effort but I question some of the results. In times of only moderate instability, such as the years 1970-73, the implicit policy advice rendered by different models often conflicts even as to the direction of policy, let alone the magnitude of the actions proposed. During periods when there is agreement on direction, such as in 1974, there are important differences in the magnitude of the actions proposed. The economic structure implicit in some of the models seems to be significantly at variance with my own perception of the way the economy works. None of this is to say that we could not learn from recent experience and that policy could not be improved in the light of that experience. Exercises such as the present comparison of policy implications of different models are useful even if they do no more than to draw attention to the difficulties of interpreting the past. Needless to say, I hope that

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exercises of this sort will do more and will help point the way
toward future improvement of policy.

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