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Reflections on U.S. Macroeconomic Policy

IN LIGHT OF EVENTS of the past several years, I find that the only advice I can give about monetary and fiscal policy is to stabilize the rate of growth of money in the neighborhood of 5 to 6 percent and to balance the high employment federal budget for the foreseeable future. Underlying this position is a very simple observation: although macroeconomics has developed apace in the postwar period, its application to policy has brought no steady improvement in the performance of the U.S. economy. While the postwar period as a whole looks much better than the years between the wars, the lack of clear improvement from 1946 to 1974 is disturbing. The case for continuing an activist stabilization policy requires a conviction that such a policy has a brighter future than is suggested by the record.

In reviewing possible explanations for the postwar experience, I am unable to convince myself that an activist policy will be superior to a "steady-as-she-goes" policy. Most of this report is devoted to a discussion of possible explanations of the postwar stabilization record. At the end, these arguments are applied to the situation now facing policy makers. In particular, I attempt to explain why my policy views are so different from those of James Tobin.

Note: The views expressed are those of the author and do not necessarily reflect those of the Federal Reserve Bank of Boston.

Explanations of the Postwar Stabilization Record

Three explanations have been offered for the mediocre record of stabilization policy: (1) the economy has become increasingly difficult to manage; (2) macroeconomic knowledge has not increased nearly as much over the postwar period as the explosive growth in the journal literature might suggest; (3) avoidable errors of stabilization policy have been frequent. These explanations will be considered in turn.

THE UNRULY ECONOMY

Under the view that the economy has become increasingly difficult to manage, policy has prevented economic performance from deteriorating even though it has not been successful in bringing about a definite improvement. I do not find this argument persuasive. Although substantial disturbances have occurred in the past decade—assassinations, conflicts over desegregation, the Vietnam War, two Mideast wars, Watergate, the Arab oil embargo, the flight of Peruvian anchovies, and so on—they have been no worse than the disturbances in the earlier postwar years. The end of World War II required an enormous reallocation of resources from wartime to peacetime uses. The Berlin crisis in 1949, the Korean War, the 1956 Mideast war that closed the Suez Canal and disrupted petroleum supplies—all threatened economic stability.

Even if recent and earlier disturbances were equally severe, one might argue that the current structure of the economy magnifies the effects of the disturbances that do occur and that, in particular, the economy now exhibits an inflationary bias that did not afflict it before.

Again, I find this argument unconvincing. There were earlier instances in which the rate of inflation was slow to decline in the face of excess capacity. In the most infamous example, wages and prices stopped falling after 1933, in spite of continuous high unemployment. In the late fifties and early sixties, the inflation rate—which had risen to about 4 percent in 1956—decelerated only slowly before stabilizing at a little over 1 percent in 1962–63, in spite of two recessions and an unemployment rate continuously above 5 percent.[1] The behavior of the inflation rate after 1969 is quite consistent with these earlier instances. By 1969 inflationary expectations

1. In the two years preceding the business cycle peak in the third quarter of 1957, the GNP deflator rose at a 3.7 percent annual rate; in the two years following the peak,

were entrenched, and the 1969–70 recession was mild compared with those of 1957–58 and 1960–61.[2]

The economy has been more inflationary in recent years, but not necessarily more inflation-prone. The problem is to explain wage and price inflation; an appeal to rising wages and prices as evidence of greater susceptibility to inflation is circular reasoning. Excluding statistics on prices and the money stock, economic data do not indicate that since, say, 1965, the system has changed in such a way as to become more susceptible to inflation. Industrial concentration is no greater than before, and imports have enhanced competitiveness in some industries. Nor do the data suggest that union power has become any greater.

As a scientific matter, one must accept the hypothesis that the nation could reduce the inflation rate nearly to zero if it were willing to accept the late 1950s' medicine of several recessions. Sadly, we have no evidence that any prescription other than recession, possibly protracted, will be efficacious.

IMPERFECT KNOWLEDGE

I have considerable sympathy with the second view—that macroeconomic knowledge has not increased as much as many might think. For one thing, some of the advance involved unlearning things that weren't true—in particular, the extreme Keynesian view that "money doesn't matter." Currently accepted views on money are, I think, much closer to those of Irving Fisher than to those of Alvin Hansen. More important, the improvement in empirical knowledge has not been sufficient to permit the theory to be of much help in devising activist policies.[3]

An activist policy requires considerable empirical knowledge, first, about the magnitude and timing of the effects of policy instruments; and second,

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it rose at a 2.0 percent rate; and in the next two—1959:3–1961:3—at a 1.3 percent rate. Although economists may differ as to whether the deceleration of inflation after 1957 should be regarded as "slow," political leaders are certain to regard a deceleration involving half, or more, of a presidential term as "slow," especially when it is characterized by distressingly high unemployment.

2. Unemployment peaked at 7.5 percent in July 1958, and at 7.1 percent in May 1961; the 1970 peak (in December) was 6.2 percent. If allowance were made for changes in the structure of the labor force between the late 1950s and 1970s, the differences between the 1970 peak and the earlier peaks would be even greater.

3. Indeed, I would argue that advances in the theory of policy over the postwar period have substantially weakened the case for activist policies. The literature on control

about the state of the economy over much of the period during which the changes in policy instruments will have their impact.

As a theoretical matter, uncertainties about the effects of policy and about economic forecasts argue not for the abandonment of activist policy, but rather for policy settings closer to historical averages than would otherwise be appropriate.[4] However, activist policy should be further tempered to avert the possible problem of "instrument instability."[5] This problem arises because economic forecasts typically apply over a period far shorter than that affected by policy changes. Depending on the nature of the distributed lag response of the economy to policy changes, it may not be possible to use policy in one period to offset the effects of policy in the previous period. Knowing whether instrument instability is in fact relevant requires substantial knowledge about the forecasts and about distributed lags; in my view, this kind of precision simply is not available in an economy whose unemployment rate is in the neighborhood of, say, 4 to 6 percent.

The empirical magnitudes of key economic parameters are still in considerable dispute. To take one example, in the recent study by Goldfeld that Tobin cites, the income elasticity of the demand for money was estimated to be 0.68,[6] well below the estimate of 1.0 generally accepted a few years ago. While Goldfeld was extremely careful and resourceful, his findings must be replicated by other investigators on other bodies of data before they can be confidently accepted. The few cases in which earlier studies have been redone give good reason for caution: equations typically have performed poorly in predicting events after the publication of the original studies.[7]

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theory demonstrates that stabilization of a complex dynamic system may require counterintuitive policies and that intuitive policies may be quite destabilizing. Moreover, relatively small differences in parameter values may determine whether or not a particular policy is stabilizing.

4. See William Brainard, "Uncertainty and the Effectiveness of Policy," in American Economic Association, *Papers and Proceedings of the Seventy-ninth Annual Meeting, 1966* (*American Economic Review*, Vol. 57, May 1967), pp. 411–25.

5. See Robert S. Holbrook, "Optimal Economic Policy and the Problem of Instrument Instability," *American Economic Review*, Vol. 62 (March 1972), pp. 57–65.

6. Stephen M. Goldfeld, "The Demand for Money Revisited," *Brookings Papers on Economic Activity* (3:1973), pp. 577–638. Hereafter, this document will be referred to as *BPEA*, followed by the date.

7. For two examples, see Charles R. Nelson, "The Prediction Performance of the FRB-MIT-Penn Model of the U.S. Economy," *American Economic Review*, Vol. 62

A good example of the dangers of trying to do too much at the current state of knowledge is the paper I wrote in the summer of 1970.[8] There I argued—with the standard caveats in both the notes and the text—that as of mid-1970 the economy was in a recession, characterized by a high, inherited, rate of inflation. The goal of policy was to reduce the inflation rate over a period of years while minimizing the cost in terms of unemployment.

Several policy options were illustrated through simulations of the Federal Reserve-MIT-Penn econometric model of the United States. The basic feature of these simulations was that as inflation, and inflationary expectations, declined, nominal interest rates would have to be pushed down by temporarily rapid growth in the money supply. Unless the nominal rate of interest fell, the real rate would rise, choking off investment and causing a recession.[9]

The argument seemed sensible to me in 1970, and seems sensible to me now. But looking back, I find that, for the ensuing period as a whole, the policy advice stemming from it was wrong. Macro policy has not been dramatically different from the stance assumed in my 1970 simulations, and yet the performance of the economy has been dramatically different.[10] What went wrong?

Perhaps the argument was basically correct but the time frame was wrong. If the lags in the effects of policy actions are longer than those incorporated in the FMP model, the model may have understated the

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(December 1972), pp. 902–17; William Poole and Elinda B. F. Kornblith, "The Friedman-Meiselman CMC Paper: New Evidence on an Old Controversy," *American Economic Review*, Vol. 63 (December 1973), pp. 908–17. Consumption studies may be a major exception to the general record of poor post-sample predictive performance. See Arthur M. Okun, "The Personal Tax Surcharge and Consumer Demand, 1968–70," *BPEA* (1:1971), pp. 167–204. Even in this study, however, consumer spending on durables substantially exceeded levels predicted on the basis of earlier studies.

8. "Gradualism: A Mid-Course View," *BPEA* (2:1970), pp. 271–95.

9. This argument would be consistent with any model containing a demand-for-money function with unitary income elasticity and a negative interest elasticity. If the income elasticity is below unity, the same basic argument holds except that a less rapid rate of money growth is required.

10. "Simulation B" in the paper had a money growth rate of 5.8 percent, continuously compounded, from 1968:4 to 1972:4, and of 9.5 percent from 1972:4 through 1975:4. The actual rate of money growth was 6.0 percent for 1968:4–1972:4 and 6.1 percent for 1972:4–1973:4. The simulation also assumed a moderately restrictive fiscal policy, with the full employment surplus rising to a little over 1 percent of GNP in 1973–75. On a calendar-year basis, the full employment budget surplus in fact has been within 0.5 percent, plus or minus, of GNP over 1971–73.

strength left over from the expansionary policies of 1965–68. Furthermore, the inflation rate over the past year may have been fed by price catch-up once controls were removed, and by fears of further controls.

Unfortunately, explanations involving the FMP model are now difficult to verify. The current version (now called the MPS model) is substantially different from the 1970 version. It would, I believe, be a very substantial undertaking to reconstruct and run the 1970 version in an attempt to identify the equations that went off track.

There is a further reason to doubt that the current body of knowledge is adequate to the demands of countercyclical stabilization policy. In a recent paper, Robert Lucas showed that the structure of the economy is, in principle, dependent on the nature of the policies followed.[11] Thus government stabilization policies may fail because they themselves will change the structure of the economy. Policy makers cannot manipulate taxes, interest rates, and other variables under the assumption that the economy will respond as it did in the past. Past responses to tax changes, for example, may have been conditioned by the view that they were required to raise (or lower) revenues. The same responses cannot be expected if tax changes become viewed as temporary and reversible stabilization tools. While there is considerable dispute about the empirical importance of this argument, it must be regarded as a significant warning. At a minimum, government decision makers must be aware that private decision makers may be basing their actions in part on forecasts of future policies and not merely on current levels of tax rates, money growth, and the like.

To summarize, technical economic analysis makes a case for continuous adjustments in policy instruments, with the adjustments being larger the larger the prospective deviation of the economy from its desired track. If I had complete control over policy, I would indeed move the levers to some extent from time to time, although by less than would policy activists. My relative inactivity would rest not on principle but rather on my skepticism about the efficacy of activist policies for improving economic performance.

AVOIDABLE ERRORS

The third and strongest argument for maintaining an activist policy in face of its apparent failures in the past is that the policy mistakes of recent

11. "Econometric Policy Evaluation: A Critique" (forthcoming).

years were not inevitable. Two types of mistakes must be distinguished: those stemming from faulty economic analysis—the issue just discussed—and those stemming from the failure of economists' prescriptions to gain political acceptance.

The issue of the political acceptance of stabilization measures is much the tougher of the two. The debate has centered on two questions. First, should a democracy rely on the discretion of the authorities or on rules? Second, if the nation is not to rely on rules, should economic policy be made by Congress, by the President, or by an independent agency?

Arthur Okun has recently summarized this debate so clearly that I can do no better than refer the reader to his discussion.[12] The flavor of his argument is that, whatever the failures of postwar stabilization policy, policy rules cannot necessarily be expected to help. The political pressures that have contributed to past mistakes in discretionary policy can also be expected to bend any rules that might be laid down. Moreover, it is surely better for economists to devote their energies to improving the political procedures underlying discretionary policy than to work toward acceptance of suboptimal policy rules in an attempt to save the nation from irrational behavior.

Okun's paper, and previous discussions of the same issues, concentrates on the question of how economic policy *ought* to be made. Advocates of discretionary policy use an analytical framework in which economic policy instruments are adjusted to optimize an objective function, given the constraint imposed by the structure of the economy. Arguing on a normative level, they believe that such an analytical policy model ought to be used, while advocates of rules have tended to argue that this model is inappropriate in a democratic society. To push this debate further, I believe that a different question should be examined. How are governmental decisions in fact made in democratic societies? Apart from the normative issue, there is a scientific issue involved that requires that the constraints imposed by the structure of the democratic political process be added to the analytical model. In the area of economic policy, future progress may depend more on advances in "positive politics" than on advances in "positive economics."

Several observations about positive politics—which are personal, undocumented, and perhaps wrong—may help to suggest the kinds of political constraints I have in mind. Successful democratic societies are charac-

12. Arthur M. Okun, "Fiscal-Monetary Activism: Some Analytical Issues," *BPEA* (1:1972), pp. 154–57.

terized less by the voting booth than by continuous, open, public debate over governmental policies and by a healthy public skepticism about the wisdom of public officials. Sensible policies that are not sold to the public either are never instituted or are repealed before they have time to become effective. Indeed, a positive-politics fact of life seems to be that legislators are frequently forced to support positions they personally believe to be unsound in order to enhance their prospects for reelection. This observation leads some economists to support policy rules—although some support them for other reasons—and others to support control over policy by the executive branch or by an independent agency.[13]

In a democracy, policy making—whatever its subject—cannot be insulated from the political process. For example, the Federal Reserve is not fundamentally independent, but severely constrained, although somewhat less so than would be the case if its governors served at the pleasure of the President rather than for fixed fourteen-year terms.

The impossibility of insulating public policy from politics is a consequence of enjoying an extremely competitive political and journalistic environment. And the same argument obviously applies to attempts to insulate automatic rules from tinkering. Neither legislated rules nor independent agencies that do not command continuing public support will survive.

It should be emphasized that the discussion is meant to be within the vein of positive analysis. I am *not* talking about how the chairman of the Council of Economic Advisers might maximize the number of votes for his party at the next election. What I *am* talking about are the constraints within which the nonpartisan ivory-tower economist ought to frame his policy recommendations.

If an economic policy is to be politically viable in the long run, I believe it must be not only economically sensible but also understandable to the public. In a successful democracy, good decisions are made when *both* the government *and* the public are well informed on the issues. Thus a policy that is merely "satisficing" given the constraint imposed by the structure

13. The argument that politics produces bad policy is not confined to economists. I suspect that every professional group feels it could carry out policies that would enlarge the national welfare if only the proper mechanism, free of "excessive" political control, were established. But, looking at this argument from a positive, as opposed to a normative, point of view, I see no evidence that government policy decisions are better in areas in which legislative oversight is minimal.

of the economy may be optimal given the combined economic and political constraints.

Policy must be understandable because in view of the frequency with which authorities have erred, or have even deliberately misled, people are justifiably wary of those who insist that the issues are too complicated to understand. At a minimum, people will want to hear the views of a number of experts; but even this approach will be of little value on controversial issues, since respected experts will be found on both sides. As a result of the questioning nature of the political process, policies dependent for their success on timely adjustments in response to events will remain unreliable. For example, political arguments (as well as professional uncertainties) about the distribution of tax burdens have distorted the timing of changes in the investment tax credit since it was introduced in 1962 so as to make them almost exactly procyclical.

An economic policy adviser confronts problems analogous to those of an investment adviser. In managing his personal portfolio the investment adviser can follow his own analysis to the full extent of his confidence in it. But managing someone else's money requires a different approach. The advice must be consistent with the client's investment objectives, his attitudes toward risk, and his willingness to act quickly when necessary and not to act at all while riding out a string of disappointments. Moreover, the adviser must retain the client's confidence that his money is not being used for someone else's benefit. Thus the adviser needs not only technical competence but the ability to explain the investment problem so that the client does not expect more than can be delivered.

In this context, what can I say about my "gradualism" paper? Could I expect to explain its argument to intelligent laymen? If the economy failed to perform as promised, could I explain, a year later, that policy making necessarily involves uncertainties, that the policy followed really was optimal ex ante in the context of a stochastic model, and that I couldn't be blamed for the unlucky roll of the dice? Could I fault the layman who suspected that I was merely rationalizing my poor performance or that my policy adjustments were really designed to help some special interest group?

Even if the policy adviser has faith in his technical knowledge to make continuous small adjustments in policy instruments, he must remember that he cannot operate the policy levers directly and that others operate them, too. Moreover, the public and Congress will, I believe, inevitably expect much greater benefits from continuous policy adjustments in the

neighborhood of full employment than economists can in fact deliver; the resulting disillusionment erodes public confidence in the ability of economists to solve *any* problems.

These constraints imposed by the workings of democratic societies can be ignored no more than those imposed by the structure of the economy. Along with the lack of economic knowledge, they are sufficient to convert an activist in principle into an advocate of neutral settings of policy instruments in a wide range of situations. If economists are to inspire public confidence that they will not play politics with their recommendations, they must construct policy guidelines that appear to be practicable most of the time.[14] Discretionary policy can then be viewed as specifying departures from the general guidelines. Debates over such discretionary departures will inevitably—and appropriately—take place within the context of partisan political situations.

If the nation is to have such guidelines, then it seems to me that they must command widespread support within the economics profession. In the view of practically all economists, guidelines defined by stable money growth and a balanced full employment federal budget would be superior to those involving stable interest rates or a budget in actual balance. The specifications of any policy guidelines should be extensively debated in terms of their general, long-run, suitability. Once a set has been accepted, however, debate over current policy should concentrate on whether there is good and sufficient reason to depart from them.

Policy in 1974

What do these arguments mean for the U.S. economy in mid-1974? In my view, the political and economic constraints leave very little freedom for policy action. There is not a strong case for departing from the guidelines of stable monetary growth and a balanced full employment budget. A basically prosperous economy suffering from the disruptions of inflation and wage-price controls should be protected from further disturbing changes in monetary and fiscal policy and given a chance to settle down.

14. The word "guideline" rather than "rule" is used here in order to leave open the question of whether a rule should be embodied in legislation. The argument is analogous to the one that the existence of a constitution is much more important than whether the constitution is in written or unwritten form.

In mid-1974, "stable" monetary policy involves a rate of growth of money (M_1) of 5 to 6 percent. Between 1966:4 and 1973:4 the rate of growth of M_1 has fluctuated around a growth trend of 6.2 percent. At the same time, a stable fiscal policy defined in terms of an approximately balanced full employment budget requires a cut in federal personal income tax rates, or an adjustment of the income brackets to which current tax rates apply, because the current inflation is swelling real tax revenues. In the long run, less expansionary policy guidelines—especially for monetary policy—will be required if the inflation rate is to be reduced to near zero; but now the policy problem is to stabilize the economy around the current trend rate of inflation of about 5 percent.

I differ with James Tobin on two counts. First, I cannot accept the economics of his argument. Second, even if his argument is correct, I do not believe the general public will accept it long enough to allow his policy to be successful.

As I see it, two aspects of Tobin's economic argument are especially questionable. First, he argues that recovery from the recession, or mini-recession, of early 1974 requires falling interest rates—indeed, a drop in short-term rates well below the lows of early 1974. If so, it would represent a rare exception to the general pattern of rising rates during business cycle expansions. Moreover, given the behavior of both financial and product markets, the decline in real gross national product in the first quarter of 1974 looks less like a typical recession than a temporary supply disturbance (largely due to petroleum shortages), analogous to the impact of a prolonged strike in a major industry.[15]

Second, and more important, there is much that economists do not understand about the real effects of inflation, and these areas of ignorance are central to the interpretation of Tobin's "Q" variable. There is, I believe, ample evidence that inflations generally alter real magnitudes from what they would be in a noninflationary environment. If there are such things as "steady-state" inflations in which real magnitudes are essentially unaffected by the inflation, they would be established only very slowly. Extreme examples of the real effects of inflation are apparent in conditions of hyper-inflation. Frank Graham's book on the German inflation of the early 1920s contains a large number of such examples. Graham reports

15. For example, in the third quarter of 1959 real GNP declined at a 4.1 percent annual rate due to a lengthy steel strike.

that an index of real (that is, deflated) stock prices, with a base of 1913 = 100, reached a postwar peak of 27.4 in November 1921, and then fell dramatically to 3.6 in October 1922, finally reaching 23.7 in October 1923, about the time of the currency reform.[16] German stock prices may have behaved that way partly because corporations found it impossible to maintain real dividends owing to the loss of purchasing power while dividend checks were being distributed. In another example, Graham reports that the index of real housing rentals fell from 15.5 in May 1921 to 0.5 in August 1923, as rent restrictions and tenant delinquencies wiped out the owners of urban real estate.[17] Few of these effects were well understood at the time.

Similar effects may now be operating to depress the U.S. stock market. The real tax burden on corporate income rises with the inflation rate. Wage and price controls apparently affected corporate profits, and though they have ended, their resumption is not improbable. Public attitudes toward profits are not favorable, as evidenced by the recent reaction to oil company earnings.

These factors can be expected to alter investment expenditures, but they are unlikely to encourage corporations and individuals to delay purchases of real goods. The earnings prospects from real goods may be low, but they are not clearly lower than the prospects from financial claims. Furthermore, although financial assets are normally less risky than real assets, long-term bonds are now among the riskiest of investments. On this interpretation, recent weakness in bond and stock prices does not foretell the end of a spending boom; on the contrary, it may reflect attempts to exchange financial assets for real assets.

Tobin's interpretation of the weakness in financial markets may, of course, be correct. But if mine is correct, accelerated growth in the money stock would help to generate accelerated inflation. The economy now is operating close to full capacity, especially in materials-producing industries, and has very little room to absorb risks on the up side. A further acceleration of inflation may jeopardize the solvency of the thrift institutions, lead to a new set of price controls far more rigid than most pro-controls economists would favor, and generate other horrors.

Tobin's recommendations are, it seems to me, unresponsive to the

16. Frank D. Graham, *Exchange, Prices, and Production in Hyper-Inflation: Germany, 1920–1923* (Princeton University Press, 1930), pp. 178–79.

17. Ibid.

public's preferences. According to my reading, the policy that reflects these preferences entails acceptance of a greater risk of recession to curtail inflation than would have been true ten years ago. Indeed, I believe that these preferences will force public officials to do something about inflation. Whether or not these preferences are entirely rational, I doubt that they will yield to economists' arguments in the near future. I believe that my position on policy ought to reflect my best professional judgment as to how these preferences can be satisfied at minimum expected cost.

Neither Tobin nor anyone else can offer a set of proposals involving controls and structural change that, based on actual experience in this or other countries, promises to reduce inflation while maintaining essentially full employment. If I could find such a set of proposals, I would join in vigorous efforts to persuade the public to tolerate inflation long enough for the reforms to take hold. But given the lack of a documented case that structural reforms can do the job, and given the recent demonstration that controls are ineffective, I find it hard to believe that the public will quietly accept continuing inflation while waiting for a Tobin-type program to work.

If economists simply argue for the acceptance of inflation, the various "somethings" that are in fact done are unlikely to reflect professional thinking about the least-cost method of reaching the public's goal of slowing inflation. The policy of least expected cost, in my opinion, is the "steady-as-she-goes policy" of 5 to 6 percent money growth and a balanced full employment budget. My best technical judgment is that a much tighter policy runs a real risk of a substantial recession while a much easier policy runs a real risk of acceleration of inflation. As I view public preferences, both of these extreme outcomes must be avoided. The only way I know to do so is to steer a middle course, risking a middling outcome.

Since the rate of inflation is now about 10 percent even though the rate of growth of the money stock has approximated my recommendation of 5 to 6 percent, does my recommendation make sense? First, it should be noted that the GNP deflator rose at an average annual rate of 4.9 percent from 1968:4 to 1973:4—not out of line with the growth of money (6.0 percent) and of real GNP (3.5 percent) over the same period. The acceleration in the rate of inflation in the past year may in part reflect fiscal stimulus in 1972 and the recent acceleration of money growth—M_1 growth was at a 6.9 percent rate from 1971:4 to 1973:4, up from a 5.4 percent rate from 1968:4 to 1971:4. For the most part, however, it probably reflects a com-

bination of special factors, including a catch-up as controls broke down. Taking the controls period as a whole, the inflation rate is not surprising; between 1971:3 and 1974:1 the GNP deflator rose at an annual rate of 5.4 percent.

Basically, then, I feel that the recent rate of inflation—8.1 percent annual rate on the GNP deflator between 1972:4 and 1974:1—embodies a trend component of about 5 percent and a temporary component of about 3 percent. The need now is to avoid overreaction to the temporary component—either by accelerating money growth to validate the higher inflation or by clamping down on money growth in an attempt to eliminate inflation quickly. A stable policy cannot be sold as a guarantor of a happy outcome, but then no economist has a scientific basis for promising very much in the present circumstances. Under a stable policy, unemployment could rise to well over 6 percent, but the probabilities of such a rise are not so great as to require more expansionary policies now. Given the risks of acceleration of inflation, and given the political problems of promptly reversing more expansionary policies should they prove inappropriate, policy instruments should remain at neutral settings until there is a clear and present danger of a substantial recession. A year or two from now, the United States could do much worse than emerge from the current situation with a mild recession fading away and an inflation rate stabilized at 5 to 6 percent. In view of public preferences on both inflation and unemployment, I have little confidence that any other policies have a genuine prospect of doing much better.

Discussion

ROBERT J. GORDON began the discussion by noting the wide divergence between Tobin's and Poole's predictions of the path of the economy, given a steady 5 to 6 percent annual growth in the money supply. While Poole saw the possibility of decelerating inflation and recovery from a mild recession in a year or so, Tobin, even in his more optimistic view, had unemployment increasing until 1978. Gordon found that his own calculations tended to support Tobin's results. Because increases in wage rates could not be expected to ease markedly for a long time, the resulting rate